Graphical: The history of a category

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Abstract

This dissertation is a cultural and conceptual history of the rise and development of graphical methods in the 19th century physical sciences. It delineates the *graphical* as a particular mode of seeing, manipulating and reasoning about physical phenomena that should be distinguished from the geometrical approaches of the 18th century. The dissertation goes beyond previous studies that have only described the use of graphical methods as a form of discipline or practice. Instead, it reconstructs a "period eye" to explain how historical actors formed patterns and associations by looking at, thinking about or manipulating graphical objects. Through this approach it is shown that the impact of graphical methods went beyond their practical use. The *graphical* provided a rhetorical and intellectual mean of mediating between different social and epistemological constraints such as the workshop and the classroom; abstract theory, empirical rules and experimental data; algebraic equations and geometrical constructions; or conflicting notions of generality and precision.

By tracing the use of "graphical" either as a qualifying adjective or a suffix, it is shown how in the late-18th and early-19th century certain operations, constructions, methods, representations or instruments came to be understood, described and organized by this category. By following the genealogy of paradigmatic diagrams (such as the phase diagrams in thermodynamics and the curves of magnetization in electrical engineering) or the pedagogical role of squared paper, it is shown how graphical objects came to be valued not just as practical paper tools but as epistemological objects. These two approaches provide the basis for an interpretation of how the historical category of the *graphical* and graphical objects were perceived and valued in the 19th century.

Contents

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· 1 ·

Introduction

This study establishes the *graphical* as a *historical category* and mode of seeing, manipulating and reasoning about physical phenomena. While there have been numerous historical studies dedicated to graphical methods and representations, few have treated consistently and coherently the *graphical* as a historical object of study. The contemporary interest in graphical objects–diagrams, methods, instruments, practices etc.–has provided historians with an analytical filter for selecting and ordering their historical material. Though historical methods have proved successful in describing the use or meaning of *particular* historical objects as part of *particular* historical contexts (i.e. *this* diagram or method used by *this* person at *this* location at *this* moment in time for *this* purpose etc.), they have provided few approaches for describing *general* historical objects. Unfortunately, a common uncritical solution has been that of "gluing" together the particular objects to make claims about the general objects.

What would it mean and what would one gain from studying the *graphical* as a general historical object instead of looking at particular graphical objects? The challenge behind this question is to understand how one can have access to such an object, the *graphical*, and what kind of object this is. At a fundamental level, historical studies have defined their object of study through an operation of *identity* and *continuity*. Thus, one can study a place, a person, a physical object, or an idea as long as one can posit that the object stays the same while changing! This apparent contradiction is solved once we conceptualize the object as defined by multiple variables which do not change all at once.[1] However, after a given time all the

1. Thomas Kuhn's paradigm shifts, sudden and discontinuous, have been explained by Peter Galison as a form of continuous change in a different domain: discontinuous shifts in theory are accomplished by continuous changes in experimental or instrumental practices. Galison's argument has been that though "there is no absolutely continuous basis in observation", one can find intercalated local continuities, see Peter Galison, *Image and Logic : A Material Culture of Microphysics* (Chicago: University of Chicago Press, 1997), esp. 797-803. For

variables which have defined the object at the starting point might have changed; then, are we still left with the same object? I will posit that the answer to this question is historically contingent, and the very purpose of the study is to understand how these operations of identity and continuity play out historically. For local changes to be historically meaningful, there always needs to exist a global object of study.[2] The usual approach of historians has been to define the global object of interest through *analytical categories*, i.e. as trans-historical objects which are conceptually useful and of interest for the contemporary reader. While the shortcomings of such an approach are not always immediately obvious, one is often forced to awkwardly hide the fact that these objects are not *historically coherent*; that is, their *continuity* and *identity* are not historically reconstructed.[3]

This study aims to understand the *graphical* not as an analytical category, but as a historical category. Its central question is not how or why certain graphical objects were used, but rather why graphical objects came to matter as *graphical* objects? Or briefly, why did the *graphical* come to matter?

1 ON HISTORICAL CATEGORIES

> Consider, for example, the activities [Vorgänge] that we call "games". I mean board-games, card-games, ball-games, Olympic games, and so on. What is common to them all?–Don't say: "They *must* have something common, or they would not be called 'games' "–but *look* and *see* whether there is anything common to all. – For if you look at them you will not see something that is common to *all*, but similarities, affinities, and a whole series of them at that. To repeat: don't think, but look! [...]
>
> And the upshot of these considerations is: we see a complicated network of similarities overlap-

his analysis Galison identified three levels of "constantly reinforced continuity": a level of pedagogical continuity, technical continuity, and demonstrative or epistemic continuity, see Galison, *Image and Logic : A Material Culture of Microphysics*, 21-22.

2. One can find it useful to conceptualize this claim in terms of a mathematical surface – though one might only have immediate access to a local description of the surface, in the form of a local coordinate system, there still exists a global object (the surface itself) defined by some global properties.

3. For example, a dictionary entry brings together various meanings under the heading of a particular word. However, the continuity between the various meanings of a word and their identity under a single heading is not historical (unless we consider the dictionary as a primary source).

> ping and criss-crossing: similarities in the large and in the small.[4]

This key passage on *family resemblances* from Wittgenstein's *Philosophical Investigations* has been highly influential in the history of science because it has allowed historians to move away from the attempts of logical positivism of reducing scientific activity to a system of rules. Instead of a logically unified and coherent description of scientific activity, historians have described the production of scientific knowledge as arising from a complicated system of "overlapping and criss-crossing" localized practices.[5]

However, Wittgenstein's central claim – "if you look at them you will not see something that is common to *all*" – is partially incorrect, and depends on his distinction between looking and thinking – "don't think, but look!". Trying to find resemblances between objects by looking and not thinking is nonsensical. Once we think, we have to admit to at least one central and crucial similarity common to all – the very name from which the question started – "Consider for example the proceedings that we call 'games"'.[6] In haste, Wittgenstein has dismissed the common name – "Don't say: 'They *must* have something common, or they would not be called 'games' "' – and has assumed, without sufficient proof, that the "activities [Vorgänge]" must have some overlapping similarities (i.e. continuity). But the very presumption that such similarities exist, and the challenge to find them, originate in the very name that has already brought together all the activities. In the end, Wittgenstein's attempt to explain why such disparate activities can be grouped together under the unifying name of "game" is doomed to fail. First, one can always find other objects that share some overlapping similarities without

4. Ludwig Wittgenstein, *Philosophical Investigations*, Rev. 4th (Chichester, West Sussex, U.K.: Wiley-Blackwell, 2009), §66.

5. Thomas S. Kuhn, *The Structure of Scientific Revolutions*, 3rd (Chicago: University of Chicago Press, 1996), 44-46; Galison, *Image and Logic : A Material Culture of Microphysics*, 765-771; David Kaiser, *Drawing Theories Apart : The Dispersion of Feynman Diagrams in Postwar Physics* (Chicago: University of Chicago Press, 2005), 208-209. Most contemporary accounts of scientific practice have appealed to such a description though under different names: the system might be described as a net, network, matrix etc., while the "overlapping and criss-crossing" could refer to a particular place, a "knot", a "trading zone", a "center of calculation", a "face to face" interaction etc.

6. Besides the very name, all the activities named "game" share something else – they are all supposed to be "activities".

belonging into the same category (who decides the boundary?), or objects that might have no overlapping similarities and which still belong to the category (or which can be added by *fiat*). Second, how can the overlapping similarities be established, and aren't some of these already presupposed by the very grouping that needs to be explained?

Historians influenced by Wittgenstein have used the concept of family resemblances to talk about *shared* features: a common set of ideas, skills or objects that came to be shared through some form of interaction. However, less attention has been paid to the role played by *labeling* in the organization and classification of objects. Nelson Goodman has described how

> [a] label associates together such objects as it applies to, and is associated with the other labels of a kind or kinds. Less directly, it associates its referents with these other labels and with their referents, and so on.[7]

The concept of a label is important because it underlines the fact that objects are brought together not only because of some shared physical properties, or a shared practice of classification, but also through the very names we use to describe the objects.[8] Because labels belong to a language they are more readily intelligible than unorganized physical objects. One needs to acquire more specialized skills to distinguish between a duck and a goose, compared to the skill required in reading the labels describing such animals.[9] I will refer to the objects described by the same label as forming a *category*. Compared to Wittgenstein's concept of family resemblances, the concept of a *category* forces us to consider the ways in which various objects are brought together not only through some form of continuity (such as shared, overlapping and criss-crossing qualities) but also through an act of identification through labeling or naming.

A category, opposed to a concept, has no unified and well-defined meaning. One might be

7. Nelson Goodman, *Languages of Art : An Approach to a Theory of Symbols*, 2d ed. (Indianapolis: Hackett, 1976), 32.

8. Most approaches in the philosophy of language have abstracted the actual role played by the labels and have focused exclusively on the process of classification; e.g.: "P is correctly predicated of an object a in virtue of its objective quality of P-ness", in Mary B. Hesse, *The Structure of Scientific Inference* (Berkeley, University of California Press, 1974), 45.

9. For the original example see Thomas S. Kuhn, *The Essential Tension : Selected Studies in Scientific Tradition and Change* (Chicago: University of Chicago Press, 1977), 293-319.

skeptical that much is gained by focusing on objects with ill-defined meaning (labels, names, words etc.) instead of trying to understand the meaning of the concepts.[10] In the end, any historical explanation ties back to a social and cultural context that connects to concepts and beliefs and not to the arbitrary signs through which these are named. This criticism is addressed in Chapter 3. However, it suffices to say, that by *category* we are not referring to words in isolation of any meaning. A category is important not only because it provides someone with a pre-made grouping, but also because it encourages one to find patterns, analogies or rules through which to motivate the given grouping. Wittgenstein's concept of family resemblance has provided historians with a justification for dismissing the attempts of some philosophers of describing the production of scientific knowledge as a rule-based activity. A scientist was formed not by acquiring a system of rules, but through training, practice, discipline etc. If any rule-like behavior (i.e. some global pattern that could be described in purely logical terms) did emerge, it was to be explained in terms of the interactions and overlaps of the localized practices. Such global patterns were seen as a consequence of *real* and *concrete* everyday practices.[11] Though partially justified, such an approach has ignored the role played by the *perceived* or *assumed* rules: "They *must* have something common, or they would not be called 'games' ". Thus, even if *analytically* we reject any form of unifying definition, *historically* the fact that such a definition is expected–"They *must* have something common"–should not be ignored. Despite the fact that there is no global agreement on meaning, there can exist a global (i.e. non-localized) object: the categories determined by the labels.[12] Michel Bréal, Ferdinand

10. See for example Quentin Skinner's criticism of Raymond Williams' *Keywords*: Quentin Skinner, *Visions of Politics* (Cambridge, U.K. ;New York: Cambridge University Press, 2002), 158-174; Raymond Williams, *Keywords: A Vocabulary of Culture and Society*, Rev Sub edition (New York: Oxford University Press, 1985).

11. "... any global argument about a new phenomenon would have to be built, like the apparatus itself, out of the integrated fragments of these subsystems", or "*In the fixing of meaning, local practices trump global issues of ontology*" in Galison, *Image and Logic : A Material Culture of Microphysics*, 558-559, 708. On the connection of realism to practice see Ian Hacking, *Representing and Intervening: Introductory Topics in the Philosophy of Natural Science* (Cambridge: Cambridge University Press, 1983).

12. Of course, the claim is not that "game" is an absolutely global object that requires no shared set of skills, such as literacy, familiarity with certain fonts etc. However, the claim is that one does not need to understand the meaning of "game" to infer a connection between various objects that carry such a label. A non-English speaker who has no concept associated with "game" could assume a connection between objects which are labeled by it.

Saussure's *maître*, described young children acquiring a language as if they were trying to guess a rule which was not yet formulated; the very supposition of such a rule, led to its formulation and creation.[13] A similar idea was expressed by Max Black in a study on metaphors: "It would be more illuminating in some of these cases to say that the metaphor creates the similarity than to say that it formulates some similarity antecedently existing."[14] Or, going back to the example of classifying ducks and geese, a young observer might not immediately realize that these birds belong to different natural kinds; however, once one is familiarized with the categories (though not with the actual practice of distinguishing or identifying these birds), one might very well suppose that some procedure for distinguishing between the birds must actually exist.

Thinking about categories instead of concepts opens the possibility for a *longue durée* history. *Historical categories*, as used in this study, are to be distinguished from the commonly invoked *actor's categories* (which are contraposed to the *analytical categories* of the historian). Though an actor's categories also refer to historical terms, in this case the historian is interested in the original terms either to preserve or to recover their historical meaning.[15] A historian could refer to an actor's category either to distinguish the contemporary and historical meaning of commonly used terms, or to preserve the original terms for a particular meaning (or concept).[16] When studying an actor's categories, the interest is either in determining its meaning in relation to its context of use, or in identifying changes in its meaning which can

13. "Il est question ici d'une règle non formulée, que l'homme s'efforce de deviner, que nous voyons les enfants tâcher de découvrir: en la supposant, le peuple la crée", in Michel Bréal, *Essai de sémantique (science des significations)* (Paris: Hachette, 1904), 80.

14. Max Black, *Models and Metaphors: Studies in Language and Philosophy* (Cornell University Press, 1962), 27.

15. One famous example is Machiavelli's concept of *virtù* which is generally referred to by the original Italian word such that it is clearly distinguished from the modern meanings of *virtue*, see Skinner, *Visions of Politics*, vol.2, 160-185.

16. See for example Pamela O. Long's study of the historical meaning of the terms "invention", "secrecy", and "theft", or Lorraine Daston and Katharine Park's study of the objects and meanings associated with wonders and curiosities (which also discusses the terms by which they were referred to). Lorraine Daston and Katharine Park, *Wonders and the Order of Nature* (New York :Cambridge, Mass.: Zone Books ;Distributed by the MIT Press, 1998); Pamela O. Long, "Invention, Secrecy, and Theft: Meaning and Context in the Study of Late Medieval Technical Transmission," *History and Technology* 16, no. 3 (2000): 223–241.

be correlated with some significant events.[17] For these reasons, an actor's categories might be better described as *historical concepts.* The historical category, however, is not restricted to a well-defined meaning and a narrow context of use but includes the whole family of resemblances. The historical category has the advantage of recovering a multiplicity of meanings; it thus includes not only the meaning intended by the author of the utterance, but also the possible and potential meanings which an audience might associate with it.[18] Most terms have a multiplicity of meanings which are known and accessible to a historical actor (e.g. the category of "game"). The struggles to keep scientific words free of any unwanted associations or parasitic connotations will be discussed in greater detail in Chapter 3.

Actor's categories have been often criticized as being "numerous, fluctuating and contested", while well-constructed analytical categories could have "stability across temporal and spatial localities and have broad applicability across the sciences".[19] In one of the more famous examples, Bruno Latour has promoted a radical program of relabeling and reorganizing the content of "science, technology and society" because "our work remains incomprehensible, because it is segmented into three components corresponding to our critics's habitual categories"; instead, Latour provided "hybrid terms that blur the distinction between the really social and human-centered terms and the really natural and object-centered repertoires".[20] Even though critical of some of these radical programs of relabeling and redrawing boundaries, historians have appreciated the role of analytical categories in revealing taken for granted assumptions or (consciously and unconsciously) hidden aspects of scientific practice. Though analytical categories play an essential role in making visible historical realities, one should

17. Though not referring explicitly to "actor's categories" but rather to the more general and abstract category of *utterances*, see Skinner, *Visions of Politics*, 84-87, 103-127.

18. For the distinction between the meaning of a text and its reception see James A. Secord, *Victorian Sensation: The Extraordinary Publication, Reception, and Secret Authorship of Vestiges of the Natural History of Creation* (University of Chicago Press, 2003).

19. Patrick Carroll-Burke, "Tools, Instruments and Engines Getting a Handle on the Specificity of Engine Science," *Social Studies of Science* 31, no. 4 (2001): 593-596.

20. Bruno Latour, *We Have Never Been Modern* (Cambridge: Harvard University Press, 2012), 3; Michel Callon and Bruno Latour, "Don't Throw the Baby out with the Bath School! A Reply to Collins and Yearley," in *Science as Practice and Culture*, ed. Andrew Pickering (Chicago ; London: The University of Chicago Press, 1992), 347.

not neglect the fact that the absence (or ignorance) of such categories to the historical actors could have significant implications. We appeal to analytical categories because they are useful to think with; thus, it should not be surprising if in the absence of such categories a historical actor would think, act and experience the world differently. Relying solely on analytical categories could mean reifying our own world as historical reality.

A historical category combines some of the advantages and disadvantages of the actor's categories and the analytical categories. Being composed of actor's categories, a historical category preserves the original meaning and experience available to the historical actors. However, because it is not limited to a narrow context, it can provide a comparable breadth to an analytical category. If the meaning and use of particular concepts is often constrained and policed by certain disciplines or institutions, the historical category breaches across such divides. *If an actor's category has an actual meaning defined in terms of a specified context, the historical category defines a composite array of potential meanings and associations that would have been historically available. If a historical concept (an actor's category) could refer to what a historical actor meant by something, the historical category is supposed to reveal what a historical actor saw or thought when confronted with something. If a historical concept points to a given meaning, a historical category points to possible meanings.*

The closest concept that can illustrate some of the implications intended by a historical category is Michale Baxandall's notion of a *cognitive style* or a *period eye* defined as "a stock of patterns, categories and methods of inference; training in a range of representational conventions; and experience, drawn from the environment, in what are plausible ways of visualizing what we have incomplete information about".[21] Baxandall's key idea was that a "stock of patterns, categories, habits of inference and analogy" lends "the fantastically complex ocular data a structure and therefore a meaning".[22] Everyday experiences such as dancing or gauging the

21. Michael Baxandall, *Painting and Experience in Fifteenth Century Italy: A Primer in the Social History of Pictorial Style* (Oxford University Press, 1988), 32,30.
22. Ibid., 29.

volume of a barrel provided the "Quattrocento man" with a series of skills to experience and discuss a painting. The claim itself might not be immediately surprising for a historian – in the end, training through repetition provides one with a set of skills for seeing things that an unskilled person would not otherwise see.[23] However, Baxandall's claim is more general than that. Instead of being concerned with how a painter learned to paint, Baxandall asked what the public (made out of trained physicians, merchants etc.) *saw* when they looked at a painting. Baxandall was interested in understanding "a public mind with different furniture and dispositions from ours", or the things one would be "less likely to see" and "more likely to see".[24]

The concepts of a *historical category* and a *period eye* are important because they draw our attention to *historically coherent* objects, that is objects which are historically connected (as perceived by a *period eye*). On the one hand, disconnected objects (as perceived by a *contemporary eye*) could have been brought together by historical actors through an act of identification or labeling; though such connections are merely formal, they could have encouraged historical actors to expect or to seek more profound and meaningful connections between these objects. On the other hand, an object could have undergone *continuous* transformations, and though the end products as perceived by a *contemporary eye* would be significantly different and disconnected, the neighboring variation could have still preserved a relation of identity with its previous iteration.

The approach of this study is inspired by David Kaiser's analysis of the dispersion of Feynman diagrams. In *Drawing Theories Apart* Kaiser relied on Wittgenstein's concept of family resemblances to criticize Bruno Latour's notion of "immutable mobiles". By pointing out the variations between various Feynman diagrams, Kaiser showed that such objects "hardly remained immutable, either in appearance, role, or meaning".[25] For Kaiser, it was the mutability

23. See Ludwik Fleck, *Genesis and Development of a Scientific Fact* (Chicago: University of Chicago Press, 2012).
24. Baxandall, *Painting and Experience in Fifteenth Century Italy*, 48, 30.
25. Kaiser, *Drawing Theories Apart : The Dispersion of Feynman Diagrams in Postwar Physics*, 281.

or plasticity of the diagrams which served their mobility and staying power, as they could be adapted to new purposes. In the end, "[l]ittle about the diagrams remained 'immutable"'.[26] However, Kaiser reconstructs the mutations of the diagrams as perceived by the *contemporary eye* of the historian. It is enough for the reader to look at two Feynman diagrams drawn in different pedagogical settings to spot the differences. For the *period eye* of the postdocs who employed the ever varying diagrams, "Feynman diagrams appeared almost like batons in a relay race – stable objects that retained their meaning and form as they were passed from one user to another in a growing network".[27] While for Kaiser "[t]his initial appearance of stability ... is misleading", for us the *historical perception* of the Feynman diagrams as "stable objects that retained their meaning and form" is essential to understand how such objects could have been applied to new problems. The staying power of the Feynman diagrams must be understood not only in terms of their ability to be tinkered with and adapted to new problems, but also in terms of their ability of being mutated such that they preserve a connection (of identity and continuity) with their previous iterations. If each time a newly mutated Feynman diagram had ceased to be connected to the old category of Feynman diagrams, it would be hard to understand Douglas Hofstadter's frustration with the "diagrammatic menagerie" he encountered as a postdoc in the 1970s:

> It was sort of like, these are semi-Feynman diagrams, but they're not quite. And everything was blurry. There was never a sense of precision....Everywhere I turned, I found ugliness, and arbitrariness, and vagueness.[28]

Thus, what matters is not only that graphical objects can be analyzed through Wittgenstein's concept of family resemblances, but rather that they were *historically perceived* as forming a family resemblance (i.e. a historical category).

26. Kaiser, *Drawing Theories Apart : The Dispersion of Feynman Diagrams in Postwar Physics*, 282.
27. Ibid., 173.
28. Hofstadter quoted in ibid., 315.

2 THE COGNITIVE DIMENSION

What calls for thinking?[29]

Past studies in the history of science have invariably focused on the *production* of scientific knowledge; this term has encouraged and validated a series of metaphors centered around construction, fabrication, work, technology, mechanisms, engines, tools, devices etc. Some of the early and most influential examples of this language can be found in Steven Shapin and Simon Schaffer's *Leviathan and the Air Pump* where "the experimental production of pneumatic facts" was explained in terms of material, literary and social technologies that were all seen as "knowledge-producing tools"; similarly, Bruno Latour and Steve Woolgar's *Laboratory Life* described "the different stages in the construction of facts, as if a laboratory was a factory where facts were produced on an assembly line".[30] Such approaches have been motivated by a shared goal of avoiding "whiggish" teleological accounts in which the discovery of scientific truth is inevitable (and can be at most delayed). Instead, the production of scientific knowledge requires effort – the establishment of trust, the creation of networks, the foundation of institutions, guided practice and exercise, the movement of objects etc. This whole infrastructure not only makes possible the production of scientific knowledge, but it also shapes it. Though profoundly insightful, such approaches have inherited a teleological residue from the "whiggish" accounts they had tried to replace. While they have provided new *explanantia*, their *explananda* have remained the same: *Why are things the way they are* or *How do things come to be*? In Harry Collins' memorable analogy for knowledge production, the goal is to explain how the ship got into the bottle.[31] However, what is at least as surprising and worthy

29. Martin Heidegger, *Basic Writings*, ed. David Farrell Krell (Harper Collins, 1993), 385.

30. Steven Shapin and Simon Schaffer, *Leviathan and the Air-Pump : Hobbes, Boyle, and the Experimental Life* (Princeton: Princeton University Press, 1985), 25; Bruno Latour, "Visualization and Cognition," *Knowledge and Society* 6 (1986): 236. I have purposefully singled out the more neutral term "production" over the common reference to "constructivism" or "constructionism" that has been mostly associated with a particular approach of history of science and STST. See also Ian Hacking, *The Social Construction of What?* (Cambridge: Harvard University Press, 1999); Jan Golinski, *Making Natural Knowledge: Constructivism and the History of Science, with a New Preface* (Chicago: University of Chicago Press, 2008).

31. H. M. Collins, "The Seven Sexes: A Study in the Sociology of a Phenomenon, or the Replication of Experi-

of consideration is not only the fact that the ship made it into the bottle, but also that one is *awed* by this. Besides the *technical* reasoning and practice required to put the ship in the bottle, there is a *reflexive* and *associative* reasoning that starts from contemplating an object such as the bottled ship. So far, the focus on the production of knowledge has provided an external description of how things come to be. But besides the world which unfolds in front of us, there is also a world of potentialities and virtualities which depends on how one looks at or what one can see when they look at the world. This study will reconstruct a *period eye* to go beyond the *actual* use of certain methods and objects as inculcated through practice, to describe the *potential* use as revealed by what "comes to mind" when historical actors think about, look at, or manipulate an object.

In the past two decades, historians have endeavored to treat on a "symmetrical footing" the practices of the experimental and theoretical sciences by extending the framework used in the study of the former to the latter.[32] Though there are methodological advantages in postulating a symmetry between two objects of study and in bringing them in close proximity, one must always keep in mind the limitations of such a comparison.[33] By appealing to concepts such as *paper tools* and *theoretical technologies*, historians have been able to describe theoretical science as a form of practice based on everyday labor, craftlike skills, regimens of training and practice, or tacit knowledge. These approaches have inspired and shaped this study. However, one must critically examine the uncritical assumptions introduced by metaphorical concepts such as those of tool or technology.

ments in Physics," *Sociology* 9, no. 2 (1975): 205–224.

32. Peter Galison and Andrew Warwick, "Introduction: Cultures of Theory," *Studies in History and Philosophy of Modern Physics* 3 Vol 29 B, no. 29 (1998): 288n2.

33. Cf.: "This asymmetry between historical accounts of experimental and theoretical work is, however, both undesirable and unnecessary", in Andrew Warwick, "Cambridge Mathematics and Cavendish Physics: Cunningham, Campbell and Einstein's Relativity 1905–1911 Part I: The Uses of Theory," *Studies in History and Philosophy of Science Part A* 23, no. 4 (1992): 632. No further motivation is given. Or, "I shall argue that this distinction between theoretical and experimental work is in many ways artificial and that by acknowledging a number of similarities between these activities we can begin to build a more symmetrical account of theory and experiment", Andrew Warwick, *Masters of Theory : Cambridge and the Rise of Mathematical Physics* (Chicago: University of Chicago Press, 2003), 12. Warwick never mentions the ways in which the distinction between theoretical and experimental work is not artificial.

The actual cognitive role played by material, external objects has remained unexplored because historians have assumed a much too close analogy between "paper tools" and "laboratory tools" without actually specifying what sets them apart: "at many levels paper tools are fully comparable to physical laboratory tools and instruments and that both kind of tools contribute to the creation of reference and meaning, or *representations* in that sense".[34] The image of the tool (the device, the technology, the mechanism etc.) is self-reifying and generates a false expectation of power and efficacy. However, the sole evidence for such power and efficacy is usually the mere manipulation, shuffling and multiplication of papers:

> In our cultures "paper shuffling" is the source of an essential power, that constantly escapes attention since its materiality is ignored. [...] It is hard to overestimate the power that is gained by concentrating files written in a homogeneous and combinable form.[35]

Historians have purposefully appealed to the concepts of paper tools and theoretical technologies to avoid talking about the "thinking" behind a scientific activity.[36] Because a historian does not have direct access to what goes inside the head of a historical actor, one can only engage with the material traces of his activity. Though such methodological restraint is commendable, lack of access to a phenomenon does not imply that such a phenomenon is irrelevant or reducible to something else. For example, Ursula Klein followed Bruno Latour's dictum – "Most of what we impute to connections in the mind may be explained by this

34. Ursula Klein, *Experiments, Models, Paper Tools* (Stanford, Calif: Stanford University Press, 2003), 245. Two remarkable exceptions are the studies of David Gooding and Reviel Netz. See David C. Gooding, "Visualizing Scientific Inference," *Topics in Cognitive Science* 2, no. 1 (2010): 15–35; David C. Gooding, "Cognition, Construction and Culture: Visual Theories in the Sciences," *Journal of Cognition and Culture* 4, no. 3 (2004): 551–593; Reviel Netz, *The Shaping of Deduction in Greek Mathematics: A Study in Cognitive History* (Cambridge University Press, 2003).

35. Bruno Latour, "Drawing Things Together," in *Representation in Scientific Practice*, ed. Michael Lynch and Steve Woolgar (Cambridge: MIT Press, 1990), 54-55.

36. It can be instructive to compare Andrew Warwick and David Kaiser's essay on "Kuhn, Foucault, and the Power of Pedagogy" with Kuhn's own writings. The essay is carefully written to avoid any mental or cognitive references. It only talks about how students "*learn* how to speak and act as scientists and engineers", but not how to *think* as scientists. The key terms are *training, practice, skill, production.* Kuhn, however, engaged with the work of cognitive scientists or Gestalt psychologists which is also reflected in his choice of expressions such as "mental sets", "mental equipment", "mental apparatus", "divergent thinking", "convergent thinking" etc., see Andrew Warwick and David Kaiser, "Conclusion: Kuhn, Foucault, and the Power of Pedagogy," in *Pedagogy and the Practice of Science: Historical and Contemporary Perspectives*, ed. David Kaiser (Cambridge: MIT Press, 2005), 393.

reshuffling of inscriptions" – in exploring "the explanatory capacity of material agency that go hand in hand with the elimination of notions referring to mental processes". Klein looked at Berzelian formulas as "paper tools" which she considered to be "material devices in the broadest sense of being exterior to mental processes, visible and maneuverable".[37] While this approach was seen as "redressing the balance between the material and mental dimensions of inscriptions", in truth, the "mental dimension" is never addressed.[38] This is a problem because the power of inscriptions has often been attributed to their ability to connect with certain mental processes. Berzelian formulas were useful not only because of their "maneuverability on paper" but also, if not especially, because of their maneuverability off paper, in the mind.[39] Unfortunately, Klein fails to recognize that powerful reasoning tools depend the least on the materiality of their support. That is not to say that material culture is irrelevant for reasoning, but rather that the relation between the two has only received unsatisfactory explanations. Busy hands scribbling on paper cannot be a substitute for reasoning.

The *tool* nature of diagrams has been invoked by David Kaiser to describe the activity of theoretical physicists as a form of *practice* defined by a set of *craftlike skills* which cannot be easily communicated at a distance. When diagrams did move, they were adapted to new kinds of calculation suited to their local setting. Following Claude Lévi-Strauss's image of the *bricolage*, Kaiser described the adaptation of the diagrams as a form of *tinkering*.[40] However, "tinkering" is just another form of the "busy hands" explanation criticized above. The fact that we can see the moving hands of the scientists (or the traces produced by such an activity) does not mean that there is nothing else going on. What the anthropological metaphor of the *tool* absconds is the mental process that guides the "tinkering" (or the "shuffling" and "re-

37. Klein, *Experiments, Models, Paper Tools*, 242-243.

38. Ibid., 242.

39. Once the manipulation of Berzelian formula has been mastered, the material support of paper was not required for actually manipulating the formulae.

40. Kaiser, *Drawing Theories Apart : The Dispersion of Feynman Diagrams in Postwar Physics*, 18-19. For the description of the variations as forms of "tinkering" see ibid., 8, 18, 134, 174, 176, 199, 207, 230, 253, 282, 308, 317, 377.

shuffling"). Lévi-Strauss' image of the bricolage suffers from a second limitation: it assumes that new tools are created solely through a process of *recombination* which uses a finite set of forms or patterns.[41] However, when regarded historically, no system is finite or closed; new rules and patterns emerge which are not just a recombination (or reshuffle) of a finite set of objects. The fact that knowledge cannot be reduced to a finite set of actions and rules was one Michael Polanyi's main insights behind his concept of *personal knowledge* or *tacit knowledge*. While Kaiser builds on Polanyi's *tacit knowledge* to explain how physicists acquired the skill to manipulate Feynman diagrams, he appeales to Lévi-Strauss's *bricolage* to explain how diagrams were modified. The paradox of this combination between Polanyi and Lévi-Strauss is that the acquisition of skills seems to require great effort (institutionalized and localized regimens of practice and training), while innovation occurs almost spontaneously through seemingly effortless acts of tinkering.

In a remark to the Vienna Circle in 1930, Wittgenstein pointed out that:

> In Cambridge I have been asked whether I believe that mathematics is about strokes of ink on paper. To this I reply that it is so in just the sense in which chess is about wooden figures. For chess does not consist in pushing wooden figures on wood. If I say, 'Now I shall get a queen with very terrible eyes and she will drive everything from the field', you will laugh. It does not matter what a pawn looks like. It is rather the totality of rules of a game that yields the logical position of a pawn. A pawn is a variable, just like 'x' in logic.[42]

Of course, Wittgenstein is only partially right. It can matter how a pawn looks like because the size and color of the pieces or the board matters can affect the concentration and attention of a player; and so does the light in the room, or the diet of the players, and a whole array of myriad factors often known only to the individual player. However, all these material factors that could affect the players' concentration play only a marginal role in the actual reasoning process of a chess player. By "mangling" a disparate array of factors within the non-descriptive

41. E.g. "The characteristic feature of mythical thought is that it expresses itself by means of a heterogeneous repertoire which, even if extensive, is nevertheless limited", in Claude Lévi-Strauss, *The Savage Mind* (University of Chicago Press, 1966), 17.

42. Wittgenstein, *Philosophical Investigations*, 103-104.

term of "work", historians of the theoretical sciences or of mathematics have become less capable of understanding what truly matters in seeing or understanding a solution. Of course, theoreticians or mathematicians often need to write things down on paper, but the heavy work (and the actual effort) does not invovle neither the paper nor the hands.

The inability of specifying all the acquired knowledge and skills (Polanyi's tacit knowledge) or all the hypotheses (the Duhem–Quine thesis) only comes to show that historians cannot have a full understanding of when a practice or a tool is productive, effective or efficient, or what can actually make it to be productive, effective or efficient. I will illustrate this line of criticism with an example from Max Wertheimer's *Productive Thinking* (1st ed. 1945).[43] Visiting a classroom in which children had been previously taught how to find the area of a parallelogram, Wertheimer was left unpersuaded by the students' exhibition of their newly acquired skill:

> "What have they learned?" I ask myself. "Have they done any thinking at all? Have they grasped the issue? Maybe all that they have done is little more than blind repetition. To be sure, they have solved promptly the various tasks the teacher has assigned, and so they have learned something of a general character, involving some abstraction. Not only were they able to repeat word for word what the teacher said, there was easy transfer as well. But–have they grasped the issue at all? How can I clarify it? What can I *do*?"[44]

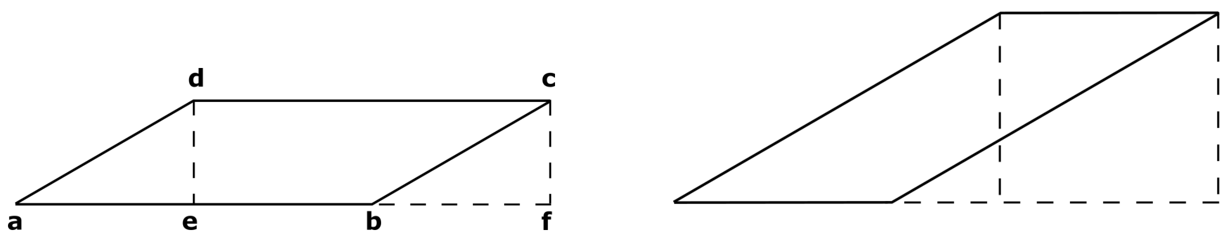
To test the real understanding of the students (what Wertheimer called *productive thinking*), he gave them a parallelogram drawn differently from the one they were accustomed to (see Fig. 1.1). Though the great majority of students were able to solve the examples chosen by the teacher, this time the reactions varied:

> Some are obvious taken aback.
>
> One pupil raises his hand: "Teacher, we haven't had that yet."

43. While there are more recent sources that one could employ, I chose Wertheimer's book because of his role in the development of Gestalt psychology which has been so influential in the development of history of science, especially through the famous duck-rabbit digram invoked by Thomas Kuhn. See Kuhn, *The Structure of Scientific Revolutions*, 111-120; Kuhn, *The Essential Tension : Selected Studies in Scientific Tradition and Change*, 6-7.

44. Max Wertheimer, *Productive Thinking* (New York: Harper & Brothers, 1959), 15.

> Others are busy. They have copied the figure on paper, they draw the auxiliary lines as they were taught, dropping perpendiculars from the two upper corners and extending the base line (see Fig. 1.1b). Then they look bewildered, perplexed.
>
> Some do not look at all unhappy; they write firmly below their drawing: "The area is equal to the base times the altitude"–a correct subsumption, but perhaps an entirely blind one. When asked wether they can show it to be true in this case, they too become perplexed.
>
> [In the footnote:] A boy from another class, observing these difficulties, whispers to me, "In our class we have learned how to work this overlapping example. It's the teacher's fault. Why didn't he teach them how to do it in the overlapping diagram too?"[45]



(a) (b)

Figure 1.1
(a) Students were taught to find the area of the parallelogram $abcd$ by dropping the perpendiculars de and cf. Because the two triangles ade and bcf were equal, and $edcf$ formed a rectangle, the area of the parallelogram was equal to the area of the rectangle, and thus equal to the product of the base and the height. (b) However, for the new parallelogram the same construction failed to produce the expected rectangle. Redrawn after Max Wertheimer, *Productive Thinking* (New York: Harper & Brothers, 1959), 14-16.

This example illustrates remarkably well the issues articulated above. Histories focused exclusively on acquired skills (and which do not take into account some form of thinking) cannot explain when and how a historical actor decides that an acquired skill or tool is applicable to a certain problem. Furthermore, the blind and mechanical use of the tool or skill (through the mere repetition of known steps) will not always give rise to a correct solution (and sometimes they will generate no solution at all). Talk about "tinkering" or "generalization" will fail to explain why some students were successful with the new task (assuming that they had not seen this problem before). The successful students managed to solve the problem not because they drew more diagrams and scribbled more assiduously, but rather because they were able to *look for* a solution and *see* a solution.[46] Of course, one might be led to say that the

45. Wertheimer, *Productive Thinking*, 15-16.
46. What *looking for* and *seeing* actually means is an answer to be provided by psychology. See for example

successful (or productive) application of an acquired skill or tool to a new problem is a skill in itself. Though, it can be described as a skill, it is not a skill *acquired* through repetition and drilling. If it were then students could never be differentiated through a problem that forced them to apply their skill in a novel situation and which tested their "true" understanding (or what Wertheimer calls their "intelligent reactions").

This issue should bring to focus an often neglected dimension of Michael Polanyi's concept of *tacit knowledge.* So far sociologists and historians have appealed to Polanyi's concept to explain why the replication of an experiment or the acquisition of a method requires direct, face-to-face contact and cannot be always achieved through purely textual (or verbalized) means.[47] However, the understanding developed by students, though it is developed through training, cannot be explained solely in terms of such training; often, it is a form of "personal knowledge" (as it was first called by Polanyi), rather than a knowledge *shared* through personal contact and direct manipulations. Even in the case of *tacitly shared* knowledge, what is transferred is not so much the knowledge but the results – the practitioners are usually certified that they can reproduce a certain result.

Technical skills are, of course, required to solve a scientific problem. Wertheimer's example, however, points to a profound distinction between the thoughtless, blind, mechanical repetition of a preexisting solution, and the intelligent, innovative, imaginative solution of a new problem. While some students could see the two diagrams in Fig. 1.1 as being the same despite being different (i.e. they could transform one into the other), others could not see any connection between the two – "Teacher, we haven't had that yet". To understand how sci-

Herbert Simon's tackling of the question: "What does an experienced chess player 'see' when he looks at a chess position?", in William G. Chase and Herbert A. Simon, "Perception in Chess," *Cognitive psychology* 4, no. 1 (1973): 55–81; see also William G. Chase and Herbert A. Simon, "The Mind's Eye in Chess.," *Visual information processing* 4 (1973): 215–281.

47. Kaiser employed tacit knowledge for its ability of emphasizing the "nontextual means of transmission", see Kaiser, *Drawing Theories Apart : The Dispersion of Feynman Diagrams in Postwar Physics*, 10-13; Warwick, *Masters of Theory : Cambridge and the Rise of Mathematical Physics*; Kathryn Mary Olesko, "Tacit Knowledge and School Formation," *Osiris* 8, no. 1 (1993): 16–29.

entists think when they use graphical objects means to be able to distinguish between such cases: between scientists who perceive graphical objects as being related and those who see them as unrelated. George Gamow used to tell his students that as a theoretical physicist you can lie down on a couch, close your eyes, and no one will know whether or not you are working.[48] This image has been rejected by historians who have rightfully insisted on showing that labor, practice and skill are essential for theoretical science. However, historians of science have failed to show why closing your eyes and thinking about a problem is an essential part of theoretical science (and not only). The great challenge ahead is to make the immaterial material without corrupting its essence (or confounding it with its material support and manifestations).

3 THE GRAPHICAL DIMENSION

There is a vast literature on scientific visualization, representation, picturing, imaging etc.[49] These studies have generally focused on the *work* required to *produce* scientific images, that is objects which can be *trusted* to "represent" or "make visible" certain aspects of nature. Special attention has been paid to the philosophical and historical tensions between the visual, logical and verbal domains. In a few cases, historians have construed diagrams and models as "cognitive tools" which can facilitate cognitive processes such as pattern-matching and visual inference.[50] While such histories of visual culture analyze the status, role, meanings etc. of

48. Kaiser, *Drawing Theories Apart : The Dispersion of Feynman Diagrams in Postwar Physics*, 7-8.

49. For a recent overview study see Klaus Hentschel, *Visual Cultures in Science and Technology : A Comparative History* (New York: Oxford University Press, 2014). See also Michael Lynch and Steve Woolgar, eds., *Representation in Scientific Practice* (MIT Press, 1990); M. Norton Wise, "Making Visible," *Isis* 97, no. 1 (2006): 75–82; Galison, *Image and Logic : A Material Culture of Microphysics*; Peter Galison and Caroline A. Jones, eds., *Picturing Science, Producing Art* (New York: Routledge, 1998); Peter Galison, "Images Scatter Into Date, Data Gather Into Images," in *Iconoclash*, ed. Bruno Latour and Peter Weibel (Karlsruhe: Zentrum für Kunst und Medientechnologie, 2002), 300–323; James Elkins, *The Domain of Images* (Ithaca: Cornell University Press, 2001); Lorraine Daston and Peter Galison, *Objectivity* (New York : Cambridge, Mass.: Zone Books, 2007); Catelijne Coopmans et al., eds., *Representation in Scientific Practice Revisited* (Cambridge, Massachusetts: The MIT Press, 2014, 2014); Wolfgang Lefèvre, *Picturing Machines 1400-1700* (Cambridge: MIT Press, 2004).

50. Gooding, "Visualizing Scientific Inference"; Gooding, "Cognition, Construction and Culture"; Netz, *The Shaping of Deduction in Greek Mathematics*; John B. Bender and Michael Marrinan, *The Culture of Diagram* (Stanford, Calif.: Stanford University Press, 2010).

an *already given* object which can be specified from the beginning of the study (*this* image, or *this* series of images), the history of the historical category of the *graphical* needs to start by bringing together its objects of interest. This study does not aim to be a synthesis of visual histories about graphical objects, but rather a history of the *graphical* category reconstructed through a *period eye* looking at and valuing various *graphical* objects. It is a history of the *graphical* within the objects.

The focus on the category of the *graphical* rather than on the graphical objects *per se* allows us to move beyond the unchallenged interpretation of graphical objects as "tools" or "working objects".[51] Though insightful and productive, such an interpretation only provides an explanation for the *isolated* role and use of graphical objects. The only fact which matters for this interpretation are the intrinsic qualities of the graphical objects and not their groupings and associations. By focusing on the category of the *graphical*, this study will reveal the role played by the historical perception of family resemblances between graphical objects. David Kaiser has alluded to a similar "associative" explanation for why Feynman diagrams sticked as opposed to the dual diagrams. One cannot explain the different "staying power" of these diagrams if they were construed as paper tools which relied solely on habit and inculcation. Kaiser's explanation invoked the "realist associations" of the Feynman diagrams based on their similarity to "real" photographs of "real" particles.[52] This appeal to "realism" has been rightly criticized by Adrian Wüthrich.[53] Rather than "realism", we consider the historical association of Feynman diagrams and particle trajectories as a more appropriate explanation. It is not the "realism" of the representations which matters but its density of meanings and associations.

Previous studies have predominantly dealt with the *isolated* histories of graphical objects such as graphical representations, graphical methods, or graphical instruments.[54] For exam-

51. For "working objects" see Bender and Marrinan, *The Culture of Diagram.*
52. Kaiser, *Drawing Theories Apart : The Dispersion of Feynman Diagrams in Postwar Physics*, 362-373.
53. Adrian Wüthrich, *The Genesis of Feynman Diagrams* (Springer Science & Business Media, 2010), 9-11.
54. For the history of graphical representations see: H. Gray Funkhouser, "Historical Development of the Graphical Representation of Statistical Data," *Osiris* 3 (1937): 269–404; Howard Wainer, *Graphic Discovery: A*

ple, for historians interested in graphical instruments such as the autographs, the "graphic method" is only "the technique of using an automatic recording instrument".[55] In contrast, for the 19th century French physiologist Étienne-Jules Marey the graphical method was a "language" for representing and inscribing phenomena which applied both to experimental plots and self-recording instruments. Not only graphical representations, methods and instruments have been analyzed in isolation from each other, but even the individual instances of these objects have remained historiographically disconnected. Historical studies have usually dealt with narrowly contextualized histories of some particular examples, while the broader works have only synthesized a thematically and chronologically disjoint array of objects.[56] Thus, a

Trout in the Milk and Other Visual Adventures (Princeton University Press, 2013); Howard Wainer, *Visual Revelations : Graphical Tales of Fate and Deception from Napoleon Bonaparte to Ross Perot* (New York: Copernicus, 1997); Thomas L. Hankins, "Blood, Dirt, and Nomograms: A Particular History of Graphs," *Isis* 90, no. 1 (1999): 50–80; Thomas L. Hankins, "A "Large and Graceful Sinuosity". John Herschel's Graphical Method," *Isis; an international review devoted to the history of science and its cultural influences* 97, no. 4 (2006): 606–633; Laura Tilling, "Early Experimental Graphs," *The British Journal for the History of Science* 8, no. 3 (1975): 193–213; for graphical methods see: Dominique Tournes, "Pour une histoire du calcul graphique," *Revue d'histoire des mathématiques* 6, no. 1 (2000): 127–161; Dominique Tournes, "L'intégration graphique des équations différentielles ordinaires," *Historia Mathematica* 30, no. 4 (2003): 457–493; Dominique Tournes, "Diagrams in the Theory of Differential Equations (Eighteenth to Nineteenth Centuries)," *Synthese* 186, no. 1 (2012): 257–288; Erhard Scholz, *Symmetrie, Gruppe, Dualität: Zur Beziehung Zwischen Theoretischer Mathematik Und Anwendungen in Kristallographie Und Baustatik Des 19. Jahrhunderts*, vol. Bd. 1 (Basel ;Boston: Birkhäuser Verlag, 1989); Erhard Scholz, "Graphical Statics," in *Companion Encyclopedia of the History and Philosophy of the Mathematical Sciences*, red. Ivor Grattan-Guinness, vol. 2 (London ; New York: Routledge, 1994), 987–93; Konstantinos Chatzis, "La Réception de La Statique Graphique En France Durant Le Dernier Tiers Du XIXe Siècle," *Revue d'histoire des mathématiques* 10, no. 1 (2004): 7–43; for graphical instruments: H. E. Hoff and L. A. Geddes, "The Beginnings of Graphic Recording," *Isis* 53, no. 3 (1962): 287–324; H. E. Hoff and L. A. Geddes, "The Rheotome and Its Prehistory: A Study in the Historical Interrelation of Electrophysiology and Electromechanics," *Bulletin of the history of medicine* 31, no. 3 (1957): 212–234, contd; H. E. Hoff and L. A. Geddes, "Graphic Registration before Ludwig: The Antecedents of the Kymograph," *Isis* 50, no. 1 (1959): 5–21; H. E. Hoff and L. A. Geddes, "The Technological Background of Physiological Discovery: Ballistics and the Graphic Method," *Journal of the History of Medicine and Allied Sciences* XV, no. 4 (1960): 345–363; Soraya De Chadarevian, "Graphical Method and Discipline: Self-Recording Instruments in Nineteenth-Century Physiology," *Studies in history and philosophy of science* 24, no. 2 (1993): 267–291; Robert Michael Brain, "The Graphic Method: Inscription, Visualization, and Measurement in Nineteenth-Century Science and Culture" (PhD diss., University of California, Los Angeles, 1996); M. Norton Wise and Robert M. Brain, "Muscles and Engines: Indicator Diagrams and Helmholtz's Graphical Methods," in *The Science Studies Reader*, ed. Mario Biagioli (Taylor & Francis, 1999), 51–66. One of the few studies which connect graphical representations to graphical instruments see Thomas L. Hankins and Robert J. Silverman, *Instruments and the Imagination* (Princeton: Princeton University Press, 1999).

55. Brain, "The Graphic Method," 8.

56. For some examples of broad histories see Hentschel, *Visual Cultures in Science and Technology*; Daniel Rosenberg and Anthony Grafton, *Cartographies of Time*, 1st ed. (New York: Princeton Architectural Press, 2010); Bender and Marrinan, *The Culture of Diagram*; Mark Greaves, *The Philosophical Status of Diagrams* (Stanford, Calif: CSLI Publications, 2002). For an example of localized history see Hankins, "A "Large and Graceful Sinuosity".

history of graphical representations or of graphical instruments reads as a long list of disparate examples.

Such studies have a common shortcoming: a methodological inability of specifying the *historical value* of these objects as *graphical* objects. While the graphical dimension of an object is valued by the contemporary eye, there is no reason to project such values onto a period eye. Not all instruments which produced a graphical trace and not all graphical methods were employed because they were valued as *graphical.* Paraphrasing Peter Galison, a commitment to one graphical object is not necessarily a commitment to all graphical objects.[57] Furthermore, a commitment to a particular graphical object because of its graphical quality is not necessarily a commitment to the *graphical* in general.

Most historical studies have construed graphical objects as innovations which punctured history through the work of a few individuals:

> Graphs, unambiguously recognizable as such, appeared in the last quarter of the eighteenth century, probably independently, in three places – in the indicator diagram of James Watt, in the lineal arithmetic of William Playfair, and in the scientific writings of Johann Heinrich Lambert.[58]

If construed solely as tools, it is hard to understand why graphical objects were not immediately embraced:

> Why, then, was the scientific climate in the 1760s such that Lambert's methods could not be assimilated nor the usefulness of graphs be recognized, whereas in the 1850s graphical representation became a popular technique? … If Forbes was able to borrow from Lambert, it is difficult to say why earlier workers did not do so.[59]

Two explanatory models have been invoked:

1. *Lack of knowledge.* Either the writings of the innovators have remained unknown and

John Herschel's Graphical Method."

57. "And yet a commitment towards one kind of visualization is not necessarily a commitment to them all", in Galison, "Images Scatter Into Date, Data Gather Into Images," 307-8.

58. Hankins, "Blood, Dirt, and Nomograms," 52.

59. Tilling, "Early Experimental Graphs."

obscure, or the productive use of such graphical objects required some particular training or knowledge:

> Lambert's graphical method did not catch on immediately, which may be attributed in part to the obscurity of much of his writing and in part to the unfamiliarity of graphs themselves. … The concept of a graph is abstract, and its meaning will seem obvious only to those who are familiar with it.[60]

A similar explanation has been offered by Kathryn Olesko who considered that the "relative unimportance in Prussian mathematics instruction of both descriptive geometry and the graphical analysis of functions" could be the reason why Franz Neumann's seminar from the mid-19th century did not rely on "more imaginative uses of graphs".[61]

2. *Trust in the precision of numbers.* Graphical methods and representation were not more readily employed because they lacked rigor and precision:

> We do not have any contemporary reactions to Lambert's graphs, but the graphs of William Playfair, which became much better known than Lambert's, brought forth the criticisms that they "lacked rigor", that they were mere "plays of the imagination" and "without importance" outside of pedagogy. Those who were used to working with tables of numbers could persuade themselves that in drawing graphs one lost the precision of the numbers themselves. It is probably for these reasons that experimental and statistical graphs did not become popular until the 1830s.[62]

Olesko has also pointed out that for "Neumann's students, the points comprising graphs lacked certainty".[63] The use of graphical methods and representations was limited by the "value they placed not only on precision measurement but also on accuracy as determined by the closeness of theoretical calculations and experimental results".[64]

Though both explanatory models rely on well established narratives within history of science (the use of tools requires particular regimens of training and practice; some objects and

60. Hankins and Silverman, *Instruments and the Imagination*, 120-1.
61. Kathryn Mary Olesko, *Physics as a Calling: Discipline and Practice in the Königsberg Seminar for Physics* (Ithaca: Cornell University Press, 1991), 252.
62. Hankins and Silverman, *Instruments and the Imagination*, 120-1.
63. Olesko, *Physics as a Calling: Discipline and Practice in the Königsberg Seminar for Physics*, 256.
64. Ibid., 252.

methods embody more closely certain moral and scientific ideals), they do not readily apply in the case of graphical objects. First, among all mathematical methods graphical methods required the least amount of knowledge and training, and in the second half of the 19th century they were easily imitated and transfered. Second, graphical objects were disseminated as the value of precision measurements steadily increased throughout the 19th century. To understand the multiplication and dissemination of graphical objects, one must first understand how these objects were seen, valued and associated through a *period eye*.

4 OVERVIEW

Each of the first three chapters is focused on a particular group of graphical objects: graphical methods, graphical instruments and graphical representations. It will be shown that the category of the *graphical* did not emerge because of the multiplication of graphical objects (as identified by a *contemporary eye*), and it was never only a simple and isolated description of a given object or method. Instead, the *graphical* was a *value-laden* label which reflected and reinforced the epistemological and pedagogical programs of those who promoted this category.

Chapter 2 follows the emergence of the label *graphical* within various specific contexts, such as Newton's *Principia*, the mathematical textbooks of 18th century French astronomers, and the newly founded École polytechnique as envisioned by Gaspard Monge and his followers. The use of *graphical* as a separate category only partially overlapping with the *geometrical* will come to reveal the increasingly strenuous place of geometry within 18th century mathematics. Chapter 3 reconstructs the *period eye* through which the action of *autographs* and self-registering instruments was described and understood by historical actors. It will be shown, against the common belief of many historians, that the initial purpose of these instruments was not that of producing inscriptions or traces; instead, historical actors perceived the main function of these instruments as being that of replacing an observer. Chapter 4 identifies the weather charts of 18th century meteorologists as the first historically coherent grouping of graphical representations. By analyzing their role and value, an explanation is given for the presence or absence of graphical representations in early 19th century publications.

Chapters 5 and 6 follow the development of two powerful conceptual, computational and representational tools of late 19th century physics: the phase diagrams of thermodynamics and the curves of magnetization of electromagnetism. While inspired by previous studies on "paper tools" and "knowledge in transit", the goal of these chapters is to explain the appeal of diagrammatic practices *before* they were institutionalized in the practice and teaching of physics and engineering. By reconstructing what some physicists *saw* when they first constructed and contemplated such diagrams and curves, these chapters reveal a new historical dimension of diagrammatic practices. Such a historical understanding of diagrammatic practices is possible only if one interprets diagrams not only as *paper tools*, but also as *graphical* objects. For this reason, the object of analysis is not the diagram *qua* tool (and the skills required to use it or apply it to new purposes), but rather its *graphical* dimension enriched by meanings generated through novel associations, patterns and analogies. In contrast with what was claimed by previous studies, these chapters show that the dissemination of particular graphical representations such as the phase diagrams and the magnetic curves were a cause and not a consequence of the acceptance and valuation of graphical representations in general.

Chapter 7 explores the pedagogical role of graphical methods in the reform of the teaching of mathematics at the secondary school level in Britain, France and Germany. If in the late 18th century (as seen in Chapter 2) the *graphical* was always part of a dichotomy – opposed to trigonometry, algebra or analysis – and was used as boundaries between various disciplines, by the early 20th century the *graphical* was invoked as a place of intersection and coordination for geometry and algebra, mathematics and experimental science, induction and deduction.

· 2 ·

Operations and Constructions

1 GEOMETRICAL OR GRAPHICAL

In the seventeen volumes of the first edition of *L'Encyclopédie, ou Dictionnaire raisonné des sciences, des arts et des métiers* (1751-1772) there is only one small entry that defined the term "graphique":

> GRAPHIQUE, adjectif, (*Astron.*) on appelle en Astronomie *opération graphique*, celle qui consiste à résoudre certains problemes d'Astronomie par le moyen d'une ou de plusieurs figures tracées en grand sur un papier, & relatives à la solution de ces problèmes. Si ces opérations ne donnent pas une solution extrèmement exacte, elles donnent en récompense la solution la plus prompte, & fournissent une premiere approximation commode, qu'on peut ensuite pousser plus loin en employant le calcul. Ainsi on employe les opérations *graphiques* pour avoir d'abord une solution ébauchée du problème des cometes, de celui des éclipses, & de quelques autres. On peut en voir des exemples dans différens ouvrages d'Astronomie. (O)[1]

The definition is striking because it restricted "graphique" only to astronomy as a type of solution for some problems related to eclipses and comets. Furthermore, "graphique" was only used in conjunction with "opération", and not "construction" or "méthode" as it will be known throughout the 19th century. The eight volume *Dictionnaire encyclopédique et biographique de l'industrie et des arts* (1881-1891) provides a term of comparison. Its supplement included a two-page entry on "graphique" that was defined as:

> Graphique. On appelle *graphique* ou *diagramme*, tout tracé composé de lignes droites ou courbes, ou d'espaces teintés ou recouverts de hachures, destiné à représenter dans son ensemble la marche d'un phénomène scientifique, industriel, économique, commercial, etc.[2]

1. Denis Diderot and Jean D'Alembert, *Encyclopédie ou Dictionnaire raisonné des sciences, des arts et des métiers* (Paris: Chez Briasson, 1751–1772), vol. 7, 859. With the exception of this entry, "graphique", "graphiques" or "graphiquement" are altogether absent from *L'Encyclopédie* or from the mathematical works of Jean d'Alembert who wrote the entry.

2. Eugène-Oscar Lami, *Dictionnaire encyclopédique et biographique de l'industrie et des arts industriels: Supplément* (Paris: Librairie des dictionnaires, 1891), 916-918.

The article aimed to illustrate the extensive use of diagrams (or "graphiques") in all spheres of activity. It briefly described: the curve of a function; the experimental curve fitting observations; the thermodynamic cycles used to compute the efficiency of engines; the meteorological curves; the curves of a moving body; diagrams produced by self-registering instruments that "today are universally applied to a multitude of observations of all sorts" and which could automatically record through a trace on a piece of paper the air temperature, the blood pressure, or the pressure inside a steam engine; polar coordinate diagrams; diagrams of discontinuous quantities that employ rectangles of different height or shading; geographical maps that indicate the population, degree of instruction, or the commercial and industrial business, etc. The other volumes included references to "statique graphique", "construction graphique", "méthode graphique", "représentation graphique", "travail graphique", "tableaux graphiques", "arts graphiques", "procédés graphiques", "configuration graphique", "tracé graphique", "indication graphique", "théorie graphique", "dessin graphique", "erreur graphique", "traduction graphique", "modèle graphique", "machines graphiques", etc.

This prolixity of terms and diagrams could mistakenly make us believe that graphical methods were hardly used before the 19th century, though that was not the case. Eighteenth century artists and engineers were taught "l'art du lavis", "le dessin en perspective ou le dessin géométral", "l'usage des plans, profils et élévations", "l'art de lever les plans et les cartes", "l'art du trait", "le tracé des ombres", etc. What should intrigue us is not the variety of forms, but rather their coalescence under the unifying category of the *graphical*. Did it make a difference if one referred to "a graphical representation" or "a graphical construction" instead of "a trace" or "a construction by rule and compass"? Though the immediate meaning of these expressions is almost identical, carefully tracking their use along a *longue durée* history can reveal new alignments of bodies of practice and knowledge, and the rhetorical means through which such realignments were carried out.

For example, though there was a continuity between the practices and diagrams used in early-modern stone and wood cutting and those of 19th century descriptive geometry, the language employed to describe these domains of activity differed in significant ways. Descriptive geometry employed a term like "graphique" to coordinate different aspects of its academic teaching – the lectures and the practical work, the theory and its applications, the relations between engineers and artists, or the opposition between geometry and analysis. The activity of stone and wood cutters was centered around "le trait", a term which cannot be fully reduced to "graphical trace" – because "les traits" were often employed not only on paper but also on materials like wood and stone, or on the surface of walls and floors – nor to "graphical method" – because "le trait" was used not as a representation or solution, but as an intermediate step that assisted one in cutting, building or assembling. Furthermore, the social identity of the workers was also constructed around the term as in the case of the *noms compagnonniques*, usually formed by combining the origin of the compagnon and some personal characteristic such as "L'Ami du Trait":

> O vous, dont la modestie, dont les talents sont connus et appréciés, ô vous Lyonnais *L'Ami du Trait*, Toulousain *La Prudence*, Suisse *Le Résolu*, Lafrance *L'Ami du Trait*, Bourguignon *Franc-Cœur*, Gascon *L'Ami du trait*, vous tous enfin, Compagnons courageux, qui, marchant dans la même voie, vous livrez à la démonstration, non seulement par métier, mais par devoir, mais par dévoûment, mais par amour pour vos semblables, continuez la tâche que vous vous êtes imposée![3]

The close study of such terminology allows one to supplement visual analyses or histories of practice which tend to group together objects that look the same or which have the same material support. A history of categories/terminology reveals the ways in which categories were used to homogenize heterogeneous groups of objects (or to bring together objects which were not immediately or intuitively grouped under one heading). Such coordination could be carried out either by grouping together various objects under the same label, or by creating relations between various groups of objects by using partial labels (see Fig. 2.1). While the study of concepts has focused on meaning, the study of categories explores the coordination

3. Agricol Perdiguier, *Le livre du compagnonage*, 2nd ed. (Paris: Pagnorre, 1841), vol. 1, 210.

of meaning through common terms or labels. The difference can be tracked back to Wittgenstein's concept of "family resemblance" [Familienähnlichkeiten]. Historians have appealed to Wittgenstein to study the break down of meanings around localized practices and their coordination along trading zones. However, their attention has been solely focused on the meaning of the concepts forming a family, without considering the implications that a family of concepts is united under one name/label/category. The very existence of vague and general words like "game" or "graphical" should become a concern and it demands further investigation.

trigonometric operations   arithmetical operations   graphical operations   chemical operations   revolutionary operations
graphical constructions
graphical arts
graphical work
graphical method
graphical language
graphical representation

Figure 2.1

1.1 "Opération Graphique" in French Astronomy

The definition of "graphique" from *L'Encyclopédie* was reproduced in *L'Encyclopédie Méthodique: Mathématiques* (1784-1789) with only the last sentence being replaced by:

> On peut en voir des exemples dans mon *Astronomie.* L'Abbé de La Caille a donné une manière commode pour trouver les longitudes en mer par une opération graphique. *Nouveau Traité de navigation*, Bouguer & La Caille, 1769. (D. L.)[4]

"D. L." were the initials of the astronomer Jérôme de Lalande (1732-1807) who wrote all the entries related to astronomy in *L'Encyclopédie Méthodique: Mathématiques.* The shift in language between the two encyclopedias is striking – while "graphique" (or any of its other forms) was never used in the other articles of *L'Encyclopédie*, Lalande's articles from *L'Encyclopédie Méthodique* referred several times to "opération graphique" (especially when describing the

4. *Encyclopédie Méthodique. Mathématiques* (Paris: Chez Panckoucke, 1784), vol. 2, 149.

construction of a dial) and to a "méthode graphique, par laquelle on peut trouver sans calcul, avec la règle et le compas, les phases d'une *éclipse de soleil* à deux ou trois minutes près, ce qui est très-suffisant pour prédire des *eclipses* en différens pays de la terre, & pour tous les usages de l'astronomie".[5] With very few exceptions, all the articles that used "graphique(s)" were signed by Lalande.[6]

Lalande's reference to the astronomer Nicolas Louis de La Caille (1713-1762) could not have been casual as it was La Caille who first made extensive use of the term "opérations graphiques" starting with a *mémoire* presented to the *Académie royale des sciences* in 1744. In this *mémoire* La Caille proposed a more precise method for determining the phases of a solar eclipse or the occultations of fixed stars by the moon. He compared his method of determining the phases of an eclipse "par le calcul" to Domenico Cassini's determinations "par des opérations graphiques".[7] This characterization is not to be found in Cassini's own descriptions of the method who only emphasized its originality and referred to its geometrical constructions. La Caille chose "graphique" instead of the common term "géométrique" because he drew a distinction between the geometrical principles underlying his method, and the actual means of computation. Both La Caille and Cassini employed a geometrical approach that projected the eclipse on a convenient plane. But while astronomers generally used only orthographic and stereographic projections, La Caille "reduced to a general method the computation of all the imaginable projections of the sphere".[8] He described his goal as finding "la manière la plus géométrique, la plus directe, de faire ce calcul & cette projection".[9] Astronomers who substituted the "boring computations [calculs ennuyeux]" with "opérations graphiques" could only

5. *Encyclopédie Méthodique. Mathématiques*, vol. 1, 592.

6. "graphique" was also used as "opération graphique" in the entry on "Trigonométrie" by d'Alembert and in a preliminary note by Prony. The term might have had a ring of novelty as it was mistakenly replaced by "géographiques" in Condorcet's entry on "méthode": "L'astronomie conserve des descriptions géographiques & des constructions géométriques"; the error was pointed out in the errata, ibid., vol. 2, 390-391; vol. 3, 176.

7. Nicolas Louis de La Caille, "Sur le calcul des projections en général, et en particulier sur le Calcul des Projections propres aux Eclipses du Soleil & aux Occultations des Etoiles fixes par la Lune," *Histoire de l'Académie royale des sciences. Année MDCCXLIV*, 1748, 205.

8. Ibid., 193.

9. Ibid., 205.

identify the time of an eclipse with a precision of half a minute – an insufficient result when compared to the precision of the observations that could determine the time up to a second.[10]

In 1740 La Caille was appointed to teach mathematics at the Collège Mazarin and shortly after he started publishing his lessons.[11] Several of his textbooks employed the distinction between "calcul" (usually referring to "le calcul de trigonométrie") and "opérations graphiques". *Leçons élémentaires d'astronomie géométrique et physique* (1st ed. 1746) described how to "determine graphically all the circumstances of the occultation of a satellite" or how to "determine graphically the phases of a solar eclipse". Though these "graphical operations" were only precise up to a few minutes, they were sufficient if one wanted to prepare for an eclipse or to use this first rough estimate within a more precise "determination through computations".[12] In *Leçons élémentaires d'optique* (1st. ed 1750) a solution could be found "par la Trigonométrique ou par une opération graphique".[13] In the *Nouveau traité de navigation* (1760), a treatise written by Pierrre Bouguer but expended after his death by La Caille, a special section was added which supplemented the trigonometric calculations with graphical operations because

> Le calcul trigonométrique est plus propre à donner de la précision aux opérations précédentes; mais il est plus long & plus susceptible d'erreurs causées par faute d'attention. Le Pilote zélé pour son art & pour son devoir, doit tâcher de faire tous ses calculs par ces deux méthodes successivement. Les opérations graphiques lui donneront son résultat en très-peu de temps, & serviront à guider le calcul trigonometrique qui lui donnera plus d'exactitude dans ce résultat.[14]

Published in several editions, La Caille's textbooks were popular and influential.[15] With

10. The precision of the "opérations graphiques" was diminished by several simplifying assumptions that were required for the graphical representation (e.g. the Earth is immobile during the eclipse); also, in certain cases the lines were almost parallel and their intersection point could not be precisely determined.

11. René Taton, ed., *Enseignement et diffusion des sciences en France au XVIIIe siècle* (Paris: Hermann, 1964), 144-145.

12. Nicolas Louis de La Caille, *Leçons élémentaires d'astronomie, géometrique et physique* (Paris: chez H. L. Guerin & L. F. Delatour, 1755), 349.

13. Nicolas Louis de La Caille, *Leçons élémentaires d'optique* (Paris: chez H. L. Guerin & L. F. Delatour, 1756), 162.

14. Pierre Bouguer and Nicolas Louis de La Caille, *Nouveau traité de navigation: contenant la théorie et la pratique du pilotage* (Paris: chez H. L. Guerin & L. F. Delatour, 1760), 258.

15. *Leçons élémentaires d'astronomie géométrique et physique* was published in 1746, 1755, 1761, 1780 (augmented by Lalande); it was translated into Latin (1759, 1765); English (1750). *Leçons élémentaires d'optique* was

them spread not only La Caille's mathematical methods but also his way of talking. La Caille's discussion of trigonometry inspired Antoine-René Mauduit, the professor of mathematics at the Collège Royal and l'Académie royale d'Architecture, who also employed La Caille's distinction in his *Principes d'astronomie sphérique* (1765) and *Leçons de géometrie* (1773; 1790).[16] In the preface to the *Principes d'astronomie sphérique* Mauduit opposed the use of logarithms to

> constructions géométriques, par lesquelles, avec la regle & le compas, on peut aisément trouver toutes les parties d'un triangle à résoudre. On les a nommées *graphiques*, parce qu'on y emploie différentes projections ou développements du triangle.[17]

Because the term "graphique" was still novel it was often followed by an explanation, such as that in La Caille's *Nouveau traité de navigation*: "opérations graphiques, c'est-à-dire, des constructions des figures faites sur le papier avec la Règle & le compas".[18] Mauduit used the term almost interchangeably with "géométrique" referring to "les solutions géométriques ou graphiques" and titling a chapter "De la Résolution Graphique ou Géométrique des Triangles sphériques quelconques". This indecision of choosing or distinguishing between the two terms can also be encountered in François Bedos de Celles's *La gnomonique pratique* (1st ed. 1760; 2nd ed. 1774): a section titled in the first edition "Maniere géométrique de tracer le Cadran horisontal" was renamed in the second edition "Maniere graphique ou géométrique de tracer le Cadran horisontal". The same change was also carried out in the Preface which now advertised the use of "deux méthodes, l'une est géométrique ou graphique; elle opère avec la simple règle & le compas; l'autre s'exécute par le calcul", or "Nous donnerons deux manieres de le décrire; l'une graphique ou géométrique, c'est-à-dire, par la regle & le compas, & l'autre par le calcul".[19]

published in 1750, 1756, 1802 (new edition), 1807, and 1810; it was translated in Latin (1757) – Taton, *Enseignement et diffusion des sciences en France au XVIIIe siècle*, 157.

16. For Mauduit's teaching see ibid., 277, 283-285, 452.

17. Antoine René Mauduit, *Principes d'astronomie Sphérique* (Paris: chez H. L. Guerin & L. F. Delatour, 1765), iii.

18. Bouguer and La Caille, *Nouveau traité de navigation*, 210.

19. François Bedos de Celles, *La gnomonique pratique ou l'art de tracer les cadrans solaires avec la plus grande précision*, 2nd ed. (Paris: Chez Delalain, 1774), xiii, 78. The same passages without "graphique" can be found in

Though a pupil of Joseph-Nicolas Delisle, Jérôme de Lalande came into frequent contact with La Caille whom he greatly admired and "gloried in having been his disciple".[20] Lalande further popularized the use of "graphical operation" in his *Astronomie* (1764, 1st ed.), which was the standard textbook in the field. For example, Lalande showed how one could find the parallax of the transit of Venus and Mercury "without computation" using instead "a very simple graphical operation and that with a precision of tenths of seconds". Otherwise,

> the work is so long by the other methods that most astronomers neglect to compute their observations, to make use & to draw some results, because they are not aware of the way I will explain how to execute the most difficult part of this work very quickly & with a sufficient precision.[21]

The Italian astronomer Antonio Cagnoli, a former student of Lalande and an avid reader of his *Astronomie*, published in 1786 a treatise on spherical trigonometry that was praised by Lalande as "le meilleur ouvrage qu'on ait fait sur la trigonometrie, et sur son application a l'astronomie".[22] In his *Traité de trigonométrie rectiligne et sphérique* (1786) Cagnoli remarked that "les opérations graphiques ne pouvant jamais atteindre à l'exactitude du calcul".[23] In a chapter on the solution of spherical triangles with the "la regle et compas" he added

> L'invention des logarithmes a rendu la résolution des triangles si précise et si prompte, que les Géomètres ne tiennent plus aucun compte, pour ainsi dire, des opérations graphiques, sujettes à des erreurs de plusieurs minutes, quelque attention, quelque soin qu'on y apporte. Cependant comme cette espèce de solutions peut être utile dans certains cas qui n'exigent pas une exactitude rigoureuse, ou pour ceux qui ne sont pas familiarisés avec le calcul, je ne laisserai pas de les indiquer en peu de mots, et sans faire usage de la méthode des projections, pour mettre ces solutions à la portée d'un plus grand nombre de Lecteurs.[24]

the first edition, François Bedos de Celles, *La gnomonique pratique ou l'art de tracer les cadrans solaires avec la plus grande précision*, 1st ed. (Paris: Briasson, 1760), iv, 59.

20. Ian Stewart Glass, *Nicolas-Louis de La Caille: Astronomer and Geodesist* (Oxford: Oxford University Press, 2012), 125, 144.

21. Joseph Jérôme de Lalande, *Astronomie* (Paris: Desaint & Saillant, 1764), vol. 2, 769-770.

22. The treatise was translated the same year into French. Joseph Jérôme de Lalande, *Bibliographie astronomique avec l'histoire de l'astronomie depuis 1781-1802* (Paris: la Republique, 1803), 598. For more on Cagnoli see Calogero Farinella, "Da Montesquieu a Lalande. Antonio Cagnoli e una specola privata del Settecento," *Studi Settecenteschi*, no. 17 (1997): 227–264.

23. Antonio Cagnoli, *Traité de trigonométrie rectiligne et sphérique* (Paris: Chez Didot fils aîné, 1786), 168.

24. Ibid., 288.

While "graphique" was not mentioned in the third edition of the *Dictionnaire de l'Académie françoise* (1740), it did appear in the fourth edition from 1762 where it was defined as:

> GRAPHIQUE. adj. de t. g. Terme didactique. Il se dit particulièrement des descriptions, des opérations, qui, au lieu d'être simplement énoncées par le discours, sont données par une figure. *Description graphique d'une éclipse de Soleil, de Lune, &c. Représentation graphique du passage de Vénus sur le disque du Soleil. Opération graphique.*
>
> GRAPHIQUEMENT. adv. Il se dit en Astronomie, Des choses dont on donne la peinture, ou une description graphique.[25]

1.2 *Operationes Graphicas* in Newton's *Principia*

While it was mainly through La Caille's and Lalande's articles and textbooks that the term "graphique" was popularized in French scientific literature, the use of the term had some precedents, especially in Latin. The terms *graphice*/*graphicè*, *Graphicarum* were used several times in Newton's *Philosophiæ Naturalis Principia Mathematica* (1st ed. 1687; 2nd ed. 1713; 3rd ed. 1726) in conjunction with a method of determining from three given observations the trajectory of a comet moving in parabola (Book 3, Proposition 41).[26] The problem had greatly troubled Newton. As late as June 1686, with the second book of the *Principia* already completed in the previous summer, Newton complained to Halley that

> The third [book] wants the Theory of Comets. In Autumn last I spent two months in calculations to no purpose for want of a good method, which made me afterwards return to the first Book & enlarge it with divers Propositions some relating to Comets others to other things found out last Winter. The third I now designe to suppress. Philosophy is such an impertinently litigious Lady that a man had as good be engaged in Law suits as have to do with her. I found it so formerly & now I no sooner come near her again but she gives me warning. The two first books without the third will not so well beare the title of Philosophiae naturalis Principia Mathematica & therefore

25. *Dictionnaire de l'Académie française* (Paris: B. Brunet, 1762), 836.

26. Isaac Newton, *Philosophiae Naturalis Principia Mathematica*, 1st ed. (London: Jussu Societatis Regiæ ac Typis Josephi Streater. Prostat apud plures Bibliopolas, 1687), 271, 494, 497, 509; Isaac Newton, *Philosophiae Naturalis Principia Mathematica*, 2nd ed. (Cambridge: Cornelius Crownfield, 1713), 273, 458, 475; Isaac Newton, *Philosophiae Naturalis Principia Mathematica*, 3rd ed. (London: William & John Innys, 1726), 263, 499. The variations across the three editions can be followed in Isaac Newton, *Philosophiae Naturalis Principia Mathematica*, ed. I. Bernard Cohen and Alexandre Koyré (Cambridge, Mass.: Harvard University Press, 1972), 392, 716f., 732, 748.

> I had altered it to this De motu corporum libri duo: but upon second thoughts I retain the former title.[27]

Several of Newton's failed attempts have been preserved.[28] The first manuscript of *De Motu Corporum* (Autumn 1684) included a brief *scholium* after one of the problems that described a way of determining the orbits of comets and their periods of revolution. The method was not applied to any observations, and only noted that "in the case computations prove troublesome to astronomers, it will be enough to determine all these things ~~by a geometrical procedure~~ by the description of lines [~~praxin Geometricam~~ per descriptionem linearum]".[29] The revised version of *De Motu* (1684-1685) presented a new method which employed a graphical determination followed by an arithmetical correction – "[a]ll these things I accomplish first graphically [graphicè] by a rough, swift operation, then graphically still but with greater care, and lastly by a numerical computation".[30] No computations were appended probably because the method proved to be defective. Newton's successful method was presented in Proposition 41, Book 3 of the *Principia* which opened with the remark:

> Having tried many approaches to this exceedingly difficult problem, I devised certain problems [i.e., propositions] in book 1 which are intended for its solution. But later on, I conceived the following slightly simpler solution.[31]

In the *Principia* Newton explained that he found the position of the comet using "partially arithmetical and partially graphical operations [per operationes partim Arithmeticas partim Graphicas]".[32] Because this method was only an approximation, the next proposition dealt

27. Letter to Edmund Halley on the Doctrine of Projectiles and Motions of the Heavens (1686).
28. Isaac Newton, *The Mathematical Papers of Isaac Newton,* ed. Derek Thomas Whiteside (Cambridge, London: Cambridge University Press, 1967), 5, 524-531; Newton, *The Mathematical Papers of Isaac Newton,* vol. 6, 57-61, 483-507.
29. Newton, *The Mathematical Papers of Isaac Newton,* vol. 6, 60-61.
30. Ibid., vol. 6, 495.
31. Isaac Newton, *The Principia: Mathematical Principles of Natural Philosophy*, trans. I. Bernard Cohen and Anne Miller Whitman (Berkeley: University of California Press, 1999), 901.
32. Newton, *Philosophiae Naturalis Principia Mathematica*, 494; Newton, *Philosophiae Naturalis Principia Mathematica*, 458; Newton, *Philosophiae Naturalis Principia Mathematica*, 499. Newton commonly opposed arithmetical and graphical operations. For example, he described the activity of an arithmetician as a substitution of numbers for lines, multiplication for "drawing into a line [ductu in lineam]", division for "application to a line [applicatione ad lineam]" and vice versa, see Newton, *The Mathematical Papers of Isaac Newton,* vol. 7, 406f.

with its correction, appropriately titled "To correct a comet's trajectory that has been found graphically [Trajectoriam Cometae Graphice inventam corrigere]".[33]

> I made all these determinations graphically [determinavi graphice] by a scale of equal parts and by chords of angles, taken from the table of natural sines, constructing a fairly large diagram [schema], that is, one in which the semidiameter of the earth's orbit (of 10,000 parts) was equal to 16 1/3 inches of an English foot.[34]

Newton's use of "graphice" was connected to a subtle but clear distinction that he drew between geometry (or rational mechanics) and mechanics (or practical mechanics). This distinction was developed in the Preface to the *Principia*, which as Niccolò Guicciardini has argued, "must be read as part of Newton's reflections on the scope and methods of geometry".[35] In the Preface it was remarked that

> the description [descriptiones] of straight lines and circles, which is the foundation of *geometry*, appertains to *mechanics*. *Geometry* does not teach how to describe these straight lines and circles, but postulates such a description. For *geometry* postulates that a beginner has learned to describe lines and circles exactly before he approaches the threshold of *geometry*, and then it teaches how problems are solved by these operations [operationes]. To describe straight lines and to describe circles are problems, but not problems in *geometry*. *Geometry* postulates the solution of these problems from *mechanics* and teaches the use of the problems thus solved.[36]

This was the distinction observed by Newton in the *De Motu Corporum* (Autumn 1684), discussed above, when he crossed "by a geometrical procedure [praxin Geometricam]" to replace it with "by description of lines [per descriptionem linearum]". The distinction was further explored in a manuscript on geometry from the 1690s. After inserting the previous passage from the Preface of the *Principia*, Newton continued with:

33. "Graphically" (*graphice*) was only used in the first two editions, see Newton, *Philosophiae Naturalis Principia Mathematica*, 509; Newton, *Philosophiae Naturalis Principia Mathematica*, 475; in the third edition the title was modified to "Inventam cometae trajectoriam corrigere", see Newton, *Philosophiae Naturalis Principia Mathematica*, 518.

34. Newton, *The Principia*, 909; Newton, *Philosophiae Naturalis Principia Mathematica*, 499. Newton also referred to derivations "by constructions of figures, and by calculations [per constructiones figurarum & computationes]", see Newton, *Philosophiae Naturalis Principia Mathematica*, 499.

35. Niccolò Guicciardini, *Isaac Newton on Mathematical Certainty and Method* (Cambridge: MIT Press, 2009), 300, for a discussion of the Preface see ibid., 293-305.

36. Newton, *Philosophiae Naturalis Principia Mathematica*, Praefatio; Newton, *The Principia*, 381-382.

> Both the genesis of the subject-matter of geometry, therefore, and the fabrication of its postulates pertain to mechanics. Any plane figures executed by God, nature or any technician you will are measured by geometry in the hypothesis that they are exactly constructed. A technician is required and postulated to have learnt how to describe straight lines and circles before he may begin to be a geometer. And it consequently does not matter how ~~by what mechanical means~~ they shall be described. Geometry does not posit modes of description: we are free to describe them [plane figures] by moving rulers around, using optical rays, taut threads, compasses, the angle given in a circumference, points separately ascertained, the unfettered motion of a careful hand, or finally any mechanical means whatever. Geometry makes the unique demand that they are described exactly....The purpose of mechanics is to form and move magnitudes in appointed figures and motions: that of geometry is neither to form nor move magnitudes, but merely to measure them. Geometry forms nothing except modes of measuring. It postulates a technician who knows how to form straight lines and circles, and teaches him how through their formation appointed magnitudes are to be measured.[37]

Geometrical solutions were "those accomplished by the ~~geometrical~~ mechanical operations of drawing lines and constructing figures accurately by dint of postulates".[38] Thus, for Newton "graphical operations" were just a subset of the larger category of "mechanical operations" (which could also include the use of optical rays, taut threads or any mechanical means), and were distinguished from geometry which only postulated the existence of exact descriptions.

The Latin "graphice", as used by Newton, was not fully translatable in English. In the first English translations of the *Principia* (1729) the term was rendered as "by scale and compass".[39]

37. Newton, *The Mathematical Papers of Isaac Newton,* vol. 7, 289ff.

38. ibid., vol. 8, 173. "In geometrical theory it is allowable to imagine that any cones or any conic section you wish are given and that these are cut by any given circles whatever, and we are thence permitted to form certain ideas of the quantities sought; and in the case of imaginative notions or ideas or ideal constructions of this sort the definitions of cones and conic sections suffice, without postulates. These constructions are, to be sure, theorems rather than solutions of problems, or, to speak more truthfully, they are porisms having a middle nature between theorems and problems", ibid., vol. 8, 183. Or again "Definition of figures suffice for discovering theorems about them; not, however, for describing the figures themselves. They are indeed had in thought through their definitions, but no so in physical reality. The straight line and circle are not had in geometry through their definitions. That they may so be had, geometers postulated to describe them. And similarly, that there may be had a cone to be cut, its description ought either to be postulated or taught through postulates: otherwise, the construction of the problems by means of conic sections will not be geometrical. Moreover, to describe a cone and to cut it, once described, by a given plane are difficult operations and ones liable to error, and for that reasons today's geometers teach how to describe a conic in the place without a construction of solids", ibid., vol. 8, 209.

39. "All which I determin'd by scale and compass ... partially by arithmetical operations, and partially by scale and compass", Isaac Newton, *The Mathematical Principles of Natural Philosophy*, trans. Andrew Motte, 2 vols. (London: Benjamin Motte, 1729), vol. 2, 329.

Only the instance of "graphice" from Proposition 10, Book 2 was translated as "graphically, or by scale and compass, collect the lengths AK, Ak", suggesting that the mathematical meaning of the term "graphically" would have been unfamiliar to many of the English readers of the *Principia*.[40] This must have been the case, as 17th and early 18th English dictionaries defined "graphical" solely as:

> **Graphical** (*graphicus*) cunningly wrought, perfect, excellent. – *Glossographia* (1656)
>
> Graphical, *curiously described*, *express*, or *accurate*. – *A new English dictionary* (1702)
>
> GRAPHICAL [of *γραφικός, Gr.*] curiously described, or after the Life, exact. – *Dictionarium Britannicum* (1730)
>
> GRAPHICE [*γραφική, Gr.*] the Art of Painting, Limning or Drawing. – *Dictionarium Britannicum* (1730)
>
> GRAPHICAL. *adj.* [γραφω.] Well delineated. – *Dictionarium Britannicum* (1730)
>
> GRAPHICALLY. *adv.* [from *graphical.*] In a picturesque manner; with good description or delineation. – *A dictionary of the English language* (1755)

Furthermore, it is doubtful that many of Newton's contemporaries would have appreciated his careful distinction between the *geometrical* and the *mechanical* (which included the *graphical*). John Flamsteed, the Astronomer Royal at the Greenwich Observatory and a correspondent of Newton, published in 1680 a method of finding the phases and times of an eclipse "Geometrically, by Scale and Compasses, without further Calculation" or "either by Calculation or Construction".[41] Flamsteed's "geometrical" method "by Scale and Compasses" and by "Construction" was very similar in nature to those of Cassini, La Caille or Lalande.

2 GRAPHICAL GEOMETRY

While in Britain Newton's distinction between the *geometrical* and the *graphical* was not immediately expressible in vernacular, in France the new term was adopted with greater ease, probably because "graphique" did not possess any entrenched associations or definitions. The

40. Newton, *The Mathematical Principles of Natural Philosophy*, vol. 2, 41.
41. John Flamsteed, *The Doctrine of the Sphere* (London: Printed by A. Godbid and J. Playford, 1680), Contents.

Emilie du Châtelet's translation of the *Principia* (completed in 1749 but only published in 1756) made use of "graphique" and "graphiquement" without appealing to any extra explanations.[42] Though the term started spreading through the work of French astronomers, its use remained quite singular throughout the 18th century. When the term is encountered outside a work of astronomy, it is only used a few times in passing. The use of "graphique" was only broadened at the beginning of the 19th century, as can be observed if one follows, for example, the titles of treatises on gnomonics or dial making, an art which involved various geometrical constructions and traces. Such a list is particularly instructive because it allows one to immediately identify the keywords that carried weight for such publishing endeavors. As Table 2.1 shows, the titles of gnomonics treatises advertised a science, art or method that was universal, general or practical and which exposed the tracing or construction of a dial. Only in 1815 was published the first treatise with "graphique" in the title. The pattern is even stronger if one considers the content of these treatises which did not make use of "graphique" to describe their geometrical constructions.[43] This claim is also confirmed by the article on "Gnomonique" from *l'Encyclopédie* which only referred to "une méthode géométrique de tracer des lignes horaires au moyen de certains points déterminés par observation" or how to "tracer géométriquement un cadran horisontal" etc.[44] The new visibility of "graphique" was a direct consequence of the redefinition and articulation of the object of engineering education at L'École polytechnique.

2.1 Géométrie graphique ou descriptive

L'École polytechnique was established in March 1794, and was known in its first year of existence as l'École centrale des travaux publics.[45] The organization and program of the school

42. Isaac Newton, *Principes mathématiques de la philosophie naturelle*, trans. Émilie du Châtelet, 2 vols. (Paris: Desaint & Saillant, 1756), vol. 1, 280-281; vol. 2, 134.
43. The sole exception was François Bedos de Celles's *La gnomonique pratique* (2nd ed. 1774) which is discussed above.
44. Diderot and D'Alembert, *Encyclopédie ou Dictionnaire raisonné des sciences, des arts et des métiers*, vol. 7, 725-726.
45. For the debut of the school see Janis Langins, *La République avait besoin de savants* (Paris: Belin, 1987).

Table 2.1
A list of the major 18th-century French treatises on dial making. Though this subject relied heavily on graphical methods, “graphique” (as a historical category) was first used in a title in 1815.

1673	*Traité de gnomonique, ou de la construction des cadrans sur toute sorte de plans*	Jacques Ozanam
1682	*La gnomonique, ou, L'art de tracer des cadrans ou horloges solaires sur toutes sortes de surfaces, par differentes pratiques : avec les demonstrations geometriques de toutes les operations*	Philippe de La Hire
1698	*La gnomonique, ou, Methodes universelles pour tracer des horloges solaires ou cadrans sur toutes sortes de surfaces*	Philippe de La Hire
1701	*La Gnomonique universelle ou la science de tracer le cadrans solaires*	Paul Richer
1720	*La gnomonique, ou l'on donne par un principe general la maniere de faire des cadrans sur toutes sortes de surfaces, et d'y tracer les heures astronomiques*	Jacques Ozanam
1742	*La gnomonique ou L'art de faire les cadrans*	D.-F. Rivard
1744	*La Gnomonique, ou la Science des cadrans*	Pierre Blaise
1760	*La gnomonique pratique, ou L'art de tracer les cadrans solaires avec la plus grande précision : par les meilleures méthodes, mises a la portée de tout le monde*	F.B. de Celles
1773	*Gnomonique mise à la portée de tout le monde, ou Méthode simple et aisée pour tracer des cadrans solaires*	Joseph-Blaise Garnier
1780	*Méthode nouvelle et générale pour tracer facilement des cadrans solaires sur toutes surfaces planes, en situation quelconque, sans calcul ni embarras d'instrumens : par un seul problème géométrique qui fait connoître l'axe & la soustylaire, la latitude du lieu, la situation du plan, la déclinaison du soleil & le parallèle du jour lors de l'opération*	J.G. de La Prise
1781	*Méthode nouvelle et générale pour tracer facilement des cadrans solaires sur toutes surfaces planes*	J.G. de La Prise
1782	*La gnomonique théori-pratique, ou les principes de géométrie, de trigonométrie rectiligne et sphérique; sur lesquels est fondé l'art de tracer les cadrans sciatériques ou solaires*	Abbé Dulac
1789	*Petit traité de gnomonique, ou l'art de tracer les cadrans solaires*	C. R. Polonceau
1806	*Gnomonique élémentaire*	[Charles Delezenne]
1812	*Gnomonique analytique, ou solution par la seule analyse, de ce problème général: trouver les intersections des cercles horaires avec une surface donnée*	Joseph Mollet
1815	*Gnomonique graphique, ou Méthode simple et facile pour tracer les cadrans solaires sur toute sorte de plans, sans aucun calcul, et en ne faisant usage que de la règle et du compas suivie de quelques problèmes curieux, relatifs aux surfaces sphérique et cylindrique*	Joseph Mollet

were chiefly designed by Gaspard Monge (1746-1818) who centered its teaching around the new subject of descriptive geometry. While the name "descriptive geometry" was only employed after 1793, its content was based on a long tradition of applying geometrical principles for constructing traces to be used in stone and wood cutting. Monge had perfected this subject as the professor of mathematics at l'École royale du génie de Mézières which, along with l'École royale des Ponts et Chaussées, was the main engineering school of the 18th century.[46] Under the lead of its founding director, Nicolas de Chastillon, l'École royale du génie de Mézières had reformed its teaching in the 1760s by transforming a set of isolated techniques particular to the activity of carpenters and joiners [appareilleurs] into a model for engineering drawing.[47] Because the reform at Mézières extended mainly to its practical disciplines, it was only at L'École polytechnique that the practice of engineering drawing was to be correlated with that of theoretical mathematical instruction.

Though the courses and the instructors of l'École centrale des travaux publics overlapped with those of l'École du génie de Mézières and l'École des Ponts et Chaussées, the new school was designed to break with the privileges and corporatism of the institutions of *l'Ancien Régime*. Its revolutionary aims were also implemented through the organization of courses. In a manuscript probably dating from the months of July 1794 and attributed to Monge, the teaching of mathematics was divided in two parts: 1. analysis and its application to geometry and mechanics; 2. "the graphical description of objects" which was comprised of descriptive geometry and drawing:

> Certains objets sont susceptibles de définition rigoureuse; leur description graphique exige l'emploi de la règle et du compas; l'art de les décrire peut être appelé la *géométrie descriptive*. Certains autres objets ont des formes trop composées pour être susceptibles de définition par im-

46. On l'École royale du génie de Mézières see René Taton, "L'école royale du génie de Mézières," in *Enseignement et diffusion des sciences en France au XVIIIe siècle*, ed. René Taton (Paris: Hermann, 1964), 559–615; Bruno Belhoste, Antoine Picon, and Joël Sakarovitch, "Les exercices dans les écoles d'ingénieurs sous l'Ancien Régime et la Révolution," *Histoire de l'éducation* 46, no. 1 (1990): 72-90.

47. Belhoste, Picon, and Sakarovitch, "Les exercices dans les écoles d'ingénieurs sous l'Ancien Régime et la Révolution," 75.

itation. C'est l'art du *dessin*.[48]

The study of descriptive geometry extended over the whole three years of the engineering program. The first year of the course dealt with the general rules and methods of descriptive geometry and their application to a series of special topics such as the trace of stone and wood cutting; the rigorous determination of shadows in a drawing; the practice of linear perspective; surveying [lever des plans et des cartes]; the description of machines. In the second year students were introduced to the general principles of architecture and their application to the construction of roads, bridges, canals or mines. The third year was dedicated to studying the construction of ports and fortifications (see Fig. 2.2).

It was not the content of the courses that was novel but rather the lines around which they were brought together. The grouping of stereotomy, architecture and fortifications under the heading of "descriptive geometry" reflected the aims of the school to unify the teaching of military and civil engineering. The connection between geometry and drawing under the heading "the graphical description of objects" was not purely formal. It was meant to make out of mathematical instruction a bridge between engineers, artists and savants rather than a distinction of the engineering corp.

Historians agree that "descriptive geometry" was first used by Monge in September 1793 in a project for "écoles secondaires pour artisans et ouvriers".[49] However, it is most likely that the term did not originate with Monge as it has sometimes been suggested.[50] Several precedents stand out. In a project on the organization of public instruction presented to the

48. Archives de l'École polytechnique, 1,1, carton n°1 reproduced in Bruno Belhoste, "De l'Ecole des ponts et chaussées à l'Ecole centrale des travaux publics," *Bulletin de la Sabix. Société des amis de la Bibliothèque et de l'Histoire de l'École polytechnique*, no. 11 (1994): Document 4.

49. Bruno Belhoste and René Taton, "L'invention d'une langue des figures," in *L'Ecole normale de l'an III, Leçons de mathématiques*, ed. Jean Dhombres (1992), 371–400.

50. Cf. Joël Sakarovitch, "La géométrie descriptive, une reine déchue," in *La formation polytechnicienne: 1794-1994*, ed. Bruno Belhoste, Amy Dahan-Dalmédico, and Antoine Picon (Paris: Dunod, 1994), 78 n4; Bruno Belhoste, *La formation d'une technocratie: l'École polytechnique et ses élèves de la Révolution au Second Empire* (Paris: Belin, 2003), 267.

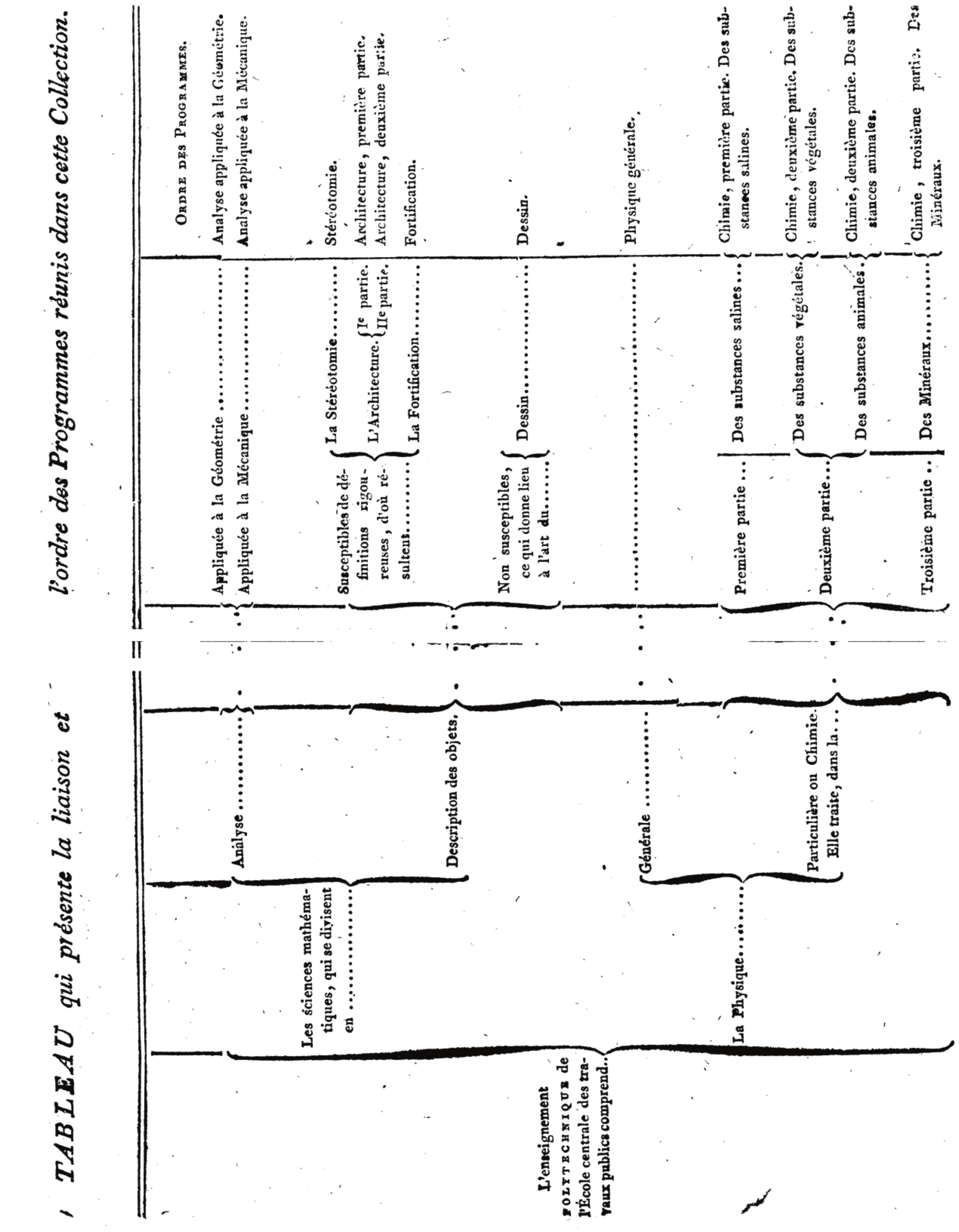


Figure 2.2
Source: *Programmes de l'enseignement polytechnique de l'école centrale des travaux publics* (Impr. nationale, 1794).

National Assembly in April 1792, Condorcet proposed a course on "la géométrie graphique, ou la manière d'arriver avec la règle et le compas aux résultats de l'arithmétique, de la géométrie, de la perspective, etc."[51] The course was to be taught by either the instructor of military art or the instructor of "general principles of arts and crafts" and was part of the class on the "application of the sciences to the arts" of a series of institutes specially designed for the perfection of the industry.[52] The term was further used in a memoir on public instruction that was drafted in July 1793 by the Bureau de Consultation des Arts et Métiers, but most probably written by Lavoisier who was a member of the bureau.[53] The program, which borrowed the structure and much of the terminology of Condorcet, imagined that courses would begin with

> l'exposition des principes élémentaires de la géométrie graphique. Le professeur s'attachera à résoudre tous les problêmes relatifs à cette science, par la règle et par le compas; il donnera des idées précises de la manière dont se forment les surfaces et les solides, dont ils se mesurent; il apprendra à rapporter à un plan toutes les parties d'un objet, à en faire la projection: de là les règles de la perspective, de la taille des pierres, de l'art de la charpente, de ce qu'on appelle le trait.[54]

Furthermore, "les écoles élémentaires des arts et d'économie sociale" were supposed to teach "la géométrie descriptive ou graphique, la stéréotomie, les principes de la composition des machines, l'évaluation de leurs effets, et tout ce qui est relatif aux arts considérés dans leurs rapports géométriques et mécaniques".[55] In a manuscript report titled "Mémoire sur l'éducation" (October 1791 - June 1792), J. H. Hassenfratz – a former student and close collaborator of Monge – proposed the creation of "une chaire d'enseignement des mathématiques graphiques".[56]

51. Jean-Antoine-Nicolas de Caritat marquis de Condorcet, *Rapport et projet de décret sur l'organisation générale de l'instruction publique: présenté à l'Assemblée Nationale, au nom du Comité d'Instruction publique* (Paris: de l'Imprimerie Nationale, 1793), 86.

52. Antoine Léon, *La Révolution française et l'éducation technique* (Paris: Société des études robespierristes, 1968), 121-126.

53. For the identification of Lavoisier as the author see Emmanuel Grison, *L'étonnant parcours du républicain J.H. Hassenfratz (1755-1827): du faubourg Montmartre au Corps des Mines* (Paris: Presses de l'Ecole des Mines, 1996), 189-190.

54. Antoine Laurent Lavoisier, *Œuvres*, vol. 6 (Paris: Imprimerie impériale, 1893), 524.

55. Ibid., 541.

56. Grison, *L'étonnant parcours du républicain J.H. Hassenfratz (1755-1827)*, 176-178.

The above sources suggest that "descriptive geometry" was probably a variation on "graphical geometry". The term was used by Condorcet to refer not to the Mézières type of practical geometry but rather to the geometry practiced with scale and compass as he translated his own term. The phrasing closely resembled a passage in Rousseau's *Émile* (1762) which must have been known to most educational reformers:[57]

> La géométrie n'est pour mon élève que l'art de se bien servir de la règle & du compas; il ne doit point la confondre avec le dessin, où il n'emploiera ni l'un ni l'autre de ces instrumens. La règle et le compas seront enfermés sous la clef, et l'on ne lui en accordera que rarement l'usage et pour peu de temps, afin qu'il ne s'accoutume pas à barbouiller: mais nous pourrons quelquefois porter nos figures à la promenade, & causer de ce que nous aurons fait ou de ce que nous voudrons faire.[58]

Strictly defined, descriptive geometry referred to a method "of representing on a drawing page with only two dimensions all three dimensional objects of nature" [explain the source].[59] Opposed to other methods (such as perspective or axonometric projections), descriptive geometry employed a correlated double projection on two planes which preserved all the geometrical aspects of an objects such as size and shape (see Fig. 2.3). This method of representation was more abstract than regular perspective and required a greater time of adjustment. To develop a similar level of intuition one often added shadows, a technique that was greatly developed and promoted by Monge.

In Monge's program for L'École polytechnique two months were allocated to learning the rules and principles of descriptive geometry, after which students were supposed to study its practical applications. After the general lessons students divided in groups of twenty were assigned to special classrooms [salles particulières] where they executed "the graphical operations that were explained in the general lesson".[60] This division purposefully mirrored the teaching of the physical and chemical sciences for which students used special laboratories

57. For the relation between Condorcet's program and Rousseau's ideas see Jean Bloch, *Rousseauism and Education in Eighteenth-Century France* (Institut et musée Voltaire, 1995), 132-135.

58. Jean-Jacques Rousseau, *Émile, ou de l'éducation*, vol. 1 (Amsterdam: Chez Jean Néaulme, 1762), 382-383.

59. Gaspard Monge, *Géométrie descriptive: leçons données aux écoles normales, l'an 3 de la République* (Paris: Baudouin, 1799), 6.

60. Belhoste, "De l'Ecole des ponts et chaussées à l'Ecole centrale des travaux publics," Document 4.

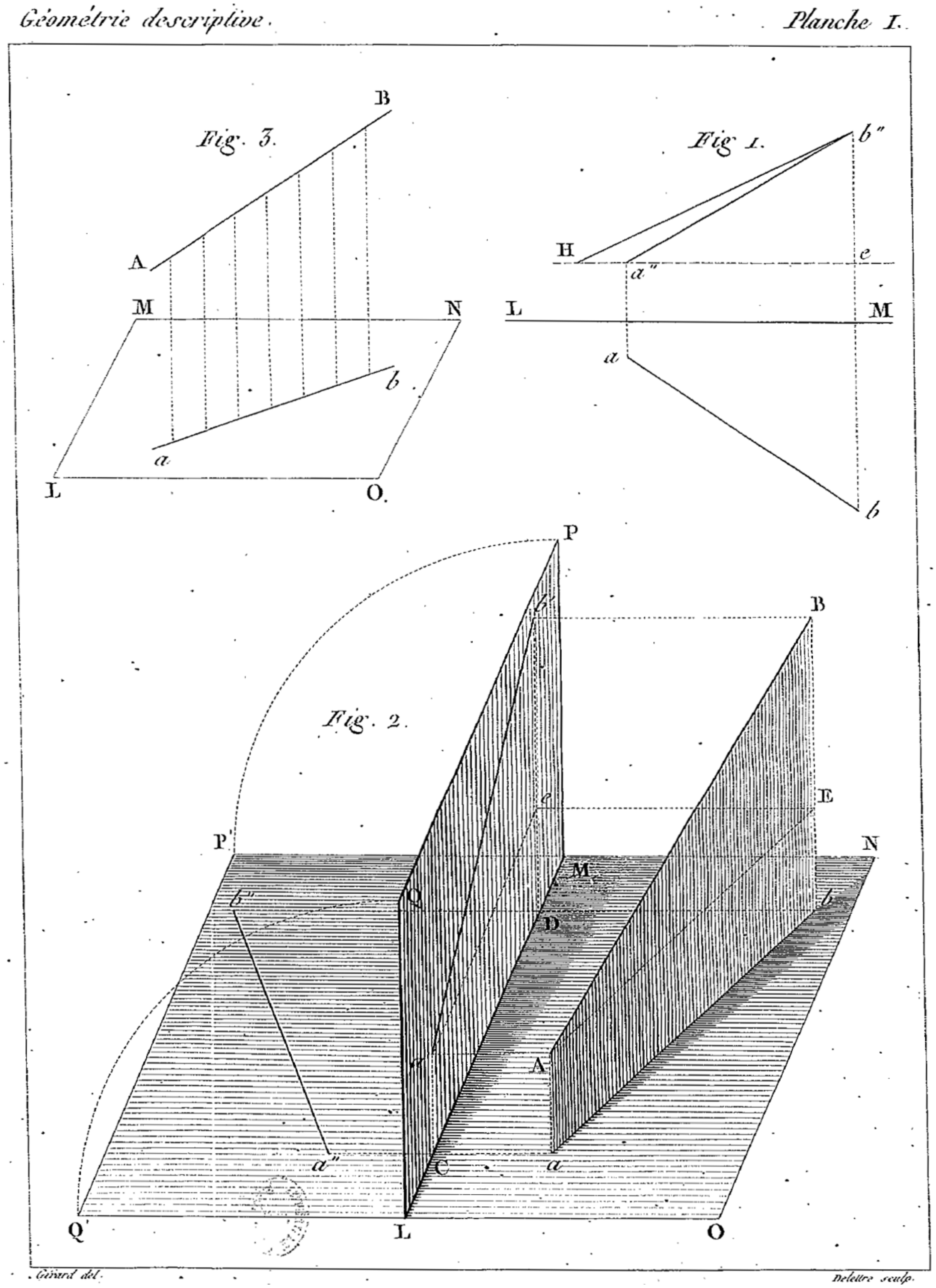


Figure 2.3
Source: Gaspard Monge, *Géométrie descriptive: leçons données aux écoles normales, l'an 3 de la République* (Paris: Baudouin, 1799), Pl. 1.

[laboratoires particuliers] to execute “chemical operations”.[61] A similar system was used by Monge for his course at l’École normale where the lectures on descriptive geometry were delivered in the amphitheater of Jardin des Plantes while students “exercised their graphical constructions” in “large drawing rooms” inside the former church of the Sorbonne.[62] The replacement of “graphical operations” by “graphical constructions” was not purely aleatory as Monge had to justify the relevance of the course on descriptive geometry for future primary school teachers. While the “graphical operations” of L’École polytechnique were a form of better learning the general rules of descriptive geometry, the “graphical constructions” taught at l’École normale were presented as necessary for the arts:

> On contribuera donc à donner à l’éducation nationale une direction avantageuse, en familiarisant nos jeunes artistes avec l’application de la géométrie descriptive aux constructions graphiques qui sont nécessaires au plus grand nombre des arts, et en faisant usage de cette géométrie pour la représentation et la détermination des élémens des machines, au moyen desquelles l’homme, mettant à contribution les forces de la nature, ne se réserve, pour ainsi dire, dans ses opérations, d’autre travail que celui de son intelligence. … Ensuite on appliquera la méthode des projections aux constructions graphiques, nécessaires au plus grand nombre des arts, tels que les traits de la coupe des pierres, ceux de la charpenterie, etc.[63]

2.2 Travaux Graphiques and Arts Graphiques

The most coherent attack against Monge’s pedagogical program was framed by Jules de La Gournerie (1807-1887) who was assigned in 1848 to teach descriptive geometry at L’École polytechnique.[64] While Monge had a been a revolutionary Jacobin and a dedicated professor of geometry who never built anything in his life, La Gournerie was the son of a monarchist and a practicing engineer who was forced by the circumstances to teach.[65] The difference between the two men was visible in La Gournerie’s *Discours sur l’Art du trait et la Géometrie descrip-*

61. Belhoste, “De l’Ecole des ponts et chaussées à l’Ecole centrale des travaux publics,” Document 4.
62. Gaspard Monge, *Géométrie Descriptive* (Paris: J. Klostermann, 1811).
63. Monge, *Géométrie descriptive*, 2-3.
64. For a detailed discussion of La Gournerie’s criticism of Monge see Joël Sakarovitch, *Epures d’architecture : de la coupe des pierres a la géometrie descriptive, XVIe-XIXe siècles* (Basel ; Birkhäuser, 1998), 331-342; Enrique Rabasa, “La Gournerie versus Monge,” *Nexus Network Journal* 13, no. 3 (2011): 715–735.
65. Rabasa, “La Gournerie versus Monge,” 717.

tive (1855) which purposefully and systematically traced not so much the history of Monge's descriptive geometry but rather the history of "l'art du trait".[66] The *Discours* condemned the emphasis on the theoretical development of descriptive geometry which allowed the British to "surpass us with respect to the modern techniques of skew arches".[67]

Two decades later, La Gournerie attacked the very core of Monge's program that had united under the term "Arts graphiques" a series of disparate domains to which descriptive geometry was directly applied. La Gournerie faulted this approach because it forcefully brought together arts with "very different natures":

> J'ai adopté dans ce Mémoire l'expression d'*Arts graphiques* parce qu'elle est employée, mais je la trouve assez impropre; elle correspond a l'idée d'aprés laquelle des arts ayant tous une partie graphique plus ou moins importante, et d'ailleurs de natures trés-diverses, avaient été compris dans le cours de Géométrie descriptive de l'Ecole Polytechnique.[68]

La Gournerie cleverly used some older reports of Prony to veil his criticism against Monge. The school of Monge was "very strict with respect to geometrical exactitude" and neglected the material circumstances involved in constructions such as the cement, the costs or the bearing pressures.[69] Because of such failures, Prony had insisted that the course on stereotomy should be taught as an applied course at L'École des ponts et chaussées.[70] For similar reasons, La Gournerie took the decision to make an independent course out of the part on stereotomy from what had been the course on descriptive geometry; this freed the course on stereotomy to delve into the technical and economical constraints of constructions. Furthermore, La Gournerie

66. La Gournerie's discourse opened with: "La Géométrie descriptive est une science nouvelle: elle ne date que de Monge; mais, considérée dans ses applications, elle continue l'ancien art du Trait. Je me propose aujourd'hui d'exposer rapidement l'histoire de cet art, d'étudier la transformation que Monge lui fit éprouver, et enfin de rechercher quels ont été les progrès des arts graphiques depuis la création de la Géométrie descriptive", Jules de La Gournerie, *Discours prononce au conservatoire impérial des arts et metiers, le 14 Novembre 1854, à l'ouverture du cours de géométrie descriptive: Discours sur l'art du trait et la géométrie descriptive* (Paris: Mollet Bachelier, 1855), 5.

67. Ibid., 36.

68. Jules de La Gournerie, "Mémoire sur l'enseignement des arts graphiques," *Journal de Mathématiques Pures et Apliquées* 19 (1874): 149.

69. Ibid., 115-116.

70. Sakarovitch, *Epures d'architecture*, 338.

criticized Monge for failing to present different graphical methods of representation that were useful in engineering practice, such as perspective or axonometric projections. Instead of trying to create a unifying theory, La Gournerie resumed teaching a series of varied graphical methods of representation.

2.3 The Graphical Language of Geometry

L'École polytechnique was mockingly named by Théodore Olivier "L'École monotechinque" because of the increasing weight given to analysis under the influence of Laplace, Poisson and Cauchy, "ces hommes qui ne connaissaient d'autre langue que l'algèbre".[71] Olivier also found fault in Chasles' remarks that "la géométrie descriptive n'est toujours qu'un instrument dont l'ingénieur se sert pour traduire sa pensée et exécuter sur le papier les opérations que la science, je veux dire la géométrie générale, lui indique."[72] Olivier employed the metaphor of language centered on "graphique" to undermine both the claims of an algebraic language and those that reduced descriptive geometry to a simple translation of rational geometry.[73]

Following Monge, Olivier explicitly distinguished between descriptive geometry as a method and as a language: "Descriptive geometry, as a method, allows us to find novel geometrical properties (unknown until then); as a language, it allows us to write and transmit to engineers geometrical truths which they can use and verify, that is to use them in their work in the field".[74] If geometry was the language of the engineer, one had to learn how "to read it and write it".[75] Descriptive geometry, as a method, allowed one to find new geometrical

71. Théodore Olivier, *Mémoires de géométrie descriptive: théorique et appliquée* (Paris: Carilian-Goeury et V. Dalmont, 1851), xi-xii.

72. Chasles quoted in Théodore Olivier, *Additions au cours de géométrie descriptive* (Paris: Carilian-Goeury et V. Dalmont, 1847), xi.

73. Olivier's rhetoric is particularly surprising because he presented the attacks coming from analysis and rational geometry as a form of reducing descriptive geometry to mere writing: "l'art des projections, qui écrit graphiquement des résultats géométriques obtenus par la géométrie rationnelle"; "La géométrie descriptive ne peut servir qu'à tracer graphiquement les résultats géométriques obtenus par l'analyse", ibid., xi-xv.

74. Ibid., xiv.

75. Théodore Olivier, *Cours de géométrie descriptive*, 2 vols. (Paris: Carilian-Goeury et V. Dalmont, 1843), vol. 1, 2.

properties; as a language, "it allows us to *write* and to communicate geometrical *truths* to the *engineers*, and to even make them verify and use these truths".[76]

Olivier considered that "geometrical ideas" were transmitted through two languages – "la géométrie algébrique" and "la géométrie descriptive", or as he commonly referred to them "la langue algébrique" and "la langue graphique".[77] The first used symbols while the later used lines, but they were both exact because they were guided by clear rules established by reasoning ("le raisonnement"). However, the principles of the two languages were different – the algebraic language employed arithmetics as its fundamental principle, that is the number, and it was applied to those geometrical problems dealing with metrical relations ("relations métriques"). The graphical language had as its fundamental principle the form ("la forme") and it was applied in geometry to the solution of problems of position ("relations de position"). Because the principles of these two languages was different so were their strengths and abilities–"leur *esprit* ou *génie* est différent":[78]

> I think one can say that descriptive geometry, or the graphical language, is eminently capable of expressing and of discovering the relations of position, and that analysis, or the algebraic language, is eminently capable of expressing and of discovering the metrical results.[79]

Because of this "la langue géométrique" of ancient geometers resembled more closely the "algebraic language" rather than the "graphical language of descriptive geometry" because one solved problems from a "metrical" and not "formal" point of view.[80]

Olivier further developed the metaphor of a graphical language not only as an expression

76. Olivier, *Additions au cours de géométrie descriptive*, xiv.
77. "In mathematics the human spirit always proceeds in the same manner, whether by employing the graphical language or the algebraic language, to arrive at the manifestation of a truth. The principles are identical in their essence in both languages; they are only manifested in different forms...", Olivier, *Mémoires de géométrie descriptive,* 171.
78. Olivier, *Additions au cours de géométrie descriptive*, xiii.
79. Théodore Olivier, *Développements de géométrie descriptive* (Paris: Carilian-Goeury et V. Dalmont, 1843), 176. Or: "C'est ainsi que ''analyse s'applique à la recherche des propriétés de relation métrique, et que la géométrie descriptive s'applique à la recherche des propriétés de relation de position", Olivier, *Cours de géométrie descriptive*, vol. 1, vi).
80. Olivier, *Mémoires de géométrie descriptive,* 61.

of geometrical ideas, but also as a form of writing and reading. One projects a given spatial system ("un système dans l'espace") on two planes, and by studying the relations between the projected lines, one can find theorems that apply to the spatial system. Thus, one can say that "it is by *reading* the projections of a system situated in the space, the same way we read *equations*, that we discover the geometrical properties written *graphically* on the *diagram* [épure], and as a result we discover, in a certain manner, the geometrical properties that exist in this spatial system [système de l'espace]".[81] Or again, that "we read a *diagram* [épure] the same way we read pages of *analysis*".[82]

Olivier described two types of images – "les figures" (or "croquis") that were drawn in perspective were used "to help the mind of the reader and allow him to better grasp the true form of a spatial system [système de l'espace]"; "les épures" were "figures rigorously constructed with scale and compass; the graphical results which are found by their construction are immediately translated in geometrical language in the text".[83] Olivier used a graphical marking to help students to distinguish more easily between "les figures" and "les épures" ("la ligne de terre" LT was shaded). The first type of drawings were used in the theoretical and oral part of the course, while the latter were used in the manual and graphical part.[84]

Olivier used the expression "read in space [lire dans l'espace]" to mean that "by looking at the vertical and horizontal projections on a diagram [épure], the student acquires the habit of conceiving the relations of position that exist between the points, lines and planes that compose a spatial system [système de l'espace]".[85] The action of reading in space by looking at a diagram was facilitated by a new notation introduced by Olivier. Given a horizontal plane H and a vertical plane V then (see Fig. 2.4):

> On remarquera sans peine que cette notation a le grand avantage de pouvoir *démontrer dans*

81. Olivier, *Additions au cours de géométrie descriptive*, xiv.
82. Ibid.
83. Olivier, *Développements de géométrie descriptive*, viii.
84. Olivier, *Cours de géométrie descriptive*, vol. 2, vi-vii.
85. Olivier, *Développements de géométrie descriptive*, vii-viii.

> *l'espace*, car on peut parler du point m, de la droite D, du plan P, et l'élève lit sur *l'épure* le point m dont les points m^v et m^h sont les projections, la droite D dont les droites D^v et D^h sont les projections, le plan P dont les droites H_P et V^P sont les traces, etc.[86]

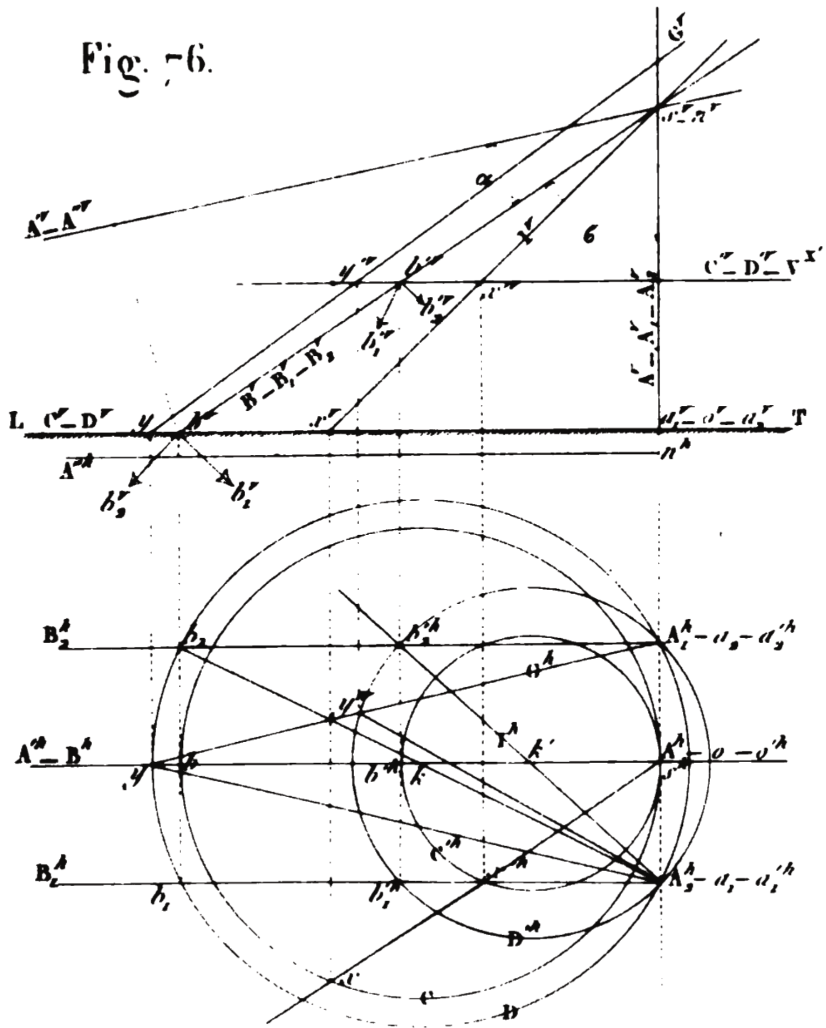


Figure 2.4
Source: Théodore Olivier, *Développements de géométrie descriptive* (Paris: Carilian-Goeury et V. Dalmont, 1843), Atlas, pl. 16.

At the level of practice and teaching, Olivier looked at geometrical and graphical points of view from a different perspective. In descriptive geometry, there were two things to be considered – the geometrical solution and the graphical construction. Olivier contrasted the "point, droite, courbe ou surface *géométrique*" and the "point, droite, courbe ou surface *graphique*"

86. Olivier, *Développements de géométrie descriptive*, vii.

because the graphical construction of a point was governed by different requirements than its geometrical construction. Because of this a "general *geometrical solution*" would often need to be modified by the "*graphical data* [données graphiques]" of the particular problem.[87] Ideally, the graphical point had to be as close as possible to the geometrical point, but this depended on the choice of methods or operations:[88]

> très souvent les solutions géométriques les plus élégantes devaient être modifiées pour les rendre graphiques; que souvent aussi elles devaient être abandonnées pour y substituer des solutions moins élégantes sous le point prêtant avec facilité aux constructions graphiques.[89]

> Il existe une grande différence entre une solution géométrique et une solution graphique; pour bien faire concevoir en quoi les deux modes de solution diffèrent l'un de l'autre, je vais prendre pour exemple la solution de deux problèmes de géométrie descriptive qui se trouvent résolus dans tous les traités, par la méthode géométrique et non par la méthode graphique. Ces deux problèmes sont l'un: la plus courte distance entre deux droites, et l'autre: la section droite d'un cylindre.[90]

Olivier gave the example of parabolas or hyperbolas for which a construction could be

> elegant and simple *from a geometrical point of view*, but could not be accepted as a *graphical construction* because in descriptive geometry one needs to construct rigorously, and also to be able to verify through new constructions the graphical results that were obtained; because, in the end, the diagram [épure] traced by the engineer has to be used to build on the spot, and it is therefore necessary for the errors, that appear inevitably on paper due to the imperfection of the instruments, not to be so large that when multiplied by the scale of the diagram we could make grave mistakes of execution on the spot.[91]

Olivier opposed the "recipes for graphical constructions" and the narrow-minded "graphical manipulations" of many instructors to what he called "the philosophy of the science of diagrammatic space [la philosophie de la science de l'espace figuré]".[92]

87. Théodore Olivier, *Compléments de géométrie descriptive* (Paris: Carilian-Goeury et V. Dalmont, 1845), 299-300.
88. The same point is also made: "Ainsi, la méthode donnée par M. Plucker dans les Annales de Mathématiques, publiées par M. Gergonne, est d'une élégance et d'une simplicité remarquables sous le point de vue géométrique; mais il me sera permis de dire que sous le point de vue graphique, elle ne pourrait être acceptée." ibid., 146.
89. Olivier, *Développements de géométrie descriptive*, 187-188.
90. Olivier, *Compléments de géométrie descriptive*, 351.
91. Ibid., 146-147.
92. Olivier, *Mémoires de géométrie descriptive,* 49.

3 CONCLUSION

If the *graphical* is looked at through a contemporary eye it would be seen as a transparent description of something drawn or inscribed. However, what is lost by this perspective is the fact that no such term was needed until the late 18th century though there was no shortage of graphical activities: quadrants were divided, maps were drawn, trajectories were traced, curves were described, etc. The category of the *graphical* became necessary not because of the multiplication of graphical objects, but rather because of the multiplication of choices. The category was formed as a contrast or a counterweight to other preestablished categories. While quadrant makers or stone-cutters had no use for a category such as the *graphical*, astronomers and geometricians needed a term to describe *equivalent* operations which were neither arithmetical nor trigonometrical. While *geometrical* could have been a satisfactory choice for many, for a scrupulous geometrician such a term would have misconstrued his activity because all his operations (either arithmetical, trigonometrical or graphical) were founded in the same science of geometry. Though other expressions were in use ("construction", "construction by rule & compass", "geometrical construction", etc.) these lacked the malleability of a single-word adjective such as *graphical*. While other professions also used a mix of graphical and non-graphical methods, these methods were employed *along* each other, and not in opposition to each other. However, in the case of astronomy and mathematics, students started being schooled in a variety of methods or operations through which they could solve the same problem. It is such choices that brought to the forefront the opposition between graphical and non-graphical methods, and with it the category of the *graphical*.

If until the end of the 18th century *graphical* played only a modest role in distinguishing between different methods and operations described in mathematics and astronomy textbooks, the revolutionary program of l'École polytechnique redefined its application. Monge and his students reorganized the older courses of les Écoles spéciales around the principle of a universal graphical technique which could coordinate between multiple fields of application. This seductive but utopic vision of engineering teaching was implemented not only through pedagogical practices, but especially through their redefinition and redescription. The category of the *graphical* was invoked to present descriptive geometry and drawing as the common language of engineers, artists and mathematicians; the term was also used to redescribe the relation between theoretical and practical lessons as one between oral lectures and the execution of "graphical operations" and "graphical work"; "graphical constructions" provided a way of presenting the new and abstract principles of descriptive geometry as directly applicable to practical arts.

However, as the next chapters will show there were several visions of a what a universal graphical language could mean. While Monge imagined a *unified* language formed around a rigid and abstract set of principles and methods, other engineers emphasized the intuitive nature of graphical methods which allowed them to be understood by anyone. In Monge's school students had to work hard to acquire the skills of learning the abstract graphical language of descriptive geometry. The other engineering traditions appealed to the *graphical* as an intuitive language which produced diagrams that could be grasped "at a glance". In this parallel tradition it was not the students who labored to produce the diagrams, but rather the diagrams worked for the student as labor-saving devices. One referred not to *the* graphical language but rather to *a* graphical method.

· 3 ·

Indicating, registering, recording

Indicateur, enregistreur, inscripteur

The 19th century rise of graphical methods and representations has been inextricably linked with the development of a special class of scientific instruments – the *autographs*, i.e. instruments which could record their variations under the form of a trace.[1] Some historians went as far as defining *the* graphic method solely as "the technique of inscribing curves with self-registering instruments".[2] While the restrictive singular could be encountered in the work of some 19th century physiologists, especially that of Étienne-Jules Marey, it remained unpopular with engineers who employed a varied array of "graphical methods" which only rarely involved autographs.[3] This minor distinction actually illustrates a more important point that has so far not been properly addressed – the gap between the *graphical* as understood through the contemporary eye and the period eye.

The contemporary eye perceives the autograph as an instrument which produces *inscriptions* or *graphs*, both categories which were absent until the late 19th century.[4] These terms are particularly misleading because they unconsciously give agency to the traces produced by the

1. Hankins and Silverman, *Instruments and the Imagination*, 128-140.

2. Brain, "The Graphic Method," 8. For a similarly restrictive use see Chadarevian, "Graphical Method and Discipline."

3. For a study that emphasizes the great variety of graphical methods and representations see Hankins, "Blood, Dirt, and Nomograms." For the indicator, the paradigmatic autograph used by engineers, see Wise and Brain, "Muscles and Engines: Indicator Diagrams and Helmholtz's Graphical Methods."

4. Chadarevian sees the autographs exclusively as "automatic inscription devices", in Chadarevian, "Graphical Method and Discipline," 287. The science sociologist Carroll-Burke also defined the purpose the graph-instruments to be the production of inscriptions: "The centre of gravity of graphing is the *activity* of producing inscriptions", in Carroll-Burke, "Tools, Instruments and Engines Getting a Handle on the Specificity of Engine Science," 605. While the associations between autographs and "inscriptions" is not completely inappropriate for some 19th century physiologists (such as the later work of Marey) it is an inappropriate category for Watt's indicator or Morin and Poncelet's dynamographs and instrument for observing the laws of falling bodies, Chadarevian, "Graphical Method and Discipline," 275; Brain, "The Graphic Method," 82, 144.

autographs beyond what can be historically established. For example, some have suggested that "the use of a recording instrument required the transition from tables to graphs".[5] Such a requirement can only exist within the contemporary eye. For the period eye many other scenarios were possible: a self-registering thermometer could have marked the minimum or maximum temperature without producing any graphical trace; or, the curves (or punched paper) produced by an autograph could have been reduced into tables, a much more manageable format.[6] Describing any trace produced by an autograph as an *inscription* conveys an ahistorical sense of materiality and mobility. In many cases the traces of an autograph were mere indications which were read on the spot, without ever being transferred to a mobile environment.

The analytical category of inscriptions, and their connection to autographs, is further misleading because it has been uncritically derived from Étienne-Jules Marey's *La méthode graphique dans les sciences expérimentales et principalement en physiologie et en médecine* (1st ed. 1878, 2nd ed. 1885). While historians who have looked at Marey's text have noticed his utopic vision of the graphical method as a universal language, they have only associated this vision with the claims Marey made for his instruments.[7] However, over the course of his career Marey consciously and consistently changed the terms in which he described the action and purpose of the autographs from self-indicating or self-registering instruments (i.e. instruments which wrote their own indications or kept their own register) to instruments that allowed a phenomenon to produce its own inscription. Not only the metaphors, but the very language that Marey chose embodied his vision of what autographs could and should do. The appeal of describing and perceiving self-registering instruments as inscribing instruments which produce inscriptions has been particularly strong among philosophers who have

5. Hankins and Silverman, *Instruments and the Imagination*, 128. The claim was made for Watt's indicator.

6. As discussed below, this was the case for the mechanical traces produced by meteorological autographs which were never published until the second half of the 19th century when photographic forms of recording and multiplication were introduced.

7. Chadarevian, "Graphical Method and Discipline"; Lorraine Daston and Peter Galison, "The Image of Objectivity," *Representations*, no. 40 (1992): 81–128.

come in contact with Marey's work. *Via* Marey, the term *inscription* (with all its variations from "appareil inscripteur" to "inscription") has entered the philosophical vocabulary through François Dagognet's work, to be picked up by Jacques Derrida and Bruno Latour.[8]

The aim of this chapter is to look at *autographs* through the period eye. To accomplish this we must first reconstruct the proper historical category to which these instruments actually pertained. This task is particularly challenging because previous studies have solely focused on a subset of the autographs, i.e. *graphic* self-recording instruments.[9] As it will be shown below, the historical category was populated with instruments which could function independently of an observer by performing the task of the observer. In Britain these instruments were identified through the common tropes "in the absence of the observer" and "keep its own register", while in France or Italy through the suffix "graphe" or "grafo" (i.e. *graph*).[10]

This grouping of instruments is conceptually important because it evades standard classifications along a fixed function (e.g. thermometers or instruments for measuring temperature), a particular mechanism (e.g. weather-*clock*, or *aneroid* barometer), a particular inventor or model (Six's thermometer), or a particular group of makers or activity (philosophical, mathematical or optical instruments).[11] Instead, it is a grouping defined by the relation between in-

8. The term was used by Dagognet in François Dagognet, *Écriture et Iconographie* (Paris: JVrin, 1973). Dagognet will later write a biography of Marey, see François Dagognet, *Etienne-Jules Marey: A Passion for the Trace* (New York: Zone Books ; 1992). Part of the genealogy described above was acknowledged in Latour, "Visualization and Cognition," 88. See also Timothy Lenoir, ed., *Inscribing Science: Scientific Texts and the Materiality of Communication* (Stanford University Press, 1998).

9. So far, Hebbel E. Hoff and L. A. Geddes multiple articles remain the most in-depth studies of this subset of the larger category of self-registering instruments. More recent studies, such as those Thomas Hankins or Soraya de Chadarevian have discussed "automatic recording" or "self-recording" instruments as if such instruments were exclusively graphical. As shown below, this was hardly the case until the second half of the 19th century. Cf. Hoff and Geddes, "The Rheotome and Its Prehistory"; Hoff and Geddes, "Graphic Registration before Ludwig: The Antecedents of the Kymograph"; Hoff and Geddes, "The Technological Background of Physiological Discovery"; Hoff and Geddes, "The Beginnings of Graphic Recording"; Hankins and Silverman, *Instruments and the Imagination*; Chadarevian, "Graphical Method and Discipline."

10. Because this study looks at the use of words in multiple languages, I will denote general linguistic objects through italics, while their specific form will be given inside inverted commas. Thus the morpheme *graph* would be represented in English by "graph", "grapher", "graphy", in French by "graphe" or "graphie", in Latin by "graphia", etc.

11. For the last grouping see J. A. Bennett, "A Viol of Water or a Wedge of Glass," in *The Uses of Experiment: Studies in the Natural Sciences*, ed. David Gooding, Trevor Pinch, and Simon Schaffer (Cambridge: Cambridge

strument and observer as an action of *replacement*, *displacement*, or *imitation*. To recover such a meaning it is necessary to understand how instruments were *categorized*, not only grouped or classified. While the latter operates through spatial divisions (proximity, hierarchies, etc.), the former operates through linguistic divisions and tropes.

Histories of scientific instruments have so far paid little attention to the historical names of instruments.[12] While a few studies have looked at controversies related to particular names, there has been no consistent endeavor for tracking the name of instruments to allow for a history of naming practices and historical trends.[13] The first challenge is to pull apart the ahistorical etymologies that are often used to interpret the name of an instrument. The first section will delineate a method for describing naming practices and the interpretation of names, while the second section will apply this approach to the label *graph*. The third section is dedicated to self-registering instruments, while the fourth section discusses Marey's redefinition of instrument labels.

1 HISTORICAL ETYMOLOGIES

1.1 The analogy of etymologies

The name given to a concept or an object is not completely random or arbitrary. Saussure's fundamental principle of *l'arbitraire du signe* can be highly misleading:

> The bond between the signifier and the signified is arbitrary. Since I mean by sign the whole that results from the associating of the signifier with the signified, I can simply say: the linguistic sign is arbitrary.[14]

University Press, 1989), 105–114; J. A. Bennett, "Early Modern Mathematical Instruments," *Isis* 102, no. 4 (2011): 697–705.

12. Maurice Daumas, *Les instruments scientifiques aux XVIIe et XVIIIe siècles*, 1st ed. (Paris, Presses universitaires de France, 1953); Gerard L'Estrange Turner, *Scientific Instruments, 1500-1900: An Introduction* (University of California Press, 1998); Gerard L'Estrange Turner, *Nineteenth-Century Scientific Instruments* (University of California Press, 1983).

13. For a study surrounding the controversy of naming the "calorimeter" see Lissa Roberts, "A Word and the World: The Significance of Naming the Calorimeter," *Isis* 82, no. 2 (1991): 198–222.

14. Ferdinand de Saussure, *Course in General Linguistics* (New York: Philosophical Library, 1959), 67.

Only much later in the *Cours* this principle was further qualified – signs can be absolutely arbitrary (i.e. unmotivated) or relatively arbitrary.[15] "Dix" and "neuf" are unmotivated, but "dix-neuf" is relatively motivated. Saussure admitted two limits to the arbitrariness of the sign brought by: 1. syntagmatic relations that allow a term like "dix-neuf" to be analyzed in terms of its components "dix" and "neuf"; 2. associative relations which connect the meaning of "dix-neuf" to other terms like "dix-huit". For Saussure the "irrational principle of the arbitrariness of the sign" was opposed by a "principle of order and regularity". The order principle was contrived by the mind, while the arbitrariness principle was generated by a "system that is by nature chaotic".[16] The proportions of arbitrariness and motivation varied from language to language; some languages made greater use of "unmotivated signs" (and could be seen as more "lexicological"), while others relied on structural rules (and were more "grammatical"). It was the study of language as a system of signs that made arbitrariness into a crucial assumption - "signs that are wholly arbitrary realize better than the others the ideal of the semiological process".[17]

Many linguists have considered Saussure's principle of arbitrariness to be a misnomer and an overstatement. Roman Jakobson went as far as calling it an "arbitrary principle" (all puns intended) and rejected Saussure's parallel between lexicon-grammar and arbitrariness-motivation.[18] While one could concede that the linguistic sign in isolation can be seen as arbitrary or unmotivated, once the sign is regarded as part of a system, the relation between the signifier and the signified is also re-evaluated and re-defined. If one were to argue from the arbitrariness principle that "it would make no difference to the linguistic transaction (the act of parole) if the word for "sister" were not *soeur* but *zoeur*, or *soeuf*, or *pataplu*" then he would run into a lot of trouble.[19] Every word is part of a complex network of associations (word fam-

15. Saussure, *Course in General Linguistics*, 131.
16. Ibid., 133.
17. Ibid., 68.
18. Roman Jakobson, "Sign and System of Language: A Reassessment of Saussure's Doctrine," *Poetics Today* 2, no. 1 (1980): 33–38.
19. Roy Harris, *Reading Saussure: A Critical Commentary on the Cours de Linguistique Générale* (Open Court,

ilies, expressions, rhymes, etc.) that would resist such a change, or would react by producing further changes.[20] Dwight L. Bolinger attempted to show that the sign is not arbitrary and that "there must be an intimate connexion between form and meaning — sufficiently close at times for form to influence meaning, and for meaning to influence form".[21] Bolinger pointed out the existence of "constellations of words having similar meanings tied to similar sounds" such as "bash", "clash", "smash", "crash", "dash", "flash", etc.[22]

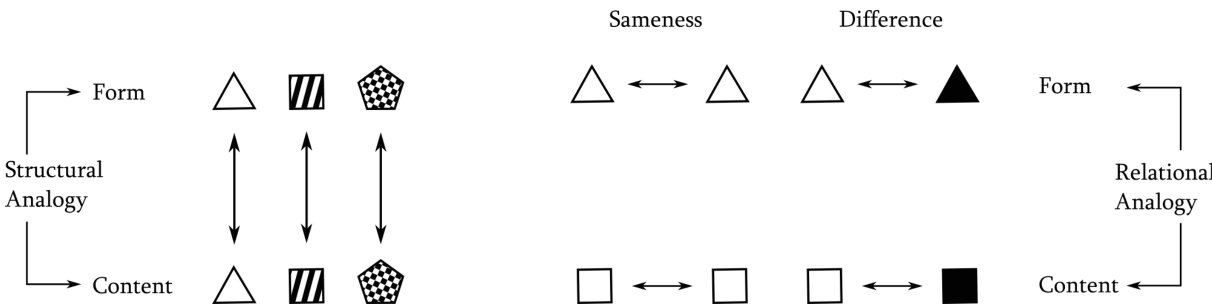


Figure 3.1
"Structural analogy" vs "Relational analogy". After Masako K. Hiraga, "Diagrams and Metaphors: Iconic Aspects in Language," *Journal of Pragmatics* 22, no. 1 (1994): 5–21.

A similar point was made by Roman Jakobson who took interest in Peirce's concept of diagrams - a type of icons that are based on the structural similarity of the signifier and the signified. He illustrated this concept through the expression "Veni, vidi, vici" for which the structure of the sentence reflects the temporal structure of Caesar's actions; similarly, "high – higher - highest" reflected the gradation of the signified through the addition of the phonemes. Jakobson has generated considerable interest for Peirce among linguists, who have introduced a further distinction between *structural diagrams* that display a correspondence between the structure of the content and the structure of the form (e.g. "Veni, vidi, vici"), and *relational diagrams* that associate sameness in form and sameness in content, or difference in form and

1987), 67-8.

20. William Keach replied to Harris' example by pointing out that because signs are not absolutely arbitrary, the French "soeur" is not completely equivalent to the English "sister" if one attempts to translate a poem by Byron. The same restriction appears for Shakespearean rhymes like "womb" and "tomb", or "breath" and "death", see William Keach, *Arbitrary Power: Romanticism, Language, Politics* (Princeton: Princeton University Press, 2015), 19.

21. Dwight Bolinger, "The Sign Is Not Arbitrary," *Boletín del Instituto Caro y Cuervo* 5 (1949): 53.

22. Ibid., 58-9.

difference in content (e.g. the phonestheme /fl/ that associates the idea of movement with "flap", "flare", "flee", "flicker", etc.).[23] These two types of diagrammatic relations are represented in Fig. 3.1.

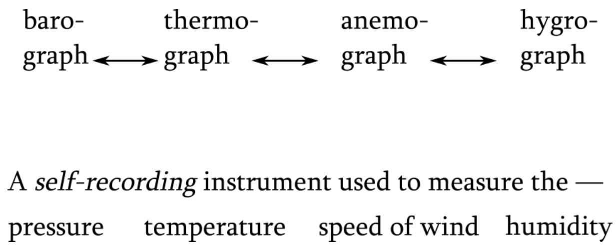


Figure 3.2
Relational analogy for *graph*.

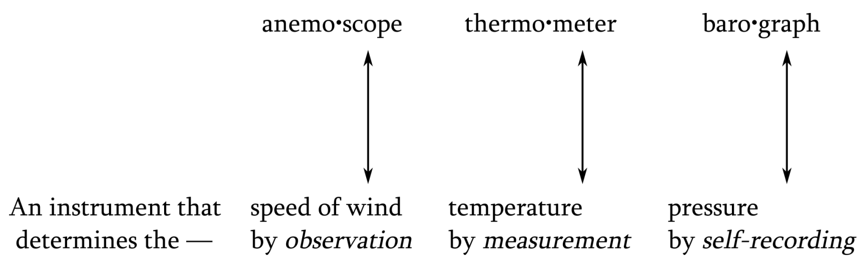


Figure 3.3
Structural analogy for *scope*, *meter* and *graph*.

The concepts of *structural* and *relational* analogy can help us understand the patterns through which language is organized. Such an analysis is particularly fruitful when applied to words with a well-established structure (see Figs. 3.2 and 3.3). However, while the above cited linguistics studies were mainly interested in describing the state of language synchronically, in what follows the concept of analogy will be applied historically.

1.2 *Scopes* and *Meters*

The productive use of "scopio" or "scopium" can be traced back to the activity of a well-connected network of Italian natural philosophers. The term "telescopio" has been attributed

23. Masako K. Hiraga, "Diagrams and Metaphors: Iconic Aspects in Language," *Journal of Pragmatics* 22, no. 1 (1994): 5–21; John Haiman, "The Iconicity of Grammar: Isomorphism and Motivation," *Language* 56, no. 3 (1980): 515–540.

to Frederico Cesi, the founder and president of the Accademia dei Lincei, who allegedly "unveiled" the term during a banquet in Galileo's honor in 1611.[24] A year later "telescopium" was first used in print by Julius Lagalla, a professor of philosophy at the University of Rome and a guest at Cesi's banquet. There is some dispute over the origin of the term because Lagalla–in a different book–has attributed the term to John Demisiani, a Greek mathematician and poet, who was also present at the Galileo dinner.[25] An unsustained claim has been made that Demisiani had also coined "microscopio", though the term probably originated in 1625 with Giovanni Faber, a botanist and secretary of the Accademia dei Lincei, who called "after the model of telescope, a microscope, because it permits a view of minute things".[26] The ending of "scopio/scopium" was so unique that when in 1612 Christopher Scheiner proposed the term "heliscopium", Cesi concluded that Scheiner must have read Lagalla's book where "telescopium" was first printed.[27] In 1617 Giuseppe Biancani, a Jesuit astronomer well-familiar with the "telescopio/telescopium", coined the term "thermoscopium" to describe an instrument with which "many things may be found out about the nature of the air".[28] The *scope* ending was productive not only in Italian or Latin. "Baroscope", probably coined by Robert Hooke, came into use in 1664 among the members of the Royal Society.[29] Hooke also introduced in the "Preface" to the *Micrographia* (1665) the term "hygroscope" to describe an instrument made from "the beard of a wild oat" which could measure the humidity of air. The

24. Edward Rosen, *The Naming of the Telescope* (New York: HSchuman, 1947), esp. 30-31. Before "telescopium" was introduced, Galileo and Kepler mainly used "perspicillum"–a term commonly used to refer to an optical lense–along with other variations such as "perspicillum duplicatum" (double lense) or "specillum". In vernacular Galileo preferred the term "occhiale". However, neither of these terms was ideal because they could not easily distinguish between the instrument and the lenses, see ibid., 4-5.

25. Ibid., 60. In a careful reading of Lincean documents Rosen has shown that while in the first accounts of the banquet Cesi only bestowed ("indidit") the name, in a later reworking of the story it was claimed that he "thought it up and bestowed it [excogitavit et indidit]", see ibid., 64. This has led Rosen to attribute the term to Demisiani.

26. Faber quoted in ibid., 23-24. For sources who attribute "microscopio" to Demisiani see ibid., 96n220, 97n227.

27. Ibid., 38, 56.

28. Biancani quoted in W. E. Knowles Middleton, *A History of the Thermometer and Its Use in Meteorology* (Baltimore: Johns Hopkins Press, 1966), 11.

29. The term is first mentioned in a letter from Robert Hooke to Beale, June 24, 1664. Because the word is not defined one can assume that it was already in use. See W. E. Knowles Middleton, *The History of the Barometer* (Baltimore: Johns Hopkins Press, 1964), 72.

microscope, an instrument on which Hooke was an almost uncontested authority, probably provided the pattern on which the new names were constructed.

The ending *meter* became productive in France in the second half of the 16th century when several mathematical instrument makers published short pamphlets to advertise a particular instrument they had designed. In 1567 Abel Foulon, "valet de chambre du roi", published a *Usage et description de l'holomètre: pour scavoir mesurer toutes choses qui sont soubs l'estandüe de l'oeil.* Foulon's example was followed by G. des Bordes in *La declaration et vsage de l'instrument nommé canomettre* (1570) and Philippe Danfrie, also a "valet de chambre du roi", who in 1597 published a *Declaration de l'vsage du graphometre: par la pratique duql l'on peut mesurer toutes distances* with a supplement titled *Traicté de l'usage du Trigometre.* This was the first instance in which the graphometer, which would become the most well-known surveying instrument, was named and described.[30] Soon after, a Breton instrument maker (Henry de Suberville) dedicated to the king Henry IV an instrument which he advertised as *L'Henry-Metre, instrument royal, et universel* (1598) with which one could "prend toutes mesures geometriques, & astronomiques", and Pierre Le Conte advertised *La fabrique et l'usage du radiometre, instrument geometrique, et astronomique* (1604). In everyday use such instruments would have been simply described as "quadrans" or "quadrans universels".[31] However, these pamphlets played a special role in both advertising the novelty of the instrument and in associating the instrument with a particular maker. Furthermore, the instruments were dedicated to the king, and as such an appropriate name was required.[32]

Thus, it is not surprising that "thermometre" (instead of "thermoscopium" or "thermoscope") was first used in a French mathematical text, the *Recreation mathematique* (1624) of H.

30. Maurice Daumas, *Scientific Instruments of the Seventeenth and Eighteenth Centuries and Their Makers* (London: Portman Books, 1989), 16. For Danfrie's activity and workshop see A. J. Turner, "Paper, Print, and Mathematics: Philippe Danfrie and the Making of Mathematical Instruments in Late 16th Century Paris," in *Studies in the History of Scientific Instrument*, ed. Christine Blondel (London: Rogers Turner, 1989), 22–42.

31. See for example the "quadrans" listed by Jehan Quenif (1557) and reproduced in ibid., 33-34.

32. See Mario Biagioli, "From Print to Patents: Living on Instruments in Early Modern Europe," *History of Science* 44, no. 2 (2006): 139–186.

van Etten, an author connected to the French Jesuit priest and mathematician Jean Leurechon, and in its Latin translation *Thaumaturgus mathematicus* (1628) by the German mathematician Caspar Ens.[33] In this case, the division in language between *scope* and *meter* reflected a more profound division in labor. As pointed out by historian Jim Bennett, in the 17th century "[m]athematical instrument makers did not become involved in the production of telescopes or microscopes"; the optical instrument makers were most often astronomer or "spectacle makers".[34]

For the next century the *meter* and *scope* endings competed with each other in the vocabulary of natural philosophers. Marin Mersenne, a philosopher and mathematician educated at the Jesuit College of La Flèche, referred to a "termoscope" (in vernacular) and to a "thermoscopium" (in Latin).[35] Blaise Pascal preferred "thermometre".[36] In *New Experiments and observations touching the cold* (1665) Robert Boyle introduced the term "barometer", in analogy with "thermometer", to avoid "circumlocutions", despite the fact that "baroscope" and "thermoscope" were also used throughout the text.[37] By the end of 1665 the *Philosophical Transactions* announced that "Modern *Philosophers*, to avoyd Circumlocutions, call that Instrument... a *Barometer* or *Baroscope*".[38] "Thermometer" and "thermoscope" were similarly interchangeable as exemplified in Thomas Hobbes' *Elements of Philosophy* (1656): "This organ is called a *Thermometer* or *Thermoscope*, because the degrees of Heat and Cold are measured

33. Middleton, *A History of the Thermometer and Its Use in Meteorology*, 20. For a while H. van Etten was considered to have been Leurechon's pseudonym but now scholars are inclined to see Etten as a student of Leurechon, see Arianna Borelli, "The Weatherglass and Its Observers," in *Philosophies of Technology: Francis Bacon and His Contemporaries*, ed. Claus Zittel et al. (Leiden; Boston: Brill, 2008), 119-121. In 1628 was published a book called *L'usage du thermomètre*.

34. Bennett, "Early Modern Mathematical Instruments," 703. Instead of grouping everything under "scientific" instruments, Bennett is careful to distinguish between the *historical* categories of *mathematical*, *optical* and *philosophical* instruments. See Deborah Jean Warner, "What Is a Scientific Instrument, When Did It Become One, and Why?," *The British Journal for the History of Science* 23, no. 1 (1990): 83–93.

35. Marin Mersenne, *Correspondance*, ed. Paul Tannery and Cornelis de Waard, vol. 3 (Paris, 1946), 191; Marin Mersenne, *Cogitata Physico-Mathematica*, vol. 2 (Sumptibus Antonii Bertier, via Iacobaea, 1644), 143. These were mentioned in Middleton, *A History of the Thermometer and Its Use in Meteorology*, 23, 27.

36. See for example the quote in ibid., 28.

37. The text was probably written two years earlier, see Middleton, *The History of the Barometer*, 71-72.

38. "A Relation of Some Mercurial Observations, and Their Results," *Philosophical Transactions (1665-1678)* 1 (1665): 153.

and marked by it".[39] For most of the 17th century "baroscope" and "thermometer" were the most common choices.[40] However, analogy often ruled against common use. The philosopher John Locke used a "baroscope" and a "thermoscope" to keep a daily register of the weather, while the *Philosophical Transactions* described an "*Aerometer*, consisting of *Hygrometer*, *Thermometer*, and *Barometer*".[41]

The first distinction between a *scope* and a *meter*, that I could encounter, was made in 1709 by Christian Wolff who distinguished between "thermometra", "barometra", "hygrometra", or "manometra" as instruments which "measure [metimur]" and the "baroscopia", "thermoscopia", "hygroscopia", or "manoscopia" as instruments which "indicate [indicant]". "Manoscopia" was hardly ever used in scientific texts, and was only introduced by Wolff to preserve the symmetry of the opposition. In a worried note, Wolff pointed out that

> commonly the words thermometer & thermoscope are synonymous. We nevertheless carefully distinguish these words, lest hereafter (which commonly happens) for the same words we have altogether different instruments, and furthermore confuse observations and because of this confusion draw erroneous conclusions from them.[42]

Wolff's distinction between *meter* and *scope* as one between measuring and showing was embraced by Chambers' *Cyclopaedia* (1728) which warned its readers:

> The *Barometer* is frequently confounded with the *Baroscope*, tho somewhat improperly; the latter, in strictness, being a Machine that barely shews an Alteration in the Weight of the *Atmosphere*; but 'tis one thing to know the Air is heavier at one time than another, and another to measure

39. Thomas Hobbes, *Elements of Philosophy* (London: Printed by R& WLeybourn for Andrew Crooke, 1656), 387.

40. For example, in an advertising leaflet named *Aeroscopium* the instrument maker John Warner presented his "baroscope" and "thermometer"; the leaflet is reproduced in R. T. Gunther, *Early Science in Oxford* (Oxford, 1920), vol.12, 302-303. The same pattern of use can be observed in the pages of the *Philosophical Transactions.*

41. For Locke's table of measurements see John Locke, "A Register of the Weather for the Year 1692, Kept at Oates in Essex," *Philosophical Transactions* 24 (1704): 1917–1937. For the description of the "*Aerometer*" see *Philosophical Transactions* 6 (1685): 1185. For more on the early history of weather measurements, and especially atmospheric pressure, see Jan Golinski, *British Weather and the Climate of Enlightenment* (Chicago: University of Chicago Press, 2010), 108-136; Richard Yeo, *Notebooks, English Virtuosi, and Early Modern Science* (Chicago: University of Chicago Press, 2014), 189-190.

42. Christian Wolff, *Aërometriae Elementa. Gesammelte Werke, II Abt., Band 37* (Hildesheim: Georg Olms Verlag), 9-10.

> how much that Difference is; which is the Business of the *Barometer*.[43]

Through Chambers the distinction made its way into Samuel Johnsons' *A Dictionary of the English Language* (1755) and *L'Encyclopédie* (1st ed. 1751), dictionaries which as Chambers' *Cyclopaedia* paid particular attention to consistent etymologies. *L'Encyclopédie* provided a similar warning to the *Cyclopaedia*: "on confond ordinairement, quoique mal-à-propos, le *barometre* avec le baroscope".[44] However, it also had to admit that "[a]u reste il n'y a plus aujourd'hui de *baroscope* qui ne soit barometre, & ces deux noms désignent absolument le même instrument".[45] Any distinction was simply dismissed by the *Encyclopaedia Britannica* (1st ed. 1771): "BAROSCOPE, the same with barometer. See BAROMETER".[46] The example was followed by technical or mathematical dictionaries, including Charles Hutton's famous *Philosophical and Mathematical Dictionary* (1795, new ed. 1815).

Besides the purely etymological distinction between *scope* and *meter*, which as *L'Encyclopédie* had to admit did not correspond to any practical reality because one could not encounter any proper baroscopes (though one could still inappropriately refer to barometers as "baroscopes"), the distinction between "thermometers" and "thermoscopes" was more subtle. Following "the excellent Wolfius", Chambers' *Cyclopaedia* (1728) explained that "all the thermometers in use are *thermoscopes*" because they can only show and not properly measure the changes in heat. Chambers' summary of Wolff's arguments were readily transcribed in *L'Encyclopédie*.[47] While the quantity measured by a barometer, i.e. the weight of air, could be clearly defined and standardized, the quantity measured by a thermometer, the amount of "hot and cold", was more illusive. The standardization of the thermometer calibration between

43. Ephraim Chambers, *Cyclopædia, or, An Universal Dictionary of Arts and Sciences* (London: Printed for James and John Knapton, 1728), 83.
44. Diderot and D'Alembert, *Encyclopédie ou Dictionnaire raisonné des sciences, des arts et des métiers*, vol.2, 77.
45. Ibid., vol.2, 89.
46. *Encyclopædia Britannica: Or, A Dictionary of Arts and Sciences* (1771), vol.1, 523.
47. Diderot and D'Alembert, *Encyclopédie ou Dictionnaire raisonné des sciences, des arts et des métiers*, vol.16, 273-274.

two fixed points was only carried out in the first half of the 18th century.[48] This difference between the principles of the instruments was not readily reflected in the symmetry of the pairs "barometer-baroscope" and "thermometer-thermoscope".

This short account of the endings *scope* and *meter* has shown that etymological constructions and interpretations were not universal. It was never the case that an instrument was given a Greek name *only* because such a name could be translated into a phrase that would, more or less, summarize the use of the instrument. Such endings became productive not because of their etymological meaning, but rather through the emergence of patterns that would be imitated through analogy. As instruments moved so did their labels which would become the templates for new naming patterns. However, analogy could act in two directions. On the one hand, new names would be generated based on a pattern. At the same time, differences in naming patterns would require matching differences in the objects that were named. Thus, as Christian Wolff warned its readers in 1709, a new danger arose – the *misnomer*, or the name constructed through the wrong kind of analogy, which throughout most of the 18th and 19th century will be labeled as a *barbarism*.[49]

1.3 The barbarism of etymologies

The potential conflict between scientific names and scientific objects came to the forefront at the end of the 18th century when a series of chemists who included Guyton de Morveau, Antoine Lavoisier, C. L. Berthollet and A. F. de Fourcroy proposed a new chemical nomen-

48. For the history of the "rational scales" see Middleton, *A History of the Thermometer and Its Use in Meteorology*, 65-114. For the difficulties in actually constructing a rationally scaled thermometer see Jean-François Gauvin, "The Instrument That Never Was: Inventing, Manufacturing, and Branding Réaumur's Thermometer during the Enlightenment," *Annals of Science* 69, no. 4 (2012): 515–549; Hasok Chang, *Inventing Temperature: Measurement and Scientific Progress* (Oxford University Press, 2004).

49. "Barbarism" was a technical word among philologists which defined it as: "Le barbarisme est de se servir de quelque mot impropre, ou de quelque phrase estrangère, et qui n'est pas naturelle à la langue, ou d'oublier des particules, des pronoms ou des prépo-sitions dans les endroits où elles sont nécessaires", in Jean François Féraud, *Dictionnaire critique de la langue française ...* (1787), 246. The "barbarism" was anything strange and foreign which did not belong to the language, or any combination of two parts which did not belong together.

clature.[50] The principles of the reform were first exposed by Guyton de Morveau in 1782 who criticized the old language of chemistry as "aussi barbare, aussi vague, aussi incohérente".[51] Morveau considered that scientific progress could only be certain and fast if

> les idées sont représentées par des signes précis & détermines, justes dans leur acception, simples dans leur ex-pression, commodes dans l'usage, facile à retenir, qui conservent autant qu'il est possible, sans erreur, l'analogie qui les rapproche, le système qui les définit & jusqu'à l'étymologie qui peut servir à les faire deviner.[52]

Morveau set out five principles for his scientific language:

1. Simple substances should have simple names, while composite substances should have composite names. Thus, the language would reflect the composite state of a substance.

2. The names had to correspond, as much as possible, to the nature of the things: "Les dénominations doivent être, autant qu'il est possible, conformes à la nature des choses".[53] While a simple, uncompounded word shared no similarity with the object it represented, "les dérivés, les composés de ce mot sont les seuls noms conformes à la nature des êtres congénères".[54] For this reason, De Morveau considered the term "huile de vitriol" inappropriate because it brought out the idea of an oily substance to something that completely lacks this character.

3. New words lacking any previous meaning or associations were to be preferred to words that induced a false idea: "Lorsqu'on n'a pas une connaissance certaine du caractère qui doit principalement déterminer la dénomination, il faut préférer au nom qui n'exprime

50. Jan Golinski, "The Chemical Revolution and the Politics of Language," *The Eighteenth Century* 33, no. 3 (1992): 238–251; Jessica Riskin, *Science in the Age of Sensibility: The Sentimental Empiricists of the French Enlightenment* (Chicago: University of Chicago Press, 2002); Maurice P. Crosland, *Historical Studies in the Language of Chemistry* (Cambridge: Harvard University Press, 1962); Ursula Klein and Wolfgang Lefèvre, *Materials in Eighteenth-Century Science: A Historical Ontology* (MIT Press, 2007).

51. Louis-Bernard Guyton de Morveau, "Mémoire sur les dénominations chimiques, la nécessité d'en perfectionner le système, et les règles pour y parvenir," *Observations sur la physique* 19 (1782): 371.

52. Ibid.

53. Ibid., 373.

54. Ibid.

rien à un nom qui pourroit exprimer une idée fausse".[55]

4. New words were preferably constructed from Greek and Latin such that the meaning was recoverable from the word, and the word from the meaning: "Dans le choix des dénominations à introduire, on doit préfèrer celles qui ont leurs racines dans les langues mortes les plus généralement répandues, afin que le mot soit facile à retrouver par le sens, & le sens par le mot".[56] The aim was to create names such that "le mot rappelle sûrement la chose à l'esprit" and to "recréer le signe conventionnel de la chose".[57]

5. "Les denominations doivent etre assorties avec soin au genie de la langue pour laquelle elles sont formees".[58]

For example, Morveau rejected the commonly used term "terre pesante" because: 1. though it was a simple substance it was not expressed through a simple word that would allow the formation of adjectives or composite words; 2. "cette expression manque de justesse, en ce qu'il n'est pas prouvé que la terre qu'on tire du spath pesant soit elle même plus pesante qu'une autre terre". Instead, Morveau proposed "barote" because the Greek root was already naturalized and "pour ne pas introduire un signe dépourvu de toute analogie, qui n'ait qu'une valeur arbitraire, j'emprunte un terme qui se relie par l'étymologie aux idées que je veux modifier".[59] "Barote" was chosen not because it was meaningless or arbitrary, but on the contrary because it was *intelligible*.[60] The term "barometer" was given as an example for what a properly con-

55. Guyton de Morveau, "Mémoire sur les dénominations chimiques, la nécessité d'en perfectionner le système, et les règles pour y parvenir," 374-5.
56. Ibid., 375.
57. Ibid., 382.
58. Ibid., 376.
59. Ibid.
60. Some historians have given a different, and somewhat misleading account of Morveau's principles: "The subsequent abandonment of the principle that a name should reflect the nature of its object marked the origin of the new chemical nomenclature. In 1782 Morveau published a call for a systematic reform of chemical names in which he denied outright Venel's and Bergman's common assumption that names should be founded in facts. Facts alone, Morveau asserted, 'say nothing to the mind'. He then casually anticipated a concept that would dominate much twentieth-century language theory, that is, the arbitrary relation of the sign to the signified. Morveau claimed that all names were essentially artificial: 'Sounds, and the words they represent', he wrote, 'in

structed scientific language could achieve. Despite the fact that etymologically the word was translated solely as "the measure of weight", the particular use of the instrument has lead to "substituer un nom à une phrase, & de créer un mot qui portât avec lui son explication".[61]

Both the proponents of the new nomenclature and its most avid critics seemed to agree on two main issues: 1. the words had to be intelligible; 2. no word functioned on its own – its meaning and sound depended on language as a whole. How one judged the intelligibility and coherence of the new nomenclature was up for debate. Lavoisier considered that the changes proposed were not possible "sans blesser souvent les usages reçus, & sans adopter des dénominations qui paraîtront dures & barbares dans le premier moment"; however, the ear was easily accustomed to new words when these were based on "un système général & raisonné".[62] The critics picked up exactly on the same issues. J. C. De La Métherie condemned terms like carbonate, nitrate, and sulphate as "harsh and barbaric words that shock the ear and are not at all in the spirit of the French language" – "des mots durs, barbares, qui choquent l'oreille, & ne sont nullement dans le génie de la langue françoise".[63] Throughout the article the expression "durs & barbares" was thrown against the new nomenclature.[64] To these faults in language, De La Métherie opposed the essential requirements of "l'harmonie" and "l'analogie".[65] The

reality have, by themselves, no relation, no conformity with things'. So, in the case of an individual substance that one 'envisions only for itself', and not in relation to any other substance, Morveau argued, any name that 'means nothing' would serve the purpose. In fact, he preferred meaningless names for such independently considered substances and recommended that nomenclators 'distance themselves as much as possible from familiar usage'. For the purpose, he advised taking roots from classical rather than vulgar languages", in Riskin, *Science in the Age of Sensibility*, 236. There is a deep contradiction in Riskin's claim that De Morveau recognized both "the arbitrary relation of the sign to the signified" and that he preferred meaningless names that meant nothing.

61. Guyton de Morveau, "Mémoire sur les dénominations chimiques, la nécessité d'en perfectionner le système, et les règles pour y parvenir," 375.

62. Antoine Laurent Lavoisier, "Mémoire sur la nécessité de réformer et de perfectionner la nomenclature de la chimie," in *Methode de nomenclature chimique*, ed. Louis-Bernard Guyton de Morveau et al. (Paris: Cuchet, 1787), 23-4.

63. J. C. De la Métherie, "Essai sur la nomenclature chimique," *Observations sur la physique* 31 (1787): 274.

64. "Muriate est dur & barbare", "fluate est dur & barbare; nous dirons sels fluoriques", "succinate est dur & barbare", "tartrite, tratrate sont durs & barbares", "oxalate est dur & bar-bare", "gallate, qui est dur & barbare", "benzoate qui est dur & barbare", "laclate qui est dur & barbare", "sormiate, qui est dur & barbare", "bombiate, qui est dur & barbare", ibid., 279-281.

65. "L'harmonie des mots est encore une considération essentielle à faire dans une langue", "On ne doit point négliger l'harmonie des mots, & on ne peut absolument s'écarter du génie de la langue", "Cependant une langue ne pourra jamais arriver à sa perfection, que lorsqu'on aura acquis des connaissances suffisantes pour en réduire

same points were raised by other French chemists who rejected the nomenclature because it is “barbare, insignifiante, & sans étymologie” (Balthazar Georges Sage), or “jargon barbare & mysterieux” (Christophe Opoix).[66] One critique ended with the rhetorical question of: “Les gens du monde pourront-ils accoutumer leurs oreilles à l’étrange dissonance & à la barbarie des termes?”.[67] On the other side of the channel the Irish chemist Richard Kirwan considered that the new nomenclature was unintelligible not because it was “sans étymologie” (as voiced by the French chemist Balthazar Georges Sage), but rather because it replaced “single names, already understood, and well known by all those that have attained any knowledge of chemistry” with new names “derived from the Greek or new unknown barbarous Latin names”.[68] Kirwan singled out Morveau’s example of the word “barometer” (which the French considered that it is “un mot qui portât avec lui son explication”) as a name which is “just as well understood by those who are totally unacquainted with its etymology, as by those to whom this is perfectly known”.[69] Against those who read too much into etymologies, Kirwan pointed out that “the instrument itself, and not its use, is denoted by the name”.[70]

Constructing satisfactory scientific names could be an impossible affair given the array of expected constraints: dignified Greek roots that could be adapted to the phonetics of modern languages and which would be intelligible to those without a profound acquaintance with classics. Faced with such a challenge, in the 1830s Michael Faraday contacted the Cambridge-don William Whewell to to help him find “two good names not depending upon the idea of a

tous les termes à l’analogie”, “On consultera autant qu’on pourra l’analogie”. The new nomenclature failed too often to respect the analogy of the language: “Elle n’a nullement consulté l’analogie dans un grand nombre de cas”, “Ils blessent l’analogie en beaucoup de circonstances”, in De la Métherie, “Essai sur la nomenclature chimique.”

66. Balthazar Georges Sage, “Lettre de M. Sage à M. de la Métherie sur la nouvelle nomenclature,” *Observations sur la physique, sur l’histoire naturelle et sur les arts* 33 (1788): 479; Christophe Opoix, “Lettre de M. Opoix, maître en pharmacie à Provins & membre de plusieurs académies, à M. de la Métherie sur la nouvelle théorie,” *Observations sur la physique, sur l’histoire naturelle et sur les arts* 34 (1789): 77-78.

67. Opoix, “Lettre de M. Opoix, maître en pharmacie à Provins & membre de plusieurs académies, à M. de la Métherie sur la nouvelle théorie,” 77-78.

68. Richard Kirwan, “Of Chemical and Mineralogical Nomenclature,” *The Philosophical Magazine* 8 (1800): 175.

69. Ibid., 173.

70. Ibid.

current in *one direction only* or upon Positive or negative".[71] Faraday could only fathom names "which a scholar could not suffer I understand for a moment" such as "Eastode & Westode", "Voltode & Galvatode", or "Alphode & Betode".[72] Whewell politely replied that these terms only expressed difference, but it "is very desirable in this case to express an opposition, a contrariety, as well as a difference. [...] They are also objectionable it appears to me, in putting forwards too ostentatiously the arbitrary nature of the difference".[73] To express opposition besides difference, Whewell appealed to a clever etymology to construct: "Anode & Cathode", "a way up and a way down". While for a Cambridge graduate the meaning could have been transparent, Faraday expressed his concern that "all to whom I have shown them have supposed at first that by *Anode* I mean *No way*".[74] Such concerns were immediately rebuffed in a scholarly vein by Whewell: "*Anodos* and *cathodos* do really mean in Greek *a way up* and *a way down*; and *anodos* does not mean, and cannot mean, according to the analogy of the Greek language *no way*."[75]

Both Whewell and Faraday (or his acquiescences, if those were not a simple ruse for Faraday to politely criticize Whewell) understood the analogy to Greek differently. While Faraday simply split the word "anode" into "an", a negation and "ode" or "way" (a term with which Faraday was already familiar), Whewell appealed to grammatical analogies which distinguished both between the function of "ode" as an adjective or a substantive.[76] As he explained to Faraday:

71. Faraday to Whewell, 1834, in Frank James, ed., *The Correspondence of Michael Faraday* (London: Institution of Electrical Engineers, 1991), vol.2 177 - letter 1711. I follow here the excellent and unfairly neglected study of S. Ross, "Faraday Consults the Scholars: The Origins of the Terms of Electrochemistry," *Notes and Records of the Royal Society of London* 16, no. 2 (1961): 187–220.

72. James, *The Correspondence of Michael Faraday*, vol.2 177.

73. Ibid., vol.2 184.

74. Ibid., vol.2 181.

75. Ibid., vol.2 182-3.

76. Whewell employed a similar grammatical analogy to dismiss the term "zetode" (i.e. "that which seeks the way") proposed by Faraday because "this word being grouped with others of the same termination might be expected to indicate a modification of electr*ode*, as eis*ode*, and ex*ode*, or an*ode* and cath*ode* do. Instead of this, it means a notion altogether heterogeneous to these, and the *ode* is here the object of a verb *zete*, contrary to the analogy of all the other verbs", in ibid., v.2, 202.

> It is true that the prefix *an* put before *adjectives* beginning with a vowel, gives a negative signification, but not to substantives, except through the medium of adjectives. *Anarchos* means without government, and hence *anarchia*, *anarchy*, means the absence of government; but *anodos* does not and cannot mean the absence of way.[77]

Too proud to admit that "anode" could present any shortcomings, Whewell condemned the faulty knowledge of Greek of anyone who misinterpreted the term:

> when introduced in company with *cathodos* no body who has any tinge of Greek could fail to perceive the meaning at once. The notion of *anodos* meaning *no way* could only suggest itself to persons unfamiliar with Greek, and accidentally acquainted with some English words in which the negative particle is so employed; and those persons who have taken up this notion must have overlooked the very different meaning of negatives applied to susbtantives and adjectives.[78]

The close level of scrutiny employed by Whewell in analyzing analogies to Greek was not unparalleled. Shortly after the Indian Rebellion of 1857, scholars from Oxford and Cambridge carried out a heated debate in the pages of the *London Times* over the correct form for "telegram". The arguments made by the two sides were resumed as such:

> If Pericles had had a telegraph from Athena to the Piraeus, and wished to say 'I telegraph,' would he say 'telegrapho' or 'tel[e]grapheo'? If he said 'telegrapho,' he would have called a telegraphic message a 'telegramma'. If he said 'telegrapheo,' he would have used 'telegraphema.' And so the question lies between the two verbs. Cambridge asserts that as 'grapho' can only be used with a preposition, Pericles most have said 'telegrapheo'. And, then Oxford quotes from Homer to prove that 'tele' *is* a preposition. Now, we believe that 'tele' is not, and never was a preposition, but merely an adverb used as a preposition; and although a vulgar fellow like Cleon might have said 'telegrapho', Pericles would have certainly used the other form.[79]

The debate started after the *Times* used on September 14, 1857 the word "telegram" to refer to the "telegraphic despatch" that was received at the foreign-office through the admirality to provide news about "the mutinies in India". As the war carried on, so did the telegrams and the "telegrams". The war in India was soon followed by "the Battle of the Telegram", as it was immortalized in the pages of the *Punch* (see Fig. 3.4).

77. James, *The Correspondence of Michael Faraday*, vol.2 182-3.
78. Ibid.
79. "Telegram," *Emerson's Magazine and Putnam's Monthly* 6 (1858): 446.

THE BATTLE OF THE TELEGRAM;

OR, LANGUAGE IN 1857.

"O FORTUNATI nimium!" the sage
Of Mantua styled the farmers of his age;
Knaves, who on Pan-pipes strove for cheese and curds,
Rough as their goats, and playful as their herds.

Such praise as this, and happiness the same,
English grammarians for themselves may claim,
So singularly clear the meaning seems
Of each new word invented in their dreams;
BROWN cannot dress (his very words I quote)
Save in a "normal" waistcoat, "normal" coat;
JONES cannot eat potatoes, if not done
In an "anhydrohepseterion."
JANE too, at Hastings, as the breeze she courts,
Her "Alee-Kephalee-akepasteer" sports.

My son, young TOM of Trin. Coll., Oxbridge, raves
In Tennysonian strains of winds and waves,
Of deep "æsthetic" gushings, gew-gaws rare,
And "crispéd" smiles, and "glory-crownéd" hair;
Of slumbrous caves where "CLARIBEL low lieth,"
Where the wind "lispeth," and the brook "replieth,"
And "telleth" tales of him who walked abroad
On "wannish" evenings with his "snow-limb'd" MAUD;
When "dry-tongued" laurels "pattered" in their talk
To "perky" larches in the garden walk!

Now TOM's young friend from Wadham, all last Long
In KEATS and MILNES and BAILY came out strong;
O'er *Hiawatha* dropped the frequent tear,
And means to win the Newdegate next year.
—And oft I saw him reading to MISS FRITH
Thy terse grammatic lays, sublime A. SMITH!
She weeps—I listen to the strain which thrills
With "passion-panting" seas, and "yearning rills,"
With "king-thoughts" grand, and "ruffian" winds that
howl
Through areas lone where "crass" policemen prowl.
She trembles as she reads—"Tear-dabbled, fair,
'That white, white face, hid in a night of hair'—
It comes!—while winks 'the penitential moon,'
Even at 'the bridegroom sea!'—it comes too soon,
I hear 'faint trickling sounds,' and 'dim halloos,'
In 'sanded bars' where JEAMES the egg-flip brews,—
My brain reels dizzy, and that white white face,
By some strange fancy has become a brace!"

Now, Sir? (as men address the mighty *Times*,)
I do protest against these novel rhymes;
How, in the name of goodness, can a star
"Yearn in its pulses" through a cloud afar?
How can a "half-smile dwell" on EMMA's lips,
"Touching, yet settling not upon the tips?"
How can "deep silence" be a "grim ravine
That never dared to laugh in Spring's bright green?"
—In vain I strive to solve these mystic strains,
And leave their riddles for TOM's clearer brains.
—And, Sir!—not only do the Poets rave
In "sensuous" raptures over Grammar's grave;
But TOM now says that our Philologists
Seem likely to proceed from words to fists,
While pugilistic Oxford dares to cram
Poor sickening Cambridge with a Telegram!
Who, when "First-Class men" scuffle, shall decide,
When each claims "every school-boy" on his side?
Lost in a labyrinth of "graphs" and "grams,"
We still should blunder 'twixt true words and shams;
Let then poor erring "Telegram," be shriven,
And take the sanction that the Press has given.

TELEGRAPH AND TELEGRAM.

By a Dublin University Poet.

HERE is a bother, here 's a to-do,
About using one letter instead of two!
And why are the Greeks to teach us to call
A thing the spalpeens niver heard of at all?
(Unless you suppose the spark in the wire
Was known to them by the name of Greek Fire).
End it with Phi, or end it with Mu,
What does it signify which you do?
End it with Mu, or end it with Phi,
The point 's not worth a potaty's eye,
Contemn such ulthrapedantic appeals,
And put your shoulders to these two wheels:
Reduce the charges, which now is plundering,
And teach the clerks to spell without blundering.

POMPEY ON TELEGRAM.

TUNE.—"*Sitch a gittin up-stairs.*"

OH! hab you heard ob de row dere am,
'Bout dis here new word Telegram?
De Cambridge and de Oxford School,
Boaf ob dem call de oder a fool.
Sitch a quotin' ob Greek, and makin' ob a riddle,
Sitch a quotin' ob Greek I nebber did see.

De word he may be foul Greek or fair,
Which him don't know and him don't care;
But him sound more tickle dis nigger's ear,
Dan any him's heard for many a year.
Sitch, &c.

De word him short, de word him sweet,
And berry pleasant to repeat,
Him 'zackly fit de nigger's lip,
And de debble may care for him scollumship:
Sitch, &c.

Derefore in *Johnson* jest you look,
When next him publish him spelling-book,
And dere I spects dere will be found
Dat lilly new word wid de lubly sound:
Sitch, &c.

De telegram a 'greeable name;
Him wish him news may be ebber de same;
De next we gets, widout no flam,
Him hope a berry good telegram:
Sitch, &c.

Figure 3.4
Source: "Pompey on Telegram," *Punch*, 1857, 177; "Telegraph and Telegram," *Punch*, 1857, 175; "The Battle of the Telegram," *Punch*, 1857, 185.

While the Oxford and Cambridge scholars were debating if "telegram" was a "barbarism" because it was not formed through proper analogy with the Greek language, the readers of the *Times* and the *Sun* were concerned with a different analogy which affected everyday use of English. One reader hoped that others "will join the crusade against the use of the new word "telegram" which "[i]f it should be adopted, half of our language would have to be changed":

> We shall have to say paragram instead of paragraph, hologram instead of holograph, photogram instead of photograph, autogram instead of autograph, geogrammy instead of geography, lexicogrammy in-stead of lexicography, astrogrammy instead of astrography, lexicogrammer instead of lexicographer, poly-grammy instead of polygraphy, stenogrammy instead of stenography; stereogrammy instead of stereography, horogrammy instead of horography, ichtyogrammy instead of ichtyography, micogrammy instead of micog-raphy, metallogrammy instead of metallography, &c.

The lawless and barbaric "telegram" remained a reference point for many 19th century philologists.[80] Writing a decade later, F. W. Farrar considered that "the word 'telegram' is a monstrusity, - 'a spot of barbarity impressed so deep on the English language that criticism never can wash away' ".[81] The American scholar R.G. White was alarmed that after "telegram" came to be accepted "[w]e have had *photogram* proposed, and *stereogram*, and — Cadmus save us ! — *cablegram*, not only proposed, but used".[82]

It is important to understand that what was opposed in such reactions was not just the ne-

80. See Fitzedward Hall, *Recent Exemplifications of False Philology* (Scribner, Armstrong & Company, 1872), 41-47.

81. Frederic William Farrar, *A Brief Greek Syntax and Hints on Greek Accidence* (1867), 49. The quote was taken from the Preface of Johnson's *Dictionary* (1755): "Such defects are not errours in orthography, but spots of barbarity impressed so deep in the English language, that criticism can never wash them away; these, therefore, must be permitted to remain untouched."

82. Richard Grant White, *Words and Their Uses, Past and Present: A Study of the English Language* (Sheldon, 1871), 233-4. After some had expressed their concern that "photograph" referred both to the "act of photographing, but also the image photographed", "photogram" was proposed as a substitute for a "photographic record". The new term reminded many of the barbaric "telegram". Others questioned the very analogy between "telegram" and "photogram" because "tele" was an adverb, while "photo" was a noun. Using this line of reasoning one concluded that both "photograph" and "photogram" would express the same thing, and "photograph" could not represent an active thing that produced the writing as in the case of "telegraph". Instead, "to photograph" was "utter barbarism"."Photograph or Photogram," *Jackson's Oxford Journal*, 1871, "Photograph or Photogram (II)," *Jackson's Oxford Journal*, 1871, "Photograms," *The Standard*, no. 2 (1885): 2; "Photograms (II)," *The Standard*, no. 2 (1885): 2; "Photograms (III)," *The Standard*, no. 6 (1885): 6.

ologism itself seen in isolation, but rather the "violation of analogy" or "the false analogy". The "genius" of a language was improved by cultivating its analogies.[83] Late 19th century linguists appealed to a similar opposition, this time between analogy which enriched a language with new words and forms, and "folk etymologies" which corrupted the language. Saussure considered that "les constructions de l'analogie sont rationnelles, tandis que l'étymologie populaire procède un peu au hasard et n'aboutit qu'a des coq-à-l'âne". Analogy was based on a form of interpretation and reasoning (no matter if erroneous or imperfect), while "folk etymology" was a direct and unconscious reaction. The latter had "quelque chose qui peut passer pour vicieux, pour pathologique" and as such "elle est plutôt une déformation".[84]

1.4 Conclusion

This section has delineated a particular attitude towards language. It was shown that naming practices are culturally and historically determined, and by following the patterns they generate one can reconstruct networks of exchange. Compared to previous analyses, one should not focus solely on the use of specific expressions or their intent (i.e. on the production of a speech act), but especially in the patterns of association they generate which make the speech acts intelligible. Michel Bréal, Saussure's *maître*, used a revealing example in an article on "De l'analogie" (1878). How can one make sense of a shop sign like "Parfumerie des écoles"? If the sign would be taken out of the context in which it is found, it would be easily misunderstood or misread, maybe as a school for making perfumes. However, the sign is made intelligible by analogy with the signs surrounding it: "Papeterie des écoles"; "Brasserie des écoles".[85]

Not only that names are generated based on some form of meaningful pattern, but they

83. Such a belief was generally held by both French and British thinkers in the 18th and 19th century. E.g. "The chief thing to be attended to in the improvement of a language is the analogy of it", in Joseph Priestley, *Lectures on the Theory of Language and Universal Grammar* (1762), 185.

84. Ferdinand de Saussure, *Cours de Linguistique générale: Édition: critique*, ed. Rudolf Engler (Wiesbaden: Otto Harrassowitz Verlag, 1989), vol.1 IR 3.1-3.11.

85. Michel Bréal, "De l'analogie," *Bibliothèque de l'Ecole des hautes études*, no. 35 (1878): 101.

are also interpreted and made meaningful by historical actors. For example, a mechanical engineer who designed optical indicators proposed the following classification:

> According to the number of variables to be measured such optical indicators can be classified as:
> (A) One-variable indicators, called "-meters", which measure only one variable on a graduated scale (e.g., extensometer, vibrometer, galvanometer);
> (B) Two-variable indicators which represent on a screen, or on a photographic film, in a two-coordinates diagram, the interrelationship of two variables. If the representation is visual the proper term is "-scope" (e.g., vibroscope, oscilloscope). If the representation is a permanent photographic record the proper term is "-graph" (e.g., extensograph, vibrograph, oscillograph, torsiograph). The distinction between "-scope" and "-graph" is not always sharply drawn and in many cases the same instrument can be used for visual inspection as well as for photographic recording.[86]

Notice, however, that this was not a simple grouping or classification of instruments based solely on some material property they possessed. It is a classification motivated by the name of the instruments, and at the same time a classification which tries to motivate these names.[87] If we had ignored the names, the classification would have been simply *conceptual* and *defined* by the number of variables to be measured or the type of representation. However, such a conceptual division is preceded by a *categorial* division imposed by the very names. It is this *categorial* division which motivates the *conceptual* division but without determining its content (because, of course, other conceptual explanations are available for the same given categories).

Bréal described the action of analogy in very similar terms: "Il est question ici d'une règle non formulée, que l'homme s'efforce de deviner, que nous voyons les enfants tâcher de découvrir: en la supposant, le peuple la crée".[88] Categories (names or labels) create meaning because such a meaning is presupposed. This connects back to Wittgenstein's discussion of family resemblances from *Philosophical Investigations*:

86. Kalman J. DeJuhasz, "On the Geometry of Optical Indicators," *Journal of the Franklin Institute* 229, no. 1 (1940): 54.
87. While the particular division and meaning associated with the names is specific to the author and his field of activity, the impulse for this style of thinking about names is much more general.
88. Bréal, *Essai de sémantique (science des significations)*, 80.

> Consider for example the proceedings that we call "games". I mean board-games, card-games, ball-games, Olympic games, and so on. What is common to them all?–Don't say: "There *must* be something common, or they would not be called 'games' "–but *look* and *see* whether there is anything common to all.[89]

After this, Wittgenstein goes on to argue that the only thing common to all these objects is "a complicated network of similarities overlapping and criss-crossing".[90] However, what is left unquestioned is the initial assumption, that "There *must* be something common, or they would not be called 'games' ". Such an assumption can only be made possible by the very existence of the name, label or category "game". What is common to all the games listed by Wittgenstein (board-games, card-games, ball-games, Olympic games) is their very name.

2 LABELING GRAPHS

2.1 The productivity of *graph*

L'Encyclopédie provides a good entry point for having an overall view of the use of "graphe" or "graphie" in the second half of the 18th century. The entries ending in these suffixes can be divided into six categories: 1. descriptions of a topic, or parts of a science or discourse: "lithographie" (or "the description of stones"), "sialographie" ("the part within animal economy which deals with saliva"); 2. a person whose area of activity is connected to some form of writing or expertise: "hydrographe" (or "a person skilled in Hydrography"), "démonographe" (or "someone who writes about demons"); 3. an art of writing, drawing or engraving: "chorégraphie" (or "the art of writing dance as song"), "horographie" (or "the art of making dials"); 4. a type of writing or document: "opistographe" (or "a large book in which one write the various things that need to be revised or corrected"); 5. a type of image: "orthographie" (or "the plane or drawing of a building which shows all parts in their true proportion") ; 6. a type of instrument: "pantographe".

89. Wittgenstein, *Philosophical Investigations*, §66.
90. Ibid.

Table 3.1
The entries ending in "graphe" or "graphie" from *L'Encyclopédie* (1st ed. 1751) grouped by the main meanings associated with *graph.* This comes to show that the use of *graph* to refer to instruments was quite singular at the end of the 18th century.

Description	Person	Art	Document	Image	Instrument
adenographie	agiographe	brachygraphie	autographe	ichnographie	pantographe
angeiographie	bibliographe	chorégraphie	apographe	mégalographie	
anthropographie	biographe	horographie	chirographe	orthographie	
chorographie	calligraphe	horologiographie	cryptographie	prosopographie	
cosmographie	chrysographes	orchésographe	epigraphe	scenographie	
géographie	cosmographe	orthographe	holographe		
glyptographie	démonographe	polygraphe	opistographe		
hydrographie	géographe	sciagraphie	paragraphe		
iconographie	hagiographes	stéréographie	phliacographie		
lexicographie	historiographe	tachéographie	syngraphe		
lithographie	hydrographe	tachygraphie			
logographie	hymnographe	steganographie			
micrographie	hypomnematographe	typographie			
myographie	néographe	orthographie			
neurographie	orthodoxographe				
numismatographie	rhopographe				
ophthalmographie	rhyparographe				
oryctographie	ropographes				
polographie	typographe				
sélenographie					
synostéographie					
sialographie					
topographie					
toreumatographie					
zoographie					
25	19	14	10	5	1

The distribution of words into these categories (see table Table 3.1) allows us to observe several linguistic phenomena. “Graphe” and “graphie” were not homogeneously productive across all categories. Most types of documents ending in “graphe” were words borrowed from Greek or Latin which reflected an ossified ending. By far the most productive category was that of “graphie” as a type of description or science. “Graphe” as the ending of an instrument will only become productive at the end of the 18th century; *L’Encyclopédie* only listed the pantograph. Also, there is a productivity across the aisle: a “cosmographie” was written by a “cosmographe”, while the art of “typographie” was exercised by a “typographe”. Such a phenomenon cannot be observed in the case of *meter* and *scope*, and as we will see, it is this cross-productivity of *graph* that will transform it into a popular ending for instruments.

Many of the terms ending in *graph* became part of the scholarly parlance during the great age of translations when works like Ptolemy’s Γεωγραφικὴ Ὑφήγησις (which came to be known as the *Geographia* or *Cosmographia*) were translated into Latin. While these translations only introduced Greek or Latin words, the ending was made productive by the early modern culture of publication. Though Greek was known only to a small number of humanists, Greek names were often employed in book titles written in Latin or vernacular to associate the content of the books with Greek knowledge or its tradition. The title of Ptolemy’s *Cosmographia* was both copied – as in Pomponius Mela’s treatise which circulated under the names *Cosmographiae liber* (1471), *Cosmographi de situ orbis* (1478), *Cosmographi Geographia* (1482), or Sebastian Münster’s *Cosmographia* (1544) – and imitated to create new titles – as in Antoine Mizauld’s *Cometographia* (1549) or Adriaen von Roomen’s *Ouranographia* (1591). Once the pattern was in place, a whole new series of titles would follow (see Table 3.2). However, while *cosmography* (“cosmographia”, “cosmographie”, etc.) was a recognized discipline (which was studied and practiced), most of the *graph* titles were employed not because they would have been recognized as pertaining to a particular field or sub-field, but rather because they provided a quasi-unique and scholarly name for a treatise. There were many unique names such as Helkiah Crooke's *Somatographia anthropine* (1616), John Greaves's *Pyramidographia* (1641), Pierre Crochat's *Astérographie* (1682), or Georg von Hartenfels' *Elephantographia* (1723).[91]

Table 3.2
A selection of books published between 1500-1750 with "graphia", "grahie" or "graphy" in the title. The title of such books both reflected and reinforced the etymological translation of *graph* as "description".

Adenographia	1656. Thomas Wharton. *Adenographia: sive, glandularum totius corporis* ***descriptio***
	1692. Antonio Nuck. *Adenographia Curiosa Et Uteri Foeminei Anatome Nova Cum Epistola Ad Amicum De Inventis Novis*
Aenigmatographia	1590. Nicolaus Reusner. *Aenigmatographia, sive Sylloge aenigmatum et griphorum convivalium*
Anemographia	1578. Ignazio Danti. *Anemographia : in anemoscopium verticale instrumentum ostensorem ventorum*
	1586. Jehan Lauron. *L'Anemographie, ou,* ***Description*** *des Vents, avec la cause, source, nature, & propriété d'iceulx*
Asterographia	1682. Pierre Crochat. *Astérographie, ou* ***description*** *des estoiles fixes et de toutes les constellations célestes*
Botanographia	1718. Casparus Commelin. *Botanographia a nominum barbarismis restituta, quam Florae-Malabaricae nomine celebrem*
Chronographia	1546. Alexander Scultetus. *Chronographia sive Annales omnium fere regum, principum, & potentatuum, ab orbe condito*
	1567. Gilbert Genebrard. *Chronographia : in duos libros distincta; prior est de Rebus veteris populi, posterior recentes historias praesertimque ecclesiasticas complectitur*
	1596. Lorenz Codomann. *Chronographia : A* ***description*** *of time, from the beginning of the world*
Cometographia	1549. Antoine Mizauld. *Cometographia : crinitarum stellarum quas mundus nunquam impune vidit...*
	1661. Eberhard Welper. *Cometographia : oder* ***Beschreibung*** *deßen im 1661...*

Continued on next page

91. Catalogue book titles can often be misleading because they reduce the page title, a two-dimensional object, to a one dimensional string of letters. Many of these book titles encoded more information, and were probably hardly known under these names. For example, in Crooke's *Somatographia anthropine* this title was written in small Greek letters (hardly legible) and it was only the subtitle that stood out.

Table 3.2 (Continued)

	1665. Jakob Honold. *Cometographia oder Kurtzer **Bericht***
	1668. Johannes Hevelius. *Cometographia, totam naturam cometarum*
Elephantographia	1723. Georg von Hartenfels. *Elephantographia curiosa, seu elephanti **descriptio***
Hecatomgraphia	1540. Gilles Corrozet. *Hécatomgraphie, c'est à dire les **declarations** de plusieurs apophtegmes, proverbes, sentences, & dictz*
	1540. Gilles Corrozet. *Hecatomgraphie : c'est à dire les **descriptions** de cent figures & hystoires, contenantes plusieurs appophtegmes, prouerbes, sente[n]ces & dictz*
Mecographia	1603. Guillaume de Nautonier. *La Mécographie, de l'eymant, c'est-à-dire, la **description** des longitudes, trouvées par les observations des déclinaisons de l'eymant*
	1603. Guillaume de Nautonier. *The mecographie of ye loadstone : Tat is to say ane **description** of the lenthes or longitudes, quhikis ar son be ye obseruations of ye loadstone*
Metallographia	1671. John Webtser. *Metallographia: or a **history** of metals*
Meteorographia	1667. Ferdinando Parkhurst. *Meteorographia: seu Meterography or the first **booke of the doctrine** of meteors*
Methigraphia	1620. Georgius Nicolasius. *Methigraphia, Sive Ebrietatis **Descriptio**, Effectvs Eivs, Et Vitia Annexa*
Micrographia	1665. Robert Hooke. *Micrographia, or, Some physiological **descriptions** of minute bodies*
	1687. Johann Franz Griendel. *Micrographia nova; oder, Neu-curieuse **Beschreibung** verschiedener kleiner Körper*
	1742. Benjamin Martin. *Micrographia nova, or, a new **treatise** on the microscope*
	1745. Henry Baker. *Micrographia restaurata or the copper-plates of Hooke's wonderful discoveries by the microscope*
	1746. George Adams. *Micrographia illustrata, or, The knowledge of the microscope explain'd*
Mumiographia	1716. Christian Hertzog. *Mumiographia Medica: oder **Bericht** von den Egyptischen Mumien*
	1718. Christian Hertzog. *Essay de mumio-graphie, ou plutôt **description** exacte et sincére d'une des plus rares et d'une des plus curieuses moumies qu'on a jamais veues en Europe*
Myographia	1684. John Browne. *Myographia nova sive musculorum omnium (in corpore humano hactenus repertorum) accuratissima **descriptio***

Continued on next page

Table 3.2 (Continued)

	1697. John Browne. *Myographia nova, or, A graphical **description** of all the muscles in humane body*
Onomatographia	1617. Luis Ballester. *Onomatographia, Sive **Descriptio** Nominvm Varii Et Peregrini Idiomatis*
Ophthalmographia	1632. Vopiscus Fortunatus Plemp. *Ophthalmographia sive **tractatio** de oculi*
	1676. Will Briggs. *Ophthalmographia sive oculi ejusque partium **descriptio** anatomica*
	1713. Peter Kennedy. *Ophthalmographia or, a **treatise** of the eye*
(O)uranographia	1591. Adriaen von Roomen. *Ouranographia, sive caeli **descriptio***
	1690. Johannes Hevelius. *Firmamentum Sobiescianum; sive, Uranographia, totum coelum stellatum*
	1710. Roberto Bellarmino. *Ouranography: Or Heaven Opened*
	1801. Johann Elert Bode. *Uranographia, sive Astrorum **descriptio***
Prosopographie	1573. Antoine Du Verdier. *La prosopographie, ou, **Description** des personnes insignes*
Pyramidographia	1641. John Greaves. *Pyramidographia: or, a **description** of the pyramids in Egypt*
Selenographia	1647. Johannes Hevelius. *Selenographia: Sive lunae **descriptio***
Somatographia	1616. Helkiah Crooke. *Somatographia anthropine, or, A **description** of the body of man*

Because we have only selected the titles ending in *graph*, one might infer that there was a disciplinary uniformity. That was not the case. Mizauld followed his *Cometographia* (1549) with a *Planetologia* (1551) which inspired Robert Greene's *Planetomachia* (1585). A century later, in 1684 the physician John Browne published a treatise on muscles, *Myographia nova* (the *nova* being in reference to the previous treatise he published in 1681), and a treatise on glandules, *Adenochoiradelogia.* The simpler name *Adenographia* had already been used by Thomas Wharton in 1656. Also among Browne's manuscripts one can find "Somatopolitia: or, The city of humane body artificially defended from the tyranny of cancers and gangreens".[92] In the century separating Adriaen von Roomen's *Ouranographia* (1591) and Hevelius' *Ura-*

92. K. F. Russell, "A List of the Works of John Browne (1642-1702)," *Bulletin of the Medical Library Association* 50, no. 4 (1962): 675–683.

nographia (published posthumously in 1690) one encounters no *graph* titles, but only books such as Johann Bayer's *Uranometria* (1603), Guillaume Pasquelin – *Ouranologie* (1615), Denis Petau's *Uranologion* (1630), etc. The unicity of the title is also observed in the case of Robert Hooke's *Micrographia* (1665) which was followed by books that had to distinguish themselves through some extra term such as "nova", "illustrata" or "restaurata" (see Table 3.2).

The scholarly ring of Greek and its ability of generating meaningful but unique names, transformed *graph* (along with other Greek suffixes such as "metria", "logia", "machia", etc.) into a highly productive ending. These titles propagated not only the ending but also a translation for it, as it is underlined in Table 3.2. *Graph* ("graphia", "graphie" or "graphy") was translated, mainly, as description. A second group of titles translated *graph* as a form of art, associated in particular with writing (see Table 3.3).

2.2 *Graph* as instrument

If the pattern of naming instruments with *scope* was based on *telescope* and with *meter* on the *graphometer* (or the *holometer*), the *graph* ending was established by the *pantograph*. The instrument was first described by the Jesuit priest Christoph Scheiner in *Pantographice, seu ars delineandi res quaslibet per parallelogrammum lineare seu cavum, mechanicum, mobile* (1631). Here, the instrument was only referred to as a "parallelogrammum lineare, Cauum, Delineatorium, Descriptorium, Graphicum".[93] The first use of *pantograph* can be found in French in the second half of the 17th century, but these examples are isolated and the word does not seem to have caught on.[94] It was absent from the 1690 and 1701 editions of Antoine Furetière's

93. Christoph Scheiner, *Pantographice, seu ars delineandi res quaslibet per parallelogrammum lineare seu cavum, mechanicum, mobile* (Grignani, 1631), 11. Also, notice that "-graphice" in the title referred to the art of drawing and not to the name of the instrument.

94. The first example I could identify is from 1654: "On Fait plusieurs sortes d'instruments pour prendre la figure d'une surface proposée, ausquels on donne des noms specieux de Cosmographe, Pantographe, Grafometre, Trigonometre, &c.", in Jean François, *La science des eaux* (chez Sebastien Piquet, 1654), 36; this sentence is to be found in multiple books by François. The only other distinct example from the 17th century is to be found in Jacques Ozanam, *Méthode de lever les plans et les cartes de terre et de mer* (1693), 208.

Table 3.3
A selection of books published between 1500-1750 with "graphia", "grahie" or "graphy" in the title. The title of such books both reflected and reinforced the etymological translation of *graph* as an "art".

Brachigraphia	1620. W Folkingham. *Brachigraphy, post-writ, or, The* ***art*** *of short-writing*
	1672. S Shelton. *Brachygraphy, or the* ***Art*** *of Short-Writing*
Cryptographia	1684. Johannes Balthasar Friderici. *Cryptographia, oder, Geheime schrifft-, mund- und würckliche Correspondentz : welche lehrmässig worstellet eine hoch-schatzbare* ***Kunst***
Horologiographia	1626. Thomas Fale. *Horologiographia. The* ***Art*** *of Dialling*
Pantographia	1631. Christoph Scheiner. *Pantographice, seu* ***ars*** *delineandi res quaslibet per parallelogrammum lineare seu cavum, mechanicum, mobile*
	1744. Claude Langlois. *Description et usage du Pantographe : autrement appellé singe*
Polygraphia	1672. William Salmon. *Polygraphice : Or, The* ***Art*** *of Drawing, Engraving, Etching, Limning, Painting, Washing, Varnishing, Colouring and Dying*
Sciographia	1635. John Wells. *Sciographia, or the* ***art*** *of shadowes*
Scotographia	1543. Abramo Colorni. *Scotographia overo* ***scienza*** *di scrivere oscvro, facilissima, et sicvrissima per qual si uoglia lingua*
Steganographia	1602. John Willis. *The* ***Art*** *of Stenographie, teaching by plaine and certaine Rules ... the way of compendious Writing*
	1606. Johannes Trithemius. *Steganographia: hoc est :* ***Ars*** *per ocevltam scriptvram animi svi volvntatem absentibvs aperiendi certa*
	1620. Daniel Schwenter. *Steganologia & Steganographia nova : Geheime, Magische, Natürliche Red unnd* ***Schreibkunst***
Stenographia	1644. Thomas Heath. *Stenographie: or, The* ***art*** *of short-writing*
	1695. William Addy. *Stenographia; or, the* ***Art*** *of Short-writing compleated in a far more compendious method than any yet extant*
Tachygraphia	1641. Thomas Shelton. *Tachygraphy the most exact and compendious* ***methode*** *of short and swift writing that hath ever yet beene published by any*
	1660. Thomas Shelton. *Tachygraphia, sive Exactissima et compendio-sissima breviter scribendi* ***methodus***
Zeiglographia	1649. Thomas Shelton. *Zeiglographia, or, a New* ***Art*** *of Short-writing never before published*

Dictionnaire universel which only included the equivalent term “Singe” under which the instrument was described. The term got more traction after 1709, when Nicolas Bion introduced an instrument that “est nommé Pantagrophe [sic]; on le nomme aussi Singe, parce qu’il sert à copier toutes sortes de desseins”.[95] In a later edition of the same treatise, Bion (or the publisher) revised this sentence and changed the name from “pantagrophe” to “pentographe”.[96] Bion, who was the “ingénieur du Roi pour les instrumens de mathématiques”, wrote a highly influential treatise on “instruments de mathématique” that was used as a source for words and definitions in Antoine Furetière’s revised *Dictionnaire universel*. “Pentographe” was added in 1727 and defined as “Instrument de Mathématique qui sert à copier toutes sortes de desseins. On le nomme aussi pour cette raison *Singe*. Voyez ce mot”.[97] The actual description of the instrument was still listed under “Singe”, as it had also been used in the previous editions from 1690 and 1708. Bion’s book was translated in English in 1723, and it included a section “Of the Pentograph, or Parallelogram”.[98] The term was subsequently included in Ephraim Chambers’ *Cyclopædia*, though this time under the form “pentagraph”.[99]

The fact that a word was included in a dictionary does not necessarily mean it was actually used. The word re-emerged in 1743 in a short pamphlet titled *Description et usage du Pantographe: autrement appelé singe* published by Claude Langlois, “ingénieur du Roi & de l’Académie royale des sciences pour les instrumens de mathématiques”.[100] Langlois also submitted the instrument that he had referred to as “Pantographe” to *L’Académie Royale des Sciences* and it was included among the five “machines et inventions” that were approved by

95. Nicolas Bion, *Traité de la construction et des principaux usages des instruments de mathematique* (chez la veuve de Jean Boudot, 1709).
96. Nicolas Bion, *Traité de la construction et des principaux usages des instrumens de mathematique: avec les figures nécessaires pour l’intelligence de ce traité* (P. Husson, 1723).
97. Antoine Furetière, *Dictionnaire universel, contenant généralement tous les mots françois, tant vieux que modernes et les termes des sciences des arts […]* (P. Husson, 1727).
98. Nicolas Bion, *The Construction and Principal Uses of Mathematical Instruments*, trans. Edmund Stone (London: Printed for John Senex and William Taylor, 1723), 86.
99. Chambers, *Cyclopædia, or, An Universal Dictionary of Arts and Sciences*, 780.
100. Claude Langlois, *Description et usage du pantographe, autrement appelé singe, changé & perfectionné par C. Langlois, ingénieur du Roi & de l’Académie Royale des Sciences pour les instrumens de mathématiques.* (S. l., France, 1744).

L'Académie in 1743. Claude Langlois was heading the most important workshop for mathematical instruments in France during this period. For almost twenty years he had been the official instrument maker of the French astronomers, and he made the quadrants and sectors for the expeditions to Peru and Laponia. After his death, his business and his position of "ingénieur du Roi & de l'Académie royale des sciences pour les instrumens de mathématiques" was taken over by his nephew Canivet.[101] Among other things, Canivet continued Langlois' work on the pantograph, and republished his pamphlet as *Description et usage du pantographe, nommé communément singe: considérablement changé & perfectionné* (1758). Langlois' new design proved to be highly successful, and with the help of the publicity from *L'Académie*, the improved instrument and its name were widely distributed. A posthumous edition of Jacques Ozanam's *Méthode de lever les plans* (1750) included a detailed description of the new instrument and Langlois was credited with improving so much the older models that it could be said that he has invented a new instrument to which "il a donné le nom de Pantographe ou Singe".[102] Ozanam's book was used to compile the description of the "Pantographe" that was included in Saverien's *Dictionnaire Universel de Mathematique et de Physique* (1753) where only the names of Scheiner and Langlois were mentioned. A very similar, but shorter entry was then included in Diderot and d'Alembert's *Encyclopédie* (1765) for "le *pantographe* ou *singe*". The main description was listed under "pantographe" while "singe" only included a short note that added "mais le vrai mot est *pantographe.* Voyez PANTOGRAPHE".[103] The image provided in Canivet's pamphlet was used in one of the plates (see Fig. 3.5). Jean-Gaffin Gallon's seven volume collection of *Machines et inventions approuvées par l'Academie royale des sciences* (1735-1777) also included an entry on the "pantographe ou singe perfectionné par M. Langlois", the

101. Daumas, *Scientific Instruments of the Seventeenth and Eighteenth Centuries and Their Makers*, 260-1; Maya Hambly, *Drawing Instruments, 1580-1980* (Sotheby's Publications, 1988), 28.

102. Jacques Ozanam, *Méthode de lever les plans et les cartes de terre et de mer: avec toutes sortes d'instrumens & sans instrumens* (C. Jombert, 1750), 214-5 Ozanam's first edition already mentioned the "pantographe" in a list of mathematical instruments (see Ozanam, *Méthode de lever les plans et les cartes de terre et de mer*, 208).

103. Diderot and D'Alembert, *Encyclopédie ou Dictionnaire raisonné des sciences, des arts et des métiers*, v. 15, 210-211.

description of which was taken from Langlois' pamphlet.[104]

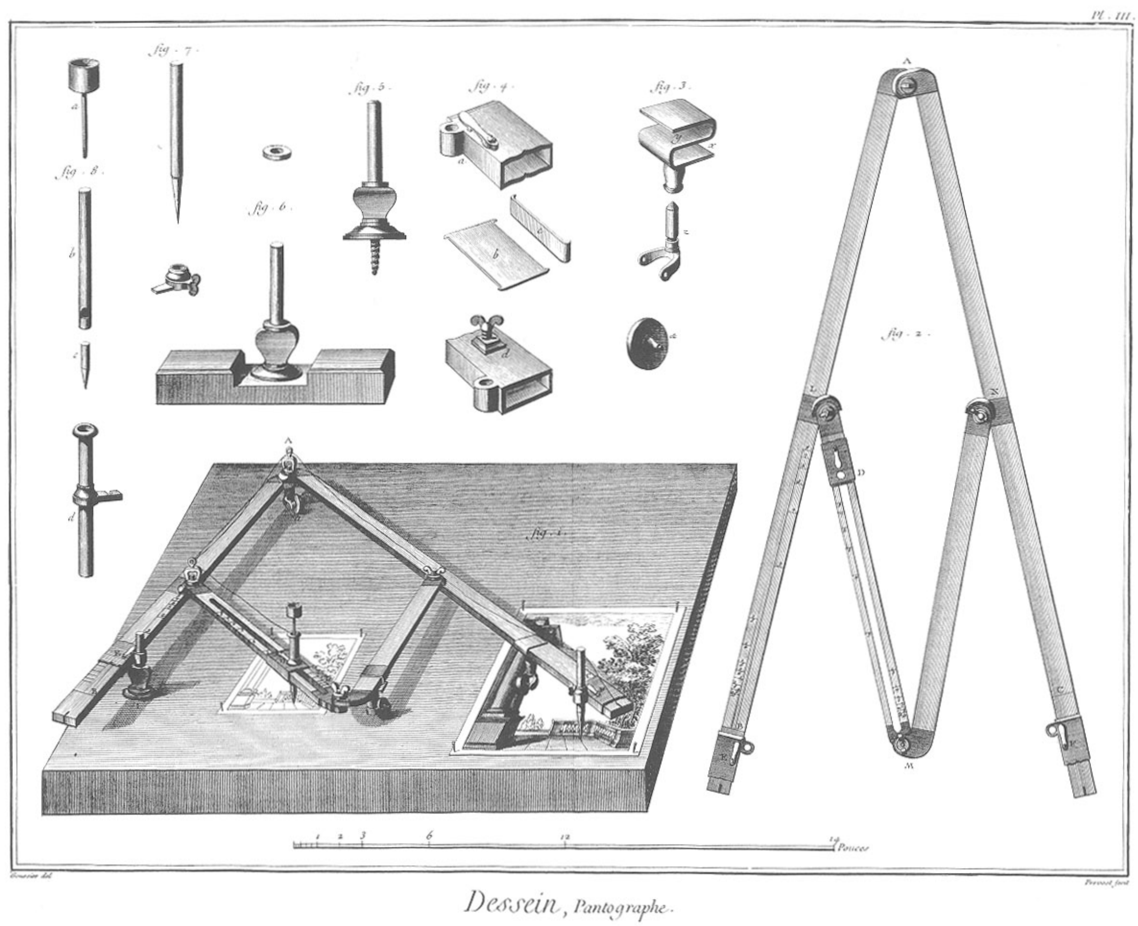


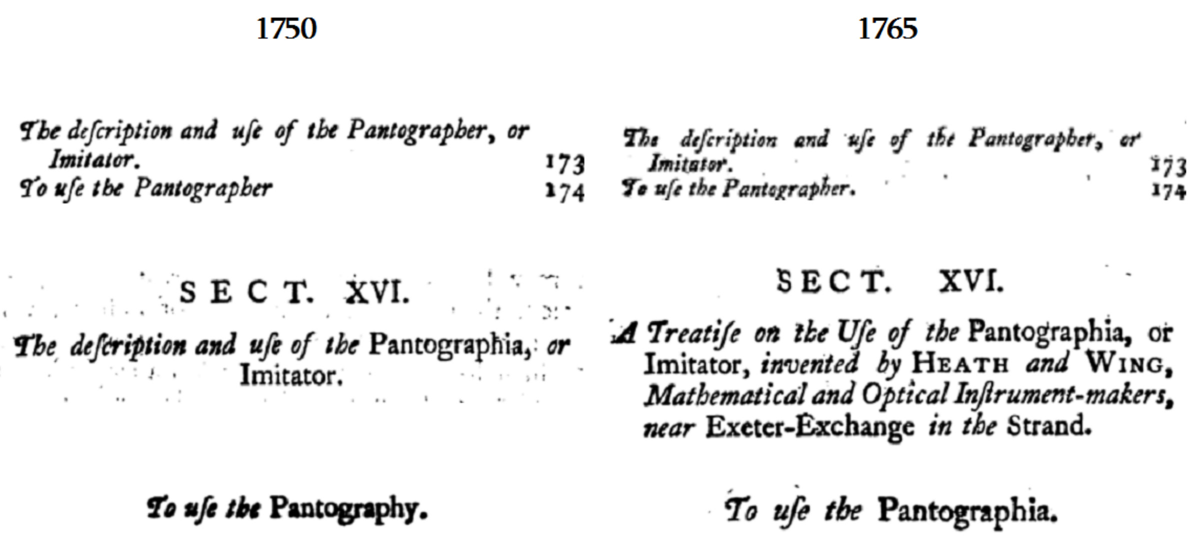
Figure 3.5
The pantograph as represented in *l'Encyclopédie* based on an illustration from Canivet's pamphlet. Source: Denis Diderot and Jean D'Alembert, *Encyclopédie ou Dictionnaire raisonné des sciences, des arts et des métiers: Planches* (Chez Briasson, 1762), vol.3, "Dessein", pl 3.

What a reversal of fortune for the names "pantographe" and "singe"! As in the case of "mètre", "graphe" ascended through the work of engineers and instrument makers associated with the French king. As such, dignified names for the instruments had to be used and the lowly "singe" had to bow in front of the noble "pantographe". At the same time, another distinction was drawn as shown in the text of *l'Encyclopédie*. The true word ("le vrai mot")

104. Jean-Gaffin Gallon, *Machines et inventions approuvées par l'Academie royale des sciences depuis son établissement jusqu'à present* (Paris: Gabriel Martin : Jean-Baptiste Coignard : Hippolyte-Louis Guerin, 1735), v. 7, 207.

was preferred to the common word ("nommé communément"). The distinction was similar to that drawn by Wolff between the common ("vulgo voces") and true meaning of words.

The term "pantograph" was taken over in English in 1750 when Hammond and Warner's new edition of *The practical surveyor* included a section on "The Pantographer, for copying of Drawings" that was based on Claude Langlois' pamphlet. The French "pantographe" was rendered as "Pantographer" in the table of contents, then as "Pantographia" in the corresponding section, and as "Pantography" in one of the subsections (see Fig. 3.6)! These were not simple typos as it is clear from the fact that the revised edition of 1765 only replaced "Pantography" with "Pantographia". Instead, these three words show how foreign the "graph" ending was in English in the mid 18th century. The only *graph* endings that were intelligible to the publishers or readers of a book such as the *The practical surveyor* were "grapher", "graphia" or "graphy". "Graph" had yet no analogy.


1750

The deſcription and uſe of the Pantographer, or Imitator. 173
To uſe the Pantographer 174

SECT. XVI.

The deſcription and uſe of the Pantographia, *or* Imitator.

To uſe the Pantography.

1765

The deſcription and uſe of the Pantographer, or Imitator. 173
To uſe the Pantographer. 174

SECT. XVI.

A Treatiſe on the Uſe of the Pantographia, or Imitator, *invented by* HEATH *and* WING, *Mathematical and Optical Inſtrument-makers, near* Exeter-Exchange *in the* Strand.

To uſe the Pantographia.

Figure 3.6
Comparison of the 1750 and 1765 editions of Hammond and Warner's *Practical surveyor*.

Once "pantograph" was established, it was soon imitated. The "Catalogue of Instruments" (1789) of the instrument maker George Adams advertised a "cyclograph, or an instrument by which circles may be described". The instrument was described in detail in Adams highly popular book *Geometrical and Graphical Essays* (1st ed. 1791). The use of the ending increased exponentially (see Table 3.4). While there were some French or German instruments which used *graph* the highest concentration of such instruments was to be found in Britain. Because many of these instruments were first described in the pages of the *Transactions of the Society of Arts*, a very distinct and localized naming pattern emerged.

Table 3.4
The first mentions of *graph* referring to instruments. The list comes to show the establishment of a new naming pattern.

Cyclograph	1789. George Adams. "an instrument by which circles may be described"
Ellipsograph	1794. Georg Friedrich Parrot. "womit man wahre Ellipsen ohne Berechnung der Brennpunkte sehr leicht beschreiben kann"
Optigraph	1807. Thomas Jones. Instrument for perspective drawing
Pronopiograph	1812. Soliel. "Instrument dont on se sert pour dessiner ce que l'on a devant soi"
Elliptograph	1813. John Farrey. "instrument for describing Ellipses"
Sectograph	1813. Thomas Jones. "Description and Use of an Instrument called 'The Sectograph', principally intended for the Purposes of dividing right Lines into equal Parte, measuring Angles, and inscribing Polygons in the Circle"
Perspectograph	1814. Edmund Turrell. "Instrument for drawing objects in perspective"
Dioragraph	1813. Simeon De Witt. "serve the purpose of drawing in perspective mechanically"
Cyclograph	1815. P. Nicholson. An instrument for drawing arcs of circles
Curvagraph	1818. W. Warcup. "an instrument for describing curved lines"
Hyalographe	1818. Clinchamp. Instrument "pour dessiner des perspectives et obtenir des épreuves du dessin"
Quarreograph	1819. Auracher von Aurach. "ein neues und einfaches Instrument um jede perspektivische Zeichnung mit der strengsten Genauigkeit aufzunehmen, und selbe im gehörigen Ton durch Schatten und Licht vollkommen zu entwerfen."
Arcograph	1821. Benj. Rotch. Similar to the cyclopgraph
Eidograph	1821. William Wallace. Instrument for copying drawings, either on an enlarged or a reduced scale.
Diagraphe	1830. Gavard. "une machine à dessiner", similar to the optigraph

Continued on next page

Table 3.4 (Continued)

Agatographe	1834. J.N. Symian. "instrument de perspective servant aux artistes pour obtenir l'ensemble d'une pose ou d'un mouvement quelconque."
Cymagraph	1837. Robert Willis. Instrument for tracing profiles and mouldings
Odontograph	1838. Robert Willis. Tooth modeling instrument
Isograph	1838. Thomas Sopwith. Instrument for transferring plans from orthographical to isometrical projections
Helicograph	1851. Francis Penrose. Logarithmic Spiral Compass

2.3 *Graph* as person

By the beginning of the 19th century, labeling a drawing instrument with a name ending in *graph* was a practice established through analogy with previous names. It was these precedents that validated the naming practice. However, if we turn our attention to the moments when the first instruments were labeled by *graph*, when there was no preestablished pattern to rely on, we will encounter a different validation procedure.

"Singe", the 18th century French word for pantograph, meant not only "monkey". It could also refer to an imitator, a counterfeiter, or a copist. Antoine Furetière's *Dictionnaire universel* (1708) explained that "On dit aussi d'un homme, que c'est un vrai *singe*, quand il affecte de contrefaire quelcun, d'imiter ses actions, ses discours, son stile." This sense of the word was used in expressions such as "ces copistes, ces *singes* de Seneque" or "peuple *singe* du maître". The name "singe" described the function of the instrument through a *personification*: it described the job accomplished by the instrument as if it was a person. This personification was preserved when "singe" was replaced by "pentographe", because the ending "graphe" was almost uniquely associated with a person (see Table 3.1). This association between a person and the instrument was preserved in English where *The practical surveyor* (1750) introduced "the Pantographer, or Imitator". It is this feature of *graph*, the ability to personify an instrument, which distinguished its use and meaning from *meter* and *scope*.

When in 1792 Claude Chappe, a clergymen, presented what many credit as the first optical telegraph he called the instrument the "tachygraphe", or "**qui** écrit vite". The name was soon changed following the advice of the count Miot de Mélito, an official in the war ministry, who proposed instead "télégraphe", or "**qui** écrit de loin".[105] Chappe's choice of "tachygraphe" was based on the well-known "tachygraphie" or " l'art d'écrire avec rapidité & par notes", a term which also generated "des tachygraphes", the people who practiced this art.[106] "Le tachygraphe", the instrument, accomplished the job of "le tachygraphe", the person. As a reward for his invention Chappe was named "ingénieur-télégraphe", a term which again eluded the distinction between person and instrument.

This insight applies also to the first autograph that was actually labeled using the ending *graph*. In 1775 it was reported that M. Courgeoles (or Courejolles) presented to Louis XV a new instrument for observing the time and physical quantities measured in meteorology such that "les physiciens qui font des observations météorologiques n'auront plus besoin de s'en occuper [des observations météorologiques]". The instrument was accordingly named "*Météorologue* ou *Météorographe*" and could "tient note de tous les mouvements de l'atmosphere pour toutes les heures de la nuit & du jour".[107] In this case *graph* described not so much the action of the instrument or the trace it produced, but rather the title of the observer that the instrument could replace.

The "Météorographe" presented to the king and exposed at Versailles in the Royal apartments was mainly a rhetorical spectacle, a promise of what such an instrument could achieve. The imprecision of the instrument was too great to have any practical value. Some improvements were proposed by Pierre Changeux who published between 1780 and 1781 a series

105. The etymological interpretations pertain to Mélito, see Miot de Mélito, *Mémoires du comte Miot de Mélito*, vol. 1 (Paris: M. Lévy frères, 1873), 36, my underline.
106. Diderot and D'Alembert, *Encyclopédie ou Dictionnaire raisonné des sciences, des arts et des métiers*, vol.15 815. Though a person who practiced the art of tachygraphy would have been called a "tachygraph(er)", this was not a recognized occupation the way a biographer or bibliographer were.
107. *Journal Encyclopedique Ou Universel*, vol. 7 (A Bouillon, 1775), 155.

of texts popularizing his self-registering meteorological instruments. Changeux's innovation was to change the writing mechanism to significantly reduce frictions: instead of a continuous action of the pencil on paper, the pencil would only come in contact with the paper every two minutes. Changeux advertised his instruments as "machines *Météorographiques*" which included "*le Barométrographe*, *le Thermométrographe*, *l'Anemométrographe*, *l'Higrometrographe*", or "*l'eudiométrographe*, pour mesurer l'élasticité de l'air ; le *manométrographe*, pour sa densité ; l'*électométrographe*, pour la quantité d'électricité qu'il contient ; le magnétométrographe, pour mesurer les variations de la boussole", or "*le pluviométrographe*, *...l'atmimétrographe* ou instrument pour mesurer l'évaporation, *...le royamétrographe* ou instrument pour marquer la hauteur précise de chaque marée".[108] Changeux added to Courgeoles's "Météorographe" eleven new instrument labels. Out of all the named instruments only the barometrographs were actually constructed. The new name was supposed to reflect the fact that the instrument did not only measure the atmospheric pressure but also "tient note par écrit, c'est-à dire, par des traces sensibles, & des variations".[109] Changeux submitted one of barometrographs to be inspected and approved by *L'Académie Royale des Sciences* who assigned Jean-Baptiste Le Roy and Mathurin-Jacques Brisson with the task. Their laudatory report emphasized both the original and innovative solution proposed by Changeux, and the utility of meteorographs for the continuous meteorological observations of phenomena that so far escaped human attention or diligence.[110] Johann Jakob Hemmer, the secretary of the Palatine Meteorological Society that had just been established in 1780, bought directly from Changeux one of his barometro-

108. Pierre Changeux, "Aux Auteurs du Journal de Physique, contenant la confirmation des avantages de l'Elecricité dans les Asphyxies, & l'annonce d'un Barométrographe & de plusieurs machines Météorographiques," *Journal de physique, de chemie, d'histoire naturelle et des arts* 13 (1780): 75-76; Pierre Changeux, "Description de deux Barométrographes ou Baromètres qui tiennent note, par des traces sensibles, de leurs variations & des tems précis où elles arrivent. Avec l'idée de plusieurs autres Instrumens Metéorographiques," *Journal de physique, de chimie, d'histoire naturelle et des arts* 16 (1780): 342; Pierre Changeux, *Météorographie, ou Art d'observer d'une manière commode et utile les phénomènes de l'atmosphère* (Paris, 1781), 32-33.

109. Changeux, "Description de deux Barométrographes ou Baromètres qui tiennent note, par des traces sensibles, de leurs variations & des tems précis où elles arrivent. Avec l'idée de plusieurs autres Instrumens Metéorographiques," 330.

110. The report is reproduced in ibid., 343-347.

graphs.[111] The fame of Changeux's instruments was considerable enough to spark a priority dispute with the Portuguese natural philosopher and instrument maker Jean Hyacinthe de Magellan.[112] Magellan, who was a member of the Royal Society and was living in London at the time, just published a "letter" in which he described the "Idée d'un Météorographe Perpetuel".[113] Magellan referred to Changeux's January letter from the *Journal de physique*, and most probably that is how he came upon the term "Météorographe". However, he did not acknowledge the source of the term, and he slyly tried to get credit for it by changing the name of the instrument "que je l'appele *Météorographe Perpetuel*".[114]

The new names, and the pattern through which they were constructed, entered Italy through the translation of the description of Changeux's instruments.[115] Less than a year later Giambattista Beccaria introduced a new instrument that he called a "*ceraunografo*":

> A clock-work rotated a circular cardboard disk, and on each side of the circumference were attached two wires: one connected to the ground, and the other connected to the outside collected the electricity of the storm. The sparks between the two wires left a trace on the paper corresponding to the time of discharge.[116]

In 1782 Marsilio Landriani described two new instruments, one he called a "croniografo" that could record the time of rain besides the amount of water, while the second instrument he called a "iometrografo" which could record the amount of rain water per hour.[117] In 1785

111. David C. Cassidy, "Meterology in Mannheim: The Palatine Meterological Society, 1780-1795," *Sudhoffs Archiv* 69, no. 1 (1985): 22.
112. For more on Magellan, but mainly in connection to his chemical enterprises, see Jan Golinski, *Science as Public Culture: Chemistry and Enlightenment in Britain, 1760-1820* (Cambridge: Cambridge University Press, 1999), esp. 122-127. Magellan was corresponding with Watt and Boulton who invented the indicator diagram.
113. J. H. Magellan, *Collection de differens traites sur des instrumens d'astronomie, physique, &c* (London: W. Richardson, 1780), 158.
114. Ibid., 159.
115. Changeux's instruments was described in Italian journals as a "barometrografo ossia stromento in cui vengno segnate sul barometro le variazioni", in "Barometrografo," *Opuscoli scelti sulle scienze e sulle arti* 2 (1779): 246–7.
116. Giambatista Beccaria, *Di un ceraunografo e della cagione de tremuoti* (Torino: Presso Giammichele Briolo, 1780).
117. Marsilio Landriani, "Descrizione di una nuova macchina Metereologica colla quale si determina la durata della pioggia," in *Opuscoli scelti sulle scienze e sulle arti*, vol. 3 (Milano, 1780), 273–76; Marsilio Landriani, "Descrizione di una macchina Metereologica," *Memorie di matematica e fisica della Societa italiana* 1 (1782): 203–224.

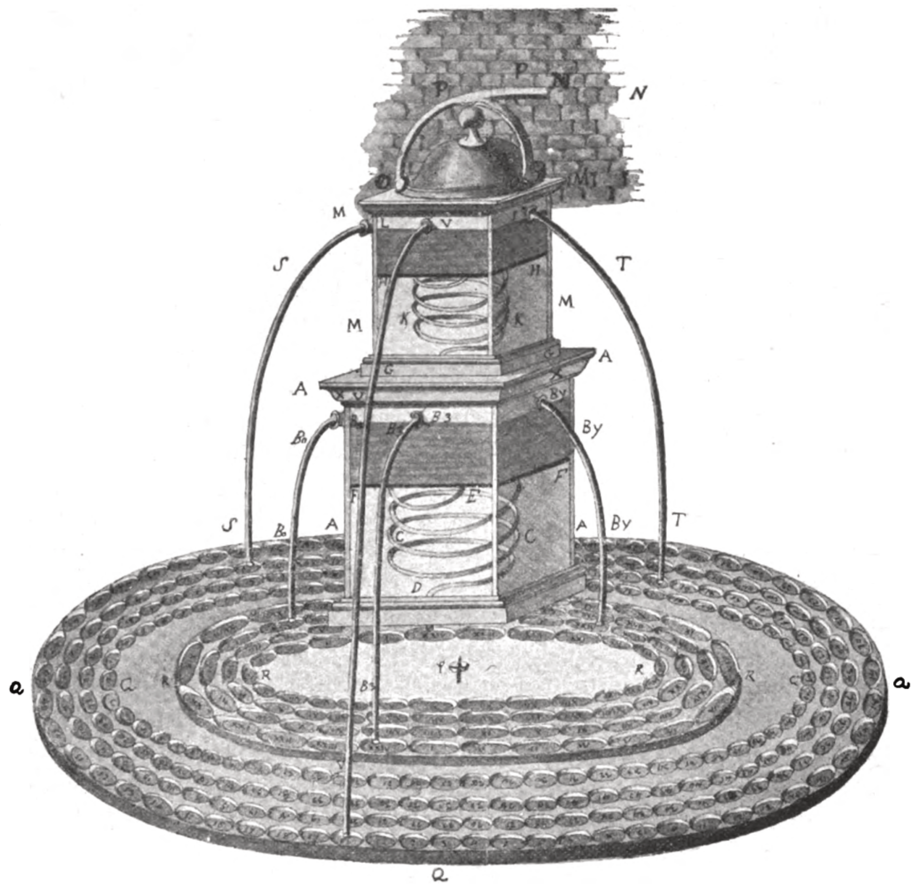


Figure 3.7
Cavalli's design for a "sismografo". Source: Cavalli's original drawing reproduced in *Annali dell'Ufficio centrale meteorologico e geodinamico italiano*, vol. 17 (Roma: Unione cooperativa editrice, 1897).

Atanasio Cavalli's described a "sismografo" which was an instrument that could determine the direction of the shock of an earthquake (similar to a "sismoscopio a mercurio") but which could also indicate the time of the earthquake (see Fig. 3.7).[118] Cavalli's "sismografo" is particularly interesting because it clearly shows that not all *graph* instruments produced a graphical trace. In a description of his meteorological observatory in Milano Pietro Moscati included among his many instruments "mie macchine meteorografiche": "anemometrografo", "elletrografo" (inspired by Beccaria's ceraunografo), "pluviografo" (an adaptation of Landri-

118. Atanasio Cavalli, "Lettera del Sig Ab Cavalli a S E il Sig Duca di Sermoneta," *Antologia romana* 12 (1786): 121–123; Cavalli, "Lettera del Sig Ab Cavalli a S E il Sig Duca di Sermoneta."

ani's croniografo), and a "atmidografo".[119] Moscati's meteorological observatory in Milano was not unique in the names it used for its instruments. A description from 1810 of the Physics Museum in Florence mentioned that "La Stanza Meteorologica" was furnished with a "Desi-aerometrografo", "Barometrografo", "Anemoscopiografo con suo anemometrografo", and "Pluviometrografo".[120]

3 IN THE ABSENCE OF THE OBSERVER

3.1 A register by mark, *not* trace

The French "météorographes" of the late-18th century were not a novel idea. Already in 1647, when only fifteen, Christopher Wren designed "a weather clock namely, with revolving cylinder, by means of which a record can be kept through the night", as he described it in a letter to his father.[121] The physician Charles Scarborough with whom Wren was lodging offered to finance the invention, however, there is no further notice about it. During his studies at Wadham College at Oxford, Wren further cultivated his interests in the "weather clock", to which he now added a thermometer he called a "weather-wheel" to form a "perpetual motion, or weather-wheel and weather-clock compounded".[122] While we do not know if Wren actually built these instruments, he did describe them to a French visitor, Balthazar de Monconys, in July 1663.[123] In Monconys' account the "weather-clock" could measure and record the di-

119. Pietro Moscati, "Descrizione d'un nuovo atmidometro orario," *Opuscoli scelti sulle scienze e sulle arti* 5 (1782); Pietro Moscati, "Descrizione dell'Osservatorio meteorologico eretto al fine dell'anno 1780," *Memorie di matematica e fisca della societa italiana* 5 (1790): 356–381.

120. *Annali del Museo Imperiale di fisica e storia naturale di Firenze*, vol. 2 (Firenze: Patti, 1810).

121. Wren quoted in Lena Milman, *Sir Christopher Wren* (Duckworth and Company, 1908), 19. For the history of these early self-registering instruments see Hoff and Geddes, "The Beginnings of Graphic Recording."

122. Christopher Jr. Wren, *Parentalia: Or Memoirs of the Family of the Wrens* (London: Osborn, 1750), 198.

123. Based on Monconys's account of Wren, historians have assumed that Monconys actually saw these instruments. "This meteorograph was seen by Balthasar de Monconys on June 10, 1663", Middleton, *A History of the Thermometer and Its Use in Meteorology*, 42; "Wren's weather clock (see Monconys, 1665, v. 2, p. 53) which he saw in June 1663", J. A. Bennett, *The Mathematical Science of Christopher Wren* (Cambridge: Cambridge University Press, 1982), 84-85. However, Monconys only claimed that Wren *told him about these instruments, but not necessarily that he saw them*: "Il ne laissa pas de me dire fort librement de son Horologe du temps… Le 12 je fus voir M. Renes [Wren], qui me dit…", Balthasar de Monconys, *Journal des voyages de Monsieur de Monconys* (Lyon: Horace Boissat, 1666), v. 2, 53-54. There is only one mention that seems to suggest that Monconys has seen the

rection of the wind, the amount of rainfall and temperature at the same time. In December 1663, Wren also described his "weather-clock" to the Royal Society, and Robert Hooke was put in charge with building such a machine.[124] The enterprise took longer than expected and only in 1678, fifteen years later, Hooke presented the new weather-clock which now included a barometer, a thermometer, a hygroscope, a rain-bucket, and a wind-vane.[125] The machine was installed in Harry Hunt's lodgings.[126] Besides the number of instruments, Hooke also changed the mechanism of recording; instead of using a pencil that leaves a continuous trace (as specified in Wren's design), the "weather-wiser" (as Hooke also called the machine) employed a hammer that stroke "punches" every quarter of an hour – a very similar design to what Changeux will use a century later.[127]

It is doubtful these instruments ever worked properly–at best they gave the impression, that if improved upon, they could accomplish their task. What interests us here is the language through which the action and purpose of these instruments was described. Everyone agreed that keeping a steady account of the weather was a particularly difficult and tedious task. First, keeping a weather diary was "very Difficult to perform so as to make it useful and instructive without a great apparatus of Barometers, Thermometers, Hygroscopes, Instruments for telling the Point of the Wind, the Force of the Wind, the Quantity of Rain that falls, the times of the Sun's shining and being overcast".[128] Second, as Wren remarked, out of all quantities that had to be measured "the greatest difficulty will be in keeping the Diary of the Winds, and Air, because it seems to require constant Attendance".[129] The purpose of the weather-clock was to meet these two challenges. Because "many changes may happen while the observer is absent

"weather-wheel", but not the "weather-clock": "J'y vis aussi la Machine de M. Renes [Wren] pour la mesure du chaud & du froid, qui est faite de fer blanc...", Monconys, *Journal des voyages de Monsieur de Monconys*, v. 2, 56. Only the "weather-wheel" seems to have actually have been built by Wren, though again we must be careful because Monconys only described how the machine looked, not if it was actually working.

124. Bennett, *The Mathematical Science of Christopher Wren*, 84-85.
125. Robert Hooke, *Philosophical Experiments and Observations* (Printed by W. and J. Innys, 1726), 41.
126. Thomas Birch, *The History of the Royal Society of London* (A. Millar, 1756–1757), v. 3, 486-7.
127. Hooke, *Philosophical Experiments and Observations*, 41-42.
128. Gunther, *Early Science in Oxford*, v. 12, 137.
129. Wren, *Parentalia: Or Memoirs of the Family of the Wrens*, 221.

or asleep", Wren imagined that

> if you visit your chamber but one half hour in the day, [the weather-clock] shall tell you how many changes of wind have been **in your absence**... Neither shall the thermometer need a constant observance, for after the same method may that be made to be **its own register**.[130]

In the minutes of the Royal Society the purpose of Hooke's weather-clock was described in similar terms as

> to keep an account of all the changes of weather, which should happen, viz. 1. The quarters and points, in which the wind should blow. 2. The strength of the wind in that quarter. 3. The heat and cold of the air. 4. The gravity and levity of the air. 5. The dryness and moistnefs of the air. 6. The quantity of rain, that should fall. 7. The quantity of snow or hail, that shall fall in the winter. 8. The times of the shining of the sun.[131]

While instruments like Wren's and Hooke's remained rare throughout the 18th century, a different type of instrument came to borrow the tropes of the weather-clock. In 1757 Lord Charles Cavendish presented a thermometer for "shewing the greatest degree of heat, which happens in any place during the absence of the observer".[132] Though Cavendish imagined that his thermometer could even be used for plunging into the sea, it was actually too delicate to be of practical use; furthermore, it could only register large steps in temperature. After designing a thermometer which could mark by a pencil its indications in "the absence of an observer", Keane Fitzgerald presented another thermometer "on the same principle ...with registers to mark the least variation that can happen during the absence of the observer". The new instrument removed the inconvenience from the friction, and "the trouble with rubbing out the mark, every time a new observation was intended".[133]

130. Wren, *Parentalia: Or Memoirs of the Family of the Wrens*, 221-224, my underline.
131. Birch, *The History of the Royal Society of London for Improving of Natural Knowledge, from Its First Rise*, v. 3, 445.
132. Charles Cavendish, "A Description of Some Thermometers for Particular Uses," *Philosophical Transactions* 50 (1757): 300–310. The first description of a maximum and minimum thermometer is attributed to Jean Bernoulli who described such an instrument in a letter to Leibniz in 1698, though he never constructed it, see Middleton, *A History of the Thermometer and Its Use in Meteorology*, 149-165.
133. Keane Fitzgerald, "A Description of a New Thermometer and Barometer," *Philosophical Transactions* 52 (1761): 146.

It was the thermometer of James Six, designed "to shew accurately the greatest degree of heat and cold which could happen in the observer's absence", that redefined meteorological instruments and observations.[134] This was the first maximum and minimum thermometer sturdy enough to be used on a large scale. In 1794, the Royal Society adopted Six's thermometer for its own meteorological observations, and their example was followed by the Kew Observatory in 1798. The success of the instrument assured Six's election to the Royal Society. For meteorological observations, Six's thermometer was gradually replaced by Daniel Rutherford's thermometer that was more precise and reliable. Employing the same language, Rutherford presented his thermometer as "fitted to mark the lowest or the highest point to which the fluid has attained in the absence of the observer".[135] However, because the Rutherford thermometer employed glass cones it forced the instrument to be mounted perfectly horizontal. For this reason Six's thermometer remained popular for sea expeditions, deep-sea measurements or balloon ascents. The thermometer was used on several arctic voyages (including John Ross and William Parry), or in measuring the average temperature in London (by Howard Luke in 1807).[136] Six did not patent his thermometer which was manufactured and improved upon by various instrument makers. One such variation on Six's thermometer was produced by the Italian physicist Angelo Bellani who referred to his instrument as "thermometrograph, or thermometer with a double indicator with which the temperature is marked in the absence of the observer [Termometrografo, ossia Termometro a doppio indice col mezzo del quale viene dinotata la temperatura in assenza dell'osservatore]" or "thermometer for inaccessible places [Termometro per luoghi inaccessibili]".[137] Among its various uses, Bellani's thermometrograph

134. James Six, "Account of an Improved Thermometer," *Philosophical Transactions of the Royal Society of London* 72 (1782): 73-4. For a history of James Six and his thermometer see Jillian F. Austin and Anita McConnell, "James Six F.R.S.. Two Hundred Years of the Six's Self-Registering Thermometer," *Notes and Records of the Royal Society of London* 35, no. 1 (1980): 49–65.

135. Daniel Rutherford, "Description of an Improved Thermometer," *Transactions of the Royal Society of Edinburgh* 3 (1794): 247.

136. Austin and McConnell, "James Six F.R.S.. Two Hundred Years of the Six's Self-Registering Thermometer," 60-62.

137. Antonio Bellani, "Memoria sopra un nuovo termometrografo ossia termometro per luoghi inaccessibili," *Giornale di fisica, chimica e storia naturale* 4 (1811): 89–110.

found its way into the practice of silk worm cultivators because "it indicates the maximum and minimum of the temperature of the rooms, where the worms are reared, whilst the rearer is absent".[138]

The titles of the papers in which such instruments were first presented is revealing for how the new grouping emerged. Cavendish titled his paper "A Description of Some Thermometers for Particular Uses" (1757), Six chose "Account of an Improved Thermometer" (1782), an example followed by Rutherford's "Description of an Improved Thermometer" (1794). However, in the 1790s a new pattern emerged. In 1791 the reverend Arthur M'Gwire communicated a "Description of a self-registering barometer", an instrument consisting of a ruled piece of paper was moved horizontally by a clock-work, while a pencil moved vertically when the mercury level in barometer changed.[139] Alexander Keith published a "Description of a thermometer, which marks the greatest degree of heat or cold, from one time of observation to another, and may also register its own height at every instant" (1798). By the 1820s the new label was well established and it was applied to either instruments "which by inspection denote the greatest heat or cold which has occurred since the last observation, or else are employed to note the temperature at any moment in the absence of the observer".[140]

It is important to notice that the self-registering instruments which produced a trace were not labeled by this feature, but rather by their ability to keep a register "at any moment" or "at every instant". Such a feature was deemed particularly important in the case of quickly varying quantities such as wind direction or speed.[141] But even in the case of temperature or air

138. Joseph Hazzi, *Letter from James Mease: Transmitting a Treatise on the Rearing of Silkworms* (Duff Green, 1828), 68.

139. Arthur M'Gwire, "Description of a Self-Registering Barometer," *The Transactions of the Royal Irish Academy* 4 (1791): 141–143.

140. *The Edinburgh Encyclopaedia* (Joseph Parker, 1832), v. 18, 13. For the common use of the label in titles see for example H. H. Blackadder's "Description of a New Register Thermometer, Without Any Index" (1826), "On the Construction of Meteorological Instruments, so as exactly to determine their Indications during Absence, at any given instant, or at successive intervals of Time" (1826); James King's "Account of a new self-registering thermometer" (1828); David Brewster's "Remarks on self-registering thermometers" (1828), etc.

141. "But the determination, however precise, of the velocity of the wind at any moment, is of little importance to meteorology; and it is **not till we can obtain some general register of the state of the wind in the absence of the**

pressure, knowing the intermediate values between the minimum and maximum value would have been crucial for determining an accurate average temperature.[142] However, the spread of these instruments was limited mainly because their measurements were fairly imprecise. Especially in the case of mercury-based barometers and thermometers the motion of the liquid transmitted only a very slight impulse to the pencil; thus, any friction with the paper greatly affected the accuracy of the registered curve. In 1832 David Brewster expressed his doubts about the utility of such instruments:

> This mechanical invention [Keith's thermometer] is very elegant, but has never met with, and is not calculated for, general adoption. Such mechanically drawn charts might please the eye or the fancy of the general observer, but we think we may lay it down as a maxim in meteorological science, that little is to be done in the present state of its advancement, by any mere mechanical contrivances not of the simplest possible nature, or which extend beyond the limits of a glass tube hermetically sealed.[143]

As discussed below, these instruments would only become popular in the 1840s when either new methods of registration were introduced, or the liquid mercury was replaced by a different principle (as in the case of the aneroid barometer). In the 1820s the main instruments that could keep a fairly precise register at every instant were the wind and tide gauges because their parts were acted upon with enough force to compensate for the friction between pencil and paper.

3.2 The Absent Observer

Why were the observers absent? Was not observation the job of an observer?

observer, by a machine of sufficient simplicity to be generally adopted, that we can hope to raise the philosophy of the wind to that importance which I am disposed to think it deserves to hold in atmospheric science; and no period was ever farther from such an acquisition than the present, when the anemometer is the most neglected of all meteorological instruments. **A register of the force or velocity of the wind in the absence of the observer** must of course include the register of its direction. An instrument for the last purpose was contrived by one Michael Lomonosow, as I found long after the contrivance I have now to describe was formed; but for the former and more important object, no plan, as far as I know, has been proposed", in James D. Forbes, "Description of a New Anemometer," *The Edinburgh Journal of Science* 2, no. 3 (1830): 32, my underline.

142. Often, in the early 19th century the average temperature was determined as the average of the minimum and maximum values.

143. *The Edinburgh Encyclopaedia*, v. 18, 15.

In the description of his improved thermometer, Six described poetically the hardships of observations that had to be carried out in the open:

> The sultry heat of the summer's days, and freezing cold of the winter's nights, which is commonly most severe at a late unseasonable hour, render it very unpleasant to be abroad in the open air, although it is absolutely necessary for the thermometer to be placed in such a situation.[144]

These observations were particularly painful when one aimed to measure extreme temperatures, because any measurement with a standard thermometer entailed that one had to be exposed in the open for long periods of time. It is not a coincidence that Six's thermometer was particularly popular on arctic expeditions and the study of glaciers.[145] Even carrying out normal measurements at sea could be particularly challenging, as one observer confessed that "[s]ea-sickness, and the unaccommodating temper of the skipper, prevented me from keeping a regular meteorological journal".[146] In other cases, the place was simply inaccessible to the presence of an observer, especially if one carried out deep-sea or high-altitude measurements. Bellani inspiredly named his version of Six's thermometer a "thermometer for inaccessible places [Termometro per luoghi inaccessibili]".[147]

The physical absence of observers charged with the tedious task of constant presence was not uncommon, especially when the observation point was remotely located outside the watchful gaze of the central observatory. In the early 1830s Thomas Young, John Herschel and Charles Babbage went to visit the tide-pole near Greenwich to find out "to their surprise, that the small house surrounding the tide pole was locked and had not been opened for some time".[148] When the first self-registering tide-gauge was erected at Sheerness in Kent by the

144. Six, "Account of an Improved Thermometer," 72.
145. For the arctic expeditions see Austin and McConnell, "James Six F.R.S.. Two Hundred Years of the Six's Self-Registering Thermometer," 60-62. A version of Six's thermometer (Bunten's "thermométrographe") was used in the early 1840s by the Swiss naturalist Louis Agassiz in his research on glaciers.
146. James King, "Observations on the Climate and Geology of New South Wales," *Edinburgh journal of science* 9 (1828): 118.
147. Bellani, "Memoria sopra un nuovo termometrografo ossia termometro per luoghi inaccessibili."
148. Michael S. Reidy, "Gauging Science and Technology in the Early Victorian Era," *The Machine in Neptune's Garden: Historical Perspectives on Technology and the Marine Environment*, 2004, 6.

engineer J. Mitchell, it was remarked that:[149]

> Yet, notwithstanding the correctness of this [previous] gauge in giving the rise and fall of tide, it requires an observer at high and low water to watch for the time. By night, no attendant is on the spot; by day, too, he is sometimes absent; and even when present, the most watchful observer often cannot tell the precise time from five to twenty minutes; as the water is at times stationary, or nearly so, for more than half an hour; moreover, sometimes it falls a few inches, and rises again. To meet these difficulties, it occurred to the civil engineer at that dock-yard, Mr. Mitchell, to cause the tide-gauge to register itself… [150]

Even when the observer was physically present, his mind might have been away. Henry Palmer, an engineer at the London Docks, addressed this problem in 1831 in a communication to the Royal Society on a "Description of a graphical register of tides and winds":

> The performance of such a machine must if well arranged be evidently free from those inaccuracies and doubts which the frequent and long-continued observations of individuals, through nights as well as days, must be liable to. It will require only the occasional attention of a superintendant to correct the time, and supply it with paper.[151]

Even professional meteorologists were afflicted with the boredom of observation. One observer called the keeping of weather journal "the veriest drudgery in science", while another recalled the "mühsame als langweilige Beschäftigung" of observation.[152]

3.3 Self-recording *Graphs*

If in French and Italian the suffix *graph* was highly productive in naming meteorological instruments since the late 18th century, the term only became productive in German and English in the 1840s.

149. Michael S. Reidy, *Tides of History: Ocean Science and Her Majesty's Navy* (Chicago: University of Chicago Press, 2008), 141-142; Reidy, "Gauging Science and Technology in the Early Victorian Era."

150. *The Nautical Magazine*, vol. 1 (Brown, Son and Ferguson, 1832), 402.

151. Henry R. Palmer, "Description of a Graphical Registrer of Tides and Winds," *Philosophical Transactions of the Royal Society of London* 121 (1831): 210.

152. James King, "Account of a New Self-Registering Thermometer," *Edinburgh Journal of Science* 9 (1828): 118; Carl Kreil, "Beschreibung eines selbstverzeichnenden Barometers und Thermometers," *Astronomisch-meteorologisches Jahrbuch für Prag* 2 (1843): 255.

The first consistent use of *graph* in German was made by Carl Kreil, an Austrian meteorologist and astronomer who in 1845 became the director of the Prague observatory and in 1851 the director of the Centralanstalt für Meteorologie und Erdmagnetismus in Vienna. Kreil undertook a full mechanization of his meteorological instruments to form a "meteorologischen Beobachtungs-Systems". His instruments, to which he referred as "autographen Apparate", "Autographen", "autographe Barometer und Thermometer", "Barometergraphen", "Thermometergraphen", etc., would register their observations at intervals of five minutes.[153] The label "autographen" was probably coined by Kreil as an alternative to the common terms "selbstverzeichnen", "selbstaufzeichnen", or "selbstaufschreiben". "Autographen" became a staple of Kreil's circle, being used almost exclusively by him and meteorologists working in Prague or Vienna, or who were in contact with Kreil.[154]

In Britain, a major shift in terminology occurred after the photographic method of registration was introduced in 1845 by Francis Ronalds at the Kew Observatory and by Charles Brooke at the Greenwich Observatory. Ronalds described the method in a paper "On photographic self-registering meteorological and magnetical instruments" which mentioned a "photo-electrograph", "thermograph", "photo-barometrograph" and "magnetograph".[155] Ronalds coined the term "magnetograph" by analogy with "electrograph", an instru-

153. Kreil, "Beschreibung eines selbstverzeichnenden Barometers und Thermometers." For a description of some of these instruments see also W. E. Knowles Middleton, *Invention of the Meteorological Instruments* (Baltimore, Johns Hopkins Press, 1969), 161-163, 181; Middleton, *A History of the Thermometer and Its Use in Meteorology*, 190.

154. Carl Kreil, *Magnetische und meteorologische Beobachtungen zu Prag*, vol. 3 (Prag: K. Kreil, 1843), 131-138; Carl Kreil, *Magnetische und meteorologische Beobachtungen zu Prag*, vol. 5 (Prag: K. Kreil, 1845), i-iii; Carl Kreil, *Entwurf eines meteorologischen Beobachtungs-Systems für die österreichische Monarchie* (Wien: Kaiserl.-Königl. Hof- und Staatsdr., 1850). The term was used by Carl Jelinek who served as an assistant at the Prague observatory between 1847-1852, see his *Beiträge zur Construction selbstregistrirender meteorologischer Apparate* (1850). Jelinek became the editor of the *Zeitschrift der Österreichischen Gesellschaft für Meteorologie* where the term was used in 1868 to describe F. Pfeiffer's "Windautograph" of in 1872 Schön's "Windrichtungs-Autograph". The term was also used by the Viennese meteorologist Carl Fritsch in *Ergebnisse der meteorologischen Beobachtungen für das Jahr 1846.* In French or English the term was only used in reports on Kreil's instruments which often translated parts of his articles.

155. Francis Ronalds, "On Photographic Self-Registering Meteorological and Magnetical Instruments," *Philosophical Transactions of the Royal Society of London* 137 (1847): 111–117.

ment and term he was well familiar with since his early experiments on electricity.[156] Despite Ronalds' preference for the suffix *graph*, the official description of the instruments at the Paris Exposition of 1855 was more formal. It only mentioned the instruments as "self-registering magnetometer" or "self-registering barometer":

> 12. Self-registering Magnetometer, for recording photographically the variations of the horizontal magnetic intensity, or of the magnetic declination. Invented by Francis Ronald, Esq., F.R.S., and constructed under his direction for the Kew Observatory.
> 13. Self-registering Barometer: for recording photographically the variations of the atmospheric pressure, with mechanical compensation for the effect of temperature. Invented by Francis Ronalds, Esq., F.R.S., and constructed under his direction for the Kew Observatory.
> 34. Specimens of the Photographic Records of the Self-registering Magnetometer and Barometer, with apparatus for measuring the ordinates of the curves.[157]

Similarly, the catalogue entries from the 1862 exhibition referred to "[p]hilosophical instruments from Kew Observatory, Richmond, consisting of self-registering magnetometers and meteorological apparatus, exhibited by the British Association for the Advancement of Science".[158] Informally, as revealed in a letter from Welsh (the Chairman of the Kew Observatory) to the Chairman of the Exhibition Committee, Ronalds' instruments were referred to as "the Bifilar Magnetograph and Barometrograph", "Self-registering Magnetograph", or the "Self-recording Barometer".[159] As Ronalds' method of photographic registration spread to the other British observatories, so did the new terminology. Radcliffe Observatory introduced in 1855 a "thermograph" and "barograph"; an "anemograph" was added in 1858.[160]

The methods of self-registration knew another breakthrough in the 1840s when telegraphy started being used. The first contribution was made by Charles Wheatstone in 1843 who

156. See his "description of a new electrograph" in Francis Ronalds, *Descriptions of an Electrical Telegraph: And of Some Other Electrical Apparatus* (R. Hunter, 1823), 47.
157. As given in the "Copy of the Labels affixed to the various Instruments and Apparatus deposited by the Kew Observatory Committee in the Paris Universal Exhibition" in "Report of the Kew Committee," in *Report of the British Association for the Advancement of Science*, vol. 25 (J. Murray, 1856), xxxii-xxxiv.
158. *Official Catalogue of the Industrial Department* (1862): xiv.
159. "Report of the Kew Committee," xxx-xxxi.
160. As seen in the annual report *Astronomical and Meteorological Observations Made at the Radcliffe Observatory.*

constructed an "electro-magnetic meteorological register" for the Kew Observatory.[161] At the British Association meeting at Cork in 1843 Wheatstone claimed that his instrument "records the indications of the barometer, the thermometer and the psychrometer every half-hour during day and night, and prints the results, in duplicate, on a sheet of paper in figures. It requires no attention for a week, during which time it registers 1008 observations".[162] The machine did not "print" a curve but actual numbers corresponding to the indications of the meteorological instruments.

The most impressive use of Wheatstone's electromagnetic registration was made by the Italian astronomer Angelo Secchi, the director of the observatory of the Collegio Romano, who presented at the 1867 Paris exposition an electrical "Météorographe" (see Fig. 3.8). The imposing instrument was more than three meters high; it carried two charts on which it registered pressure, temperature, the time and amount of rain, and the direction and speed of wind. Some witnesses considered Secchi's meteorograph to have been the main attraction of the exposition, "la pièce capitale et le joyau de la science à l'Exposition universelle".[163] The instrument was rewarded with the Grand Prix of the Exposition. Its success was assured not only by its complexity and aesthetics, but especially by the fact that it was fully operational during the exposition.[164]

The success of Secchi's "Météorographe" encouraged many instrument makers to either construct similar compounded-machines, or to rename their instruments as "meteorographs". G.W. Hough, from the Dudley Observatory in Albany, had presented in 1865 "an automatic registering and printing barometer" (which produced both a "linear diagram or curve of atmospheric pressure" and a printed record "to avoid the tedium and uncertainty of measuring

161. Robert P. Multhauf, *The Introduction of Self-Registering Meteorological Instruments* (Washington: Smithsonian Institution, 1961), 106-107.
162. Charles Wheatstone, "Report on the Electro-Magnetic Meteorological Register," in *Report of the British Association for the Advancement of Science*, vol. 13 (London: John Murray, 1844), xl.
163. *Les mondes* 13 (1867), 537.
164. Paolo Brenni, "Il Meteorografo di Padre Angelo Secchi," *Nuncius* 8 (1993): 224.

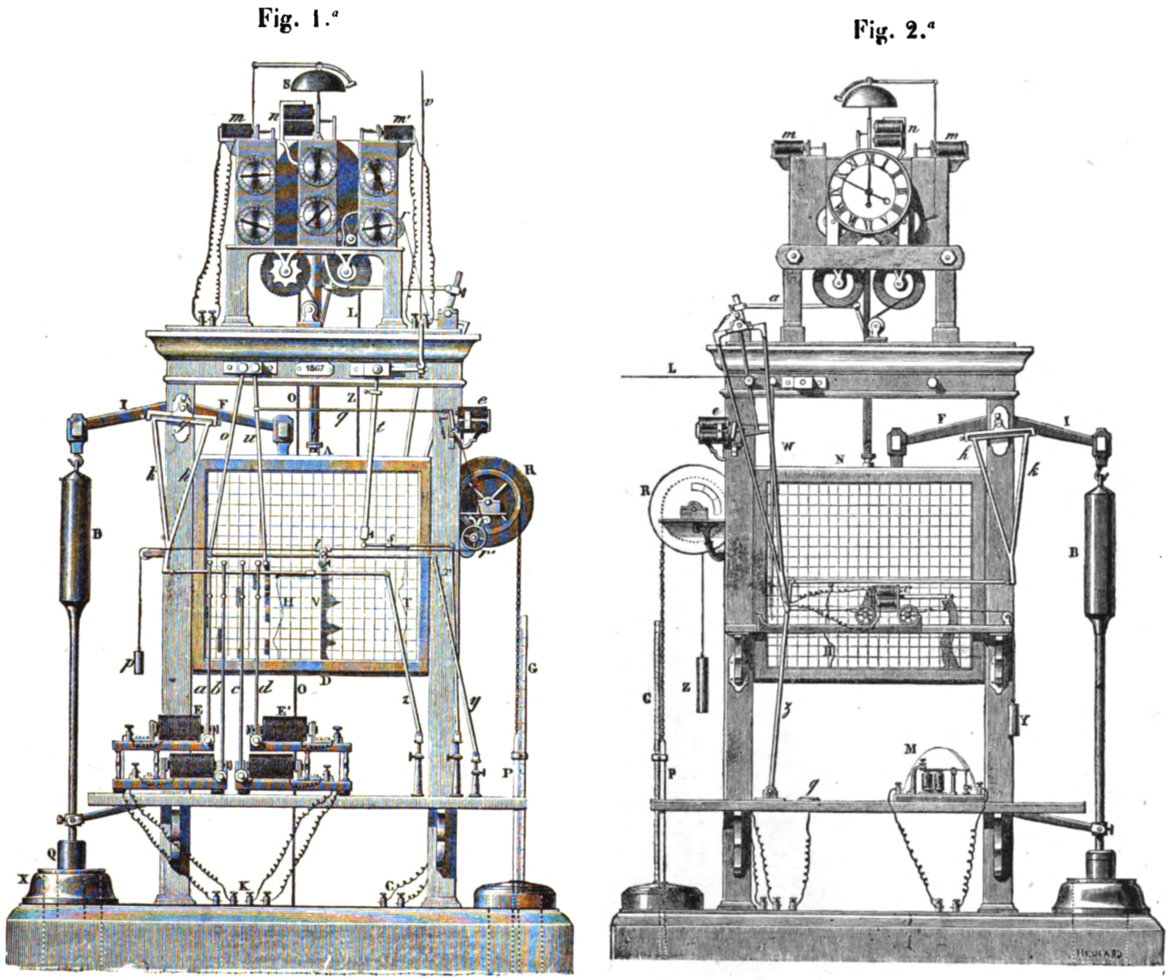


Figure 3.8
Secchi's "Météorographe". Source: A. Secchi, *Descrizione del Meteorografo dell'Osservatorio del Collegio Romano* (Roma: Belle Arti, 1870).

up such results"); in 1871 he advertised a "new meteorograph, for the automatic registration of meteorological phenomena".[165] In Switzerland the director of the Bern observatory Heinrich von Wild presented in 1865 a "Universal-Registrir-Apparat" which registered on the same strip of paper the indications from a "Thermometer", "Barometer", "Windrichtungsmesser", "Windstärkemesser", "Regenmesser"; a few years later the machine constructed in the telegraphic workshop of Hasler & Escher was advertised as a "Universal-Meteorographen", which

165. G. W. Hough, *Description of an Automatic Registering and Printing Barometer* (J. Munsell, 1865), 4; *American Journal of Science* 50 (1870): 287.

now included a "Barograph", "Thermo-Hygrograph", and "Anemo-Ombrograph".[166] In Belgium F. van Rysselberghe built a "meteorograph" (1873) embodying a unified method of registration (or "universal system of meteorography"), while in Sweden A.G. Theorell built both a "Typendruck-meteorograph" which printed its register similarly to Wheatstone's recorder (it was presented at the London Exhibition in 1871) and a "Meteorographe" which supplied its indications as curves.[167]

The emergence of *graph* as a favorite ending for labeling self-registering instruments was not a straightforward practice. Until the 1840s, self-registering instruments were rarely labeled with *graph* in Britain. The name *thermometrograph*, commonly used on the continent to refer to Six's thermometer, was almost never employed in Britain. The adoption of the label occurred not because there was a surge in self-registering instruments, but rather because a new technology was introduced, photography, which produced a "continuous registration" or a "continuous record".[168] It was the association with photography (and photographic records) which made "recording" an alternative to "registering". Though in the 1850s and 1860s the two terms were often interchangeable, in 1868 a Report of the Meteorological Committee of the Royal Society included a special chapter on "self-recording instruments" described as instruments which could "record continuously", while the label "self-registering" was preserved for instruments such as Six's thermometer.[169] If the instruments presented in 1855 by the Kew committee were labeled "self-registering magnetometer" or "self-registering barometer", in the 1872 report to the Royal Society, such instruments were described as "self-recording": "The several self-recording instruments, registering respectively the Pressure, Temperature,

166. H. Wild, "Die selbstregistrirenden meteorologischen Instrumente der Sternwarte in Bern," *Repertorium für physikalische technik für mathematische und astronomische instrumentenkunde* 2 (1867): 161–201; G. Hasler, "Anemo-Ombrograph," *Repertorium für physikalische technik für mathematische und astronomische instrumentenkunde* 11 (1875): 98–101.

167. A.-G. Theorell, "Description d'un Meteorographe Enregistreur," *Nova acta Regiae Societatis Scientiarum Upsaliensis.* 7 (1869). For a brief history of meteorographs see Middleton, *Invention of the Meteorological Instruments*, 245-263.

168. *Report of the Meteorological Committee of the Royal Society, for the Year Ending 31st December 1867* (London: Eyre, 1868), 17, 28.

169. Ibid., 27.

Vapour-tension, Rainfall, and Wind, have been maintained in constant action…"[170]

While the distinction between a self-registering instrument such as Six's minimum and maximum thermometer, and the new self-recording instruments was straightforward, both "record" and "continuous" remained ambiguous. A thermometer that would measure the temperature at a fixed hour could have been advertised as "self-recording" (see Fig. 3.10). Some distinguished between the "absolutely continuous" records (such as the photographic records) and the "intermittent" records produced by most mid-19th century mechanical or electrical self-registering instruments which only recorded the observations at fix intervals of time.[171] The former did that to minimize frictions, while the latter tried to save their battery. These "intermittent" continuous self-recording devices would only gradually be replaced by a new generation of instruments that employed pencils controlled by levers and cylinders with smoke-blacked glazed paper. One of the earliest, popular devices of this type was produced by Louis Breguet.[172] Wheatstone's "meteorological recorder" or A.G. Theorell's "Typendruck-meteorograph" typed the actual measurements. Though such a feature was clearly appreciated by some, it was dismissed by the French physicist Rodolphe Radau because "it only offers isolated readings, expressed in digits; it is certainly preferable to have a continuous trace which talks to the eyes [qui parle aux yeux]."[173]

3.4 *Grams*, or traveling records

Even when the self-recording instruments did produce quasi-continuous traces, the interaction with these traces could have been significantly different. An electrical instrument had the ability of registering multiple quantities on the same piece of paper. In this case, the value

170. "Report of the Kew Committee for the Fifteen Months Ending October 31, 1872," *Proceedings of the Royal Society of London* 21 (1872): 41.
171. *Illustrated and Descriptive Catalogue of Standard, Self-Recording, and Other Meteorological Instruments* (London, 1870), 5.
172. Rodolphe Radau, *Études sur l'Exposition universelle de 1867* (Paris: impr. de Renou et Maulde, 1867), 39.
173. Ibid., 16.

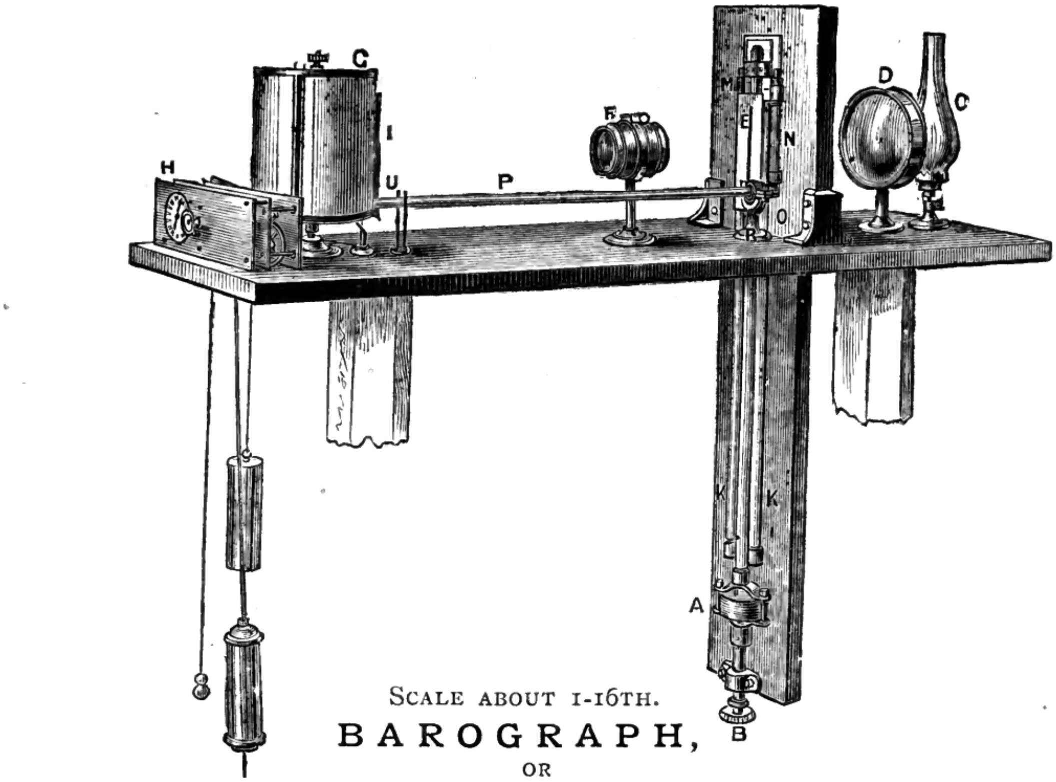
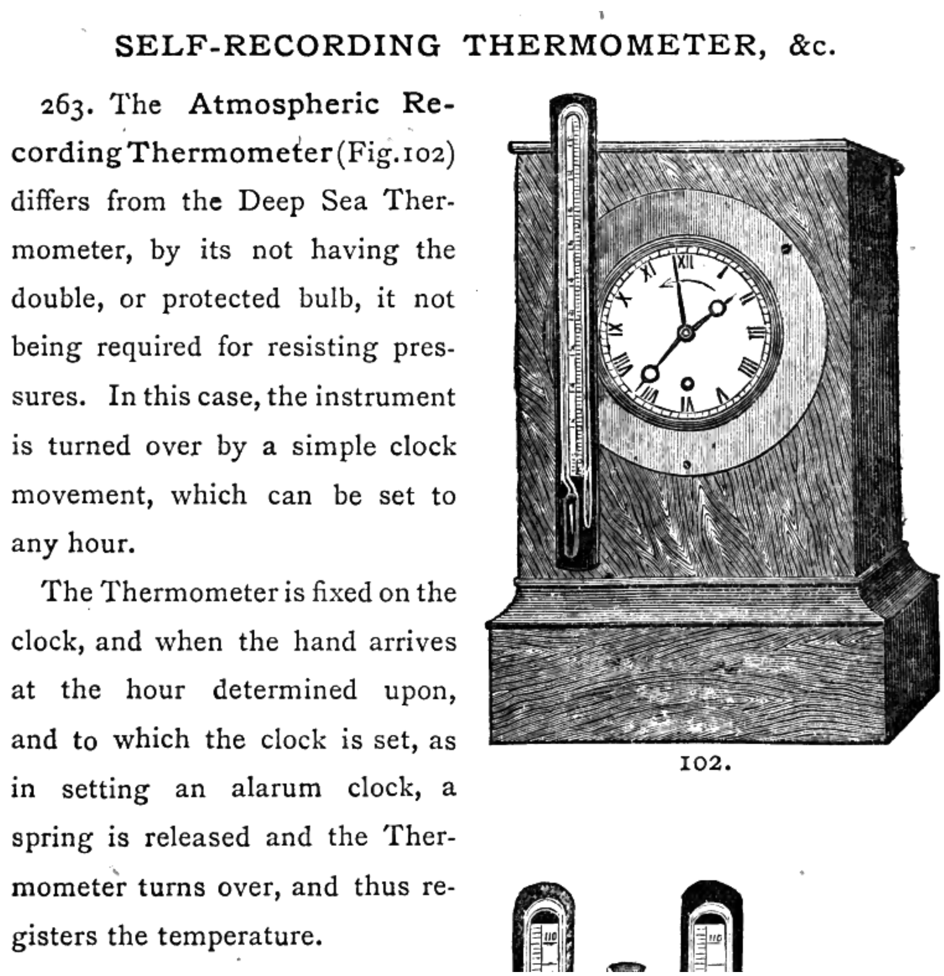


Figure 3.9
Source: *Illustrated and Descriptive Catalogue of Standard, Self-Recording, and Other Meteorological Instruments* (London, 1870), 7.

of the record resided particularly in its immediate comparison of the registered quantities. Furthermore, both mechanical and electrical recorders allowed the direct observation of the recording process. If for a self-registering instrument, like Six's thermometer, the observer would have been most probably absent when the instrument marked its maximum or minimum point, the continuous action of a self-recording instrument presented a spectacle. As in the case of Secchi's meteorograph or Breguet's barograph, many of these instruments were purposefully designed to allow the direct observation of the functioning instrument without any need to interact with it. Breguet's barograph could function undisturbed for a whole week.

The recordings of photographic instruments, however, could not be observed until the sensitive paper was removed from the covering cylinder. Even then, extra steps had to be



SELF-RECORDING THERMOMETER, &c.

263. The **Atmospheric Recording Thermometer** (Fig. 102) differs from the Deep Sea Thermometer, by its not having the double, or protected bulb, it not being required for resisting pressures. In this case, the instrument is turned over by a simple clock movement, which can be set to any hour.

The Thermometer is fixed on the clock, and when the hand arrives at the hour determined upon, and to which the clock is set, as in setting an alarum clock, a spring is released and the Thermometer turns over, and thus registers the temperature.

102.

Figure 3.10
Source: *Illustrated and Descriptive Catalogue of Standard, Self-Recording, and Other Meteorological Instruments* (London, 1870), 72.

taken until the record was readable. While most self-recording instruments with a mechanical mean of registration used ruled paper which allowed the direct reading of measurements, the photographic records had to be manually tabulated using a special instrument, a glass plate engraved with vertical and horizontal lines.[174] In most British observatories the instruments were not left to function unattended. Every two hours, the light was cut from the photographic paper for exactly four minutes. Meanwhile an observer read a standard thermometer and barometer to find "what ought to be the true reading of the curves at the moment when the

174. For a description of the instrument see *Report*, 34. The earliest such instrument seems to have been introduced for tabulating the curves produced by magnetographs – see *The Report of the Kew Committee of the British Association for 1860-1861.*

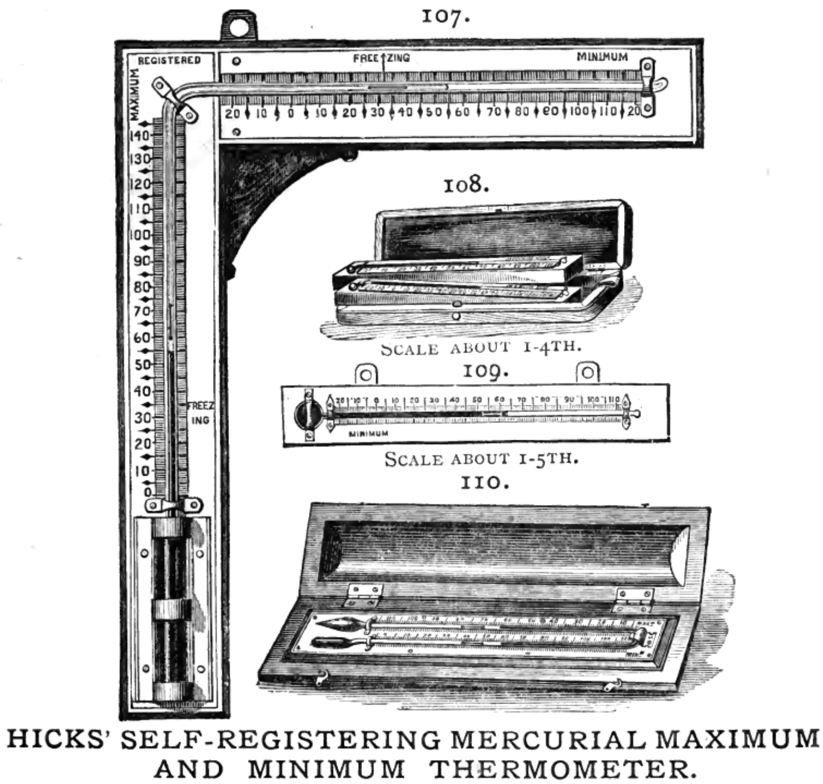


Figure 3.11
Source: *Illustrated and Descriptive Catalogue of Standard, Self-Recording, and Other Meteorological Instruments* (London, 1870), 75.

light is cut off".[175]

At first, the circulation of the photographic records was rather limited. At the Royal Greenwich Observatory, after the barometric and thermometric photographs were reduced into tables, "[n]o further reference is made to the photographic sheets; they are, however, bound in volumes, and are carefully preserved in the Record Room of the Observatory".[176] However, G.B. Airy, the Astronomer Royal of Greenwich Observatory, noted in his report to the board of visitors that "lithographs of Photographic sheets have been prepared, in conformity with the instructions of the Visitors, for insertion in this volume".[177] The Meteorological Committee of the Royal Society also considered publishing their instrumental records, but "the expense

175. *Report*, 34.
176. *Reduction of Twenty Years Photographic Records* (London: Eyre, 1878), 8.
177. George Biddell Airy, "Report of the Astronomer Royal to the Board of Visitors, 1849," *Greenwich Observations in Astronomy, Magnetism and Meteorology made at the Royal Observatory, Series 2* 9 (1849): 14.

of photolithography was found to be serious, so that it was resolved to have tracings of the curves made in the office"; a limited sample of copies was sent to meteorologists to collect opinions if such an endeavor was useful.[178] Starting in 1876, at the request of the *Times* "a copy of the traces of the self-recording instruments on a reduced scale" was sent by the Kew Observatory to the newspaper, the costs being covered by the proprietors.[179]

As the meteorological records started traveling, they were defined using the suffix *gram* in analogy with the pair telegraph-telegram. Edward Sabine studied the lunar-diurnal variation of the magnetic declination based on the "photograms" he obtained from the Kew Observatory. As the term was novel and was not used in other scientific publications, Sabine added a laconic explanation – "The term Photogram is adopted in place of Photograph in conformity to modern usage".[180] The term "magnetogram" entered the conversation after the publication of "Results of a Comparison of certain traces produced simultaneously by the Self-recording Magnetographs at Kew and at Lisbon; especially of those which record the Magnetic Disturbance of July 15, 1863". While "magnetogram" was not used in this report, readers of the report had referred to its curves as "The Kew and Lisbon magnetograms".[181] Soon the labeling pattern generated "barograms", "thermograms", "anemograms", etc.

Because processing the curves required man-power and expertise, the photographic records moved from the observatories where they were produced and tabulated to a central meteorological office:

> The photograms, with tabulations carefully prepared from them, are transmitted monthly by Mr. Stewart, the Superintendent of the Kew Observatory, to Mr. Scott, the Director of the Meteorological Office in London, where the results are computed and embodied in Tables, of the nature of those which are now presented.[182]

178. *Report of the Meteorological Committee of the Royal Society, for the Year Ending 31st December 1868* (London: Eyre, 1869), 21.
179. *Catalogue of the Special Loan Collection of Scientific Apparatus at the South Kensington Museum* (George E. Eyre and William Spottiswoode, 1876), 373.
180. Edward Sabine, "On the Lunar-Diurnal Variation of the Magnetic Declination Obtained from the Kew Photograms in the Years 1858, 1859, and 1860," *Proceedings of the Royal Society of London* 11 (1861): 73.
181. See *The Reader* 4 (20 August, 1864), 236.
182. Edward Sabine, "Results of the First Year's Performance of the Photographically Self-Recording Meteoro-

The photograms from the secondary observatories (Aberdeen, Armagh, Falmouth, Glasgow, Stonyhurst and Valencia) were first sent to Kew where they were centralized, and then sent to the Meteorological Office in London:

> The photograms and the tabulations prepared from them at the several observatories are transmitted monthly to Kew, where they undergo careful examination, and revision if required; and at the expiration of a second month they are sent, with the records prepared at Kew itself, to the Meteorological Office, where, under the direction of Mr. Scott, they are formed into Tables, and used for all meteorological purposes for which they may be available.[183]

A surveillance system was put in place. The observatories were in charge with regulating the self-recording instruments to make sure they all functioned based on the Greenwich mean time; several times a day the standard barometer had to be read as a measure of calibration and control; every change made in the instruments, every peculiarity in the curves had to be inserted in the journal with the exact time specified. The curves, journals and tabulation were sent to the Central Observatory at Kew every Thursday. Here, an assistant was supposed to observe each curve and check for light, finger marks, bad photography or appearance of bagging; he had to check the timing of the curves, and make sure all the dates matched; check the accordance of the barograph and standard readings, the accuracy of subtractions. All curves and tabulations with deficiencies were brought to the Kew director who made the necessary remarks on the curves and tabulations. The director then communicated to the Meteorological Committee all cases of failure, along with forty remeasurements for each month and every observatory (see Figs. 3.12 and 3.13). The barograms, thermograms and anemograms were thus part of a carefully designed system of observation and registration. However, this time it was not nature but the instruments themselves which were under constant observation.

logical Instruments at the Central Observatory of the British System of Meteorological Observations," *Proceedings of the Royal Society of London* 18 (1869): 3.

183. Sabine, "Results of the First Year's Performance of the Photographically Self-Recording Meteorological Instruments at the Central Observatory of the British System of Meteorological Observations," 7.

I.—WEEKLY FORM FOR REGISTERING DEFICIENCIES.

____________________ BAROGRAMS, &c.

(***Received at Kew,*** ____________________.)

Tabulation No. and corresponding Documents.

Points noticed at Kew.	Results and Remarks.
1. Deficiency in number of documents sent -	
2. Errors in numbering and writing upon them -	
(A.) Want of light in curves - - -	
(B.) Bagging in do. - - -	
(C.) Finger marks, &c. in do. - - -	
3. Action of clock - - - -	
4. Regulation of do. - - - -	
(D.) Action of clock-stop - - -	
5. Errors in dating curves - - -	
(E.) Do. in entry or date of entry of journal readings of standard into tabulation sheets - -	
6. Do. in date of entry of tabulated readings into tabulation sheets - - - -	
7. Do. of subtraction in subsidiary tables -	
8. Do. of tabulation discovered by subsidiary tables	
(*c.*) Do. in calculating residual correction - -	
(*d.*) Do. in applying residual correction - -	
9. Ten remeasurements - - - -	
(1.) *Greatest difference* - - -	
(2.) *Mean difference irrespective of sign* -	
(3.) *Residual difference* - - -	

Figure 3.12
Source: *Report of the Meteorological Committee of the Royal Society, for the Year Ending 31st December 1868* (London: Eyre, 1869), 68.

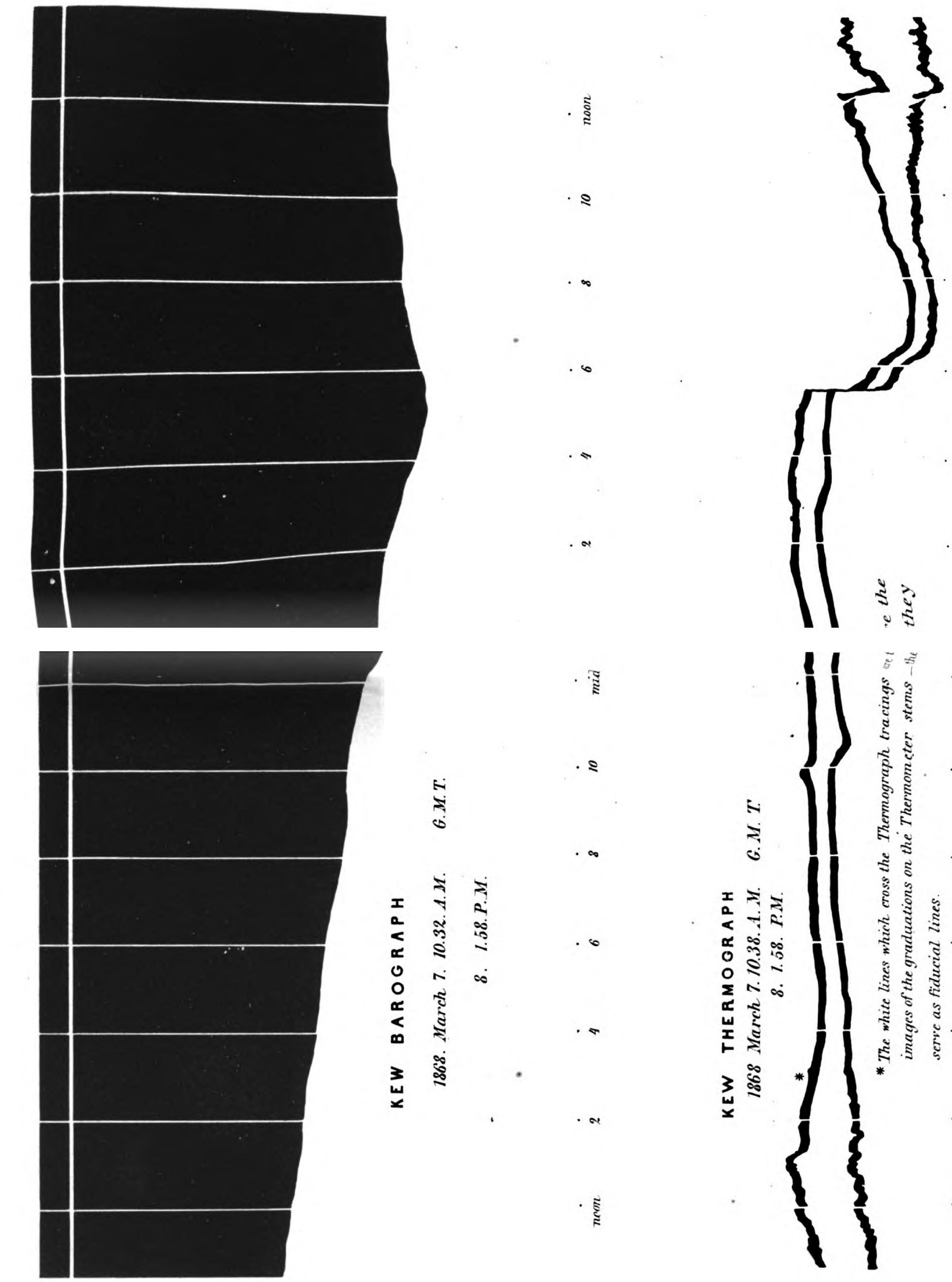


Figure 3.13
Source: *Report of the Meteorological Committee of the Royal Society, for the Year Ending 31st December 1867* (London: Eyre, 1868), pl V.

3.5 From *indicateur* to *enregistreur*

While "register", "registering" or "self-registering" were hugely popular terms in England at the beginning of the 19th century, that was not the case in France where "indicateur" and "indications" dominated the list of names referring to similar instruments or machines. We get a glimpse of these differences in an essay written by l'abbé Gossier in which he resumed some of Charles Babbage's ideas from the recently published *On the Economy of Machinery and Manufactures* (1832). Gossier was confronted with the problem of finding an adequate translation for Babbage's "registering operations" and "machine for registering". The closest French equivalent was "indicateurs", but this did not capture the real essence of these instruments or machines. The essential difference for Gossier was that the *indicator* only displayed an instantenous value corresponding to the very instant at which the instrument was looked upon. A *registering* machine would keep track of a process that carried out over a time interval:[184]

> L'espèce de machines dont nous avons à vous parler comprend les instruments connus en France sous le nom générique d'*Indicateurs*, et que les anglais, avec un peu plus de bonheur dans l'expression, appellent *machines registrantes*, ou *qui tiennent registre*, *machines à registrer*, ou *à enregistrement* (*registering machines*, *machines for registering*). Ces machines, en effet, tiennent registre, car, d'elles-mêmes, sans que le maître soit obligé de s'en occuper ou même d'être présent, elles enregistrent, soit le nombre de certaines opérations multipliées et identiques, et, par conséquent, fatigantes à compter, soit les actes et les omissions de certains agens et employés, sur la fidélité ou l'exactitude desquels nous ne pouvons ou nous ne devons pas entièrement compter. A prendre le nom français *indicateur* dans son acception ordinaire, une montre, une horloge, sont des instruments indicateurs, parce qu'ils nous indiquent l'heure qu'il est au moment où nous les consultons. Cependant, dans cet usage, usage le plus ordinaire d'une montre et d'une horloge, elles ne sont pas, à parler strictement, des machines qui présentent un registre ou un état de ce qui s'est passé ; quoique nous soyons assez portés à croire qu'elles deviennent véritablement instruments à registrer quand nous nous en servons pour connaître combien de secondes, par exemple, combien de minutes ou d'heures se sont écoulées pendant une opération quelconque, ou depuis un instant déterminé. Mais si après une marche à pied ou une course en voiture, une machine me montre combien d'espace j'ai parcouru ; si une autre me dit combien de personnes, dans mon absence, ont passé par une barrière, combien de mesures de liquide ont été retirées d'un vase, combien de pouces cubiques de gaz ont été consommés, combien d'aunes d'étoffe sont passées sous un cylindre, dont l'usage m'est payé à raison de la longueur de la pièce, alors toutes ces machines

184. A "registering machine" could have also been translated or associated with a "compteur".

> entrent dans la classe de machines à registrer, ou machines registrantes.[185]

While there are no other examples that so clearly expressed the problem of naming and translating "indicateur" into "machine à registre", we can still track the shift in the actual practice of naming meteorological instruments. Meteorological instruments that in England were labeled as "self-registering" were described in France by "indication" or "indiquer". For example, Gay-Lussac presented his minimum and maximum thermometer as "un thermomètre propre à indiquer des maxima ou des minima de température".[186]

The shift from "indicateur" to "enregistreur" began in the 1850s, triggered by the novel photographic and telegraphic methods of registration developed in Britain. For example, French journals referred to Ronalds' method "sur l'enregistrement photographique de l'électromètre, du baromètre, du thermomètre et du magnétomètre de déclinaison".[187] If in 1851 Théodose Du Moncel had submitted to the French Academy a "Description de deux anémomètres à indications continues", in 1853 he published an article "Sur les enregistreurs électriques" that opened with a discussion of "enregistreurs météorologiques".[188] Moncel further pushed the terminology in his famous and popular treatise *Exposé des applications de l'électricité* (1856) where he dedicated two special sections to the "enregistreurs et appareils météorologiques" and "enregistreurs et appareils astronomiques". For a while the new terminology was applied to the name of the instruments (or their class) rather than describing their operation. For example, Antoine and Edmond Becquerel's *Traité d'électricité et de magnétisme* (1856) defined the instruments as:

> ***Appareils enregistreurs***. Dans les observatoires où l'on cherche à avoir des **indications** non interrompues touchant les variations du magnétisme du globe, on a disposé des appareils capables

185. J. F. Gossier, "Essai sur les indicateurs, ou instruments à registre," *Précis analytique des travaux de l'Académie des sciences, belles-lettres et arts de Rouen*, 1833, 75-76.
186. Joseph Louis Gay-Lussac, "Description d'un thermomètre propre à indiquer des maxima ou des minima de température," *Annales de chimie et de physique* 3 (1816): 90–91.
187. *L'institut* 15 (1847): 78.
188. Théodose Du Moncel, "Sur les enregistreurs électriques," *Mémoires de la Société des sciences naturelles de Cherbourg* 1 (1852): 193–221.

> de tracer eux-mêmes leurs **indications**. Nous citerons les appareils construits par M. Broocke, et qui ne sont autres que les trois magnétomètres de M. Gauss, mais disposés de façon à ce que leur position donne lieu à des **indications** tracées par la photographie et capables **d'indiquer** quelles ont été à chaque instant les positions des barreaux aimantés, et par conséquent les variations de l'intensité magnétique.[189]

By the time of the 1867 Paris exposition, the label was well rooted and the meteorographs and self-recording instruments were described as "appareils météorographiques", or more broadly as "appareils enregistreurs".[190]

4 THE REDEFINITION OF *GRAPH*

The paradigmatic domain that has come to be associated with *graph* instruments is physiology. In 1847, the German physiologist Karl Ludwig described one of the first self-registering instruments applied to physiology, however, without naming it. It was Alfred Volkmann who first called Ludwig's instrument the "Kymographion" in 1849-1850.[191] Ludwig's "Kymographion" inspired a series of new instruments which borrowed not only the design but also the name of the instrument: in the early 1850s Hermann von Helmholtz developed an instrument which he first called "Froschzeichenmaschine [Frog-drawing-machine]" but later renamed "Myographion", while in 1855 Karl Vierordt described his "Sphygmographen".[192]

The instruments developed by the three German physiologists were a source of inspiration for Étienne-Jules Marey who in his doctoral thesis, *Recherches sur la circulation du sang à l'état physiologiques et dans les maladies* (1859), provided a detailed discussion of Vierordt's

189. Antoine César Becquerel and Edmond Becquerel, *Traité d'électricité et de magnétisme*, vol. 3 (Paris: Didot, 1856), 106, my underline.
190. Radau, *Études sur l'Exposition universelle de 1867*.
191. "Ich werde dieses ingeniöse Instrument (welches der Kürze wegen Kymographion heissen mag)…", in Alfred Wilhelm Volkmann, *Die Hämodynamik* (Leipzig: Breitkopf und Härtel, 1850), 120.
192. In a letter to du Bois-Reymond from June 13, 1854 Helmholtz wrote: "The physiologist institute in Gießen has constructed their own frog drawing apparatus or 'myographion', as I would like to pompously call it from now on" (Helmholtz quoted in Henning Schmidgen, *The Helmholtz Curves: Tracing Lost Time* (New York: Fordham University Press, 2014), 192-193 n28). For Vierordt see Karl Vierordt, *Die Lehre vom Arterienpuls* (Braunschweig: Vieweg, 1855), 21.

sphygmograph; the name he used to refer to this class of instruments was "appareils à indications continues". The key word around which the variations of meaning were constructed was "indication", such as: "indications graphiques du mouvement" or "appareils à indication graphique".[193] In 1859 Marey designed his own sphygmograph, which won him an honorable mention in an experimental physiology competition, the Prix Montyon, and an invitation to demonstrate the instrument at the court of Napoleon III.[194] The following year Marey continued advertising his modified "sphygmographe" in a pamphlet and in a report to the Académie des Sciences. In the title of both of these texts the sphygmograph was presented as "un nouvel appareil enregistreur". However, in the text Marey only referred to "instruments à indications continues" without any mention to "enregistreur(s)".[195] The patent for the instrument submitted by Marey on December 31, 1860 was titled: "Appareil dit: Sphygmographe, propre à observer et inscrire les pulsations du cœur".[196] Most probably Marey came to value the new name shortly before the publication of these texts and did not bother to make further corrections. However, this was not a fluke. In his future publications Marey would carefully and consistently replace "instruments à indications continues" by "appareils enregistreurs". What caused the sudden shift? Though Marey designed the sphygmograph, the instrument was actually manufactured by Louis Breguet (or Bréguet) one of the leading French instrument makers famous especially for his telegraphic and meteorological instruments.[197] Breguet was particularly famous for his clockwork mechanisms which he used in manufacturing self-recording

193. Étienne-Jules Marey, "Recherches sur la circulation du sang à l'état physiologiques et dans les maladies" (PhD diss., 1859), 29-30, 43, 114.

194. Marta Braun, *Picturing Time: The Work of Etienne-Jules Marey (1830-1904)* (Chicago: University of Chicago Press, 1992), 17.

195. Étienne-Jules Marey, *Recherches sur le pouls au moyen d'un nouvel appareil enregistreur, le sphygmographe* (Paris: E. Thunot et cie, 1860), 5; Étienne-Jules Marey, "Recherches sur la forme et la fréquence du pouls au moyen d'un nouveau sphygmographe, ou appareil enregistreur des pulsations," *Comptes rendus hebdomadaires des séances de l'Académie des sciences*, 1860, 634.

196. See Laurent Mannoni, "Le sphygmographe, une invention en trois étapes," in *Sur les pas de Marey: Sciences et cinéma*, ed. Thierry Lefebvre, Jacques Malthête, and Laurent Mannoni (Paris: L'Harmattan/SEMIA, 2004), 74n73.

197. Mannoni has suggested that Breguet was not the first manufacturer of Marey's sphygmograph, but he has not been clear when the collaboration of the two actually started. The fact that the image of Marey's sphygmograph from 1860 already carried Breguet's brand (see Fig. 3.14) suggests that the collaboration was already started in 1860. Cf. ibid.

instruments such as barometrographs and thermometrographs. And of course, such instruments were labeled as "appareils enregistreurs".[198] The improvements brought by Breguet to the sphygmographe allowed him to obtain a patent in his own name in 1869.[199] Until Marey actually started manufacturing his instrument (and interacting with instrument makers) he only used the language of "indications" which was specific to physics textbooks.

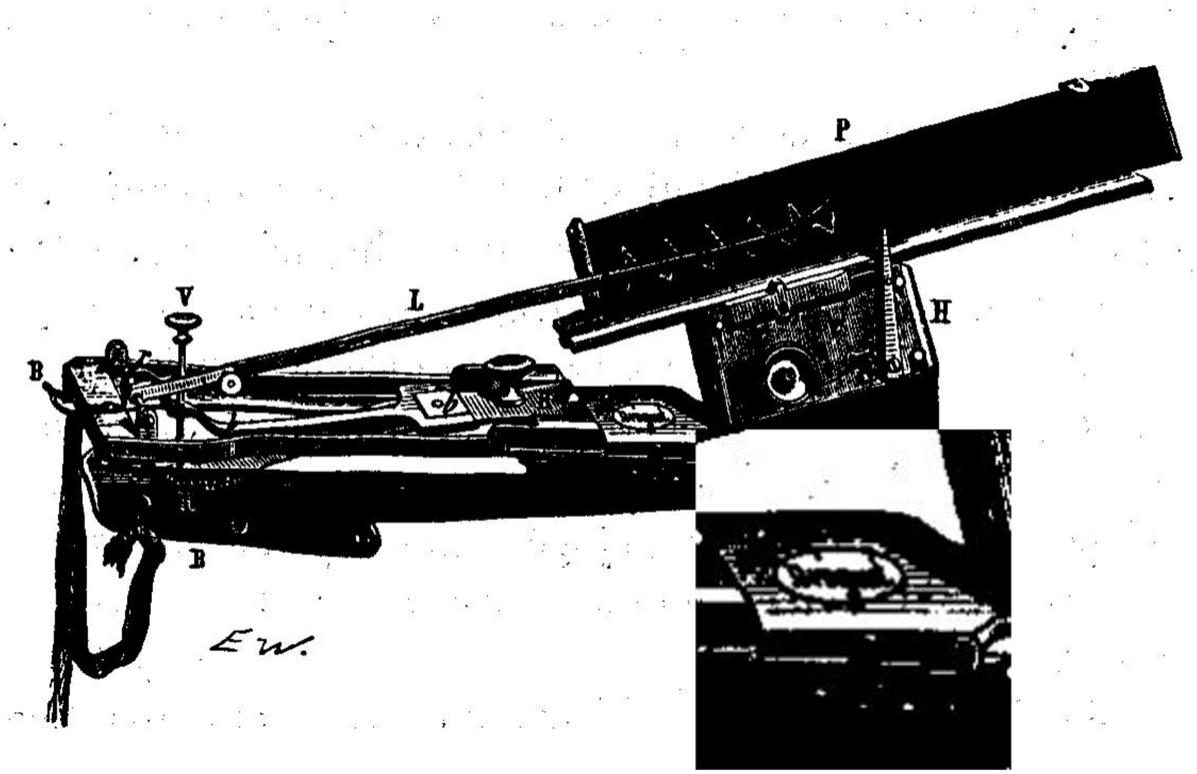


Figure 3.14
The sphygmograph designed by Marey and built by Louis Breguet. Source: Étienne-Jules Marey, *Recherches sur le pouls au moyen d'un nouvel appareil enregistreur, le sphygmographe* (Paris: E. Thunot et cie, 1860), 7.

In 1860 Marey described Poncelet and Morin's instrument for illustrating the law of falling bodies as:

> **Les instruments à indications continues** ont fourni le moyen de réaliser ces espérances. Tout le monde connaît la machine de Morin imaginée pour démontrer les lois du mouvement dans les corps qui tombent; c'est le type le plus simple de ce genre d'appareils qui ont introduit une

198. See Louis Bréguet, *Catalogue illustré: appareils et matériaux pour la télégraphie électrique, instruments divers, électricité, physique, mécanique, météorologie, physiologie* (Paris: impr. de S. Raçon, 1873).
199. See Mannoni, "Le sphygmographe, une invention en trois étapes," 45.

> véritable révolution dans l'étude des mouvements variés.[200]

In 1863, the label for this instrument was changed to:

> Nous rappellerons en quelques mots **l'appareil enregistreur** que MM. Poncelet et Morin ont construit pour déterminer le mouvement d'un corps qui tombe dans l'espace; la description de cette machine si simple fera comprendre aisément le mécanisme de tous **les instruments enregistreurs** employés par les physiologistes.[201]

The chapter on "Des appareils enregistreurs" from *Du mouvement dans les fonctions de la vie* (1868) opened with a detailed description of Morin and Poncelet's machine which was built to "enregistrer les lois de la chute des corps" and seemed to be "le premier type d'un enregistreur parfait".[202]

French textbooks described Poncelet and Morin's instrument as "un appareil, à l'aide duquel on peut observer directement le mouvement de chute libre des corps" or "appareil à indications continues [...] qui permet de vérifier directement les lois de la chute des corps".[203] Nowhere in these textbooks were the terms "enregistreur(s)" or "enregistrer" ever used. This choice closely followed the original descriptions of Morin and Poncelet who only talked about "indications". Morin referred to this machine as an "appareil chronométrique à cylindre et à style pour observer les lois du mouvement".[204] Otherwise, the expression most favoured by Morin to refer to this type of objects was "à style": "indicateur à style", "dynamomètre à style", "appareils chronométriques à style", etc.[205] The action of such instruments was not to register (Morin hardly ever used "enregistre(r)") nor to inscribe, but "trace".[206] Marey borrowed

200. Marey, *Recherches sur le pouls au moyen d'un nouvel appareil enregistreur, le sphygmographe*, 5-6, my underline.
201. Étienne-Jules Marey, *Physiologie médicale de la circulation du sang* (A. Delahaye, 1863), 49, my underline.
202. Étienne-Jules Marey, *Du mouvement dans les fonctions de la vie* (Paris: Germer Baillière, 1868), 108, 112.
203. Paul Quentin Desains, *Leçons de physique*, vol. 1 (Paris: Dezobry, E. Magdeleine et cie, 1857), 24; Nicolas Deguin, *Cours Élémentaire de Physique*, vol. 1 (Eugene Belin: Paris, 1854), 56.
204. Arthur Morin, *Notions Fondamentales de Mécanique* (Paris: Hachette, 1855), 255.
205. Morin, *Notions Fondamentales de Mécanique*, 54, 255; Arthur Morin, *Description des appareils chronométriques à style, propos à la représentation graphique et à la determination des lois du mouvement, et des appareils dynamométriques* (Metz: S. Lamort, 1838).
206. In practice Morin's "trace" acted very much as a "register". For example: "Les indications des flexions du

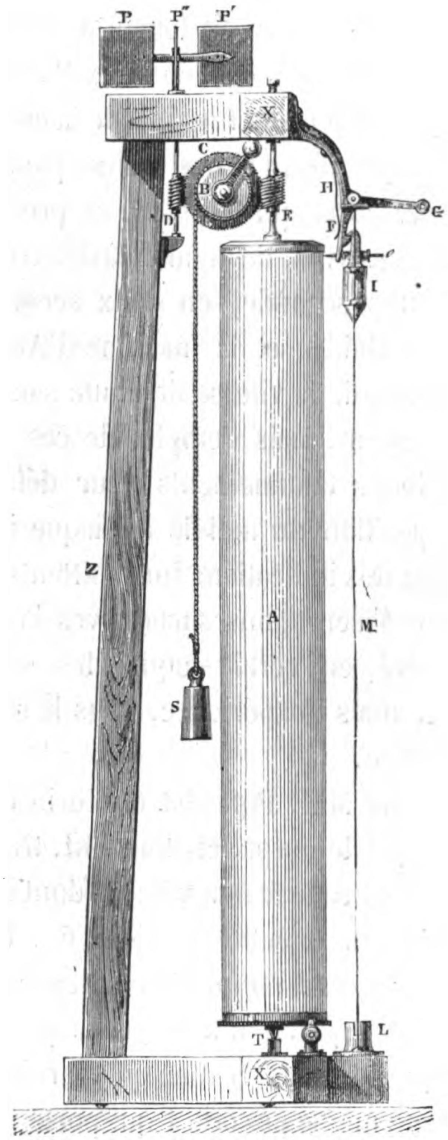


FIG. 12. — Machine de MM. Poncelet et Morin pour enregistrer les lois de la chute des corps.

Figure 3.15
While Marey copied the image of Morin's instrument from Desains's *Leçons de physique* (1857), he redescribed the purpose of the instrument from "observer directement le mouvement" to "enregistrer les lois". See also Table 3.6. Source: Étienne-Jules Marey, *Du mouvement dans les fonctions de la vie* (Paris: Germer Baillière, 1868), 108.

the image of Morin's instrument from Desains's *Leçons de physique* (1857) but redescribed the purpose of the instrument from "observer directement le mouvement" to "enregistrer les lois" (see Fig. 3.15).

After 1874 Marey shifted once again his terminology and replaced "enregistreur(s)" with "inscripteur(s)". In *La Méthode graphique* (1878) Morin's instrument was redescribed as:

> La physique et la mécanique ont réalisé de grands progrès par l'emploi des **appareils inscripteurs**. C'est à Poncelet qu'on doit l'invention de plusieurs de ces instruments que le général Morin a réalisés et dont il a tiré de remarquables résultats. Le plus connu de ces instruments est celui qui sert à déterminer les lois de la chute des corps.
> ...la machine de Poncelet et Morin semble être le premier type d'**appareil inscripteur** parfait, il n'en est pas moins vrai que, dès le commencement du dernier siècle, on essaya d'**écrire automatiquement certains phénomènes**.[207]

There are several ways through which one can observe the radical and consistent difference in the language used by Marey. Table 3.5 contrasts Marey's description of two thermographs to show how both the category and the individual pieces were redefined in terms of the new language. Table 3.6 displays an instance of Marey's triple re-description of Morin's machine from "Appareils à indications continues", to "Appareils Enregistreurs", and "Appareils Inscripteurs". Fig. 3.16 presents a word-count distribution to contrast the use of "enregistre(r)" and "entregistreur(s)" compared to "inscrit+inscrire", "inscripteur(s)", and "inscriptions" in Marey's major books. This makes clearly visible the sudden and consistent shift which has occurred in the mid-1870s.

ressort doivent être obtenues d'une manière indépendante de l'attention, de la volonté ou des préventions de l'observateur, et par conséquent fournies par l'instrument lui-même au moyen de traces ou de résultats matériels qui subsistent après l'expérience", Morin, *Notions Fondamentales de Mécanique*, 35.

207. Étienne-Jules Marey, *La Méthode graphique dans les sciences expérimentales et particulièrement en physiologie et en médecine* (Paris: G. Masson, 1878), 111-113.

Table 3.5
Marey's description of two thermographs, one centered on "enregistrer" while the other on "inscrire".

"Du thermographe, appareil enregistreur des températures" (1864)	"Sur un nouveau thermographe" (1881)
L'emploi des **appareils enregistreurs** dans les expériences physiologiques m'a déjà permis d'apprécier avec une grande exactitude les phénomènes qui se traduisent par un mouvement, quelque faible et quelque instantané qu'il soit. …Enfin, les contractions des différents muscles peuvent être **enregistrées** avec tous leurs caractères d'intensité, de forme et de durée.	Il y a une quinzaine d'années que je recherche un instrument capable d'**inscrire** les variations de la température animale en deux points du corps, afin d'observer les changements que des influences de différents ordres exercent sur la répartition de la température. …Des tubes flexibles en cuivre rouge réunissent les boules thermométriques aux **appareils inscripteurs**.
1° Cet appareil devait **enregistrer** l'intensité et la durée de tous les changements de température d'un point quelconque. 2° Il fallait pouvoir réunir deux ou plusieurs de ces instruments pour obtenir une **indication** simultanée des changements survenus dans la température de plusieurs points.	L'**inscription** simultanée des températures superficielle et profonde montre que, dans les troubles vasomoteurs, la température animale éprouve des variations de sens inverses dans les régions centrales et périphériques du corps.
Au moment où cette soupape est ouverte, on peut amener le **levier enregistreur** à zéro…	…les changements de courbure de ce dernier actionnent un **levier inscripteur**.

4.1 The language of *inscription*

The evidence amassed so far proves that at some point in the mid-1870s Marey carefully and consistently redescribed the action and purpose of his instruments. What was there to be gained from all this trouble? The distinctions between "enregistreur" and "inscripteur", as revealed by the language use, is subtle but revealing. In 1868 Marey described the "enregistreurs" as:

> …appareils qui fonctionnent d'**eux-mêmes**, et livrent à l'expérimentateur un graphique formé d'une ligne continue sur laquelle on peut lire et analyser à son aise toutes les phases du **phénomène enregistré**.[208]

In 1878 the emphasis was shifted from the instrument that keeps its *own* register or which registers a phenomenon, to a *self*-inscribing phenomenon: "Quand on se sert d'appareils inscripteurs, on obtient sans aucune peine **les courbes que trace lui-même le phénomène qui**

208. Marey, *Du mouvement dans les fonctions de la vie*, 106-7, my underline.

Table 3.6
Marey's re-descriptions of Morin's machine.

Appareils à indications continues	Appareils Enregistreurs	Appareils Inscripteurs
Les instruments à indications continues ont fourni le moyen de réaliser ces espérances. Tout le monde connaît la machine de Morin imaginée pour démontrer les lois du mouvement dans les corps qui tombent ; c'est le type le plus simple de ce genre d'appareils qui ont introduit une véritable révolution dans l'étude des mouvements variés. Étienne-Jules Marey, *Recherches sur le pouls au moyen d'un nouvel appareil enregistreur, le sphygmographe* (Paris: E. Thunot et cie, 1860), 5-6.	La physique a réalisé un immense progrès dont les sciences naturelles peuvent profiter aujourd'hui. L'invention des **appareils enregistreurs** me semble destinée à renouveler la face de la biologie… Le premier appareil qui ait fourni de bons graphiques en ce genre est celui que les généraux Poncelet et Morin ont imaginé pour déterminer les lois de la chute des corps. …la machine de MM. Poncelet et Morin parait être le premier type d'un **enregistreur** parfait… Étienne-Jules Marey, *Du mouvement dans les fonctions de la vie* (Paris: Germer Baillière, 1868), 106-112.	La physique et la mécanique ont réalisé de grands progrès par l'emploi des **appareils inscripteurs**. C'est à Poncelet qu'on doit l'invention de plusieurs de ces instruments que le général Morin a réalisés et dont il a tiré de remarquables résultats. Le plus connu de ces instruments est celui qui sert à déterminer les lois de la chute des corps. … la machine de Poncelet et Morin semble être le premier type d'**appareil inscripteur** parfait… Étienne-Jules Marey, *La Méthode graphique dans les sciences expérimentales et particulièrement en physiologie et en médecine* (Paris: G. Masson, 1878), 111-113.

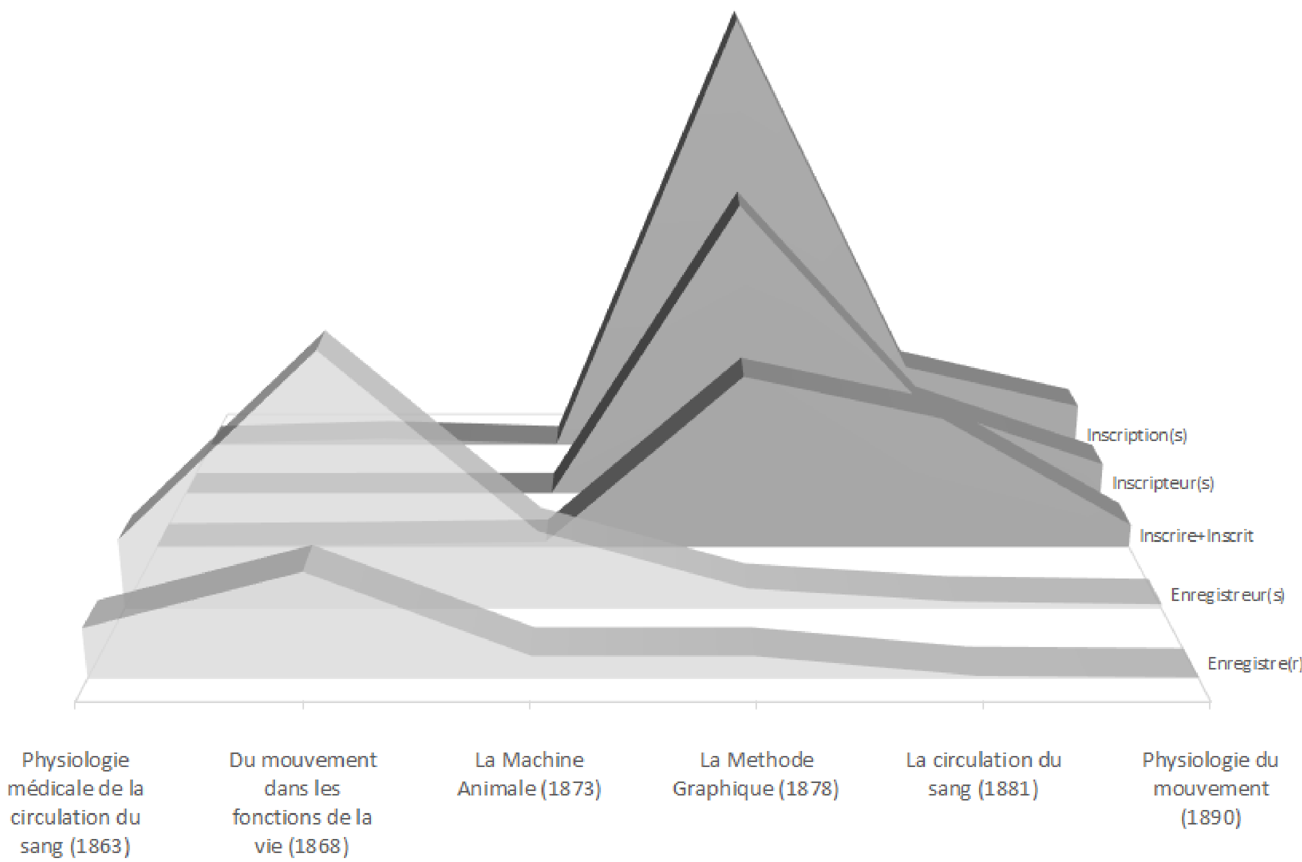


Figure 3.16
A word count distribution of "enregistre(r)" and "enregistreur(s)" compared to "inscrit+inscrire", "inscripteur(s)", and "inscriptions" in Marey's major books.

s'inscrit."[209] This was not an isolated instance, but rather a new pattern. Marey referred to "le tracé que le Poisson inscrit lui-même" and called the self-inscription of the phenomena "le langage des phénomènes eux-mêmes".[210] The role of the instruments was to "forcer le phénomène lui-même à inscrire mécaniquement son début et sa fin".[211] The pulse, motions, or variations would all "inscribe themselves", as reflected in the reflexive pronoun: "s'inscrit", "s'inscrire", "s'inscrivent", etc. (see Table 3.7).

209. Marey, *La Méthode graphique dans les sciences expérimentales et particulièrement en physiologie et en médecine*, 107, my underline.
210. Ibid., 543, iii.
211. Ibid., 139.

Table 3.7
Self-inscribing phenomena as described by Marey.

les phénomènes électriques devront s'inscrire
des actes qui doivent s'inscrire
le phénomène qui s'inscrit
un mouvement vibratoire qui s'inscrira
les appuis du pied droit s'inscrivent
l'action musculaire s'inscrit
le mouvement s'inscrit
s'inscrivaient les secousses musculaires
les changements de volume du cœur s'inscrivaient
la courbe des espaces parcourus s'inscrit
l'évaporation s'inscrirait
les différents degrés de dilatation s'inscrivent
les secousses s'inscrivent
les deux tracés s'inscrivent

The metaphor employed by Marey was not new. In 1825 the Weber brothers designed a device which would allow the waves to "depict themselves [sich selbst abbilden]".[212] When photography was introduced for solar observations, astronomers considered that the sun "imprime lui-même son passage".[213] However, by trying to trace the language of inscriptions we can find a more direct connection. Until Marey embraced "appareils inscripteurs", the expression was hardly ever used.[214] However, talk about "inscription" did occur in one field – that of acoustics.

In the late 1850s Édouard-Léon Scott de Martinville, a typographer and stenographer, presented to various academic societies "l'invention de la graphie du son et de sa fixation", which he named "phonoautographie".[215] This invention was described in terms of two defin-

212. Ernst Heinrich Weber and Wilhelm Weber, *Wellenlehre auf Experimente gegründet* (Leipzig: bei Gerhard Fleischer, 1825), 109; See Christa Jungnickel and Russell McCormmach, *Intellectual Mastery of Nature* (Chicago: University of Chicago Press, 1990), v. 1 46-49.
213. Hervé Faye, "Sur Les Photographies de l'éclipse Du 15 Mars," *Comptes rendus hebdomadaires des séances de l'Académie des sciences* 46 (1858): 708; See Jimena Canales, *A Tenth of a Second: A History* (Chicago: University of Chicago Press, 2009), 106-110.
214. The *Annales* of the Paris Observatory referred to a self-recording magnetometer or "l'appareil inscripteur de M. Brooke"; see *Annales de l'Observatoire de Paris. Mémoires* 7 (1863), 242; *Annales de l'Observatoire de Paris. Observations* 15 (1861), 51.
215. Édouard-Léon Scott de Martinville, *Le Problème de la parole s'écrivant elle-même* (Paris: l'auteur, 1878), 43, 29; see Jonathan Sterne, *The Audible Past: Cultural Origins of Sound Reproduction* (Duke University Press, 2003).

ing metaphors: writing and photography. On the one hand, "phonoautographie" was to sound what photography was for light. It was a "fixation graphique de la voix" which could produce "une impression naturelle des phénomènes sonores" or "un diagramme naturel".[216] On the other hand, it was "une écriture phonoautographique" achieved by "fixer une plume à ce fluide fugitif, impalpable, invisible" and "forcer la nature à constituer elle-même une langue générale écrite de tous les sons".[217] Through Scott's phonoautograph "la voix humaine s'écrit elle-même".[218] The double metaphor of the photographic *fixation*" and the *writing* of sound were merged into the expression "inscription automatique des sons de l'air", part of the title of the *mémoire* he submitted to the French Academy in 1861.[219]

Shortly after Edison introduced his phonograph, Scott attacked the American inventor and his invention in a self-published book, *Le Problème de la parole s'écrivant elle-même* (1878), which also collected his previous writings on his "phonoautographe". Besides questioning the originality of Edison's phonograph, Scott also condemned it as a misnomer:

> la tres-grande majorité du public *lisant* ne sait pais qu'il existe un appareil français, non pour *répéter*, mais pour *écrire* la parole, selon la signification du mot *phonographe*. […]
> Lisons vite l'écriture du phonographe, car c'est une écriture, le mot phonographe (ou mieux phonégraphe) voulant dire 'le son qui ecrit'. Le problème que s'est posé M Edison, comme le nom de l'appareil l'indique, est bien d'écrire la parole. L'écrit-il, en effet? Cette apparence de ponctuation que l'on voit sur la feuille d'étain après l'opération offre-t-elle les caractères d'une écriture, ou si l'on veut, d'une *graphie*? [220]

The American phonograph could only repeat sounds, but not write them; accordingly, it would have been more properly called a "phoneglyph", a pun on the hill-and-dale grooves on the cylinder which Scott associated with hieroglyphs. Definitely, not the natural language of sound. It was only his "phonoautographe" that could still hold the merit of making "la pa-

216. Scott de Martinville, *Le Problème de la parole s'écrivant elle-même*, 62, 54.
217. Ibid., 40, 30.
218. Document from 1857 reproduced in ibid., 39.
219. Édouard-Léon Scott de Martinville, "Inscription automatique des sons de l'air au moyen d'une oreille artificielle," *Comptes rendus hebdomadaires des séances de l'Académie des sciences* 53 (1861): 108–111.
220. Scott de Martinville, *Le Problème de la parole s'écrivant elle-même*, 3-4, 10.

role s'écrivant elle-même".

Despite Scott's idiosyncrasies and his unflinching goal of forcing sound to write itself down, his treatment of sound shared many characteristics with mid-19th century French physicists. Pierre Adolphe Daguin's *Traité élémentaire de physique* (1st ed. 1855) established a pattern in how Duhamel's vibrating diapason was to be discussed in French textbooks for the next half a century. The chapter on acoustics in Daguin's *Traité* included a special section titled "Méthode Graphique" which opened with: "Dans cette méthode, la plus exacte de toutes, le corps vibrant trace lui-même ses vibrations...". Duhamel's vibroscope exemplified the application of the method.[221] Other textbooks copied the exact division and phrasing: Jules Jamin's *Cours de physique de l'École polytechnique* (1859) – "MÉTHODE GRAPHIQUE. — On doit enfin à M. Duhamel une méthode générale beaucoup plus simple qui consiste à faire tracer par le corps sonore lui-même les vibrations qu'il exécute"; A. Ganot's *Traité élémentaire de physique expérimentale et appliquée* (11th ed. 1864) – "Dans la méthode graphique due à M. Duhamel (211), c'est le corps sonore qui trace lui-même ses vibrations".[222] In the 1860s, the textbooks started using "inscrire" to describe the action of Scott's "phonoautographe" (this term was quasi absent until then). The French translation of Wilhelm Wundt's *Traité élémentaire de physique médicale* added a new section to the original: "Méthode graphique pour mesurer le nombre des vibrations sonores. Rhonautographe" which talked about "le corps sonore inscrit lui-même ses vibrations sous forme d'une courbe ondulée".[223] Meanwhile Desains' *Leçons de physique* (1860) integrated more of Scott's terms and described how "on peut forcer un corps qui résonne à inscrire lui-même ses vibrations sur un tableau quelconque, et l'on conçoit immédiatement l'usage de ce tracé graphique".[224]

221. P. A. Daguin, *Traité élémentaire de physique théorique et expérimentale* (Paris: Édouard Privat, 1855), v. 1, 466.

222. Jules Jamin, *Cours de physique de l'École polytechnique* (Paris: Mallet-Bachelier, 1859), v. 2 449; A. Ganot, *Traité élémentaire de physique expérimentale et appliquée*, 11th ed. (Paris: Chez l'Auteur, 1864), 224.

223. Wilhelm Wundt, *Traité élémentaire de physique médicale*, trans. Ferdinand Monoyer (Paris: Baillière, 1871), 216.

224. Paul Quentin Desains, *Leçons de physique* (Paris: Dezobry, E. Magdeleine et cie, 1860), v. 2, 9. Desains' *Leçons de physique* is probably the place from where Marey took the image of Morin's machine – see Fig. 3.15

Marey had long compared the graphical method to a universal language of science which was comprehensible across national barriers because it employed “des expressions tellement naturelles”.[225] Marey was also fond of a second linguistic metaphor:

> j’aimerais mieux comparer l’étude des sciences naturelles au travail des archéologues qui déchiffrent des inscriptions écrites dans une langue inconnue; qui essayent tour à tour plusieurs sens à chaque signe, s’aidant à la fois des conditions dans lesquelles chaque inscription a été trouvée, et de l’analogie qu’elle présente avec des inscriptions déjà connues, et n’arrivent enfin qu’en dernier lieu à la connaissance des principes à l’aide desquels ils enseigneront à d’autres à déchiffrer cette langue.[226]

While these two metaphors were already in place in the 1860s, they were only fused in 1875 when Marey’s laboratory started collaborating with the French Société de linguistique to develop means of registering (or inscribing) vocal sounds.[227] Their efforts were presented in an article by Charles Rosapelly on “Inscription des mouvements phonétiques” (1876).[228] Now, the two metaphors, of the graphical method as a universal language and of science as an archeology of inscriptions written in an unknown language, were fused and materialized in a concrete image (see Fig. 3.17). Marey’s *mémoires* from this period had already switched to the new terminology.

4.2 Description / Inscription

While the meaning of *graph* has always remained closely associated with the metaphor of writing, its interpretation has changed from *description* to *inscription.* While both these are connected to writing, they underline different aspects of what is written: its content or its form.

and Desains, *Leçons de physique*, v. 1 24.

225. Marey, *Du mouvement dans les fonctions de la vie*, 82.

226. ibid., 24.

227. For the connection of Marey to phonology and acoustics see Brain, “The Graphic Method,” 231-308; Giusy Pisano, “L’acoustique de la parole par la méthode expérimentale de l’abbé Rousselot,” in *Sur les pas de Marey: Sciences et cinéma*, ed. Thierry Lefebvre, Jacques Malthête, and Laurent Mannoni (Paris: L’Harmattan/SEMIA, 2004), 219–241; Dagognet, *Etienne-Jules Marey*, 33-36.

228. Étienne-Jules Marey, *Physiologie expérimentale: Travaux du laboratoire de M. Marey* (Paris: Masson, 1876), v. 2 109-131.

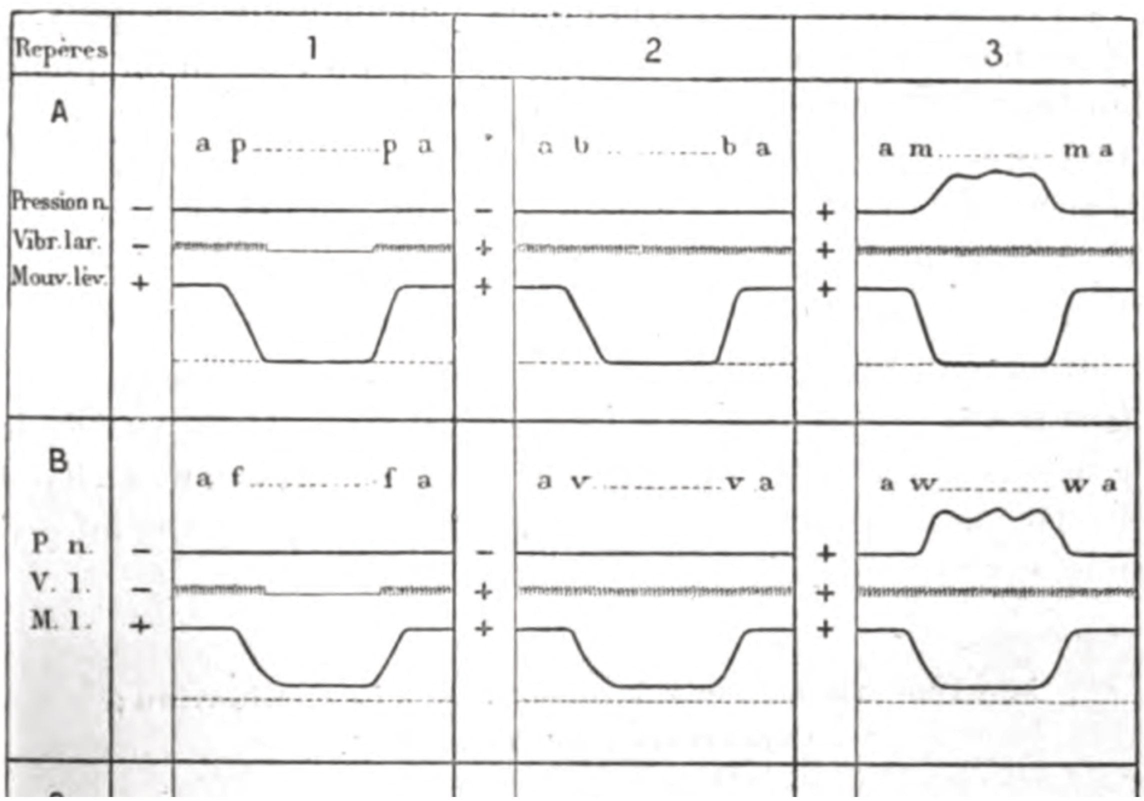


Figure 3.17
Phonemes "complètement caractérisés par la méthode graphique". Source:Étienne-Jules Marey, *Physiologie expérimentale: Travaux du laboratoire de M. Marey* (Paris: Masson, 1876), v. 2, 125.

Example: the king's description of the land was inscribed in stone. Each of these terms has its own set of central metaphors. If initially inscriptions were engravings in stone or bronze capable of enduring over long spans of time, modern forms of engraving such as lithographs and photography have added a new metaphor to this term, that of a faithful copy made directly after the original, if not by the original. As we saw above, Marey employed both of these metaphors – the inscription as the archaeological artifact that had to be deciphered, and as the faithful copy of the phenomenon. Should all scientific forms of writing be collapsed under the category of *inscriptions*? Such an approach might mean not to see the forest for the trees; or even worse, to be tempted by the central metaphors implied by *inscription* such as materiality, immutability, faithfulness, etc. There are good reasons why a geometrician *describes* a circle and does not *inscribe* it;[229] why one referred to *graphical* or *descriptive* geometry and not *inscriptive* geometry; or why a map is a description of a place and not an inscription of it.[230] Opposed to modern academic writings which have often been seduced to see an inscription in almost everything, for Marey there was a clear distinction to be made between inscriptions and descriptions: "Comment attendrait-on sans impatience le jour où de longues et obscures descriptions feront place à de saisissantes images!"[231]

5 CONCLUSION

The Handbook to the *Special Loan Collection of Scientific Apparatus* (1876) identified three classes of instruments according to the method of reading them:

> In the self-recording class the observer leaves the instrument to itself, and examines the record at his own convenience.
> In those which depend on eye observations alone, the observer must be there to look at the indicator of the instrument, but he does not touch it.
> In the third class, which depend on eye and hand, the observer, before taking the reading, must make some adjustment of the instrument.[232]

This division, full of meaning for a mid-19th century person versed in the world of meteorological instruments, is generally ignored by the contemporary eye. Self-recording instruments, understood broadly and ahistorically as inscription-producing devices, could equally well span all three categories. Through a period eye, Six's thermometer would be included in the first group, while Watt's *indicator* in the second. For a contemporary eye, Six's thermometer would not come to mind when thinking about self-registering instruments, while Watt's indicator would be seen simply as a self-recording instrument. The crucial distinction between

229. The "inscription" of a geometrical figure employs a different meaning of "inscribe" from the one discussed here.
230. Cf. Bruno Latour and Steve Woolgar, *Laboratory Life: The Construction of Scientific Facts* (Princeton, N.J.: Princeton University Press, 1986).
231. Étienne-Jules Marey, "De la méthode graphique dans les sciences expérimentales et de ses applications particulières à la médecine," in *Congrès périodique International des Sciences Medicales, Bruxelles, 1875* (Bruxelles: Manceaux, 1876), lxviii.
232. *Handbook to the Special Loan Collection of Scientific Apparatus 1876* (London: Chapman & Hall, 1876), 14.

the contemporary and period eye is that the former perceives the instruments and their action in isolation, while the latter defines such instruments in relation to an observer. That is, the products of the instrument are secondary to the spatial and temporal relations which define human interactions.

The first category of self-registering instruments is defined by instruments which can function in the absence of the observer, i.e. *at a distance*, *for long periods of time*. The observer is not replaced in the name of some form of mechanical objectivity, but rather he is displaced. It is still the human observer which needs to observe and control the self-registering instruments. The record of the instrument is valuable only so far as it can match, satisfactorily, the record of the human observer. As revealed through the use of *graph*, man and instrument stand in a relation of equality. In the second category, the observer is *fixed in space* in front of the instrument, but does not need to act instantaneously. He can simply observe the indications unfolding. This was the case with Watt's indicator or Morin's machine, which though they could have been made self-registering, that would have defeated their purpose in the first place – which was that of seeing the action of the engine or the fall of the body unfold in front of the observer. In the third category, the observer has to act in the right place, at the right time, that is in a *fixed space* and at a *fixed time*.[233] Because these three categories are defined in terms of the human interaction with the instrument, they each enforce their own epistemic virtues.[234]

Marey's *inscripteurs* completely reshuffled the three categories. As seen above, Marey was schooled in the tradition of Poncelet's and Morin's indicators through which a phenomenon could be observed unfolding.[235] At the same time, Marey drew from the technical expertise of

233. For the temporal localization of the observer for such observations see Simon Schaffer, "Astronomers Mark Time: Discipline and the Personal Equation," *Science in Context* 2, no. 1 (1988): 115–145; Canales, *A Tenth of a Second*.

234. See Daston and Galison, *Objectivity*, 39-42. The relation between observer and instrument parallels that between scientific self and scientific representation.

235. For the early association between indicators and Helmholtz's myograph see Wise and Brain, "Muscles and Engines: Indicator Diagrams and Helmholtz's Graphical Methods."

instrument makers specialized on self-registering instruments, such as Louis Breguet. However, Marey moved away from the language that defined the instrument in relation to the observer, to define the phenomena in relation to its inscription. In both equations the third term was suppressed. If a self-registering instrument only registered its variations under the constant action of a phenomenon, so a phenomenon could inscribe itself only through an instrument. The action of Marey's self-recording instruments was equally diffused. Marey's *inscripteurs* claimed to be self-recording, though they required the hand and eye of the observer to be adjusted, and operated not in the absence but "sous les yeux de l'observateur".[236]

236. Marey, *La Méthode graphique dans les sciences expérimentales et particulièrement en physiologie et en médecine*, 210.

· 4 ·

Curves and Traces

The dissemination of graphical representations (plots or graphs which are "unambiguously recognizable" by a contemporary eye) has long puzzled historians.[1] Such modern looking objects can be spotted in the most disconnected places such as the pages of a medieval manuscript of scholastic philosophy or in the description of a 17th century weather clock.[2] Even more shocking is the vivid contrast between the graphical prolixity of a couple of individuals such as Johann Heinrich Lambert and William Playfair compared to their arid, ungraphical contemporaries. Historians have tended to agree that even though graphs began to appear around 1770 (due to Lambert), they "only became common around 1820".[3] As I will show in this chapter, this view is founded on a faulty methodological approach.

1 WEATHER CHARTS

Historians of graphical representations have so far undervalued or ignored a well established graphical tradition which starts in the mid-17th century and extends throughout the 18th century: the charts of weather. In a pioneering and comprehensive study on the graphical representations of statistical data, H. Gray Funkhouser pointed out, in connection with a remark made by William Playfair, that

> The occurrence of meteorological graphs before Playfair's time is probable, yet a search in the places most likely to yield an answer to the question, the *Philosophical Transactions of the Royal Society of London*, the *Comptes rendus* of the French Academy of Sciences and lesser journals, reveals no graphs of that nature. The *Philosophical Transactions* carried meteorological data but

1. See Hankins and Silverman, *Instruments and the Imagination*, 52.
2. See above Chapter 2 or Matthias Schemmel, "Medieval Representations of Change and Their Early Modern Application," *Foundations of Science* 19, no. 1 (2014): 11–34.
3. Hankins, "Blood, Dirt, and Nomograms," 52; Tilling, "Early Experimental Graphs," 196; Hankins and Silverman, *Instruments and the Imagination*, 9.

always in tabular form.[4]

While Laura Tilling in her seminal paper on "Early Experimental Graphs" (1975) acknowledged the existence of a single "graphical presentation" in the *Philosophical Transactions* from 1724, she reached a similar overall conclusion to that of Funkhouser:

> No great interest seems to have been aroused by these early attempts at automatic recording, and certainly nobody was inspired to present data produced by *non*-automatic methods in graphical form. Whilst tables of meteorological data make frequent appearances in the *Philosophical transactions* throughout the century, only once do we find a graphical presentation, that of Nicolaus Cruquius's barometric observations in 1724, and he made no attempt to analyse the data in any way. No similar graphs appear again until the 1820s. This absence is typical of the majority of eighteenth-century scientific journals. Even where exceptions occur, there is no attempt to analyse the results given; that is, to take advantage of the graphical mode of presentation.[5]

The same conclusion has been reached more recently by Howard Wainer, who like Tilling acknowledged the existence of a few outlying examples but pointed out the otherwise remarkable absence of graphical representation with the single exception of Lambert:

> Before Playfair, the use of data graphics although not completely unheard of, was rare. During the entire eighteenth century no graphs were to be found in any volume of the following journals:
>
> *Acta Eruditorum*; *Annals of Philosophy*; *Edinburgh Journal of Science*; *Mémoires de l'Académie des sciences* (Paris); *Mémoires présentés par divers savans à l'Académie royale des sciences* (Paris); *Journal of Natural Philosophy*; *Novi Commentarii* [sic]; *Academiae Scientiarvm Imperialis Petropolitanae*; *Observations sur la Physique*; *Philosophical Transactions of the Royal Society of Edinburgh*
>
> The only European journal containing graphs during this entire century was the *Mémoires de l'Académie Royale des Sciences et Belle-Lettres* (Berlin), in papers by Lambert in the mid- to late eighteenth century (about thirty line graphs showing a variety of physical phenomena like evaporation rates) and one paper by Benjamin Thompson in an article on ballistics.[6]

Though historians have acknowledged a few single individual cases of weather charts, they have failed to delineate their *historical coherence* – i.e. their *historical continuity* and *identity*. In what follows I will show that by the end of the 18th century, weather or barometric charts

4. Funkhouser, "Historical Development of the Graphical Representation of Statistical Data," 289.
5. Tilling, "Early Experimental Graphs," 196.
6. Wainer, *Graphic Discovery*, 47-48.

formed a recognizable and well-individualized historical object that pertained to a specific scientific community and form of practice. Furthermore, the remarkable graphical activity of individuals like Johann Heinrich Lambert or William Playfair should be understood in relation to this graphical tradition and not as its origin.

The scientific study of the weather required the daily measurement of an ever expanding series of quantities (temperature, air pressure, humidity, amount of rain, wind direction or velocity, etc.) at uncomfortable hours and in exposed conditions. Then, measurements and observations had to be written down in an orderly fashion. These operations were carried out every day (if not several times a day) for a long and uninterrupted period of time of months or years. Besides the effort of upkeeping the instruments, a weather diary was an extremely tedious and painful affair. As such, any mean of facilitating this task was more than welcomed. As described in the previous chapter, several attempts were made in the 17th and 18th century to design and build weather instruments that would keep a register of the weather at every instant. Most commonly, the variations indicated by an instrument were to be recorded by a trace on a moving cylinder or disk. While for the contemporary eye the salient feature of such instruments is the graphical representations that it produced, the early modern eye admired their ability to make unnecessary the presence of an observer. The curves produced were only registered or recorded indications with little value as graphical traces. Because of this, and because such self-registering instruments were not perfected until the 19th century, these traces played little role in the development of graphical representations before the 19th century.[7]

It was a different type of graphical representation that was popularized through the scientific study of the weather. In 1683 Martin Lister, a prolific member of the Royal Society, presented at Oxford "his way of keeping the Account of the Barometer which is the most

7. Cf. Hankins and Silverman, *Instruments and the Imagination*, 128, where in the case of Watt's indicator "a recording instrument required the transition from tables to graphs".

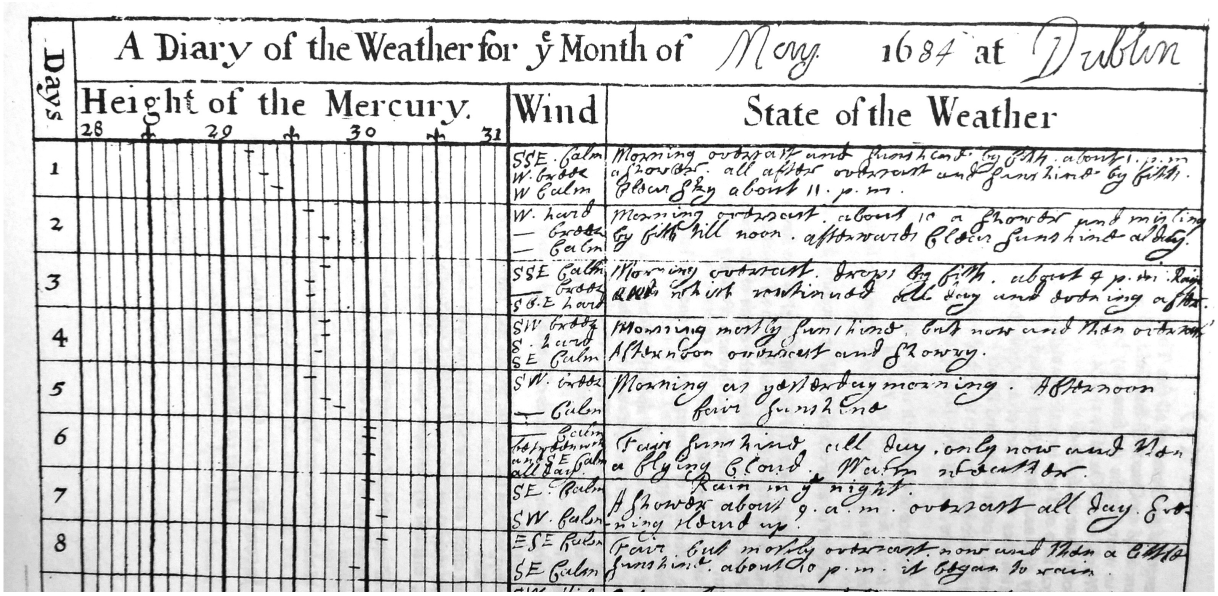


Figure 4.1
Molyneux's monthly weather diary kept after Lister's method. Source: R. T. Gunther, *Early Science in Oxford* (Oxford, 1920), v. 12, 208.

easy and exact that wee ever saw", remarked a member of the audience.[8] Instead of writing down the numbers for the height of mercury one only cut across the line corresponding to the value indicated by the barometer (see Fig. 4.1). While such a method did produce a graphical representation of sorts, it was construed only as a "compendious" or "commodious" way for "Registering the Baroscope's motions" or keeping a "Diary of the Weather".[9] The method was embraced with some enthusiasm by William Molyneux and Robert Plot, who each had a plate engraved after Lister's method in the hope that others will also start keeping their own weather diaries.[10] Plot – one of the founders of the Philosophical Society at Oxford, keeper of the Ashmolean Museum and professor of chemistry at Oxford – further popularized Lister's method in the pages of the *Philosophical Transactions* where he described it as a "new and easy invention of observing the rise and fall of the Mercury in the Barometer (by parallel

8. Aston quoted in Gunther, *Early Science in Oxford*, v. 12, 41; Birch, *The History of the Royal Society of London for Improving of Natural Knowledge, from Its First Rise*, v. 4, 212-213.
9. Molyneux quoted in Gunther, *Early Science in Oxford*, v. 12, 136-139.
10. For the engravings see ibid., v. 12, 208, 304. Plot appealed to John Warner, a mathematical instrument maker who advertised his instruments in a leaflet called *Aeroscopium*, ibid., v. 12, 302-305.

lines drawn from every decimal part of each month of its whole extent)" (see Fig. 4.2).[11] The same year Molyneux presented an account in which he compared the history of the weather at Oxford and Dublin, most probably using his own barometric charts along with those kept by Plot.[12]

Plot's and Molyneux's diaries or plates failed to generate any further enthusiasm in Lister's method.[13] Only in the 1720s the *Philosophical Transactions* published two other graphical tables submitted by the Dutch polymaths Nicolaas Cruquius and Pieter van Musschenbroek (see Figs. 4.3 and 4.5). Though these tables resembled the British charts of 1684, they served a different purpose and emerged out of a different culture. Nicolaas Cruquius was a surveyor turned cartographer who kept a daily account of the weather (almost uninterruptedly between 1706 and 1734) along with astronomical observations.[14] Cruquius believed that changes in weather could be correlated with the motion of heavenly bodies. Cruquius also published astronomical charts of the latitude of various planets for a whole year (see Fig. 4.3). Both his cartographic background and interest in astronomical observations were reflected into the weather charts which introduced a new symbolic language to represent the variation in barometric pressure along with the constitution of the sky, or the strength and direction of the wind. In the charts he submitted to the Royal Society both barometric pressure and temperature were represented by curves, but the latter were suppressed in the published plate, most probably for clarity.[15] A few years later Musschenbroek employed a very similar notation and layout with the excep-

11. Robert Plot, "Observations of the Wind, Weather, and Height of the Mercury in the Barometer, through out the Year 1684; Taken in the Musaeum Ashmoleanum at Oxford...," *Philosophical Transactions* 15 (1685): 930. To simply interpret Plot's plate as a graphical representation in the modern sense would mean to completely neglect its purpose and use. Cf. Wainer, *Graphic Discovery*, 13-15.

12. Gunther, *Early Science in Oxford*, v. 12, 192.

13. For the great variety in tabulation during the late 17th century see Lorraine Daston, "Super-Vision: Weather Watching and Table Reading in the Early Modern Royal Society and Académie Royale Des Sciences," *Huntington Library Quarterly* 78, no. 2 (2015): 187–215.

14. A.F.V. van Engelen and H.A.M Geurts, *Nicolaus Cruquius (1678-1754) and His Meteorological Observations* (De Bilt: KNMI, 1985), 29, 44-45.

15. In most 18th century weather charts only the atmospheric pressure was represented by a curve because this was the quantity whose variation had to be predicted and understood. One of the few exceptions, Plot's plate engraved after Lister's method treated both pressure and temperature symmetrically, see Gunther, *Early Science in Oxford*, v. 12, 304.

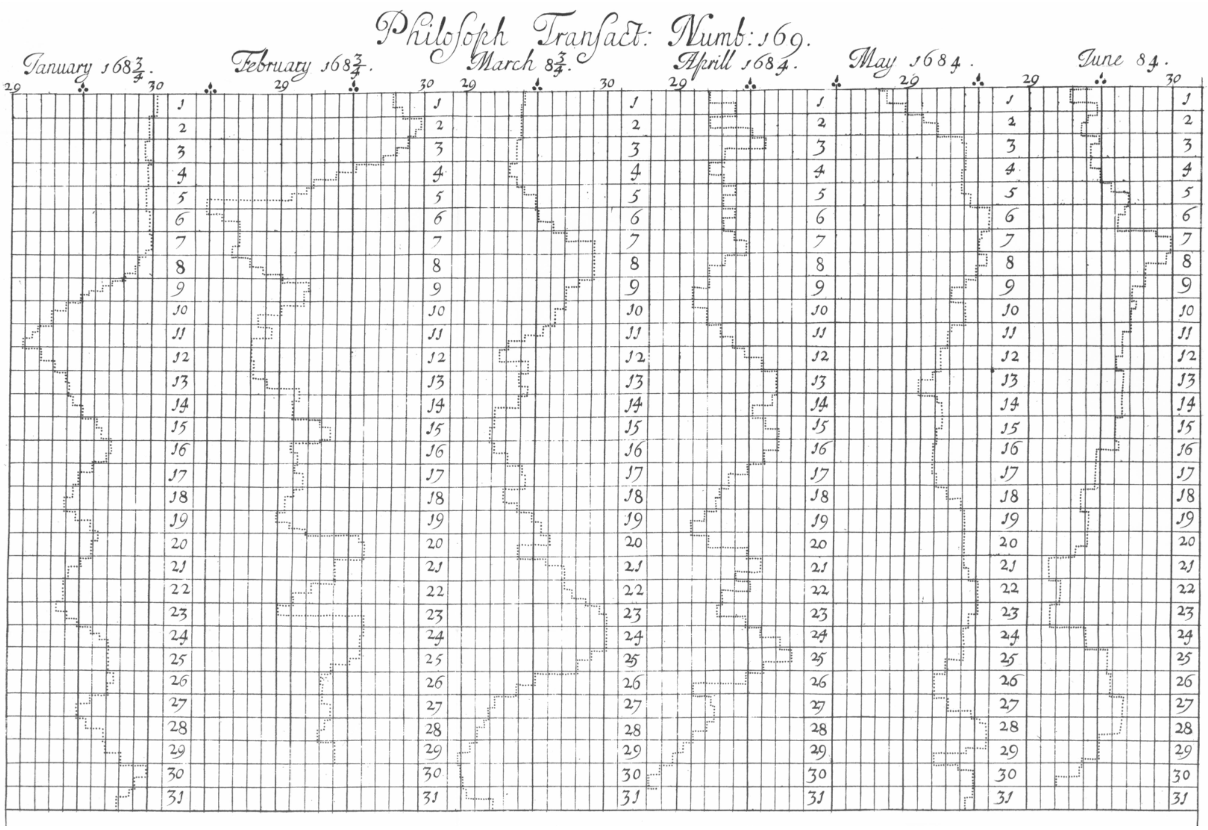


Figure 4.2
Plot's weather diary popularizing Lister's method for registering the variations of the barometer. Source: Robert Plot, "Observations of the Wind, Weather, and Height of the Mercury in the Barometer, through out the Year 1684; Taken in the Musaeum Ashmoleanum at Oxford...," *Philosophical Transactions* 15 (1685): 930–943.

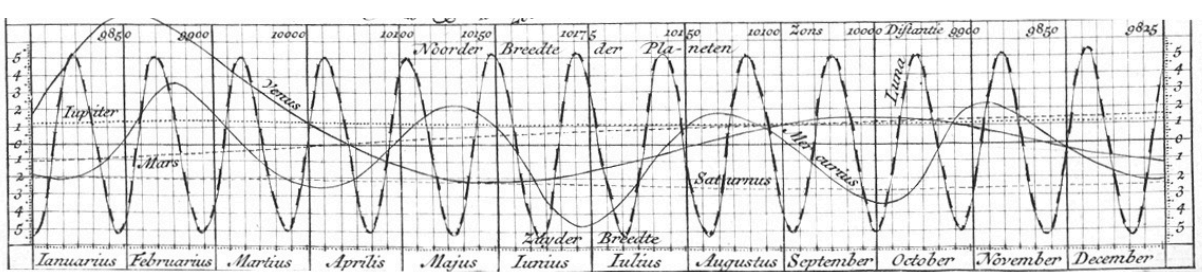


Figure 4.3
A fragment of Cruquius's astronomical chart which shows the latitude of various planets throughout the year. Source: *Loop, plaats, en distantie der planeten, uyt den aardbol, voor 't jaar 1731*, in collab. with Nicolaas Cruquius, Leiden, 1731.

tion of some minor changes (he employed numerical values for both the intensity of the wind and temperature).

Why were these weather charts constructed? In a letter to the Royal Society Cruquius alluded to the work and innovation he put into his table:

> in order that all things may be presented to the mind for simultaneous consideration, I spent a great deal of time exploring many different avenues of approach. At length I came up with the following carefully arranged Table, which I present as a Contribution on my part: i.e. I dedicate it to You.[16]

While tables were often praised because they could be parsed "at a glance", Cruquius' and Musschenbroek's charts had the important advantage of also displaying "at a glance" the diligence and virtuosity of their authors.[17] Both Musschenbroek and Cruquius carried out at least three daily observations (of pressure, temperature, atmospheric conditions, wind direction and intensity) for a whole year, an unprecedented feat for early 18th century weather measurements.[18] To these Musschenbroek would also add magnetic declination and inclination, and quantity of evaporated water (see Fig. 4.4).[19] On their charts each day was divided into three parts to account for these measurements (in Musschenbroek's chart the dots probably corresponded to individual measurements). While a conventional table would have required more than a thousand rows, their graphical charts could display all this unprecedented effort and attention on a single large sheet of paper.

While Lister's method was meant to be used as a "compendious" mean of *registering* measurements (and especially barometric pressure), Cruquius' and Musschenbroek's charts were

16. Cruquius quoted in Engelen and Geurts, *Nicolaus Cruquius (1678-1754) and His Meteorological Observations*, 115.

17. For the reading of tables and the unfounded hope that tables could allow one to decipher weather patterns see Daston, "Super-Vision."

18. For the spotted record of other observers see ibid., 198, 213. One should also consider that Cruquius' and Musschenbroek's tables were only a summary of their actual measurements. Cruquius also developed an original "table of winds", described by Musschenbroek in his *Essai de physique* (Leiden, 1739): 897-898.

19. Magnetic declension was not specified in Musschenbroek's chart from *Physicae experimentales* (1729) but was later added in his reports to the Royal Society or the French Academy, see Fig. 4.4 and Pieter van Musschenbroek, "Ephemerides Meteorologicae, Barometricae, Thermometricae, Epidemicae, Magneticae, Ultrajectinae," *Philosophical Transactions* 37 (1731): 357–384; Daston, "Super-Vision," 194, 201.

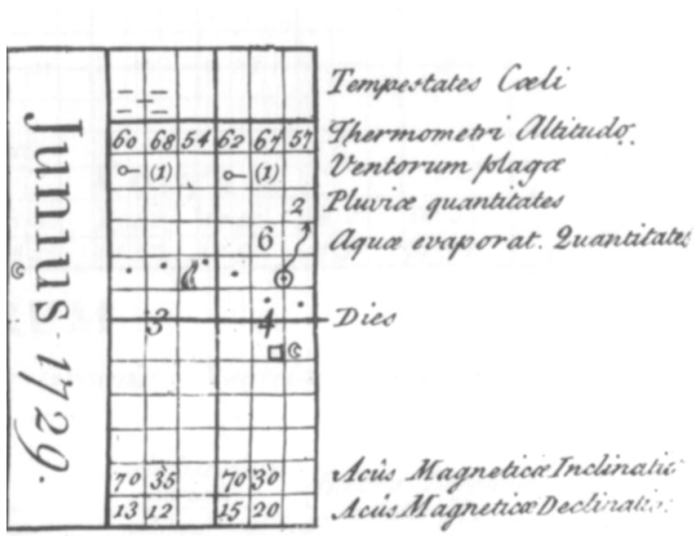
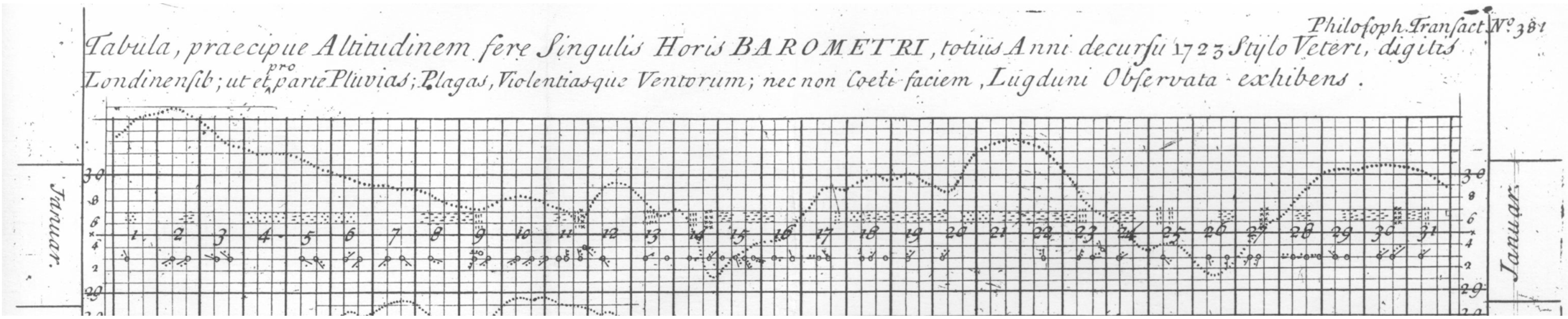
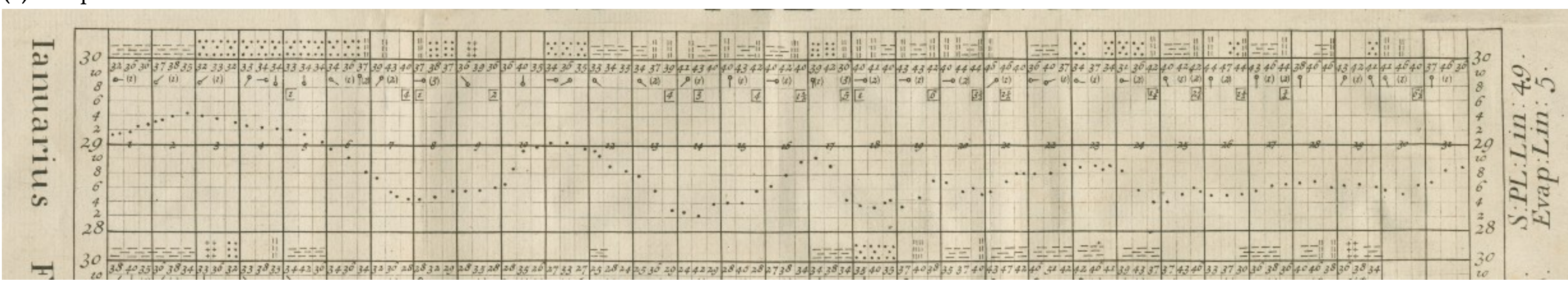


Figure 4.4
Compared to Muschenbroek's previous chart (see Fig. 4.5), this table also included magnetic inclination and declination, and quantity of water evaporated. The legend in the margin of the table was added by the editors of the *Philosophical Transactions.* Source: Pieter van Musschenbroek, "Ephemerides Meteorologicae, Barometricae, Thermometricae, Epidemicae, Magneticae, Ultrajectinae," *Philosophical Transactions* 37 (1731): 357–384.



(a) Cruquius 1724



(b) Muschenbroek 1729

Figure 4.5

Dutch Weather Charts. Each day was divided in three parts corresponding to a different measurement. The dotted curve represented the variation of atmospheric pressure, the dashes and dots within a square represented the atmospheric conditions (rain, snow, fog, etc.), while the line with a circle represented the direction of the wind (the intensity of the wind was indicated by dots in Cruquius charts, while Musschenbroek used numbers). In Cruquius original chart temperature was also represented by a curve, while Musschenbroek used numerical values. Source: (a): Nicolaus Cruquius, "Observationes Accuratae Captae, Anno Xvij = Xxiij Lugduni Batavorum, Delphis Bataviae, & in Pago Rhenoburgo," *Philosophical Transactions* 33 (1724): 4–7; (b): Pieter van Musschenbroek, *Physicae experimentales et geometricae de magnete, tuborum capillarium vitreorumque speculorum attractione, magnitudine terrae, cohaerentia corporum firmorum dissertationes: ut et ephemerides meteorologicae ultrajectinae* (Lugduni Batavorum: Luchtmans, 1729).

an exquisite mean of *presenting* an ever growing number of observations. Cruquius started keeping a weather diary in 1706, but his earliest graphs (still "elementary and clearly experimental examples") date from 1721.[20] The graphical charts which summarized a whole year (compiled for 1723, 1724 and 1725) were expressly made for the Royal Society.[21] Similarly, though Musschenbroek kept a "meteorological and magnetical diary" from 1728 until 1758, he only constructed seven large tables for the years 1728-1734. Though Cruquius' and Musschenbroek's contributions were highly valued, their tables were of limited use in print. The Royal Society only published Cruquius' first chart for 1723 and only a selection of Musschenbroek's chart for 1729 (only the full month of January and a few days from five other months were published).[22]

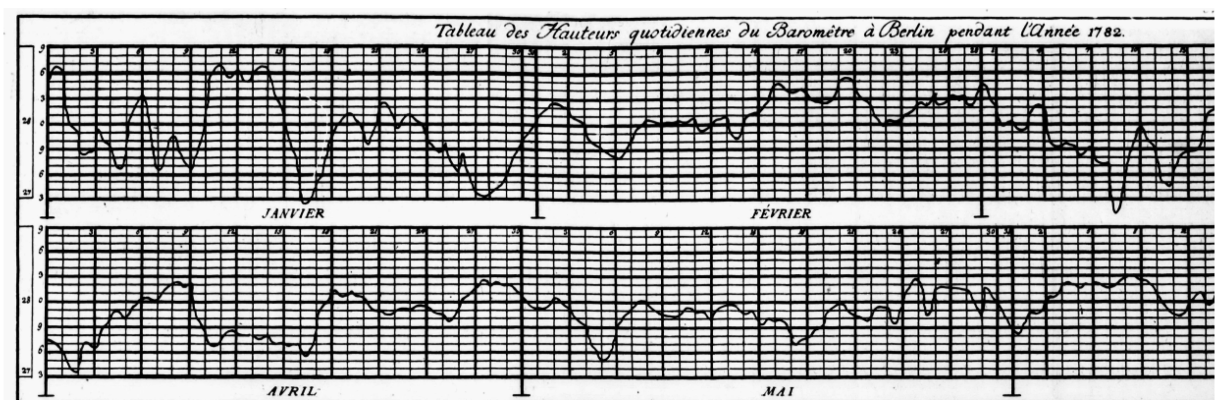


Figure 4.6
A fragment of one of Beguelin's "Tableau des Hauteurs quotidiennes" which he published between 1769 and 1788 in the *Mémoires de l'Académie* of Berlin. Source: *Nouveaux Mémoires de l'Académie Royale des Sciences et Belles-Lettres* 13 (1784): 255.

After a hiatus of a few decades, weather charts came back into fashion in 1769 when Nikolaus von Beguelin was charged by the Berlin Academy with daily meteorological observations. For the next eighteen years Beguelin published an annual barometric chart in the *Mémoires de*

20. Engelen and Geurts, *Nicolaus Cruquius (1678-1754) and His Meteorological Observations*, 28.
21. ibid. No correspondence between Cruquius and the Royal Society has been preserved after January 1726 when Cruquius sent his graphical chart for the previous year, see ibid., 35.
22. If Cruquius' chart only appeared in the pages of the *Philosophical Transactions*, Musschenbroek's charts were more widely distributed being also included in *Physicae experimentales* (1729) and *Uitgeleeze Natuurkundige Verhandelingen* 1 (1733).

l'Académie royale des sciences et belles-lettres of Berlin (see Fig. 4.6).[23] As Cruquius and Musschenbroek before him, Beguelin carried out three daily measurements of which only a summary of the monthly averages and extreme values was published in the *Mémoires de l'Académie* of Berlin. This great discrepancy between the volume of observations and the small amount that was being reported probably motivated Beguelin to also publish his barometric charts, titled "Tableau des Hauteurs quotidiennes". In 1783 the *Ephemerides: Societatis Meteorologicae Palatinae* (a meteorological journal founded in the same year) started publishing a table with Beguelin's daily measurements for the whole year; the thirteen-column table took over twenty pages (see Fig. 4.7).[24] After Beguelin had to retire due to bad health, his successor–Franz Karl Achard–published for several years in the *Mémoires de l'Académie* of the Berlin academy the full table of measurements which extended over sixty pages. However, no barometric charts were published.[25]

Beguelin's barometric charts did not go unnoticed. In 1773 the French journal *Observations et Mémoires sur la Physique, sur l'Histoire Naturelle et sur les Arts et Métiers* published a short note about Beguelin's style of reporting meteorological observations and suggested as a further improvement also charting the variation of temperature or even superimposing the two curves on the same diagram.[26] Louis Cotte, the leading French meteorologist of the late 18th century, also referred to "M. Beguelin's method" and remarked that it was adopted by various observers in Russia, Germany, Switzerland and France.[27] Cotte was not exaggerating at all.

23. See *Histoire de l'Académie Royale des Sciences et Belles Lettres*, v. 25 (1771) and *Nouveaux Mémoires de l'Académie Royale des Sciences et Belles-Lettres*, v. 1 (1772) - v. 17 (1788).

24. For Beguelin's observations see also Theodore S. Feldman, "Late Enlightenment Meteorology," in *The Quantifying Spirit in the Eighteenth Century*, ed. J. L. Heilbron, Robin E. Rider, and Tore Frängsmyr (1990), 170-171.

25. See the *Mémoires de l'Académie royale des sciences et belles lettres* between the years 1788-1793.

26. "Manière très simple de faire les observations météorologiques," *Observations et Mémoires sur la Physique, sur l'Histoire Naturelle et sur les Arts et Métiers* 1 (1773): 427–430. The charts were also noticed in the *Histoire et mémoires de la Société des sciences physiques de Lausanne* (1789) which remarked that meteorological measurements should be published as was done by the Berlin academy, "par le moyen d'une courbe qui en fait sentir les nuances" (212).

27. However, Cotte considered that despite its clear advantages such a method had an obvious shortcoming because it did not allow one to manipulate the observations to obtain monthly averages. For these reasons, numerical tables were indispensable, see Louis Cotte, *Mémoires sur la météorologie, pour servir de suite & de supplément au Traité de météorologie publié en 1774* (De l'Imprimerie royale, 1788), 7-8. A decade later, Cotte

OBSERVATIONES BEROLINENSES

Autore Beguelin.

Januarius.

Dies.	Hora. Obſerv.	Barom.	Th. juxta barom. ſuſpenſ.	Th. libero aëri expoſ.	Hygr.	Declin.	Ventus.	Pluvia.	Evap.	Luna. long. h. 12 p.	Coeli fac.	Meteora.
		dig. lin. dec.	gr. dec.	gr. dec.	gr. dec.	gr. min.	direct. vires	part. lin.	lin. dec	° ′		
1	8 mat.	28, 5, 2	5, 6	—13, 5	32, 3	17, 43	N O 1			♌ 8, 34	☉	
	2. 1/2 p.	6, 4	4, 6	—11, 7	32, 3		O 1				☉	
	10 veſp	6, 4	4, 6	—13, 3	30, 0		O 1				☉	
2	8 mat.	28, 1, 6	4, 0	—7, 0	30, 3		S 2				═ ═ a.	
	2. 1/2 p.	27, 11, 9	4, 9	—3, 5	21, 9		S S W 2				═ ═ a.	
	10 veſp	11, 0	4, 3	1, 0	18, 0		S S W 2				═ ═ a.	⁂ h. 1 pom. & h. 6
3	8 mat.	27, 8, 4	4, 9	4, 0	17, 5		S 2				═ ═ a.	⁂ h. 10-11 mat.
	2. 1/2 p.	8, 8	5, 2	5, 3	16, 8		S W 2			♍ 5, 35	═ a.	
	10 veſp	9, 0	5, 3	3, 0	18, 4		S W 3				═ ═	⁂ h. 9-12. veſp.
4	8 mat.	27, 11, 0	5, 8	4, 3	16, 0		S W 2				═ ═ a.	⁂ minut. a ſummo
	2. 1/2 p.	10, 3	6, 2	6, 0	16, 8		W S W 3				═ ═	mane pergit. ⁂
	10 veſp	9, 0	6, 8	5, 3	14, 0		W S W 3				═ ═	⁂

Figure 4.7
Beguelin's table of meteorological measurements as published in the *Ephemerides.* Source: *Ephemerides: Societatis Meteorologicae Palatinae. Observationes Anni 1782* (1784): 78.

In the 1780s some of the memoirs of the Petersburg academy, *Novi Commentarii Academiae Scientiarvm Imperialis Petropolitanae*, included a "Tabula Variationum Barometri" (v. 19-21, 1775-1776) or a barometric chart comparing barometric curves for various locations (v. 15, 1770). In one instance, the same style of representation was used to represent the variation of the tide for the Cronstadt canal.[28] The *Commentationes Societatis Regiae Scientiarvm Gottingensis* (v. 4, 1782) also included charts of temperature and pressure for the year 1779-1780.

Starting in the 1770s barometric curves were also used by Marc-Auguste Pictet, a Swiss physicist and meteorologist.[29] Pictet published the first curve in 1778 to show how one could "represent with a curved line the course of the barometer during the whole year …where the inflexions more or less frequent, or more or less pronounced, make visible at a glance the

confessed that he used monthly barometric curves to find the average pressure for the corresponding month, see Louis Cotte, "Recherches météorologiques," *Observations sur la physique, sur l'histoire naturelle et sur les arts* 41 (1792): 275.

28. See *Acta Academiae Scientiarvm Imperialis Petropolitanae* (v. 2, 1780).

29. It has been alleged that Pictet started drawing such curves in 1774, independently of Beguelin – see *Journal de physique* 54 (1802): 411.

nature of the course of atmospheric variations".[30] If Beguelin's barometric charts were part of a publication strategy, Pictet's curves were mainly private. In a letter from 1780 to the Swiss meteorologist Horace-Bénédict de Saussure, Pictet confessed that he decided to "always trace this curve for my own satisfaction, and I will inquire how much it would cost to engrave it and we will then talk if it is worth the effort".[31] He chose such curves because tables did not contain any daily observations, but also because "cela paraîtra assez joli à ceux qui ne le connaissent pas et ne déplaîra pas à ceux qui le connaissent".[32]

Pictet also exchanged barometric curves with his network of correspondents. He either constructed the curves himself based on the observations he received from his informants, or he distributed special sheets on which such curves could have been easily drawn by others.[33] By 1820, barometric curves were freely circulating in both directions between Pictet and his correspondents:

> Non seulement tu ne m'ennuyes point mon cher Edouard en m'adressant tes lettres et tes courbes, mais, si j'en ai le temps, je veux riposter et t'envoyer les miennes sur le même canevas et pour le même intervalle, en sorte que tu pourras comparer les marches du Thermomètre et du Baromètre dans les deux pays.[34]

Besides being used as a form of tabulation, barometric curves were also employed for argumentative purposes to make visible alleged patterns in the variation of atmospheric pressure. Because of the known effect of the moon on the rising and falling of tides, it seemed more than likely that a similar effect should also apply to the atmosphere leading to a "tidal" variation of atmospheric pressure. The problem was tackled by Johann Heinrich Lambert who relied on forty years of measurements compiled by Giuseppe Toaldo, the professor of astronomy at

30. Marc-Auguste Pictet, "Considérations sur la météorologie et résultats d'observations faites à Genève pendant l'année 1778," *Mémoires de la Société établie à Genève pour l'encouragement des Arts et de l'Agriculture*, 1778, 165.
31. Marc-Auguste Pictet, *Correspondance: Sciences et Techniques* (Genève: Slatkine, 1996), v. 1, 615-616.
32. Ibid., v. 1, 615.
33. In one case, he built curves that compared the temperature and pressure for St-Bernard and Genève. See ibid., v. 1, 217, 510-518, 615-617; v. 2, 300-301, 738; v. 4, 53, 61-62, 550.
34. Ibid., v. 1, 518-520.

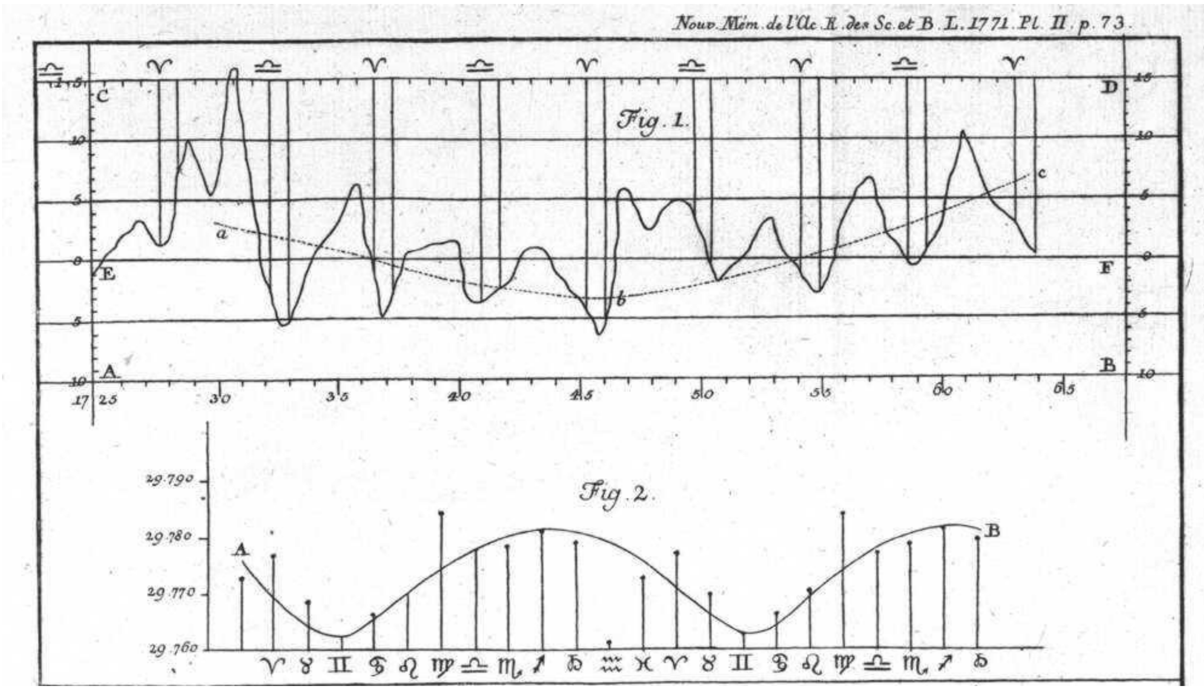


Figure 4.8
Lambert's barometric curves which correlated the variation of the barometer with the astrological position of the moon. Source: Johann Heinrich Lambert, "Observations sur l'Influence de la Lune dans le poids de l'Atmosphère," *Nouveaux Mémoires de l'Académie Royale des Sciences et Belles-Lettres*, 1773, 66–73.

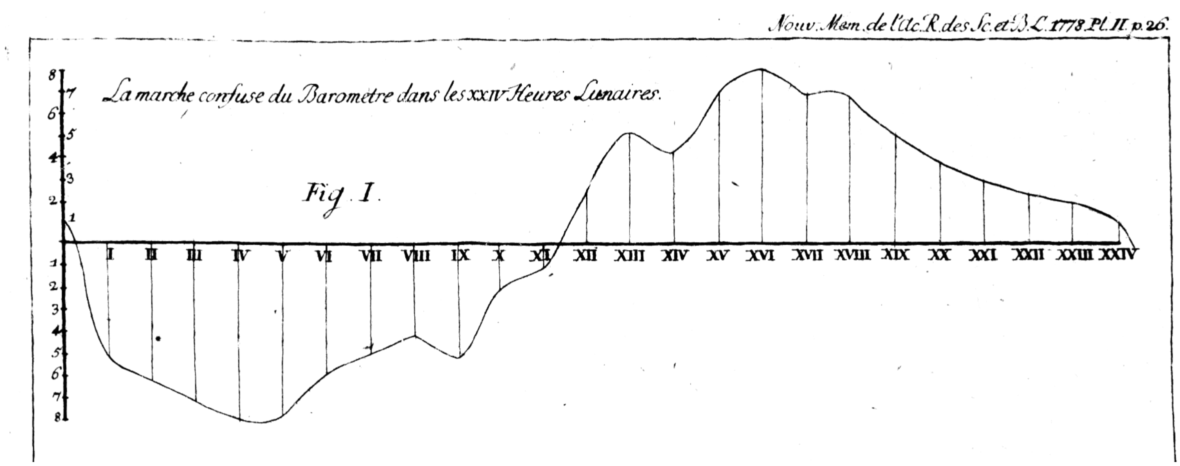


Figure 4.9
Toaldo's barometric curve which correlated the variation of the barometer with the 24-hours of the moon. It followed Lambert's model. Source: Giuseppe Toaldo, "Observation d'une variation particulière dans le baromètre," *Nouveaux Mémoires de l'Académie Royale des Sciences et Belles-Lettres* 9 (1780): 45–56.

Figure 4.10
One of the several barometric charts published in the *Ephemerides*. Such charts usually tried to correlate the variation of the barometer with the position of the moon. Source: Antonio Strnad, "Observationes meteorologicae unius lunarius synodici factae pragae bohemorum 1785," *Ephemerides Societatis meteorologicae palatinae observationes anni 1785*, 1787, 596ff.

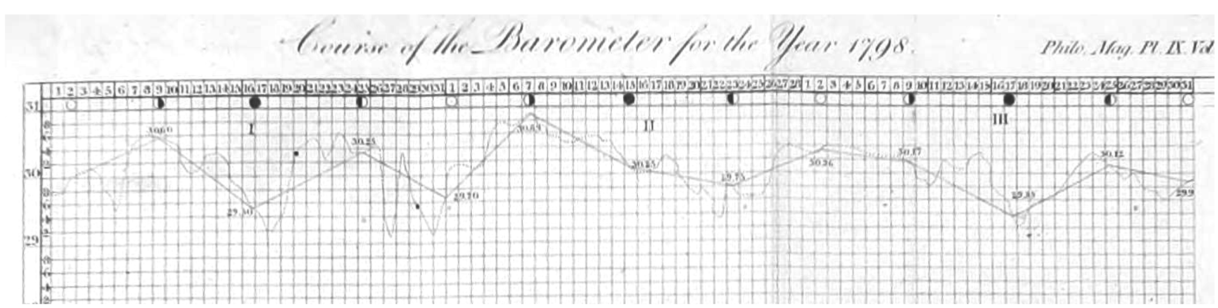


Figure 4.11
Source: Luke Howard, "On a Periodical Variation of the Barometer, Apparently Due to the Influence of the Sun and Moon on the Atmosphere," *Philosophical Magazine Series 1* 7, no. 28 (1800): 355–363.

Padua.[35] Lambert constructed a curve based on Toaldo's tables to show that the lowest point of the average barometric pressured was closely associated with the zodiac sign of the moon's apogee (see Fig. 4.8). Though Toaldo had not previously employed any curves, after Lambert's short observations, he also followed suit to represent the variation of the atmospheric pressure with respect to the twenty-four hours of the moon. The "tidal" effect of the moon was shown by a curve which Toaldo called "la *marche confuse du baromètre*, à cause qu'elle résulte de toutes ces observations mêlées ensemble" (see Fig. 4.9).[36] The first and few graphical representations published in the *Ephemerides* in the 1780s were also dedicated to the influence of the moon on barometric oscillations (see Fig. 4.10).[37] A very similar barometric chart was used in 1800 by Luke Howard to show a relation between the phases of the moon and the atmospheric pressure (see Fig. 4.11).[38]

1.1 A paradigmatic curve

By the end of the 18th century, the curve of barometric pressure had become a *paradigmatic* curve through which other graphical representations were understood and defined. When J.A. Eytelwein presented a chart of the water level of the river Oder between 1782-1791, he remarked that in constructing the chart he followed "the examples found in the memoires

35. Johann Heinrich Lambert, "Observations sur l'Influence de la Lune dans le poids de l'Atmosphère," *Nouveaux Mémoires de l'Académie Royale des Sciences et Belles-Lettres*, 1773, 66–73.

36. Giuseppe Toaldo, "Observation d'une variation particulière dans le baromètre," *Observations et Mémoires sur la Physique, sur l'Histoire Naturelle et sur les Arts et Métiers* 20 (1782): 91. Because the effect that Toaldo was trying to illustrate was extremely feeble, he did not plot directly the variation of the pressure, but its variation with respect to the average (represented by the zero line). For reactions to Toaldo's claims about the effect of the moon see the source cited in Cassidy, "Meterology in Mannheim," 15-16. For a detailed history of the problem see O. B. Sheynin, "On the History of the Statistical Method in Meteorology," *Archive for History of Exact Sciences* 31, no. 1 (1984): 56-62.

37. Another similar graphical representation was used in Vincentii Chiminello, "De Diurna Nocturnaque Oscillatione Barometri Monitum," *Ephemerides Societatis meteorologicae palatinae observationes anni 1784*, 1786, 230–233. For other examples of studies from the *Ephemerides* that discussed the influence of the moon on atmospheric pressure see Cassidy, "Meterology in Mannheim," 21-22, n58.

38. Luke Howard, "On a Periodical Variation of the Barometer, Apparently Due to the Influence of the Sun and Moon on the Atmosphere," *Philosophical Magazine Series 1* 7, no. 28 (1800): 355–363.

of different academies" of how to display the values of pressure.[39] The concept behind the barometric curve was also extended outside the field of meteorology. Already Beguelin and Pictet had imagined that the "course of the barometer" could be translated into a "courbe de la vie" which would represent the amount of happiness or unhappiness corresponding to each day (see Fig. 4.12).[40]

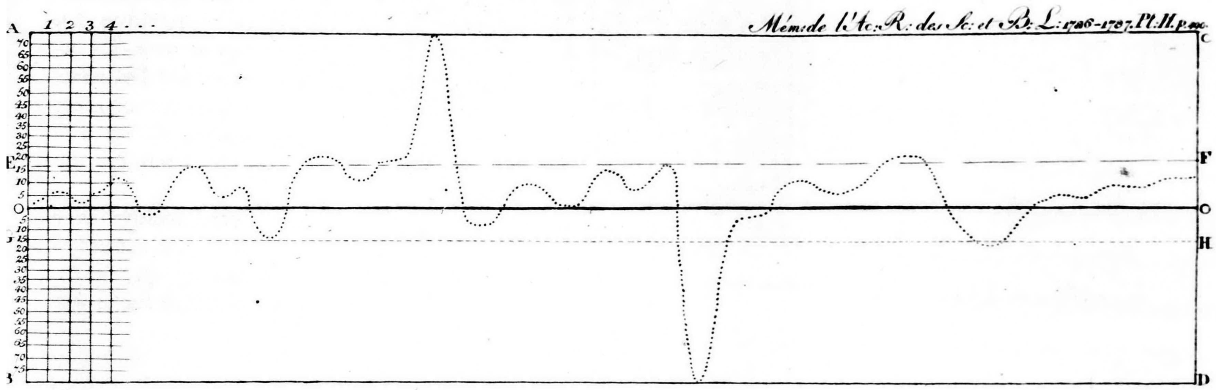


Figure 4.12
Beguelin's representation of "la courbe de la vie" drawn in analogy with his barometric charts (see Fig. 4.6). Source: Nicholas Beguelin, "Réflexions sur les Plaisirs et les Peines de la vie, comparés à l'égard du nombre, des fréquents retours, et de la multitude des genres," *Mémoires de l'Académie Royale des Sciences et Belles-Lettres* 1 (1792): 481–490.

The most impressive extension of the weather charts was that of William Playfair's *Commercial and Political Atlas: Representing, by Means of Stained Copper-Plate Charts, the Progress of the Commerce, Revenues, Expenditure and Debts of England during the Whole of the Eighteenth Century* (1786). Two decades later, Playfair acknowledged that he owed the invention of his economic charts to his brother John Playfair – the chair of Natural Philosophy at the University of Edinburgh – who

> made me keep a register of a thermometer, expressing the variations by lines on a divided scale. He taught me to know, that, whatever can be expressed in numbers, may be represented by lines.

39. J. A. Eytelwein, "Von dem Nutzen einer Wasserstandsscale, nebst Anweisung zur Verfertigung derselben," *Sammlung nützlicher Aufsätze und Nachrichten die Baukunst betreffend für angehende Baumeister und Freunde der Architektur* 2, no. 1 (1798): 26. The chart was reproduced in Hentschel, *Visual Cultures in Science and Technology*, 48.

40. Nicholas Beguelin, "Réflexions sur les Plaisirs et les Peines de la vie, comparés à l'égard du nombre, des fréquents retours, et de la multitude des genres," *Mémoires de l'Académie Royale des Sciences et Belles-Lettres* 1 (1792): 481–490; Pictet, *Correspondance: Sciences et Techniques*, v. 2, 479-483.

> The Chart of the thermometer was on the same principle with those given here; the application only is different.[41]

While this passage has been well-known to historians, its real meaning has often been underestimated because not enough examples of weather charts were known to motivate Playfair's claim.[42] As we have seen above, such claims are faulted by a bias in sampling and definition. While it is the case that journals like the *Philosophical Transactions* din not publish weather charts between 1731 (Muschenbroek) and 1800 (Luke Howard), such a fact carries little weight if one is to make a more general claim about the use of weather charts in *meteorological* work and publications. Not only that, but the weather charts that were published in other journals were impactful enough to be noticed and imitated. The impact of these charts has so far been undervalued by historians because they did not match the complexity of Lambert's graphs and graphical analysis. For this reason, Lambert along with Watt and Playfair have come to be seen as the sole possessors of "the graphical method" of representation. Despite the fact that Lambert's graphical prolificity did not have a direct impact on his contemporaries, historians have insisted to connect Lambert with anyone else who used graphical representations. For example, Tilling only acknowledged Beguelin's weather charts through the lens of Lambert's graphs:

> During the period of Lambert's publications in the journal, Beguelin presented some meteorological data in graph form every year. But soon after Lambert had died, in 1777, the old habit of publishing tables of meteorological data was resumed.[43]

Even in more recent studies like Hankins and Silverman's *Instruments and the Imagination* (1995), Lambert and Watt are presented as the origin of Playfair's charts. Either because "[i]t is quite possible that" John Playfair, William's brother, "was familiar with Lambert's *Pyrometrie*,

41. William Playfair, *An Inquiry Into the Permanent Causes of the Decline and Fall of Powerful and Wealthy Nations* (London: Greenland & Norris, 1805), xvi.

42. See Funkhouser, "Historical Development of the Graphical Representation of Statistical Data," 289; Tilling, "Early Experimental Graphs," 196; Wainer, *Graphic Discovery*, 47-48; Hankins and Silverman, *Instruments and the Imagination*, 127-128.

43. Tilling, "Early Experimental Graphs," 206. As seen above, Beguelin published his barometric charts until he retired and his endeavor was most probably independent of Lambert.

because Lambert's graphs were also temperature data", or because William Playfair worked as a machinist and draftsman for Boulton and Watt, "[h]e may have taken the idea of graphs from Watt, although this seems unlikely, considering the early date (1785) of Playfair's Commercial and Political Atlas".[44]

The likelihood of these scenarios is evaluated strictly in terms of access (to a book, or a person) and chronology. However, as shown through the example of the weather charts, what was imitated was never the general method of graphical representations or the idea of a graph. Instead, historical actors always referred to a particular form of representation as the source for their graphical innovations. The point was not lost on Playfair's contemporaries such as Alexander von Humboldt who in *Essai politique sur le royaume de la Nouvelle-Espagne* (1811) traced the genealogy of his charts and Playfair's charts back to the barometric curves:

> Cette méthode graphique est analogue à celle que M. Playfair a employée le premier, et d'une manière très ingénieuse, dans son atlas commercial et politique, et dans ses cartes statistiques de l'Europe. Sans attacher beaucoup d'importance à ces esquisses, je ne puis les regarder comme de simples jeux d'esprit étrangers à la science. Il est vrai que la carte que M. Playfair a donnée des progrès de la dette nationale de l'Angleterre, rappelle le profil du pic de Ténériffe; **mais depuis long-temps les physiciens ont indiqué, par des figures tout à fait semblables, la marche du baromètre, et la température moyenne des mois**. Il seroit ridicule de vouloir exprimer par des courbes des idées morales, la prospérité des peuples, ou la décadence de leur littérature. Mais tout ce qui a rapport à l'étendue et à la quantité, est propre à être représenté par des figures géométriques. Les projections statistiques qui parlent aux sens sans fatiguer l'esprit, ont l'avantage de fixer l'attention sur un grand nombre de faits importans.[45]

There are two ways in which we can interpret Playfair's and Humboldt's remarks. First, because their charts had some claim of novelty it might have been important to acknowledge any source of inspiration. However, the charts remained original not despite, but because of their analogy with the barometric charts; as Humboldt pointed out, Playfair's charts were "très ingénieuse". Second, while Humboldt acknowledged the influence of Playfair's charts, he also

44. Hankins and Silverman, *Instruments and the Imagination*, 127-128.

45. Alexander von Humboldt, *Essai politique sur le royaume de la Nouvelle-Espagne*, vol. 1 (Paris: Chez F. Schoell, 1811), 185-186, my underline.

mentioned the barometric charts probably because he wanted to provide his readers with an analogy such that his graphical method would be more readily intelligible and acceptable.

If a *contemporary eye* might perceive or understand the barometric charts as graphical representations, for a *period eye* graphical representations came to be defined through analogy with the barometric charts. Late 18th century sources employed the term "graphical" to describe the methods, operations or constructions through which a curve was generated rather than the curve itself. One of the earliest instances in which a curve was described as a *graphical* object, "tracé graphique", can be found in a footnote of J. B. Biot's translation of Ernst Gottfried Fischer's *Physique mécanique* (1806). Biot had been a student at l'École Polytechnique in its founding year 1794 and had been probably inculcated with the new term "graphique" (see Chapter 1). More importantly, Biot introduced the new concept of "tracé graphique" through the paradigmatic example of the barometric charts:

> Le tracé graphique est la manière la plus commune de rassembler comparativement de longues suites d'observations barométriques. On se sert pour cela d'une longue bande de papier, au milieu de laquelle on trace une ligne droite qui la traverse d'un bout à l'autre. Cette ligne est destinée à représenter la hauteur moyenne du baromètre dans le lieu de l'observation. On la divise en un certain nombre de parties égales, qui sont destinées à représenter des jours; puis, parallèlement à cette ligne, et tant au-dessus d'elle qu'au-dessous, on en trace plusieurs autres à des distances égales, comme par exemple d'une demi-ligne. Lorsqu'on a observé le baromètre un tel jour, si sa hauteur est la moyenne, on marque d'un trait le point de la ligne principale qui correspond à ce jour-là; s'il est plus haut d'une demi ligne, on porte l'observation sur la première parallèle, au-dessus de la ligne moyenne; s'il est au-dessous de la hauteur moyenne, on porte l'observation au-dessous de cette ligne, sur la parallèle qui lui correspond. On porte ainsi successivement les observations de tous les jours, chacune au rang et à la hauteur qui leur convient. On peut même, et cela est plus exact, repéter les observations plusieurs fois par jour, et les porter de même chacune à leur place, en divisant en parties égales l'intervalle qui correspond à un jour; et si, par tous les points ainsi déterminés, on fait passer une ligne courbe qui les unisse, et qui en suive toutes les irrégularités, cette ligne, par ses ondulations, représentera fidèlement l'état du baromètre dans les époques successives où l'on aura observé.
>
> Je connais, en Suisse, un propriétaire fort instruit, qui tient ainsi, depuis plusieurs années, un tableau très exact d'observations barométriques, faites trois fois par jour, avec un fort bon baromètre. Il a eu soin de noter l'état de l'atmosphère, près de chaque observation: or, à l'inspection de ce tableau, on voit que, dans le très grand nombre des cas, lorsque le baromètre a baissé, il est tombé de la pluie; et au contraire, lorsqu'il s'est élevé, le temps est devenu serein. On

> aperçoit par intervalles des exceptions à cette règle; mais elles sont beaucoup moins nombreuses que les cas dans lesquels elle se vérifie. Cette connaissance peut être fort utile à l'Agriculture; et la personne dont je parle en tirait elle-mêmc un très grand parti.[46]

With few alterations, Biot repeated this description of the "graphical trace" as exemplified by barometric curves in his later publications such as the textbook *Traité de physique expérimentale et mathématique* (1816) or the article on "Meteorology" from *Dictionnaire de Science médicale* (1819). I have quoted the whole passage to reveal two elements. First, Biot did not instruct his students how to apply a *general* graphical method or representation to plot the variation of barometric pressure. Instead, he instructed his readers how to construct a barometric chart. Second, Biot was less interested in pointing out the inventor of the graphical method, as much as a man who made constant and productive use of this method. Almost without a doubt, the Swiss to whom Biot was referring was Marc-Auguste Pictet.[47] This comes to show how the *period eye* perceived and valued graphical representations – not as a general method of representation which has an inventor, but rather as a particular method (or tool) which can be put to good use.

2 GRAPHICAL CONSTRUCTIONS

2.1 Published Graphs

As seen above, graphical representations have eschewed the attention of historians that have focused exclusively on some particular journals. However, the problem is not only to know where to look, but also to know what to look for. A graphical representation is not a well-defined object that only exists in its published form. It can very well be part of scientific practice, without ever becoming visible in print. Plots were not published merely because they were drawn, but rather because they played a role in the overall structure of the published

46. Ernst Gottfried Fischer, *Physique mécanique*, trans. Jean-Baptiste Biot (Paris: Bernard, 1806), 191-192.
47. For the correspondence between Biot and Pictet see Pictet, *Correspondance: Sciences et Techniques*, v. 2 124-151.

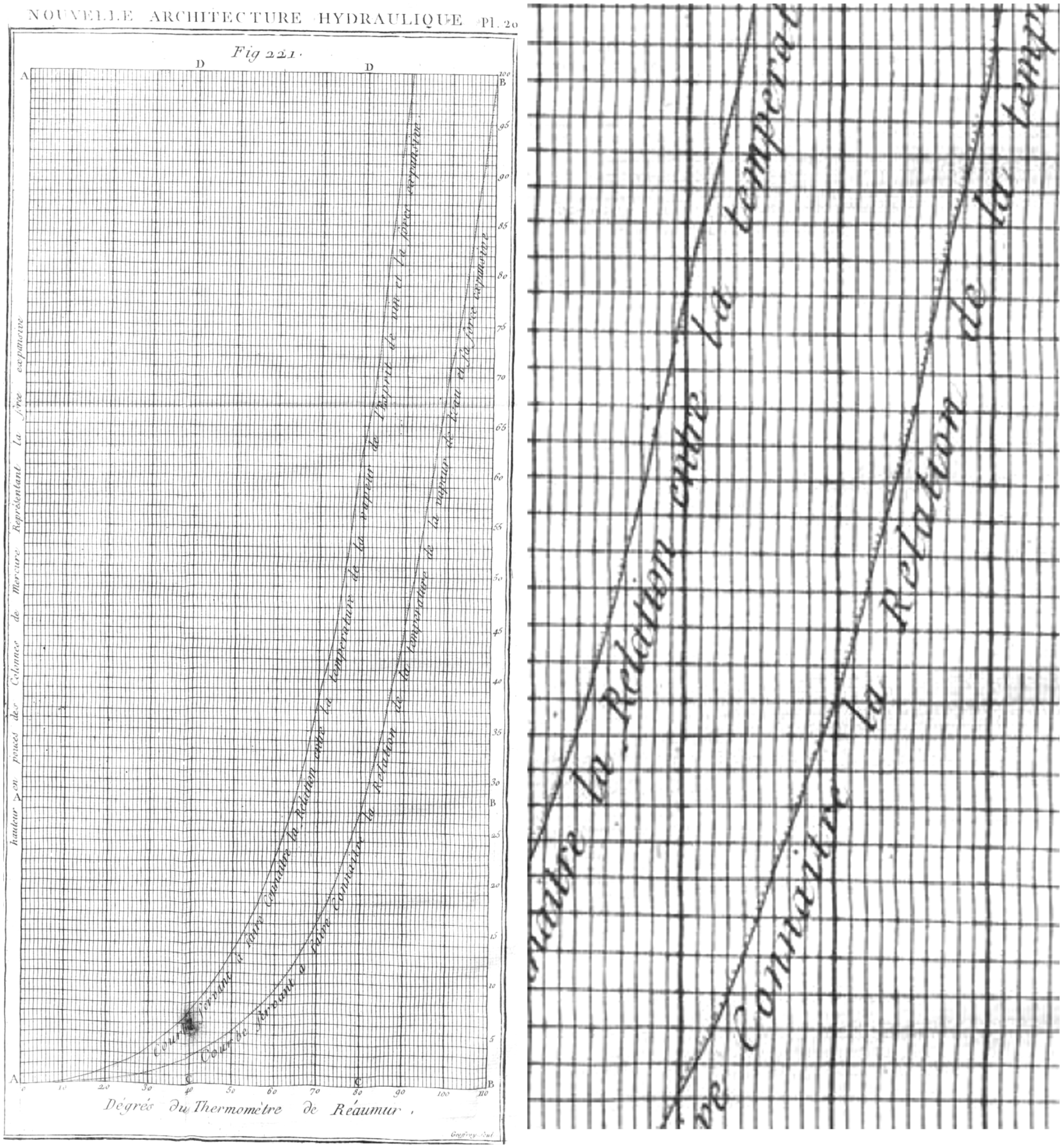


Figure 4.13
Betancourt's experimental curves (dotted line) and the curves of the interpolation formulas (continuous line). Source: Gaspard Riche de Prony, *Nouvelle architecture hydraulique* (Paris: F. Didot, 1790), v. 2, pl 20.

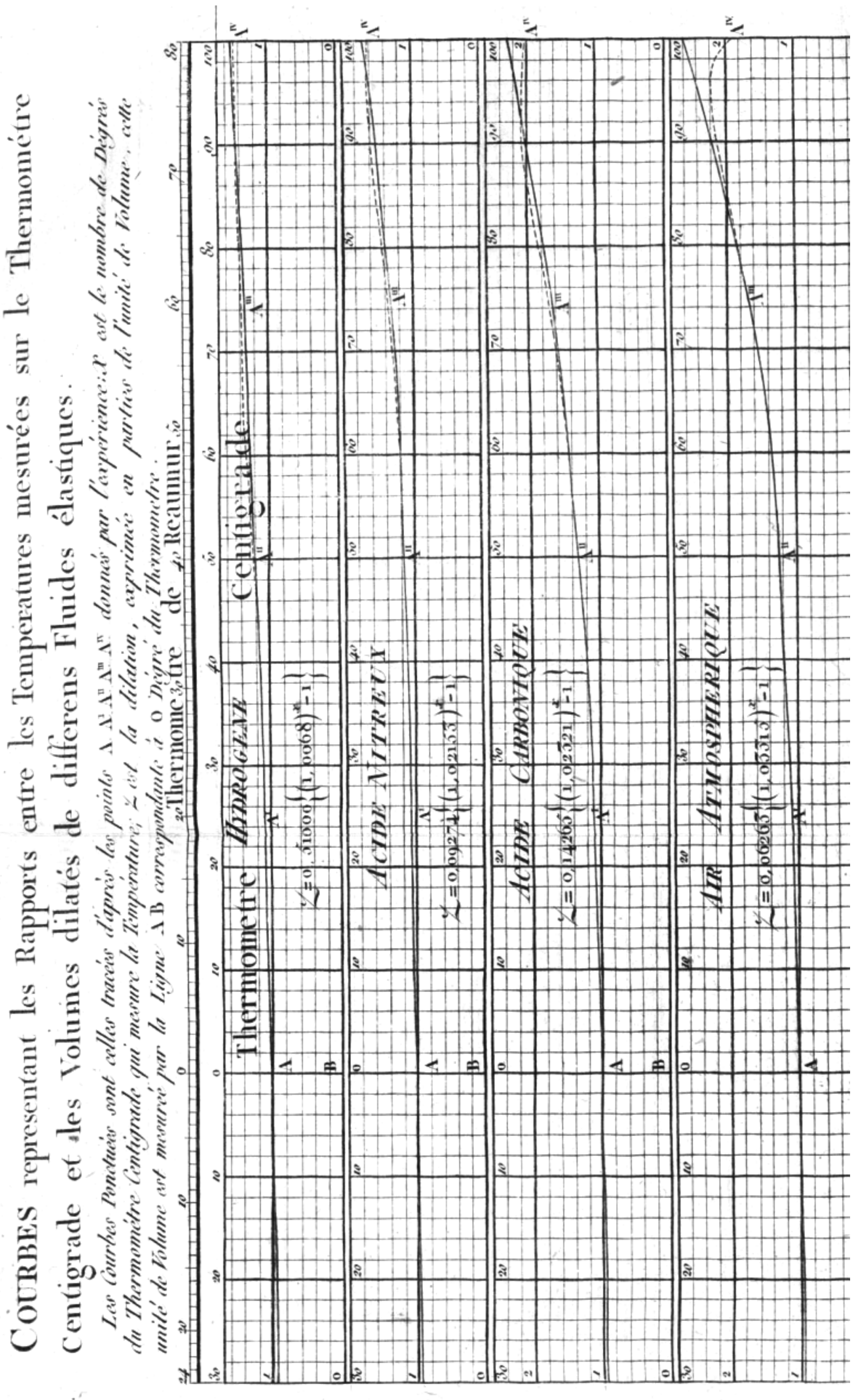


Figure 4.14
Prony's experimental curves (dotted line) and the curves of the interpolation formulas (continuous line). Source: Gaspard Riche de Prony, *Nouvelle architecture hydraulique* (Paris: F. Didot, 1790), v. 2, pl II.

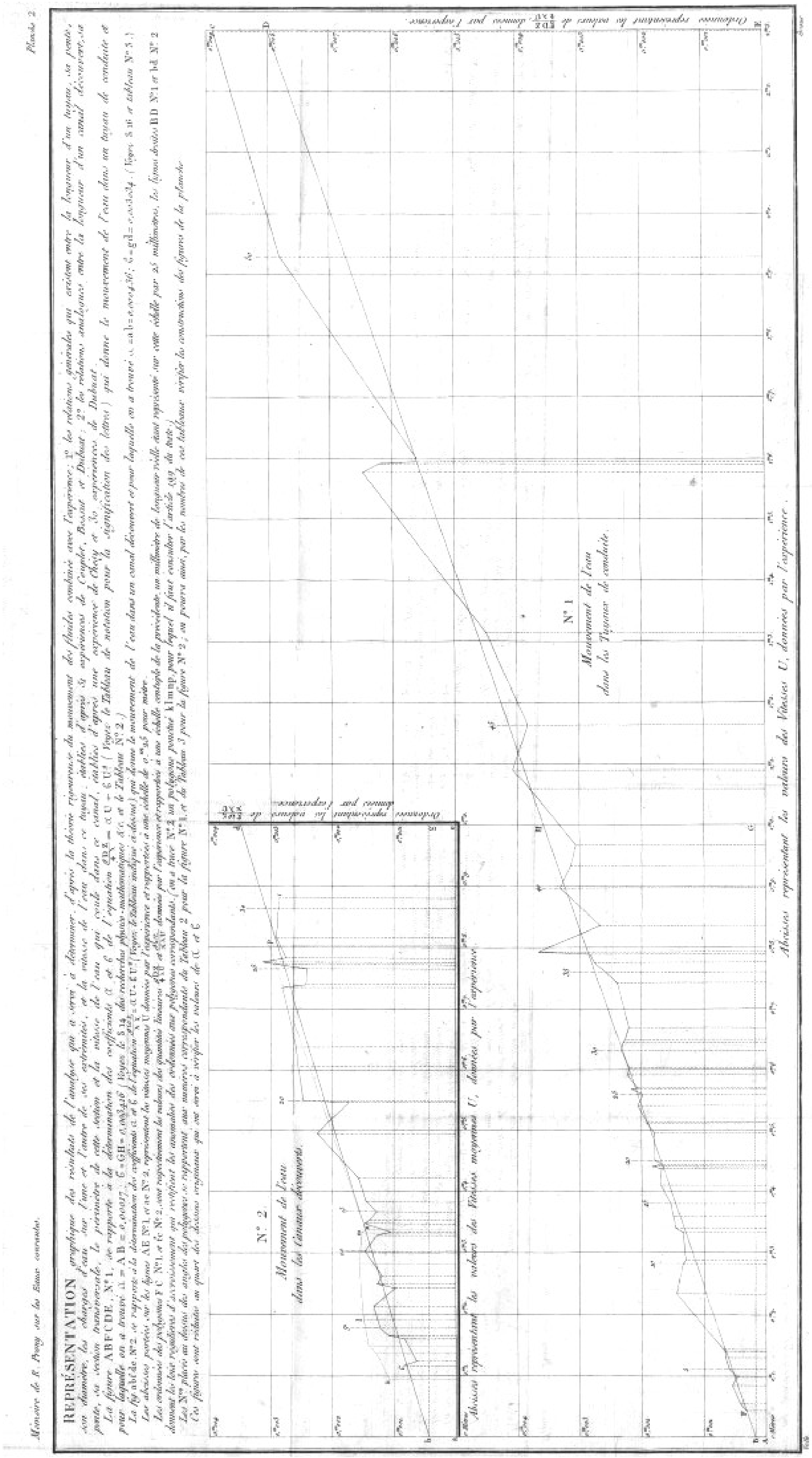


Figure 4.15
Plot No.1 preserved the original size of the drawing on which the linear coefficients were determined, while plot No.2 was reduced to a quarter. Source: Gaspard Riche de Prony, *Recherches physico-mathématiques sur la théorie des eaux courantes* (Paris: Imprimerie impériale, 1804), pl. 2.

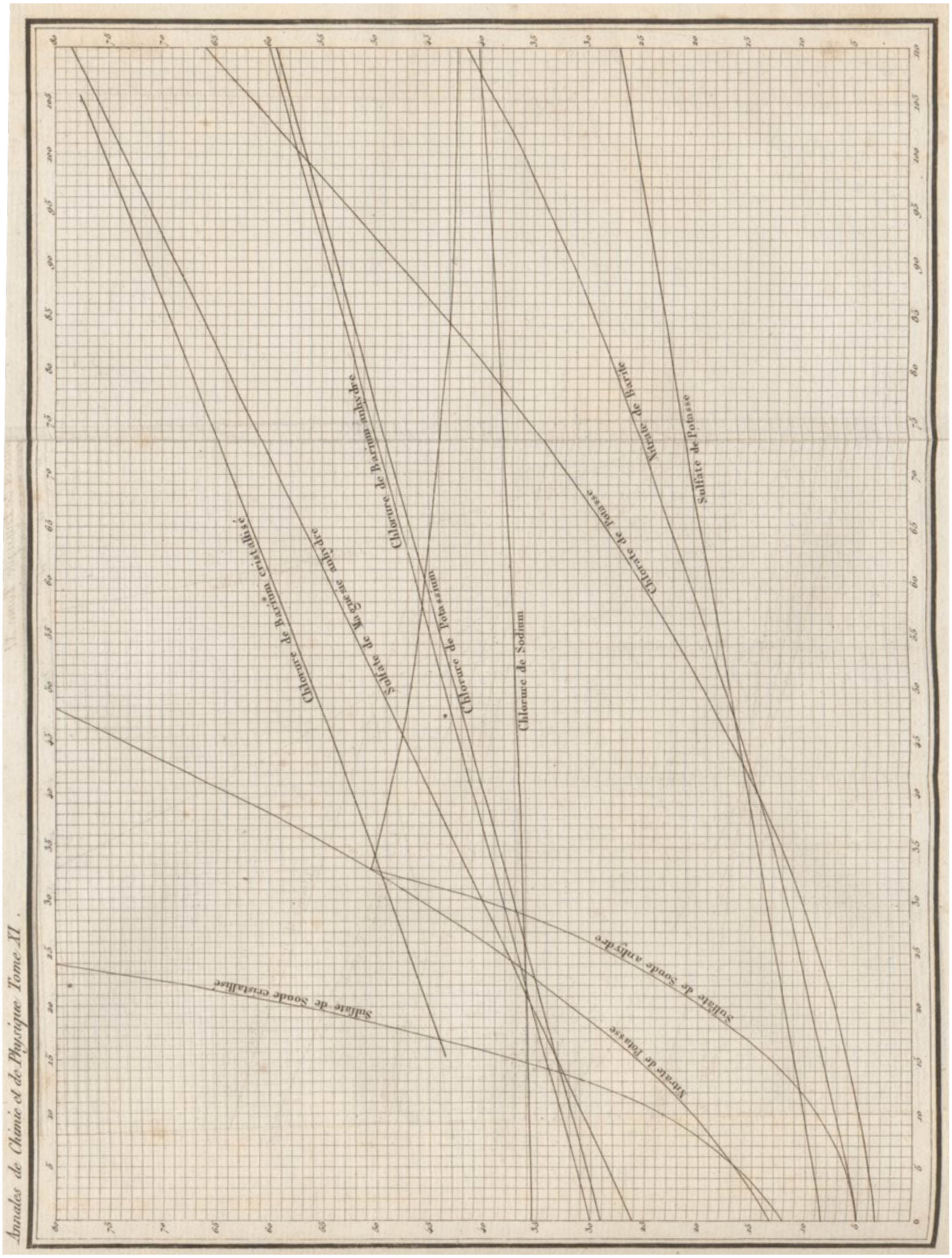


Figure 4.16
Part of the experimental data was fitted to lines, while the sodium sulfate, potassium nitrate, potassium chlorate or barite nitrate were represented through experimental curves. Source: Joseph Louis Gay-Lussac, "Premier mémoire sur la dissolubilité des sels dans l'eau," *Annales de chimie et de physique* 11 (1819): 296–315.

work.

In 1790 Agustín de Betancourt, a young Spanish engineer, submitted to the French Academy a *mémoire* on the expansive force of steam. After carrying out his experiments and measurements, Betancourt wrote, "one of my first concerns was with tracing a curve with temperature on the abscissas and pressure on the ordinates to bring out the regularity [of the measurements] and the connection between them".[48] Though the resulting curve was highly regular, he failed to find any function that could match the entirety of the curve. Gaspard de Prony provided him with a general interpolation formula

$$y = e^{\mu+\lambda x} - e^{\mu+\lambda' x} - e^{\rho+\lambda'' x} + e^{\rho'+\lambda''' x}$$

and a method for determining the coefficients. Betancourt then plotted on a plate attached to his *mémoire* the experimental and empirical curves which in the words of the review committee "coincided almost perfectly".[49] Betancourt's article became one of the commonly referenced sources in the early 19th century on the expansion of steam. Prony, who provided the interpolation formula, summarized Betancourt's article in his monumental *Nouvelle architecture hydraulique* (1790-1796). In the second volume of the treatise he even reproduced the original plate along with a series of similarly constructed plots which were meant to show the agreement between his formulas and experimental measurements (see Figs. 4.13 and 4.14).

In a volume published a few years later, *Recherches physico-mathématiques sur la théorie des eaux courantes* (1804), Prony provided his readers with an impressively large folded plate (see Fig. 4.15). This time, however, the plate was not used to present the agreement between experimental measurements and empirical formulas, but rather to *graphically* represent a graphical construction for how to determine the coefficients of a linear equation based on experimental

48. Agustín de Betancourt, *Mémoire sur la force expansive de la vapeur de l'eau* (Paris: Laurent, 1790), 15-16.
49. Ibid., vii. For the original plate see ibid., pl. 2.

results.[50] Prony recommended that one use a sheet of up to one or two meters for the abscissas.[51] Because in the book he provided the value of coefficients determined both by his graphical construction and by analytical methods, Prony insisted to reproduce the sheet he used at a 1:1 scale. Because the value of the plot was purely to exemplify the size and steps of the construction (and maybe to encourage students to practice on the actual plate), the second plot was reproduced on a smaller scale, 1:4, as duly noted by Prony.[52]

Solubilité du chlorure de potassium.

Température.	Chlorure dissous par 100 p. d'eau.
0^0,00	29,21 ;
19,35	34,53 ;
52,39	43,59 ;
79,58	50,93 ;
109,60	59,26.

Figure 4.17
Table of experimental results for potassium chloride. Source: Joseph Louis Gay-Lussac, "Premier mémoire sur la dissolubilité des sels dans l'eau," *Annales de chimie et de physique* 11 (1819): 296–315.

In a paper from 1819 Gay-Lussac presented his experimental results on the dependence of solubility on temperature through tables, empirical equations and a plot (Figs. 4.16 and 4.17). Though there were overlaps between these three modes of presentation, none was fully reducible to the others. In those cases for which the temperature and the concentration were linearly related, Gay-Lussac provided an empirical formula. It was this line (and not the experimental data) that was plotted. However, if the relationship between temperature and concentration could not be approximated to a line, Gay-Lussac chose to plot directly the experimental data as curves without fitting it to any empirical formulas. The choice was motivated as such:

> It would have been possible to represent [these curves] by an algebraic expression; but their graphical trace has the advantage to give immediately and without further calculation, with a precision

50. Gaspard Riche de Prony, *Recherches physico-mathématiques sur la théorie des eaux courantes* (Paris: Imprimerie impériale, 1804), xviii-xxi.
51. Ibid., 63.
52. Ibid., 70. This method of representation was subsequently followed by Saint-Venant in A. de Barré Saint-Venant, "Mémoire sur des formules nouvelles pour la solution des problemes relatifs aux eaux courantes," *Comptes rendus de l'Académie des sciences* 31 (1850): 283.

> at least as good, the solubility for all the temperatures in the experimental range.[53]

Gay-Lussac's plate was constructed as a final product of his experimental observations and fitted lines. The intermediate graphical steps were never represented. For example, after presenting his measurements for potassium chloride (see table Fig. 4.17), Gay-Lussac noted:

> **If** we construct these results on the abscissa the degree of temperature, and on the ordinate the quantity of salt dissolved in 100 parts of water, we would see that they **can be** represented by a straight line.[54]

On his plate Gay-Lussac never drew these experimental points to show that they could be fitted by a line, instead he drew directly the line. While Prony employed a graphical construction as an intermediate step to determine the coefficients of a linear equation by drawing a line through his experimental points, Gay-Lussac appealed to an analytical method to determine his lines of solubility. He computed the tangent of the line from his table of measurements after which he drew the line through an experimental point which he considered to be the most precise.

Let's take a moment to contrast the three examples just discussed. As I have insisted, regarding a plot simply as a graphical representation tells us too little about either the role of the plot within the structure of the argument presented in the published paper, or within the process of research and publication. There are two factors that we need to keep in mind: 1. what is the order in which tables, equations and graphs interact? 2. in what phase of the interval research-publication was a graph produced?

As seen above, Betancourt mentioned constructing a plot immediately after he had obtained a series of experimental measurements. The plot was constructed to evaluate the regularity of the curves and if they could be approximated to any known curve. Gay-Lussac invoked a similar plot when he motivated his choice to fit some of his experimental results

53. Joseph Louis Gay-Lussac, "Premier mémoire sur la dissolubilité des sels dans l'eau," *Annales de chimie et de physique* 11 (1819): 313-314.
54. Ibid., 308, my underline.

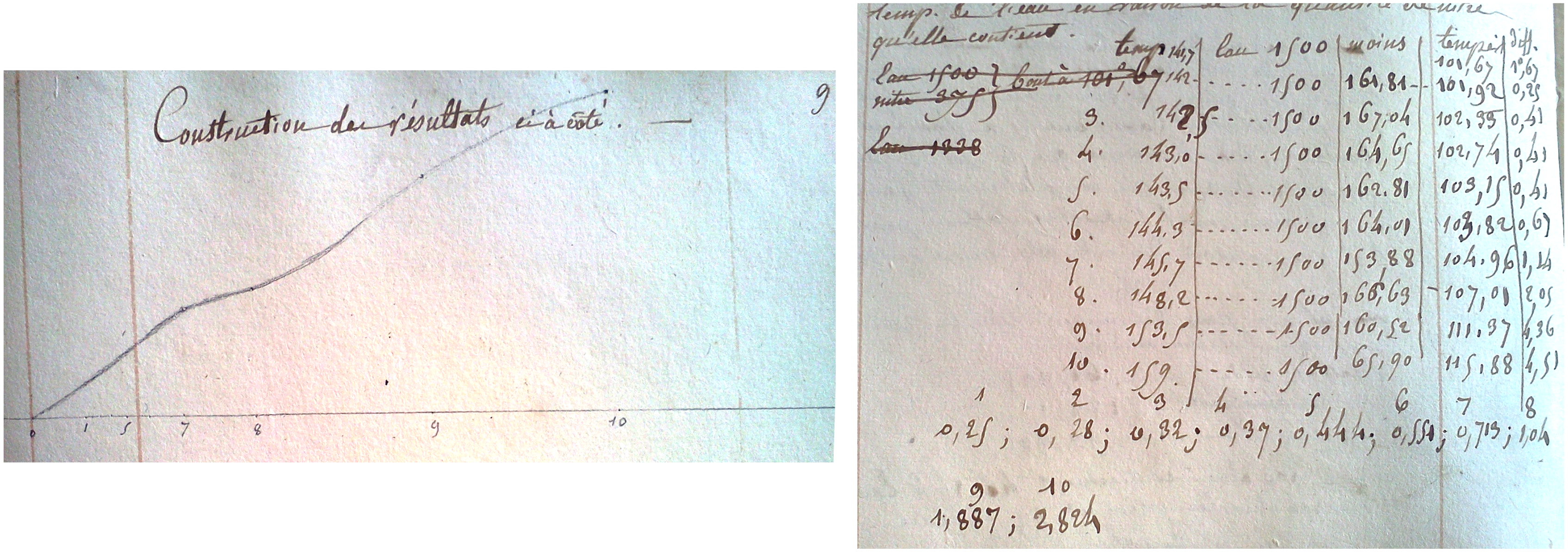


Figure 4.18
Gay-Lussac's "construction des résultats". Though Gay-Lussac often employed such graphical representations, these were never published with the exception of the solubilitly curves (see Fig. 4.16). Source: © Archives de l'École polytechnique (Palaiseau, France), IX GL 5.9.

by a line. The existence of such unpublished plots is not a simple speculation (see Fig. 4.18). If the relation between the experimental data was linear one had a choice of determining the coefficients either graphically or analytically. If the coefficients were to be determined graphically probably a second graph was produced at a large scale, and on special paper, to minimize errors. If the relation between the measurements was more complicated requiring a special interpolation function, such a function might have been plotted against the experimental data to test for agreement.

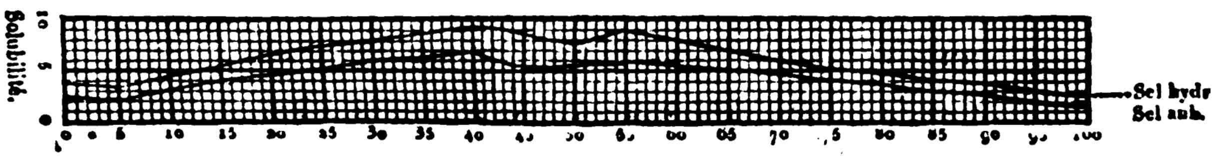


Figure 4.19
"The irregularity of these curves has not allowed the substitution of the direct experimental results with a simple equation that allows to obtain immediately for a known temperature the corresponding solubility." Source: J. Pelouze, "Mémoire sur l'action mutuelle de l'acide phosphorique et de l'alcool," *Annales de chimie et de physique* 52 (1833): 43.

Usually such plots would not have been published because they carried little weight for the overall argument. Any reader could verify on his own that some relation between the experimental results could be approximated by a line. Most often the author of the paper published a table with the difference between the experimental results and the empirical formula with which he fitted the results. So why did Betancourt and Prony bother to publish their plots? Because they were making an implicit claim that a non-trivial empirical formula could satisfactorily approximate the whole span of the experimental results. While it was always rather easy to find a first or second degree polynomial that would approximate parts of an experimental curve, finding a single empirical formula was a much more difficult and important task (because this would have been a first step in connecting the experimental results to a theory). Otherwise, an author might have been motivated to publish a plot if it was highly irregular and no satisfying formula could be found. This was Gay-Lussac's choice, and the choice of J. Pelouze who published a series of solubility curves because "the irregularity of these curves has not allowed the substitution of the direct experimental results with a simple equation that allows to obtain immediately for a known temperature the corresponding solubility" (see Fig. 4.19).[55]

Though for the contemporary eye Betancourt's and Gay-Lussac's curves might seem to be similar graphical representations, they are distinguished by an essential feature: their place within the argument of the paper. In the case of Betancourt, the curves occupy a middle step between his experimental results and the empirical formula, which could be seen as the finished product of the paper. The curves themselves are just a confirmation step. Gay-Lussac's curves of solubility, however, were the finished product of the paper because they summarized both the experimental results and the interpolation lines. Because the curves embodied the final results, and they were not just a step in the confirmation of the results, Gay-Lussac's curves will have a significantly different history than Betancourt's curves.

2.2 The pedagogy of Graphs

Though not present in print, graphical representations could have very well been used in the classroom. J. H. Hassenfratz, the professor of "physique générale" at l'École Polytechnique from 1794 to 1814, detailed in a pedagogical note how a series of experiments should be presented in a physics lesson in front of a "large assembly". Tables were inconvenient because it took a long-time for one to perceive a relation between numbers. Instead, Hassenfratz presented a method that "had been successfully employed at l'École Polytechnique" and which could also "retain the attention of the audience by making it follow the resulting chain of ideas". The method consisted in "representing quantities by lines, placing the lines in a convenient ratio, and connecting them with a curve that passed through their ends". Hassenfratz's note included two plates with some examples of experimental curves based on experiments

55. J. Pelouze, "Mémoire sur l'action mutuelle de l'acide phosphorique et de l'alcool," *Annales de chimie et de physique* 52 (1833): 43.

by Newton, Desaguliers, or Coulomb (see Fig. 4.20).[56] Hassenfratz also recommended the use of graphical methods in constructing curves in one of his scientific articles where he listed the advantages of such a method: simplicity in constructing and reading such curves, and its ability to reveal regularities and irregularities. A curve was also attached.[57]

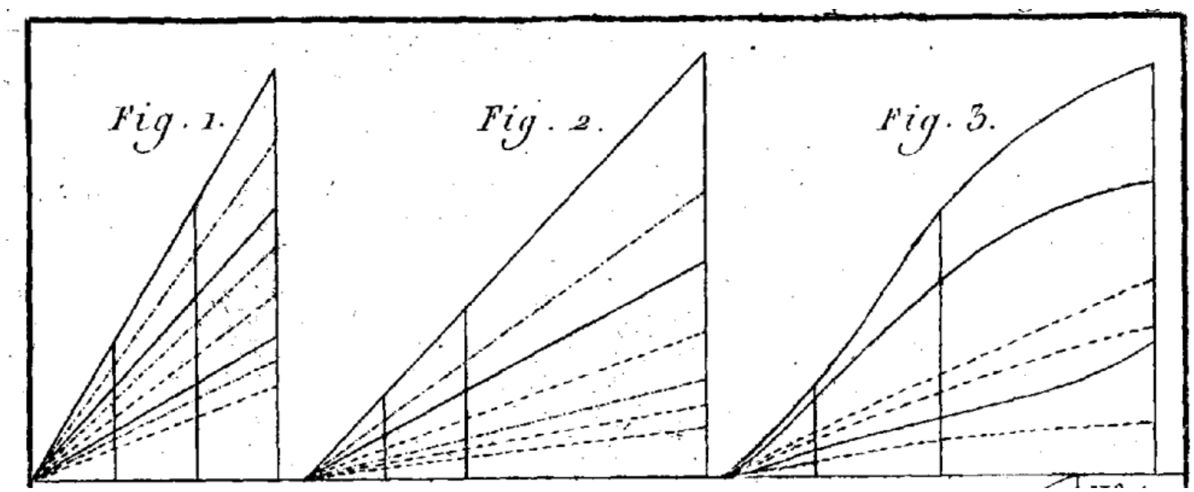


Figure 4.20
An example of Hassenfratz's pedagogical curves. All three figures represented the resistance of strings on the abscissa and their thickness on the ordinates for different torsion cylinders. Source: J. H. Hassenfratz, "Physique générale. De l'enseignement de cette science," *Journal de l'École Polytechnique* 2, no. 6 (1799): 372–308.

It is important to notice that in his pedagogical note Hassenfratz advocated for the use of graphical representations as a method of teaching and of presenting experimental results, and not as a required exercise or practice for students. Even so, in the first decades of the 19th century experimental curves were only rarely presented to students, and almost never as part of a general graphical method. Gay-Lussac's *Cours de chimie* (1828) did not include any experimental curves, not even his curves of solubility. Only when discussing the solubility of potassium nitrate – which he described as having a very variable solubility – Gay-Lussac mentioned that

> Si l'on voulait représenter la marche de sa solubilité par une courbe dont les abscisses représenteraient les degrés de chaleur et les ordonnées, les quantités de sel dissous, on trouverait qu'aux

56. J. H. Hassenfratz, "Physique générale. De l'enseignement de cette science," *Journal de l'École Polytechnique* 2, no. 6 (1799): 372–308.
57. J. H. Hassenfratz, "De l'Aérométrie. Troisième mémoire," *Annales de chimie* 27 (1798): 127-128.

> abscisses représentant plus de 100 degrés, correspondraient bientôt des ordonnées infinies.[58]

Gay-Lussac's *Cours de physique* (1828) only included two curves: one related temperature to the elastic force, while the other represented the variation of temperature throughout the year (see Fig. 4.21). In both cases the curves were provided solely to illustrate a feature of the relations, without any general discussion of graphical representations.[59]

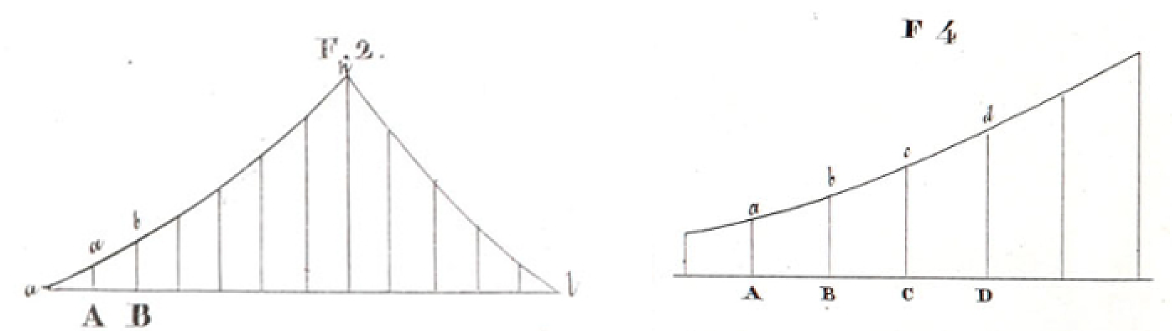


Figure 4.21
Curves representing the relation between temperature and elastic force, and the variation of temperature throughout the year. Source: Joseph Louis Gay-Lussac, *Cours de physique* (Paris: Grosselin, 1828), pl 10, 13.

Other French chemistry textbooks had a similar attitude. While they alluded to Gay-Lussac's results and his graphical construction, they did not reproduce the actual curves. Jean-Baptiste Dumas' imposing eight volume *Traité de chimie appliquée aux arts* (1828-1846) was accompanied by an atlas with 147 plates, none of which included any experimental plots. The pages of these textbooks, however, need not have fully reflected what was carried out in a classroom. In 1831, *L'École Centrale des Arts et Manufactures* which had been co-founded by Dumas in 1829, included in the description of its general chemistry course that the students "are required to produce various drawings, and in particular the curves of solubility of salts, of concentration and the boiling point of acids, etc."[60]

Gay-Lussac's curves of solubility only started being included in French chemistry text-

58. Joseph Louis Gay-Lussac, *Cours de chimie* (Paris: Pichon et Didier, 1828), Leçon 10, 16-17.
59. Joseph Louis Gay-Lussac, *Cours de physique* (Paris: Grosselin, 1828), 421, 508.
60. *École Centrale des Arts et Manufactures. [A prospectus.]* (Paris: H. Fournier, 1831).

books in the late 1840s. It is hard to identify when or why this shift occurred. The first use of the curves in France was in a translation of a German textbook–*Lehrbuch der Chemie* (1831) by Eilhard Mitscherlich, a pupil of Berzelius–which included an in-text, small sized reproduction (see Fig. 4.22). Soon, other textbooks followed suit, such as Alexandre-Edouard Baudrimont's *Traité de chimie générale et expérimentale* (1844), Victor Regnault's *Cours élémentaire de chimie* (1st ed. 1847-1848; 2nd ed. 1849-1850) or Jules Pelouze and Edmond Fremy's *Cours de chimie générale* (1848).[61] Each of these textbooks included a large folded plate of the curves (see Fig. 4.24). By this point, the curves were reproduced not only in the textbooks but were also part of the classroom teaching as was the case of Edmond Fremy's "Cours de chimie" (1847-1848) taught at l'École polytechnique where each individual curve was drawn and discussed separately (see Fig. 4.23). The curves were introduced directly without any previous discussion of a general graphical method of representation. The sole justification given by Fremy was that "l'étude de ces courbes présentent quelque fois des résultats assez curieux et qui peuvent avoir de l'intérêt dans les arts".[62]

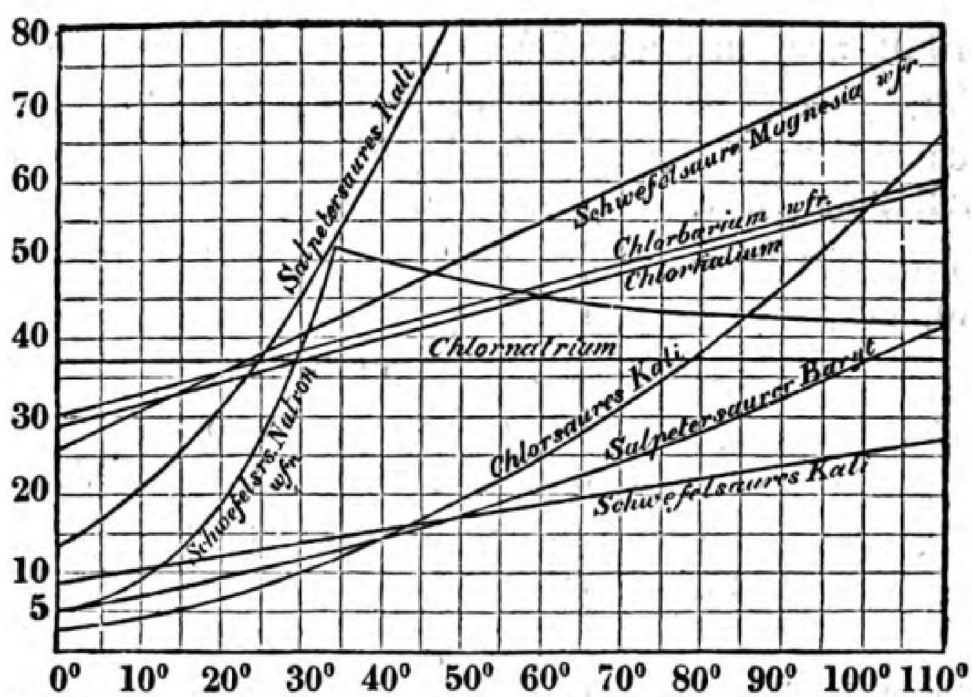
In his several books Étienne Marey always invoked the solubility curves as an important example of the application of the graphical method in science:

> whatever question we ask about these two variables, solubility and temperature, the graph [le graphique] answers to it immediately, without any effort. Refer instead to the numerical tables that contain the same elements, and you will see what difficulties we encounter to represent the relations that are here very easily grasped.[63]

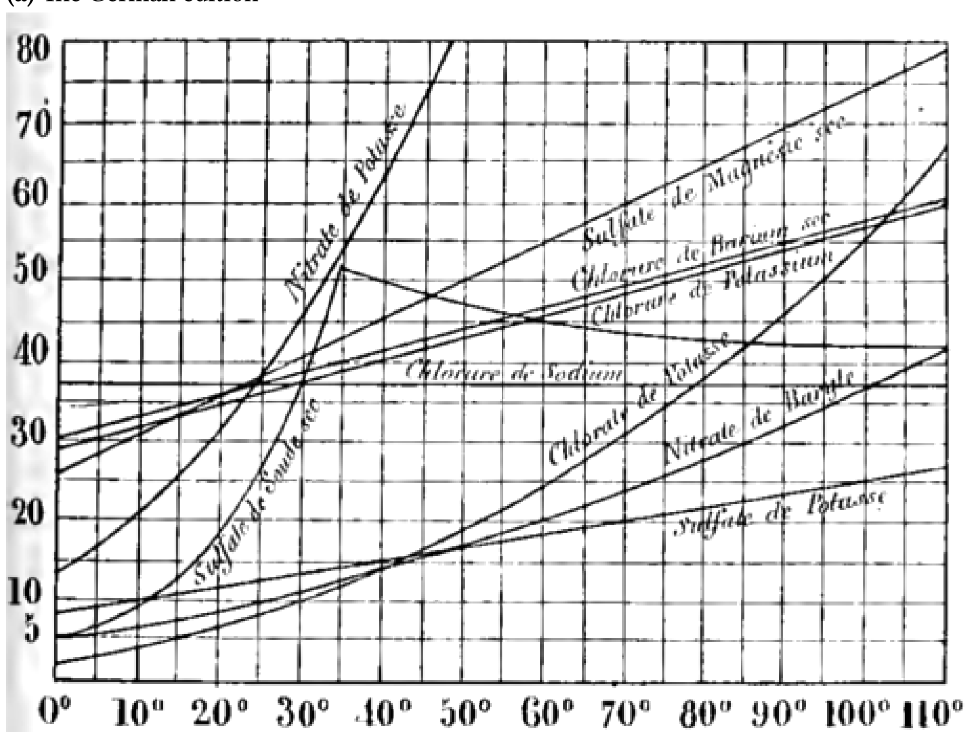
Twenty years later, Marey was even more radical:

> whatever question we ask about these two variables, solubility and temperature, the graphical table answers to it in an instant; in addition, it makes room for some comparisons and general views that a numerical table would not allow.[64]

61. Alexandre-Edouard Baudrimont, "Traité de chimie générale et expérimentale," 1844, v.1, 472-473; Victor Regnault, *Cours élémentaire de chimie*, 2nd ed. (Paris: Victor Masson, 1849), v.2, 70-73; J. Pelouze and E. Fremy, *Cours de chimie générale*, vol. 3 (Paris: Victor Masson, 1848), atlas, pl. xvi; v.2, 322.
62. E. Fremy, "Cours de chimie, 2e division, 1e année, 1847-1848" (Paris, 1847), 340.
63. Marey, *Du mouvement dans les fonctions de la vie*, 99.
64. Étienne-Jules Marey, *La Méthode graphique dans les sciences expérimentales et principalement en physiologie et en médecine* (G. Masson, 1885), 51.

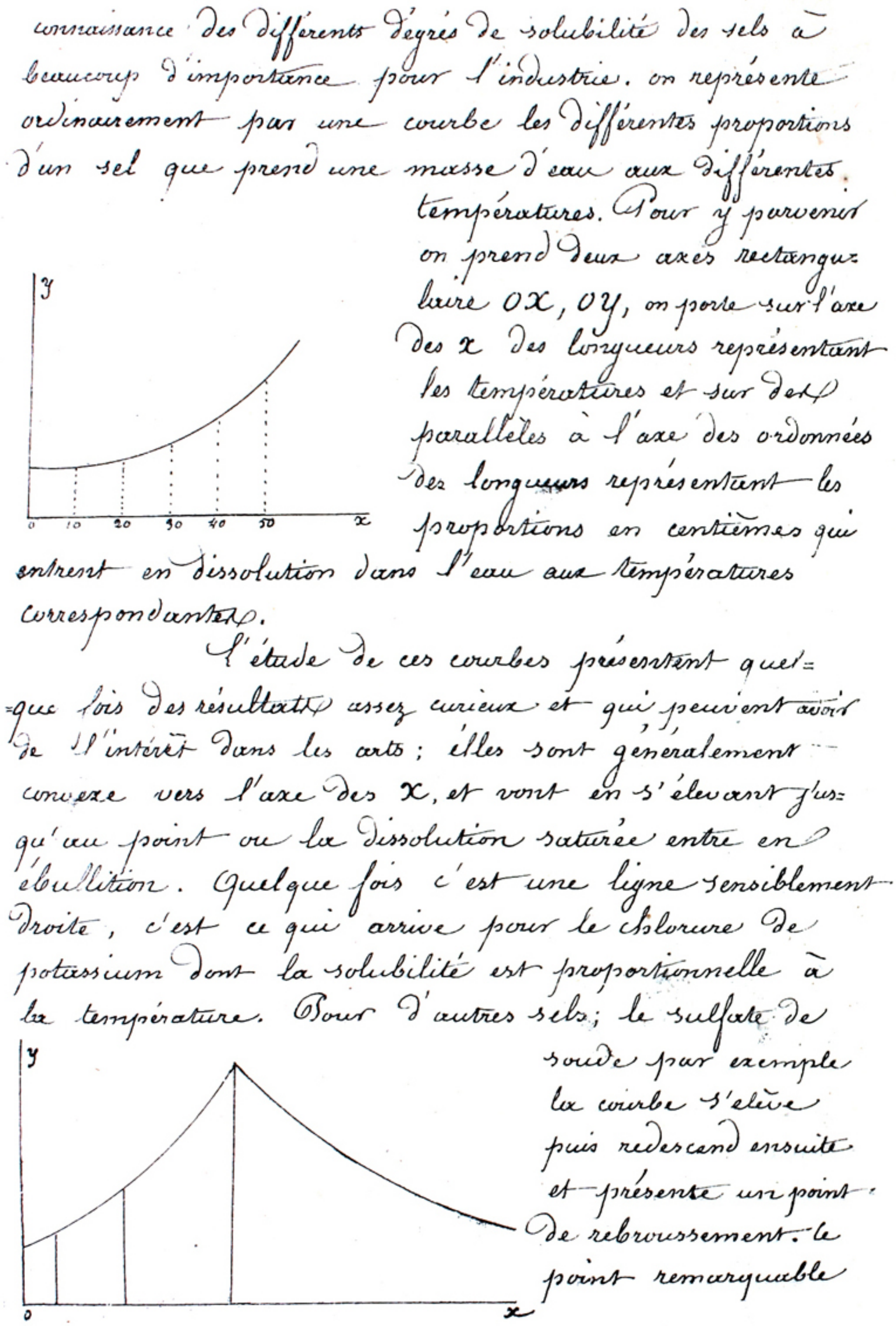


(a) The German edition



(b) The French translation

Figure 4.22
The earliest reproductions of Gay-Lussac's curves of solubility in a textbook. Source: (a): Eilhard Mitscherlich, *Lehrbuch der Chemie*, vol. 1 (Berlin: Ernst Siegfried Mittler, 1831), 287; (b): Eilhard Mitscherlich, *Élémens de Chimie*, trans. Benoît Valérius, vol. 1 (Bruxelles: Louis Hauman et Compe, Libraires, 1835), pl. 17.



connaissance des différents degrés de solubilité des sels a beaucoup d'importance pour l'industrie. on représente ordinairement par une courbe les différentes proportions d'un sel que prend une masse d'eau aux différentes températures. Pour y parvenir on prend deux axes rectangu=laire OX, OY, on porte sur l'axe des x des longueurs représentant les températures et sur des parallèles à l'axe des ordonnées des longueurs représentant les proportions en centièmes qui entrent en dissolution dans l'eau aux températures correspondantes.

L'étude de ces courbes présentent quel=que fois des résultats assez curieux et qui peuvent avoir de l'intérêt dans les arts; elles sont généralement convexe vers l'axe des x, et vont en s'élevant jus=qu'au point ou la dissolution saturée entre en ébullition. Quelque fois c'est une ligne sensiblement droite, c'est ce qui arrive pour le chlorure de potassium dont la solubilité est proportionnelle à la température. Pour d'autres sels; le sulfate de soude par exemple la courbe s'élève puis redescend ensuite et présente un point de rebroussement. le point remarquable

Figure 4.23
Gay-Lussac's curves of solubility as presented in Edmond Fremy's "Cours de chimie" (1847-1848) taught at l'École polytechnique. The curve of each solution was studied and discussed individually. Source: E. Fremy, "Cours de chimie, 2e division, 1e année, 1847-1848" (Paris, 1847), 340, © Archives de l'École polytechnique (Palaiseau, France).

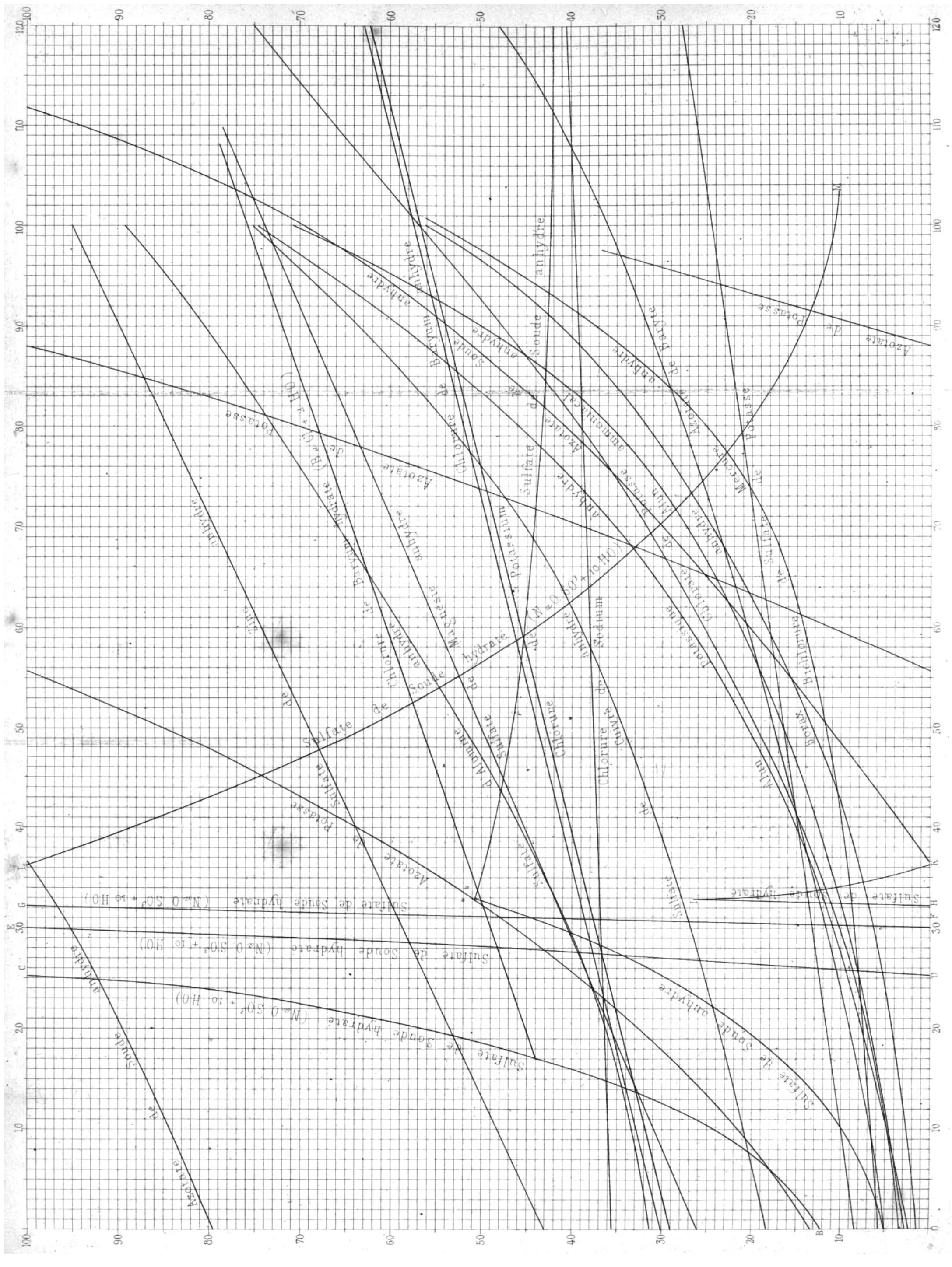


Figure 4.24
Regnault's plot of solubility curves was much more crowded than Guy-Lussac's diagram (Fig. 4.16), not so much because more salts were added, but also because Regnault represented the curves of sodium sulfate hydrate or potassium nitrate over their whole temperature range (Fig. 4.26). Source: Victor Regnault, *Cours élémentaire de chimie*, 2nd ed. (Paris: Victor Masson, 1849).

Marey's books included a reproduction of the solubility curves based on Regnault's chemistry textbook (see Fig. 4.25).[65] Compared to Marey, Regnault's position was more reserved. In his chemistry textbook, he only pointed out that "the curve can be used to find the solubilities at all intermediate temperatures."[66] It was interpolation that was the main advantage of such curves. This was also Regnault's main improvement over Gay-Lussac's curves. Because the curve of hydrated sodium sulfate did not fit into the plot, as it reached the limit of the solubility scale before 25°C, Regnault employed an ingenious solution to represent the curve for the entire range of temperatures: he drew the continuation of the curve on the same plot (see Fig. 4.26).[67]

The four volumes of Regnault's *Cours élémentaire de chimie* contained 689 in-text images. However, there was only one graph - attached as a folded plate - representing the curves of solubility. The *Cours* was also published in a condensed one volume edition under the title *Premiers éléments de chimie* (1st ed. 1850)[68] While the abridged volume used 142 figures, it did not provide any image of the solubility curves. Instead, it only mentioned the curve (reproducing part of the original text).[69] Once again, it was not the general practice of graphical representations that was embraced, but only a very particular and special representation.

2.3 The graphical method of Victor Regnault

Victor Regnault studied at the École Polytechnique and École des Mines, and spent time in Liebig's laboratory in Giesse.[70] In 1836, he became Gay-Lussac's assistant, and in 1840 he was appointed chemistry professor at L'École Polytechnique. Though trained as a chemist, Reg-

65. Marey, *Du mouvement dans les fonctions de la vie*, 97-98; Marey, *La Méthode graphique dans les sciences expérimentales et principalement en physiologie et en médecine*, 49-51.
66. Regnault, *Cours élémentaire de chimie*, v.2, 70.
67. Ibid., v. 2, 70-73.
68. Anders Lundgren and Bernadette Bensaude-Vincent, *Communicating Chemistry: Textbooks and Their Audiences, 1789-1939* (Science History Publications/USA, 2000), 279.
69. See Victor Regnault, *Premiers éléments de chimie* (Paris: Garnier, 1861), 233-234.
70. Robert Fox, *The Caloric Theory of Gases: From Lavoisier to Regnault* (Oxford: Clarendon Press, 1971), 296.

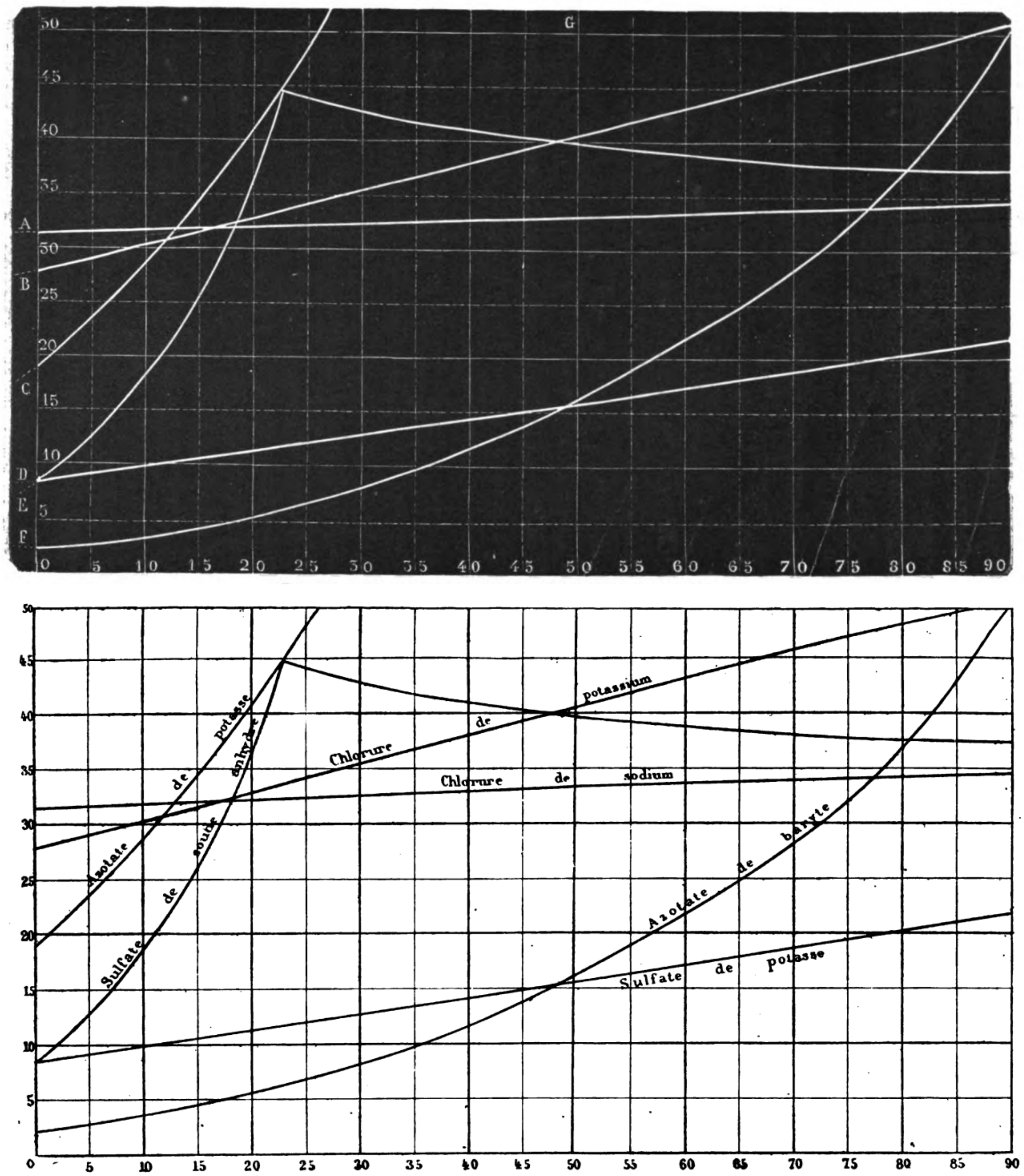


Figure 4.25
Étienne Marey's curves of solubility based on Regnault's *Cours élémentaire de chimie* (see Fig. 4.24). Source:Étienne-Jules Marey, *Du mouvement dans les fonctions de la vie* (Paris: Germer Baillière, 1868), 98; Étienne-Jules Marey, *La Méthode graphique dans les sciences expérimentales et principalement en physiologie et en médecine* (G. Masson, 1885), 49.

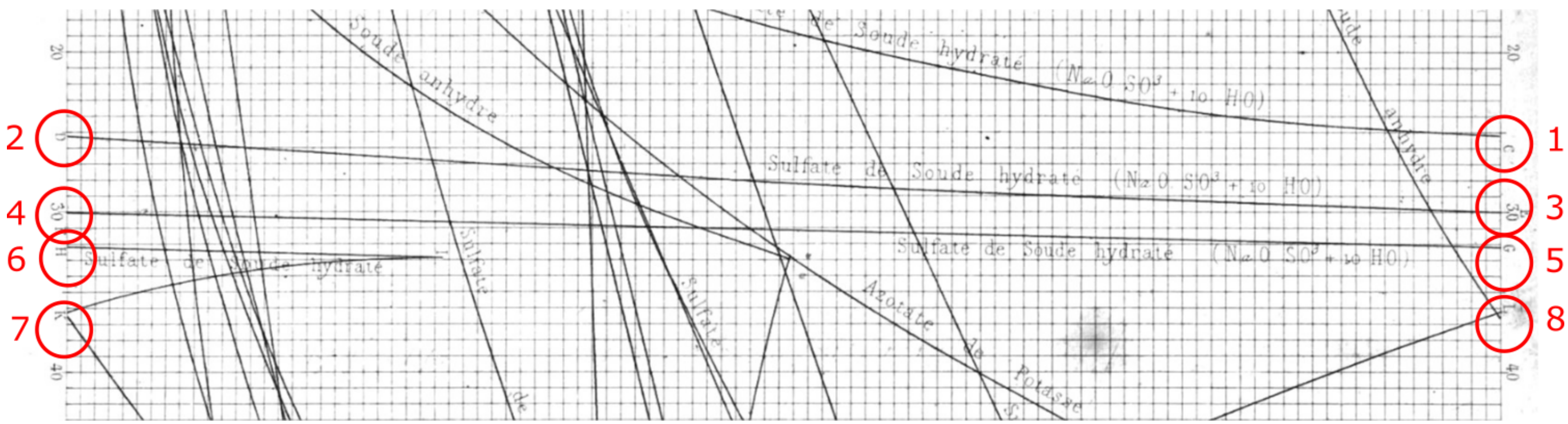


Figure 4.26
Enlarged section of Fig. 4.24. The curve of sodium nitrate [soude hydrate] though it reached its upper limit at 1, was continued on a different scale by the curve starting at 2 and ending at 3, which was then continued by the curve 4-5, 6-7, and the descending curve from 8. The continuity of the curve was indicated by Regnault through alphabetically ordered letters.

nault became increasingly interested in experiments on heat. With the financial support of the Minister of Public Works, he dedicated almost thirty years to "the systematic redetermination of all the experimental data that could conceivably be required in the theory and practice of steam-engines and other heat engines".[71] His results became an example of experimental precision, almost unsurpassed in accuracy and reliability for most of the 19th century.[72]

While sharing some of the features of Gay-Lussac's solubility curves, in the hands of Victor Regnault the graphical method became a mean not only of representing experimental results, but also one of selection and correction. In an article from 1844 Regnault set out to find an interpolation formula to relate the elastic force of steam to temperature. To estimate the agreement between his proposed formulas and the experimental results, Regnault chose to first rectify the experimental measurements by graphically constructing an experimental curve on a large scale. It was the numbers "given by the graphical curve constructed from observations" that were compared with the values generated by the empirical formulas.[73] Most importantly, these experimental curves were never published. The article produced three interpolation formulas for different ranges of temperature (a unified formula was left for future research), and a table with the elastic force computed from the interpolation formulas at different temperatures and the difference between these computed values and the values read from the graphical curves.

This initial study of steam was amplified in a work which will span three volumes over two decades, and which aimed to "determine the principal laws and the numerical data that enter into the calculation of steam engines".[74] A central role was given to "the graphical method",

71. Fox, *The Caloric Theory of Gases*, 298.
72. Ibid., 299.
73. Victor Regnault, "Mémoire sur les forces élastiques de la vapeur d'eau," *Annales de chimie et de physique* 11 (1844): 331.
74. Victor Regnault, "Relation des expériences entreprises par ordre de monsieur le ministre des travaux publics, et sur la proposition de la Commission Centrale des Machine à Vapeur, pour déterminer les principales lois et les données numériques qui entrent dans le calcul des machines à vapeur," *Mémoires de l'Académie Royale des Sciences de l'Institut de France.* 21 (1847): 1–748.

which "when properly constructed is preferable to all methods of interpolation by computation".[75] While Regnault considered interpolation formulas to be "sufficiently exact for most applications", if one wanted to know some values with greater precision "it is better to obtain them from graphical constructions executed on the immediate experimental results". If in the 1844 paper Regnault did not publish any of the plots he used to correct the experimental results, he now made a great effort to publish an impressive plate with experimental curves (see Fig. 4.27). The choice was made not because the results had to be visualized or analyzed, but because many of Regnault's final tables of results were "derived from graphical constructions executed with great care on the immediate experimental results".[76] As such, the acceptance of the accuracy of his experimental results depended on the acceptance of the accuracy of his graphical constructions. For this reason, Regnault described at large all the procedures through which the engravings on the plate were constructed so that "anyone could judge the degree of confidence that one must have in our graphical constructions".[77]

Regnault took particular pride in his large plate and described its construction in great detail. He considered graphical constructions on commercial "papiers divisés" to be unreliable because they were not rigorously divided, nor was the paper a reliable support because it "is moist when the figures are drawn, and when drying out contracts irregularly in different directions". For these reasons Regnault decided to execute his graphical constructions directly on a copper sheet that "we have divided ourselves with the utmost care". This plate inscribed by Regnault himself was only deepened by the engraves and used to print the plates of the book.[78]

First, Regnault drew two perpendicular axes and divided each one of them in 100 divisions.[79] Initially he wanted to print the plot on one meter-squared paper, but as he could not

75. Regnault, "Relation des expériences sur des Machine a Vapeur," 316.
76. Ibid., 234-238.
77. Ibid., 316.
78. Ibid.
79. For a detailed description of the graphical constructions see ibid., 316-328, 574-581.

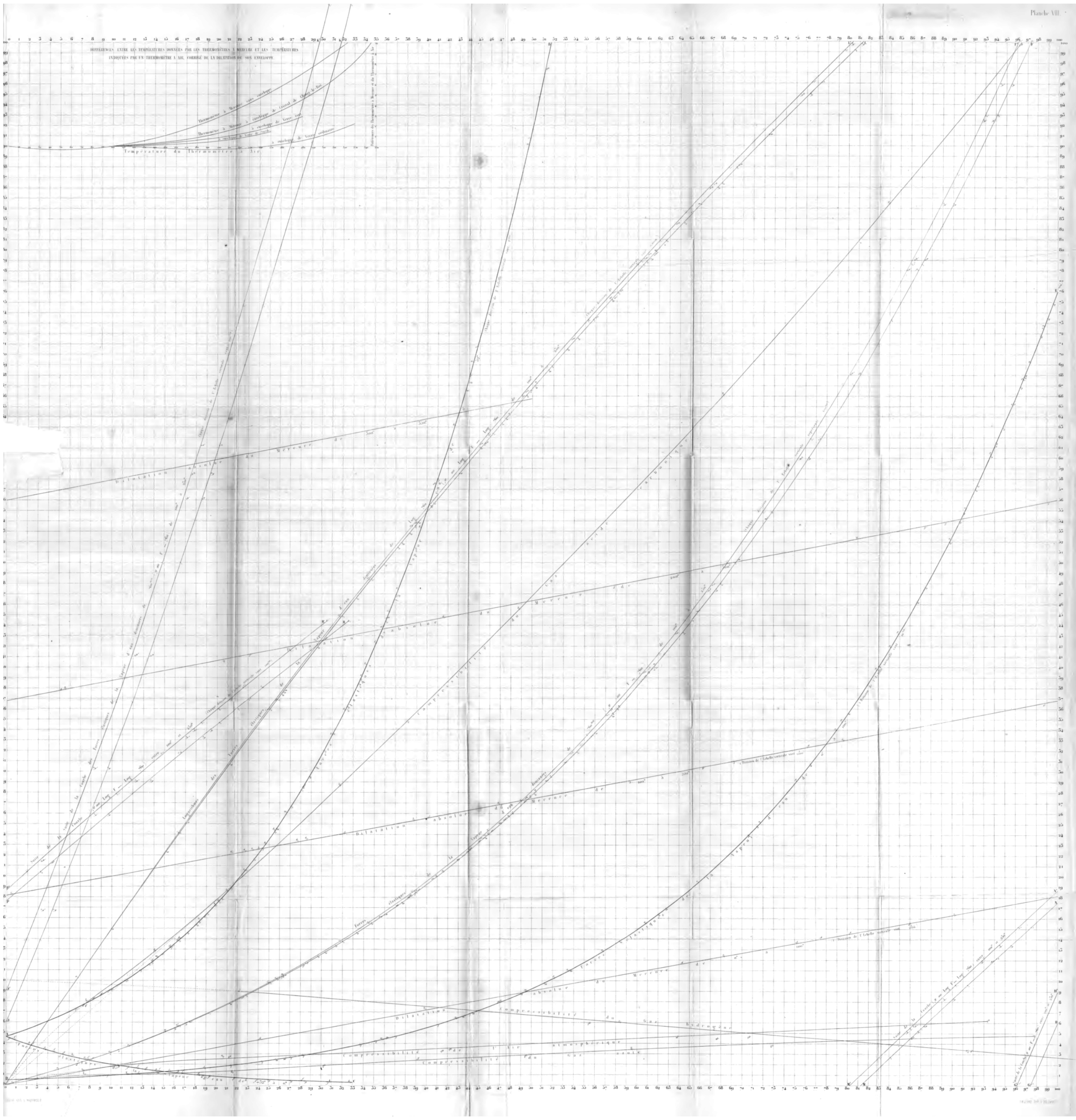


Figure 4.27
Source:Victor Regnault, "Relation des expériences entreprises par ordre de monsieur le ministre des travaux publics, et sur la proposition de la Commission Centrale des Machine à Vapeur, pour déterminer les principales lois et les données numériques qui entrent dans le calcul des machines à vapeur," *Mémoires de l'Académie Royale des Sciences de l'Institut de France.* 21 (1847): v. 1 pl. VIII.

easily find in commerce anything of this size he settled for the size of 80 squared-centimeters. For this reason, each division corresponded to 8 millimeters. Using a special micrometer he inscribed points with a precision of five significant digits. The precision of the inscription was crucial for using such a plot to read out interpolated points with the same accuracy. Because the interpolated results obtained from the experimental curves were so crucial in his evaluation of empirical formulas and constructing tables, Regnault reminded again his readers that "I have traced myself on the copper plate all the small crosses" representing the points that have coordinates equal to the numerical values given by direct observation.[80] Equal care was taken for tracing the lines of the empirical formulas: "I have marked myself this trace with a light line on the copper plate and afterwards the artist has given it with the burin the required depth to allow the reproduction of the drawing".[81]

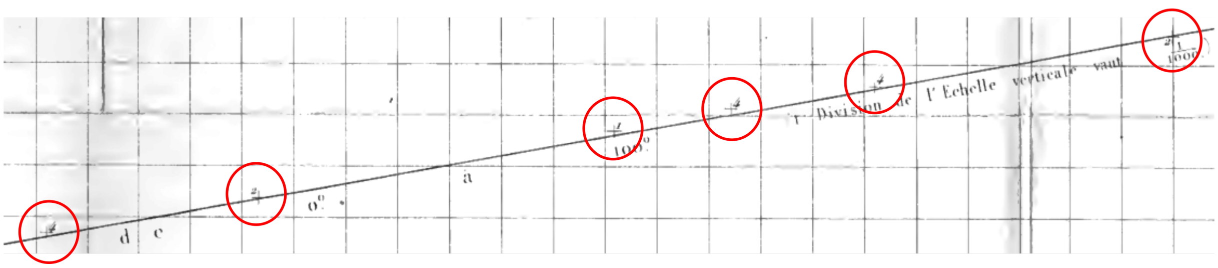


Figure 4.28
Regnault numbered the points corresponding to different series of experiments. By his own admission, he tried to keep the final curve close to the series of experiments that he trusted the most. In this case the series 2 was privileged over series 4.

For Regnault graphical analysis was part of the error analysis. Because he carefully distinguished between series of measurements by marking every experimental point with a number corresponding to a different series, one could use the plate "to distinguish at first sight the variations caused by accidental observation errors" (which were visible for the same series of experiments) and "the constant errors which depend on the diversity of methods that were employed" (which were visible when the curves of different series of experiments did not

80. Regnault, "Relation des expériences sur des Machine a Vapeur," 320.
81. Ibid., 581.

overlap but were separated by small distances).[82] On the engraved plate Regnault only traced "the curves that I admit as the definitive expression of my experiments, and that are capable of satisfying the ensemble of the observations". For this reason, he "drew the curve closer to the series [of measurements] in which I confided the most that they were exact".[83] The reader was left to judge how much the curves that satisfied Regnault departed from the other series of measurements. However, for Regnault this was "la courbe qui représente réellement le phénomène".[84]

3 CONCLUSION

In *Instruments and the Imagination*, Hankins and Silverman have attempted to explain why Lambert's graphical methods had little impact on his contemporaries:

> Lambert's graphical method did not catch on immediately, which may be attributed in part to the obscurity of much of his writing and in part to the unfamiliarity of graphs themselves. We do not have any contemporary reactions to Lambert's graphs, but the graphs of William Playfair, which became much better known than Lambert's, brought forth the criticisms that they "lacked rigor", that they were mere "plays of the imagination" and "without importance" outside of pedagogy. The concept of a graph is abstract, and its meaning will seem obvious only to those who are familiar with it. Those who were used to working with tables of numbers could persuade themselves that in drawing graphs one lost the precision of the numbers themselves. It is probably for these reasons that experimental and statistical graphs did not become popular until the 1830s.[85]

Despite Hankins and Silverman's claim, there were contemporary reactions to Lambert's graphs. However, these reactions were mostly negative because, in the words of the historian of meteorology Theodore Feldman, Lambert's "handsome curves rest more on imagination than reality."[86] Feldman has contrasted Lambert's studies on hygrometry with the exact experimental physics and precise measurements of his Swiss contemporaries Jean-André Deluc

82. Regnault, "Relation des expériences sur des Machine a Vapeur," 316, 580.
83. Ibid., 581.
84. Ibid., 428.
85. Hankins and Silverman, *Instruments and the Imagination*, 120-1.
86. Theodore S. Feldman, "The History of Meteorology, 1750-1800: A Study in the Quantification of Experimental Physics" (PhD diss., University of California, Berkeley, 1983), 51-56.

and Horace-Bénédict de Saussure.[87] Saussure sardonically remarked that Lambert had been more concerned with "tracer géométriquement la marche de l'hygrometre" than with the hygrometer proper.[88] While Saussure identified himself as a "physicien" he referred to Lambert as the "grand géomètre", "célèbre mathematicien", "philosophe".[89] Several times in the essay Saussure made a point of honor and constraint for not having employed any curves:

> Il eût été plus élégant de faire passer une courbe uniforme & réguliere par toutes ces variations; mais ici, comme par-tout ailleurs, je me suis imposé la loi de suivre pied à pied l'expérience, sans prétendre l'assujettir à des idées métaphysiques de régularité & de symmétrie[90]

Thus, if for the *contemporary eye* Lambert's curves might be seen as a modern and useful method of representing and analyzing experimental data, for the *period eye* they could have easily embodied an antiquarian spirit of geometry and metaphysics. The negative reactions were not against the graphs or the graphical method in general (objects that did not exist, or were not perceived as such) but against imperfect experimental results used to find doubtful regularities. The fact that graphical representations were not condemned or mistrusted altogether is evidenced by the barometric charts of Beguelin or Pictet. Pictet even mentioned these curves in a letter to Saussure (see above). Furthermore, one is mistaken to try to find reactions to "Lambert's graphical method"; instead, what can be found are reactions to *particular* representations. One was not imitating a general method or a general representation, but rather a way of approaching a particular problem. That was the case of Toaldo's barometric charts which were drawn after Lambert's. Even among Gay-Lussac's manuscripts, there is a curve of magnetic declension which clearly imitated one of Lambert's magnetic curves, or a descendant of it (see Fig. 4.29). The two curves, when put together, clearly show the difference between Lambert's geometrical curve and Gay-Lussac's graphical curve; while the former is highly regular, especially at the origin, the later has an irregular kink. While Lam-

87. Feldman, "The History of Meteorology, 1750-1800," 62-63.
88. Horace Bénédict de Saussure, *Essais sur l'hygrométrie* (S. Fauche, 1783), ix-x.
89. Ibid., ix, 143, 146, 196, 330.
90. Ibid., 126.

bert's graphical representation are unique in volume and purpose, they were not disconnected from the larger culture of the 18th century. Among other influences, Lambert studied under Musschenbroek in Leiden (1757-1758) who became a main source of inspiration for his studies on hygrometry.[91] The connection between Musschenbroek's weather charts and Lambert's graphical methods have so far been left unaddressed.

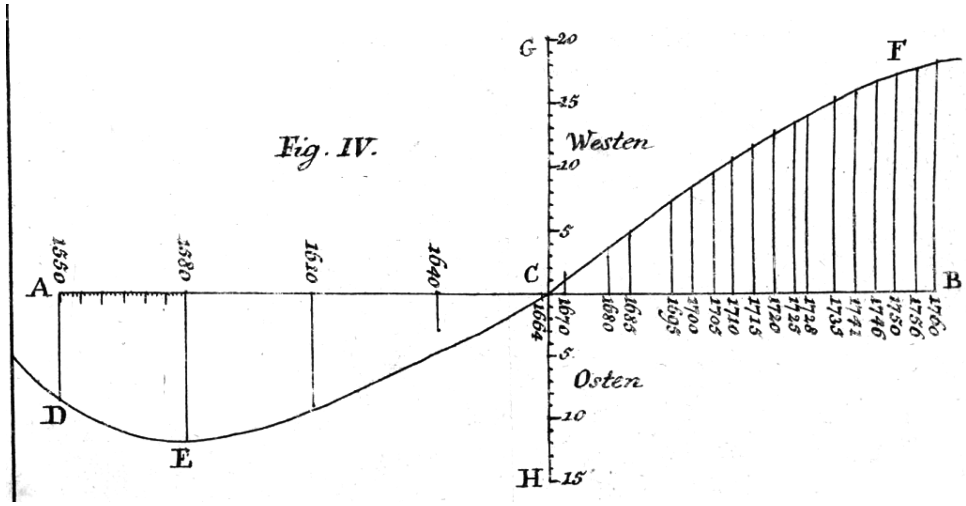
This chapter has argued that to understand the spread of graphical representations one must follow *coherent historical* objects that pertain to specific scientific communities and forms of practice. It is the spread of lowly and unimpressive weather charts or curves of solubility which can account for the development and acceptance of more abstract and general forms of graphical representation. It is also crucial to distinguish between the various contexts and disguises in which a graphical representation might be encountered: as a form of registration, opposed to a form of presentation; as a mean of calculating a numerical value, opposed to a mean of supporting a claim of concordance; as a quick draft for understanding the regularities of experimental results, or as a carefully drawn, large plate summarizing all the results. Each context and disguise imposed its own forms of visibility and mobility.

Though Regnault's large plate of experimental curves was widely discussed, carefully studied and universally appreciated, it was almost never reproduced in books. In *La méthode graphique*, a book which compared graphical representations to a universal language, Marey had to settle for a wordy description of Regnault's plate:

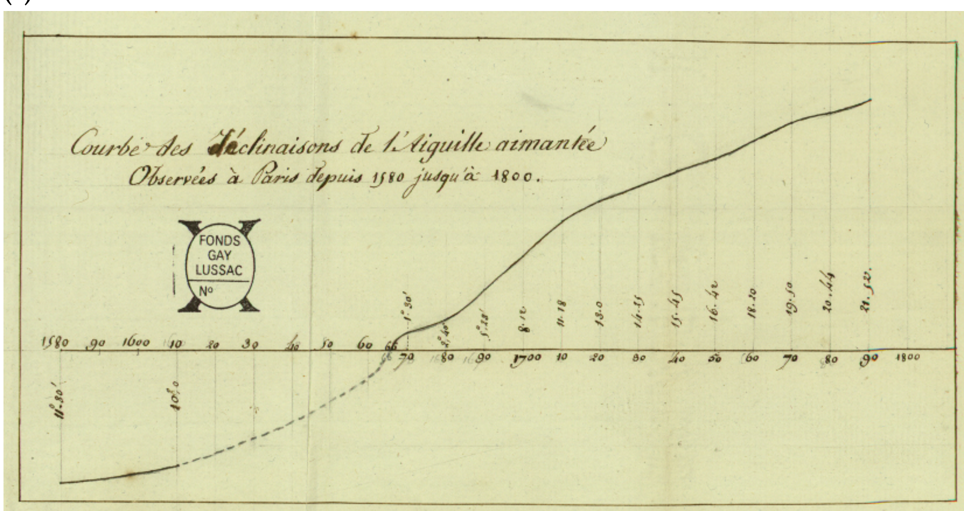
> On doit à Regnault plusieurs tableaux de ce genre; l'un des plus célèbres est celui qui représente aux différentes températures la force élastique de la vapeur d'eau, la dilatation du mercure, la compressibilité de l'air et celle de l'azote. Sur la même feuille sont encore indiquées les corrections que l'on doit faire aux thermomètres à air, suivant la nature de leur enveloppe. Comme les dimensions de ce tableau sont considérables à cause de la multiplicité des détails dont il est chargé, nous ne pouvons en donner ici la reproduction même partielle. [a footnote added: Ce tableau se trouve à Paris, chez Gauthier-Villars.][92]

91. Maarten Bullynck, "Johann Heinrich Lambert's Scientific Tool Kit, Exemplified by His Measurement of Humidity, 1769–1772," *Science in Context* 23, no. 1 (2010): 68; Johann Heinrich Lambert, "Essai d'hygrométrie ou Sur la mesure de l'humidité," *Histoire de l'Académie Royale des Sciences et Belles Lettres* 25 (1771): 70-72.

92. Marey, *La Méthode graphique dans les sciences expérimentales et particulièrement en physiologie et en*



(a) Lambert



(b) Gay-Lussac

Figure 4.29
Curves of magnetic variation over the year. Source: (a): Johann Heinrich Lambert, *Beyträge zum Gebrauche der Mathematik und deren Anwendung* (Berlin: Verl. des Buchladens der Realschule, 1765); (b): © Archives de l'École polytechnique (Palaiseau, France), IX GL 3.M.

Regnault's plate was substituted for Gay-Lussac's curves of solubility because "le principe sur lequel ces tableaux graphiques sont construits et la clarté des relations qu'ils expriment ressortent suffisamment de la figure".[93] In some cases, one of the most famous representations can also be the least visible and least re-presentable.

médecine, 48.
93. Marey, *La Méthode graphique dans les sciences expérimentales et particulièrement en physiologie et en médecine*, 48.

· 5 ·

States of Matter

1 ANDREWS' ISOTHERMS

In 1869 Thomas Andrews (1813-1885, FRS 1849) – the professor of chemistry at Queen's College in Belfast – presented a paper to be read as the Bakerian Lecture at the Royal Society titled "On the Continuity of the Gaseous and Liquid States of Matter".[1] The paper put forward the novel claim that "the gaseous and liquid states are only distant stages of the same condition of matter, and are capable of passing into one another by a process of continuous change."[2] Andrews studied the variation of the volume with the change of pressure for various temperatures. While similar experiments had been carried out before by Cagniard de la Tour (1822), Michael Faraday (1826), or Victor Regnault (1847), Andrews' experiment had an advantage because he used carbonic acid which could be liquefied within an accessible range of temperatures and pressures. Also, while previous experimenters used metal containers and only deduced the state of the fluid from the steepness of the variation of volume with pressure, Andrews could actually observe the state of the fluid inside his glass tubes. At the pressure point where one expected liquefaction to start, something unexpected happened: instead of seeing the carbonic acid separating into two clearly demarcated states, "the most careful examination fails to discover any heterogeneity".[3] Because above 30.9°C the liquid and gaseous states were visually indistinguishable, Andrews called this temperature the *critical point*. What was the state of the fluid above the critical point? Was it a gas or a liquid? Andrews argued that "we

1. It seems that contrary to common belief, the paper was not delivered by Andrews' himself who was still in Belfast when the paper was read – see J. S. Rowlinson, "The Work of Thomas Andrews and James Thomson on the Liquefaction of Gases," *Notes and Records of the Royal Society of London* 57, no. 2 (2003): 145-146.
2. Thomas Andrews, "The Bakerian Lecture: On the Continuity of the Gaseous and Liquid States of Matter," *Philosophical Transactions of the Royal Society of London* 159 (1869): 589.
3. Ibid., 584.

have no valid grounds of assigning it to the one form of matter any more than to the other".[4] Beyond the critical temperature, the fluid was not in a new state of matter but rather in "the intermediate states which matter assumes in passing, without sudden change of volume, or abrupt evolution of heat, from the ordinary liquid to the ordinary gaseous state".[5] Andrews' experiment showed not only that there was no liquefaction above the critical temperature, but also that one could "continuously" move from the gaseous to the liquid state. If one were to start in a gas state at some high temperature, and increase the pressure to 150 atmospheres and then decrease the temperature below 30.9°C, the fluid would end in a liquid state without any abrupt changes of its state, such as liquefaction:

> during the whole of this operation no breach of continuity has occurred. [...] The closest observation fails to discover anywhere indications of a change of condition in the carbonic acid, or evidence, at any period of the process, of part of it being in one physical state and part in another.[6]

While the evidence for his novel claims was based on the visual observations and table of measurements, Andrews also constructed a plot of the experimental results (Fig. 5.1).[7] Though Andrews did not directly connect it to any of his arguments, the plot provided a satisfactory illustration because the meaning of the curves was encoded in their shape: the tilted lines corresponded to the gaseous state (to make this more readily visible, the curves of a perfect gas obeying Mariotte's law were also added in the upper-right corner of the plot); the vertical lines represented the "fall from the gaseous to the liquid state" (i.e. the liquefaction process); the almost horizontal lines corresponded to the liquid state. The changing shape of the intermediate curves displayed the process through which the fluid transitioned from the liquid state into the gaseous state. The "continous" change between the gaseous and liquid states above 30.9°C was associated with the shape of the curves: "[t]he graphical representation of these experiments [the curves above 30.9°C], as shown in the preceding page, exhibits some

4. Andrews, "The Bakerian Lecture," 588.
5. Ibid.
6. ibid., 587.
7. The original orientation of Andrews' diagram was later rotated by 90 degrees in Maxwell's *Theory of Heat* (1871), see Fig. 5.8.

marked differences from the curves for lower temperatures" because the curves above the critical temperature did not exhibit a fall as "abrupt as in the case of the formation of the liquid at lower temperatures".[8]

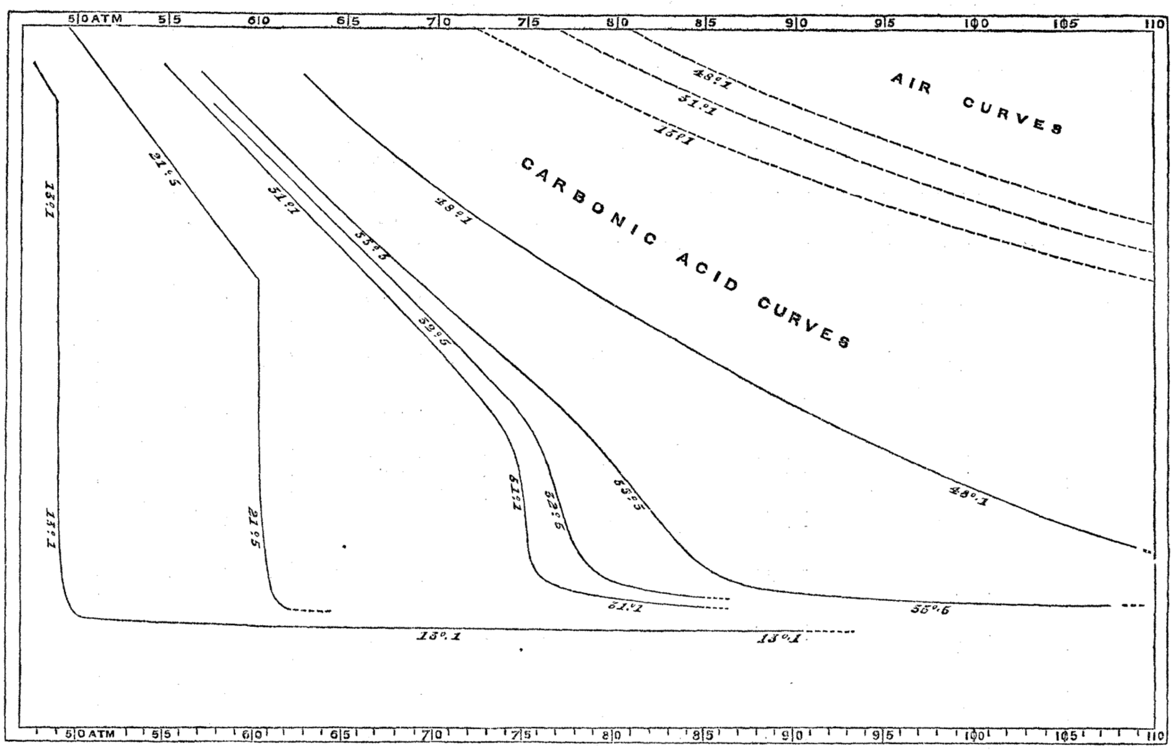


Figure 5.1
"The graphical representation of these experiments [the curves above 30.9°C], as shown in the preceding page, exhibits some marked differences from the curves for lower temperatures." The curves above the critical temperature did not exhibit a fall "as abrupt as in the case of the formation of the liquid at lower temperatures". This graphical difference was correlated with Andrews' discovery that above the critical temperature the liquid and gaseous states were physically indistinguishable. Source: Thomas Andrews, "The Bakerian Lecture: On the Continuity of the Gaseous and Liquid States of Matter," *Philosophical Transactions of the Royal Society of London* 159 (1869): 575–590.

Very soon, Andrews' experimental setup and experimental results attracted the attention of his peers. His results were noticed by theoretically inclined physicists such as Clerk Maxwell, J. W. Gibbs and J. D. van der Waals, while his experiments were continued in England by W. Ramsay and S. Young (first at the University College in Bristol, and later at the University College London), in France by L. P. Cailletet (École Normale Supérieure) and E. H. Amagat

8. Andrews, "The Bakerian Lecture," 584-5.

(Faculté Libre des Sciences in Lyons), in Switzerland by R. Pictet (University of Geneva), in the Netherlands by H. Kamerlingh Onnes (University of Leiden), and in Poland by S. Wroblewski and K. Olszewski (University of Krakow).[9] The extension of Andrews' observations to other gases provided an incentive for high-pressure and low-temperature experiments which culminated in a race for the liquefaction of the permanent gases: oxygen in 1877 by Cailletet and Pictet (independently), nitrogen in 1883 by Wroblewski and Olszewski, methane in 1886 by Olszewski, hydrogen in 1898 by J. Dewar, and helium in 1908 by H. Kamerlingh Onnes. While the interest in the liquefaction of various gases predated Andrews, his work made clear the relation between pressure and temperature (and it showed why certain gases could not be liquefied even at very high pressures if the temperature was above the critical point) and explained previous failures to liquefy the permanent gases. Along with Andrews' ideas and apparatus, his graphical representation of the isotherms also spread and soon became ubiquitous in both theoretical and experimental physics.

While the success of Andrews' experiment and its special position at the intersection of experimental and theoretical concerns could explain the wide diffusion of the curves, their origin is not immediately obvious. In the mid-19th century graphical representations of experimental data were present in scientific publications, most consistently in those of German scientists. In Britain, some of the most esteemed experimentalists of the first half of the 19th century, such as Michael Faraday or James Joule, never produced experimental plots. Andrews, who kept a close correspondence with other important experimentalists like Faraday or Jean-Baptiste Dumas, was closer to this rule rather than an exception – the only plots he

9. Kostas Gavroglu, "The Reaction of the British Physicists and Chemists to van Der Waals' Early Work and to the Law of Corresponding States," *Historical Studies in the Physical and Biological Sciences* 20, no. 2 (1990): 199–237; Kostas Gavroglu and Yorgos Goudaroulis, "Heike Kamerlingh Onnes' Researches at Leiden and Their Methodological Implications," *Studies in History and Philosophy of Science Part A* 19, no. 2 (1988): 243–274; Rowlinson, "The Work of Thomas Andrews and James Thomson on the Liquefaction of Gases"; Faidra Papanelopoulou, "Louis Paul Cailletet: The Liquefaction of Oxygen and the Emergence of Low-Temperature Research," *Notes and Records of the Royal Society of London* 67, no. 4 (2013): 355–373; Dirk van Delft, *Freezing Physics : Heike Kamerlingh Onnes and the Quest for Cold*, in collab. with Koninklijke Nederlandse Akademie van Wetenschappen (Amsterdam: Koninklijke Nederlandse Akademie van Wetenschappen, 2007).

ever published were the isotherms for carbonic acid. The choice of coordinates (pressure on the abscissa and volume on the ordinates) was also surprising because it differed from the conventional choices. By far the most famous graphical representations of experimental results were the curves produced by Victor Regnault (1847) who used the temperature on the abscissa and pressure on the ordinates.[10] This would have been a natural choice for an experimentalist because in most experiments temperature was the independent quantity that was controlled and whose variation produced an effect in the volume or pressure. In the study of steam engines, the common representation was the indicator or volume-pressure diagram – with volume on the abscissa and pressure on the ordinates. However, Andrews' original diagram had no direct connection to this type of representation; only later, the diagram was rotated by 90° to match the orientation of the axis in the indicator diagram.

The source of Andrews' isotherms was, most probably, James Thomson (1822-1892), William Thomson's elder brother. James was the professor of civil engineering at Queen's College in Belfast and a close friend of Andrews from whom he learned about his experiments. In May and June 1862 (long before Andrews presented his results to the Royal Society in 1869), Thomson wrote several notes interpreting the implications of Andrews' results accompanied by what he called "sketches of curves" (Fig. 5.2). While Andrews' curves represented the variation of pressure with volume for different temperatures, a natural choice given the fact that he had over twenty volume-pressure points for six different temperatures, Thomson chose to "sketch" the relation of temperature and volume for different pressures, a surprising choice because each curve was extrapolated from a couple of temperature points (see Fig. 5.2). However, opposed to Andrews, Thomson's sketches were not simply reproducing the experimental results, but were meant to be a guide for future experiments: "The sketching of probable features or approximate forms of these curves may serve useful purposes in indicating desirable courses for experimental investigation."[11] While Andrews' experiments showed that above a

10. For Regnault's curves see Chapter 2.
11. James Thomson, *Collected Papers in Physics and Engineering* (Cambridge: Cambridge University Press, 1912),

certain "*critical*" temperature the carbonic acid fluid did not show any sudden transition from liquid to gas, Thomson used his sketch to extend this claim and introduce the analogue concept of critical pressure:

> Now it seems clear that we have a similar superior limit of pressure, so that for pressures above that limit we meet with no discontinuity in the possible volume or in the possible quantity of heat.[12]

While Andrews identified the critical temperature by direct observation of the fluid inside the glass tubes, Thomson derived the existence of a critical pressure from the behavior of the curves. The "continuous" transition between gaseous and liquid states which Andrews observed in his glass tubes was interpreted by Thomson *graphically*: the curves below the critical pressure displayed a "discontinuity", i.e. they were not smooth as the curves above the critical pressure.

Thomson went beyond observing the shape or smoothness of the experimental curves to propose a series of hypothetical or theoretical curves. While their existence was to be decided experimentally, their shape and properties were not arbitrary but were based on a form of *graphical reasoning*. Thomson imagined the change in the pressure curves as a rotation of the tangent drawn through the inflexion point of the curves. If one continued rotating the tangent beyond its vertical position (corresponding to the curve p_4 in Fig. 5.3), then the shape of the curves would have changed while still remaining smooth (see the dotted curves in Fig. 5.3). Thomson considered that "it would seem probable that the angle would go on increasing beyond 90° in which case the curve would stand thus: – [see Fig. 5.4]".[13] A second notion of continuity was at play. Thomson postulated the theoretical "continuity" (i.e. smoothness) of curves below the critical pressure (the dotted portion of p_1, p_2, p_3) by arguing that it was probable that the slope of the tangent at the inflexion point could be increased continuously.

319-320.

12. Thomson, *Collected Papers in Physics and Engineering*, 321.
13. Ibid., 324.

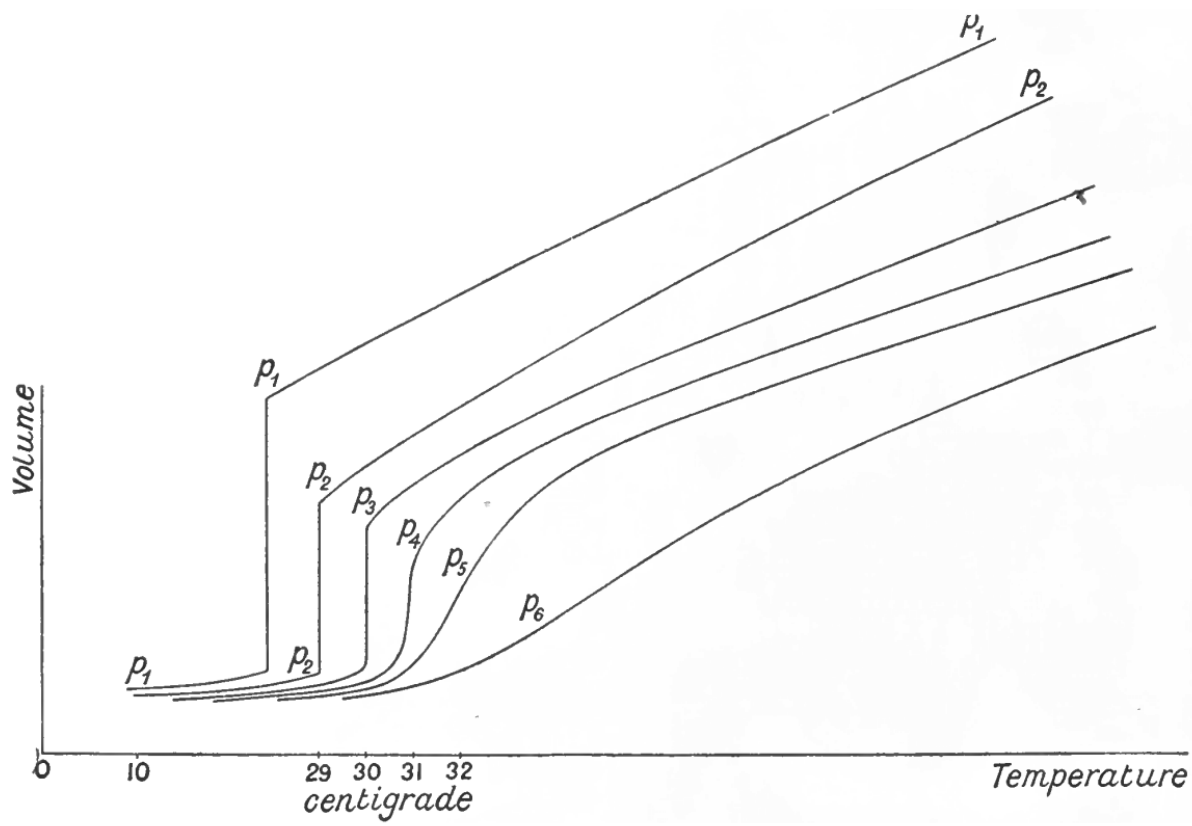


Figure 5.2
This sketch from James Thomson's notebooks was meant to show "the relation between the wave in p_4, p_5, p_6 as compared with the abrupt rise in p_1, p_2, p_3"; the curves p_1, p_2, p_3 show a "discontinuity" (i.e. the curves are not smooth). Source: James Thomson, *Collected Papers in Physics and Engineering* (Cambridge: Cambridge University Press, 1912), 320.

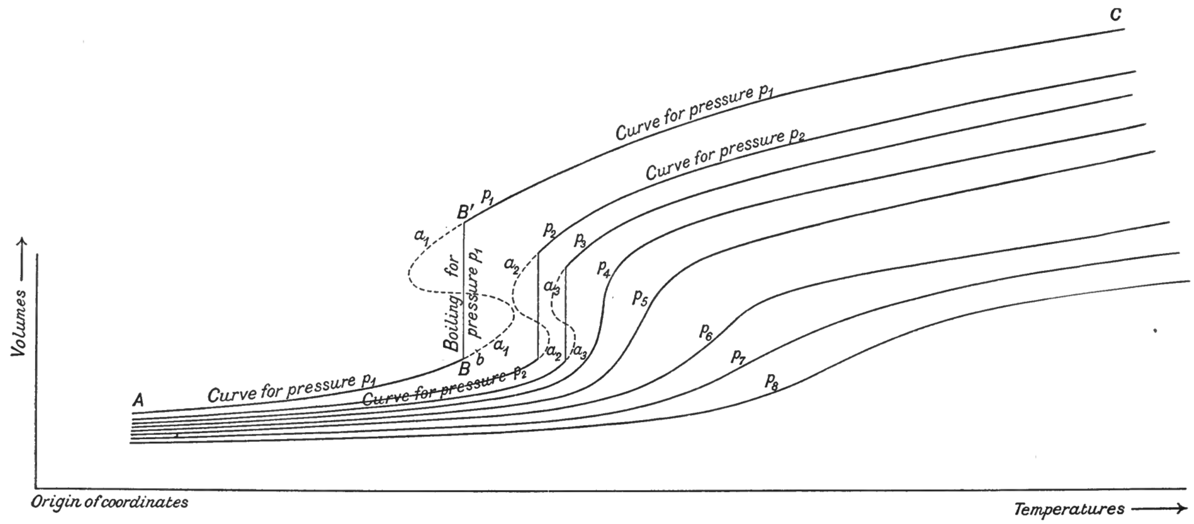


Figure 5.3
The dotted curves represented Thomson's theoretical curves which extended the continuous transition between gaseous and liquid states below the critical point. The existence of this continuity (which had not been observed experimentally by Andrews) was inferred by Thomson from a *graphical* argument: Thomson imagined the morphing of the curves p_8, p_7, p_6, p_5, p_4 by following the rotation of the tangent at the inflexion point of the curves. For the critical pressure p_4 the tangent would have been vertical. If the curves continued morphing beyond this point following the same rule, they would have looked as the dotted curves drawn by Thomson (see Fig. 5.4). Source: James Thomson, *Collected Papers in Physics and Engineering* (Cambridge: Cambridge University Press, 1912), 322.

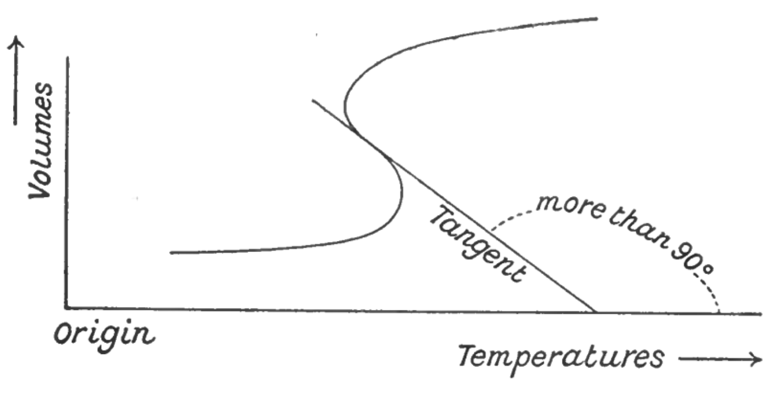
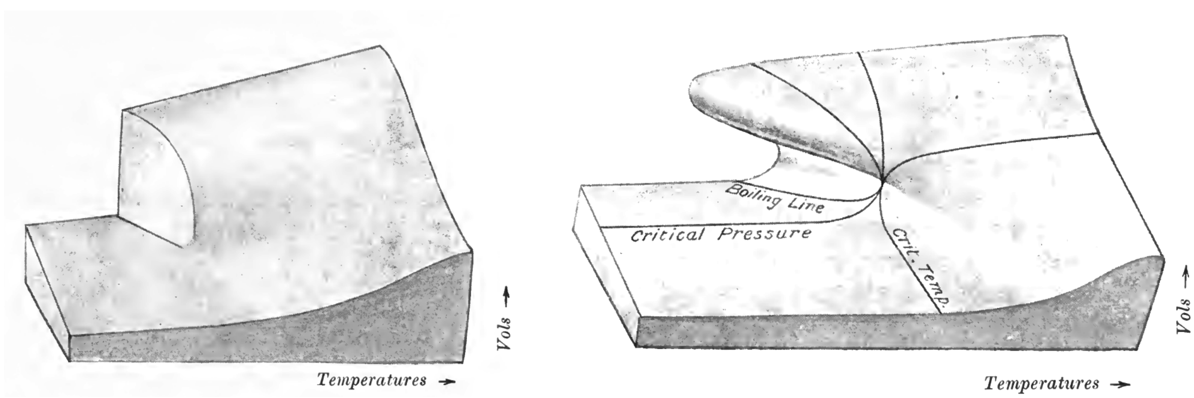


Figure 5.4
Thomson imagined the change in the pressure curves as a rotation of the tangent drawn through the inflexion point of the curves. If one continued rotating the tangent beyond its vertical position (corresponding to the curve p_4 in Fig. 5.3), then the shape of the curves would have changed as depicted in this diagram. Source: James Thomson, *Collected Papers in Physics and Engineering* (Cambridge: Cambridge University Press, 1912), 324.

Because Thomson believed that the continuous transition found by Andrews manifested itself not only above a critical temperature, but also above a critical pressure, he decided to carve out in wood a curved surface to better exhibit "the relation between liquids and their gases, or rather the various conditions of pressure, temperature, volume, and quantity of heat in which a fluid can exist" (see Fig. 5.5).[14] Such a surface would not only represent experimental results, but as in the case of the sketched curves it could also guide future experiments:

> It is only by experiment that the exact form of the curved surface can be found; but some of the facts of the case being already certainly known it is possible already to carve or mould some of the chief features of the curve surface, and these may serve to aid in showing the courses along which further experimental researches might best be directed, and also to aid in understanding the correlation of experimental results; and to aid in forming opinions in advance of experiments as to what is likely to result in intermediate cases between various experimental results which may already or at any time be arrived at.[15]



(a) "Model cut out between June 6 and June 9, 1862" **(b)** "Cut 9th May 1869"

Figure 5.5
The axes for these models were temperature, volume and pressure (pointing out of the paper). Because these variables could not be varied independently, it was only the surface of the model that described the physical states of the fluid. Thomson made other models. Source: James Thomson, *Collected Papers in Physics and Engineering* (Cambridge: Cambridge University Press, 1912), 277.

Despite the originality and importance of these ideas, Thomson refrained from publishing his notes until Andrews first published his whole experimental account. He only incidentally mentioned his ideas in a letter to his brother William from 1862:

14. Thomson, *Collected Papers in Physics and Engineering*, 321.
15. Ibid.

> ...I thought there may be a continuous set of states between the steam and the water, for constant pressure as shown by the above curve [Fig. 5.6]: the dotted part representing possible but unstable states of the fluid in respect to the quantity of heat in a given mass of it. [...]
>
> I may say that nothing in this letter nor in what William Bottomley told you is a disclosure of what Dr. Andrews has found out by his experiments. He has shown me his experiments but his view of their indications or their meaning has been usually essentially different from mine: – but whatever has occurred to me does not belong to myself and must be strictly suppressed until Dr Andrews publishes his experiments and the views which he will finally decide on adopting as to them.[16]

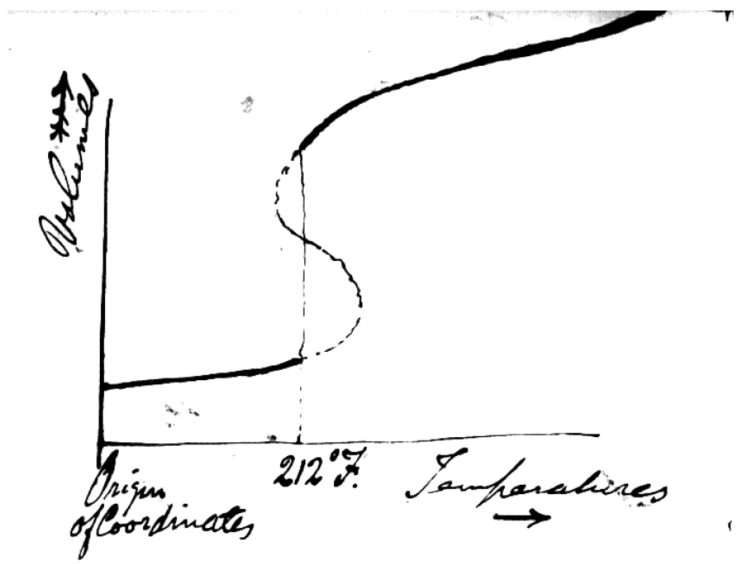


Figure 5.6
Thomson's theoretical curve. Source: James Thomson to William Thomson, 30 June 1862, T471, Add. MS 7342, Kelvin Papers (microfilm edition), Cambridge University Library.

Soon after Andrews' Bakerian Lecture, Thomson also started advertising his ideas in front of the Royal Society and the British Association for the Advancement of Science. The premise of his argument was that although there is a "practical breach of continuity" when a liquid is boiled or vapors are liquefied, "there may exist, in the nature of things, a theoretical continuity across this breach having some real and true significance".[17] To support the hypothesis of a "theoretical continuity", Thomson added a plot with the same features as his 1862 "sketches of

16. James Thomson to William Thomson, 30 June 1862, T471, Add. MS 7342, Kelvin Papers (microfilm edition), Cambridge University Library.

17. James Thomson, "Considerations on the Abrupt Change at Boiling or Condensing in Reference to the Continuity of the Fluid State of Matter," *Proceedings of the Royal Society of London* 20 (1871): 2.

curves", but superimposed on Andrews' experimental curves (Fig. 5.7).

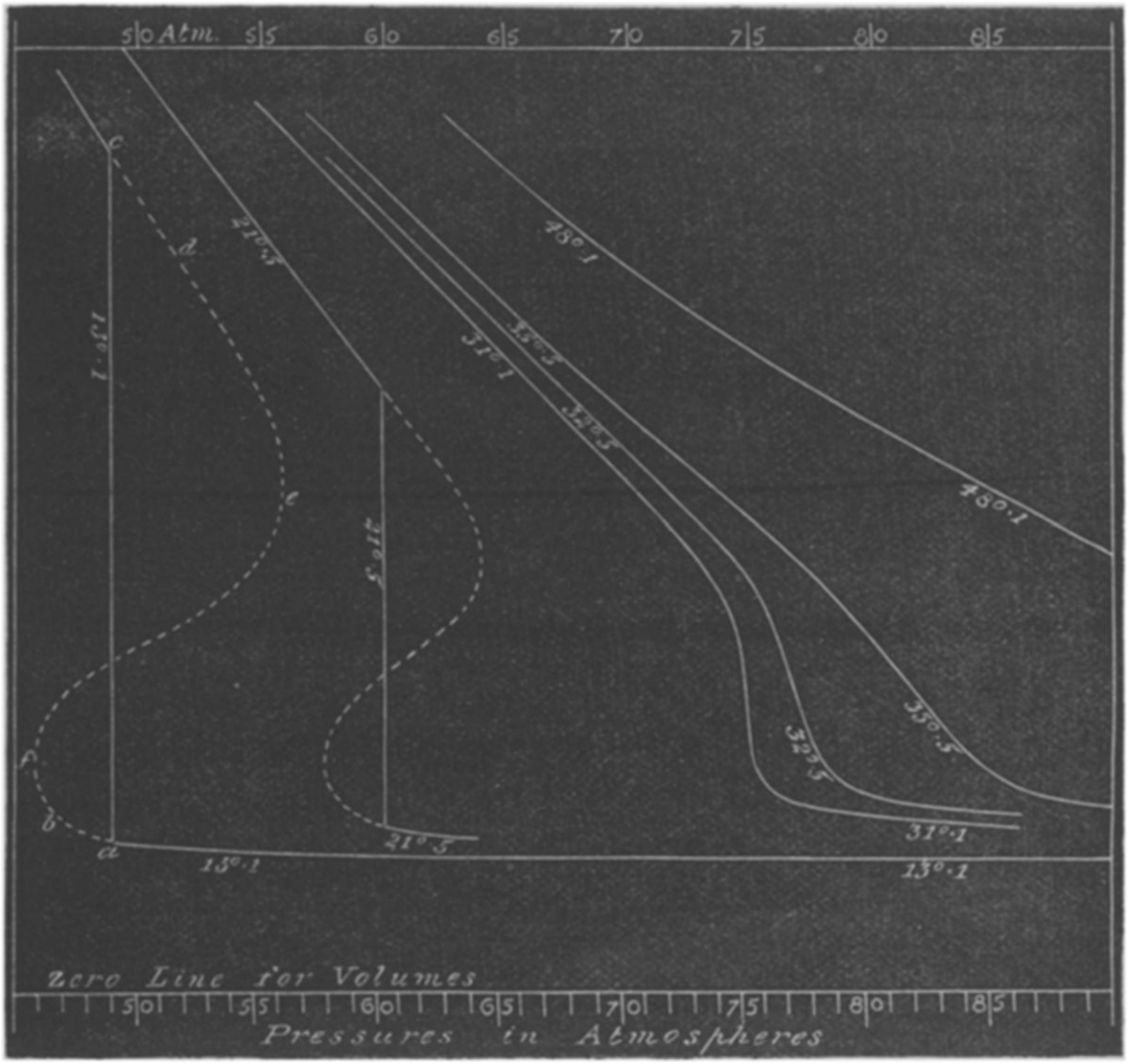


Figure 5.7
Thomson re-used the digram previously published by Andrews (see Fig. 5.1), to which he added the dotted curves corresponding to the postulated continuous but unstable transition from a gaseous to a liquid state. In his notes from 1862, Thomson only used a volume-temperature diagram. Source: James Thomson, "Considerations on the Abrupt Change at Boiling or Condensing in Reference to the Continuity of the Fluid State of Matter," *Proceedings of the Royal Society of London* 20 (1871): 4.

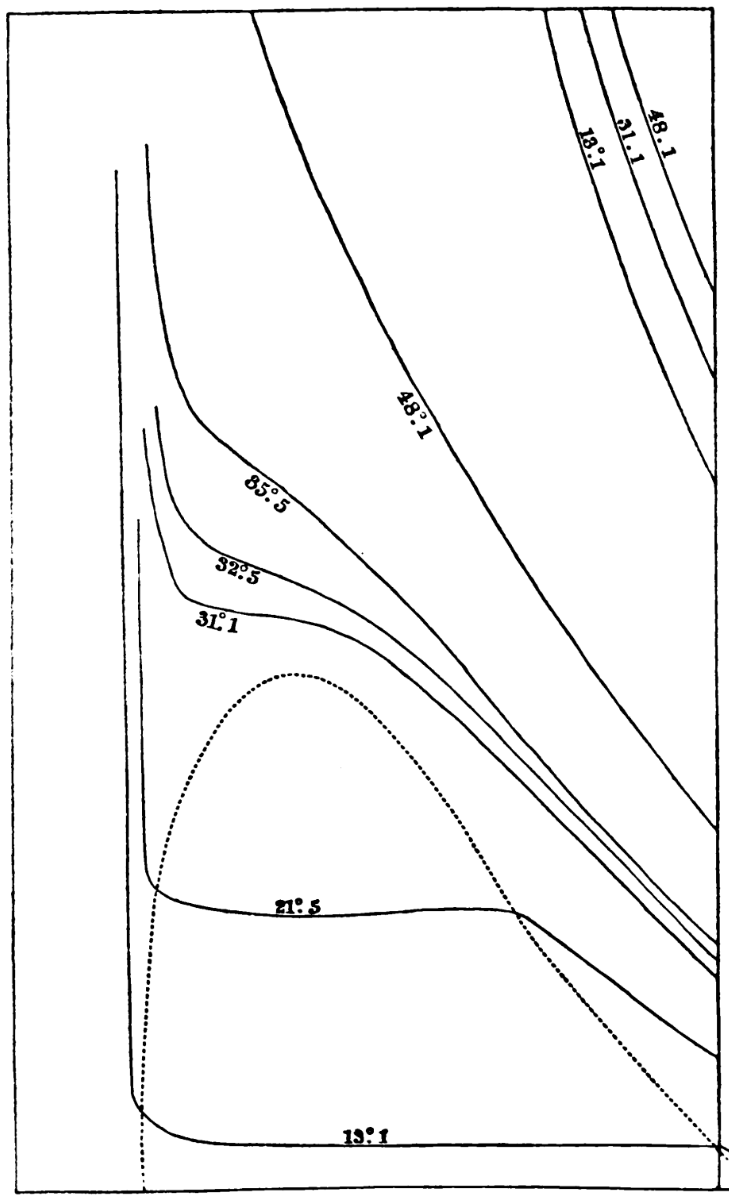
Andrews' experimental results and Thomson's interpretation immediately attracted James Clerk Maxwell's attention. Maxwell was just in the midst of publishing his *Theory of Heat*

(1871) and he took a keen interest in both Andrews and Thomson.[18] After talking directly with Thomson at Glasgow and attending Andrews' lecture at the Royal Institution, Maxwell added an account of their contributions in the *Theory of Heat*.[19] While Maxwell remained faithful to their original ideas, there were small changes in his presentation. Because other physicists like Gibbs or van der Waals learned about Andrews and Thomson from Maxwell's textbook, it is worth paying close attention to Maxwell's presentation of the graphical arguments.

First, Maxwell rotated the diagram used by Andrews and Thomson such that the axes were consistent with the indicator diagrams he employed throughout the textbook (Fig. 5.8). In Maxwell's hands, Andrews' isotherms were not just a graphical representation of experimental results, but were experimental results expressed in a graphical language. The *Theory of Heat* provided a gradual introduction in the use of indicator diagrams (which amounted to 16 out of the 41 drawings in the book), "as a means of explaining and representing to the eye the working of a fluid".[20] For Maxwell, the indicator diagram was the product of a practical method developed by James Watt to trace "every part of the action of the steam", of a geometrical method introduced by Émile Clapeyron to analyze the working of a Carnot cycle, and of a pedagogical method developed by William Rankine in his textbook on steam-engines.[21] This placed the indicator diagram at the intersection of practice, experiment, theory and training.

A second intervention of Maxwell was to add a dotted line on top of Andrews' diagram "showing the region within which the substance can exist as a liquid in the presence of its vapour".[22] This small alteration increased the clarity of the diagram as it offered a geometrical interpretation of the critical point which had not been represented in the original version. The dotted line also imposed a different perception of the diagram which could be seen not only as

18. James Clerk Maxwell, *The Scientific Letters and Papers of James Clerk Maxwell*, ed. P. M. Harman, 3 vols. (Cambridge: Cambridge University Press, 1990), v. 2, 668-669, No. 381.
19. Ibid., v.2 668, No. 381.
20. James Clerk Maxwell, *Theory of Heat*, 1st ed. (London: Longmans, Green and Company, 1871), 102.
21. Ibid.
22. Ibid., 119.



Isothermals of Carbonic Acid.

Figure 5.8
Maxwell's reproduction of Andrews' diagram (see Fig. 5.1), with slight modifications. In the *Theory of Heat* (1871) Maxwell consistently employed indicator-diagrams with the volume on the abscissa and the pressure on the ordinates. Besides flipping the diagram, Maxwell also added the dotted line which showed the region in which the substance could exist as a liquid in the presence of its vapor. Maxwell described how "we can convert carbonic acid gas into liquid without any sudden change of state"; the only condition was that the curve of the process would not cross into the region between the dotted curves. One could start at the bottom-right side at low pressure and temperature, increase the temperature and move to the upper-right side, then increase the pressure and move to the upper-left side, and then decrease the temperature and move to the lower-left side. While "during this process no sudden change of state can be observed, but carbonic acid at 50°F and under a pressure of 100 atmospheres has all the properties of a liquid." Source: James Clerk Maxwell, *Theory of Heat*, 1st ed. (London: Longmans, Green and Company, 1871), 119-124.

the space in which experimental curves were drawn, but also as a space (or region) on which the physical states of the substance were mapped.[23]

Besides the diagram with Andrews' experimental curves, Maxwell added a second drawing with Thomson's hypothetical curves (Fig. 5.9). Though the differences in presentation were minor, they remain suggestive of the direction in which Maxwell was moving. While Thomson used his hypothetical curve only "to suggest desirable courses for experimental researches", Maxwell aimed to understand its geometrical properties. For this purpose, he drew the lines BF and DH which had no physical meaning, but which bounded the region in which any horizontal line cut Thomson's curve in three different points. Maxwell deduced that:

> The literal interpretation of this geometrical circumstance would be that the fluid at this pressure, and at the temperature of the isothermal line, is capable of existing in three different states. One of these, indicated by C, evidently corresponds to the liquid state. Another, indicated by G, corresponds to the gaseous state. At the intermediate point E the slope of the curve indicates that the volume and the pressure increase and diminish together. No substance having this property can exit in stable equilibrium, for the very slightest disturbance would make it rush into the liquid or the gaseous state. We may therefore confine our attention to the points C and G.[24]

The few pages about Thomson and Andrews that Maxwell wrote in the *Theory of Heat* (1871) played an important role in the doctoral dissertation of the young Dutch physicist J. D. van der Waals (1837-1923). The title of van der Waals' thesis, "On the continuity of the gaseous and liquid states" [Over de continuiteit van den gas- en vloeistoftoestand] (1873) was almost identical to that of Andrews' Bakerian lecture from 1869. Both Andrews' and Thomson's ideas about the continuity of states were present in van der Waals' understanding of the concept:

> The expression, "continuity of the gaseous and liquid state", is perhaps the most suitable, because the considerations are based on the idea that we can proceed continuously from one state of aggregation to the other; geometrically expressed, both portions of the isotherm belong to *one* curve, even in the case in which these portions are connected by a part which cannot be realized.[25]

23. For another example of a diagram that employed not only curves, but also regions of space see Maxwell, *Theory of Heat*, 137.
24. Ibid., 125
25. J. D. van der Waals, *On the Continuity of the Gaseous and Liquid States*, ed. J. S. Rowlinson (Amsterdam ; New York: North-Holland, 1988), 125.

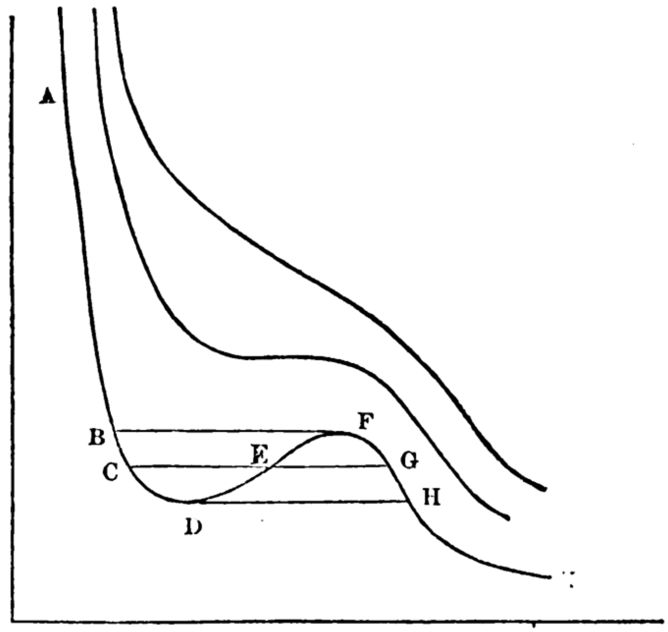


Figure 5.9
"...the isothermal line, as deduced from experiments of the ordinary kind, will consist of the curve ABC, the straight line CG, and the curve GH. But it has been pointed out by Prof. J. Thomson that by suitable contrivances we may detect the existence of other parts of the isothermal curve [the parts CD and FG]... The state of things, however, represented by the portion of the isothermal curve DEF, can never be realised in a homogeneous mass, for the substance is then in an essentially unstable condition, since the pressure increases with the volume. We cannot, therefore, expect any experimental evidence of the existence of this part of the curve, unless, as Prof. J. Thomson suggests, this state of things may exist in some part of the thin superficial stratum of transition from a liquid to its own gas, in which the phenomena of capillarity take place." Source: James Clerk Maxwell, *Theory of Heat*, 1st ed. (London: Longmans, Green and Company, 1871), 125-127.

Van der Waals always insisted that his concept of continuity had been developed independently, and was based on a physical reasoning about the molecular nature of the liquid and the gaseous states. Laplace's theory of capillarity led him "to establish the connexion between the gaseous and liquid condition, the existence of which, as I afterwards learned, had already been suspected by others".[26] Laplace's theory of capillarity led van der Waals to consider the

26. Waals, *J.D. van Der Waals*, 125. Much later, in his Nobel Prize lecture from 1910, van der Waals also attributed his concept of "continuity" to a physical reasoning about the molecular nature of the liquid and gaseous states: "Clausius' treatise was a revelation for me although it occurred to me at the same time that if a gas in the extremely dilute state, where the volume is so large that the molecules can be regarded as points, consists of small moving particles, this is obviously still so when the volume is reduced; indeed, such must still be the case down to the maximum compression and also in liquids, which can only be regarded as compressed gases at low temperature. Thus I conceived the idea that there is no essential difference between the gaseous and the liquid state of matter – that the factors which, apart from the motion of the molecules, act to determine the pressure must be regarded as quantitatively different when the density changes and perhaps also when the temperature changes, but that they must be the very factors which exercise their influence throughout. And so the idea of

effects of intermolecular forces, while Clausius' virial theorem suggested a method to him through which he was able to derive an equation of state:[27]

$$\left(p + \frac{a}{v^2}\right)(v - b) = (1 + a)(1 - b)(1 + \alpha t) \tag{5.1}$$

While this equation was compatible with the behavior of gases and under the appropriate limits it reduced to the equation of the ideal gas, "some hesitation may be experienced in extending it to liquids, because by doing so we implicitly ignore the difference between the two states".[28] To prove that the equation could equally well represent both states, van der Waals appealed to Andrews' experimental results: "the experiments of Andrews and the values of b calculated from them show that doubts as to the legitimacy of the extended method may be safely abandoned".[29] Using the equation of state, he calculated the critical temperature to be 32.5°C (compared to Andrews' experimental result of 30.9°C), which he considered to be "very satisfactory" given the errors in estimating a and b.[30]

The evidence supporting van der Waals' equation was not only numerical but also graphical. The Eq. (5.1) reduced to a cubic equation for v, which implied that "*at a given pressure and temperature there are either three volumes possible or else only one*".[31] Van der Waals traced the curve of Eq. (5.1) for the temperature of $13.1°$C (see van der Waal's Fig.6 reproduced in Fig. 5.10). While this curve did not match Andrews' isotherms, it did correspond to Thomsons' curve "which is quite analogous to that deduced from my equation. In order to show this agreement graphically I have taken from Maxwell the curves in figs. 7 and 8, and contrasted them with fig.6, which gives my isothermal".[32] The *graphical* agreement that van der

continuity occurred to me", in *Nobel Lectures, Physics 1901-1921* (Amsterdam: Elsevier, 1964), 254-255.

27. M.J. Klein, "The Historical Origins of the Van Der Waals Equation," *Physica* 73, no. 1 (1974): 34.
28. Waals, *J.D. van Der Waals*, 194.
29. Ibid.
30. Ibid., 202.
31. Ibid., 195.
32. Ibid., 196.

Waals referred to was quite unusual because he did not compare directly his equation with the experimental results by plotting them on the same diagram. Instead, the agreement was based on the features of the diagram. To make the visual resemblance clear, van der Waals employed the same lines and points as Maxwell did. However, the status of the curves had changed. Van der Waals considered his equation of state to be a "universal physical law" and, as such, he rejected Maxwell's line CG as an alternative path – "The idea of joining C and G by a straight line, as is done by Maxwell, is not a happy one, but is calculated to throw one off the right track."[33]

Maxwell was one of the first to salute van der Waals' original ideas, though he found fault with the use of Clausius' virial theorem:

> The results obtained by M. Van der Waals by a comparison of this equation with the determinations of Regnault and Andrews are very striking, and would almost persuade us that the equation represents the true state of the case. But though this agreement would be strong evidence in favour of the accuracy of an empirical formula devised to represent the experimental results, the equation of M. Van der Waals, professing as it does to be derived from the dynamical theory, must be subjected to a much more severe criticism.
> It appears to me that the equation does not agree with the theorem of Clausius on which it is founded.[34]

Most probably prompted by van der Waals' remark about the line CG, Maxwell tried to find a physical connection between Thomson's and Andrews' curves.[35] He supposed that the medium passed from C to G along the path $CDEFG$, and returned back to C along the line GC; since "the temperature has been constant throughout, no heat can have been transformed into work". The heat transformed into work was represented by the areas CDE and EFG – "Hence the condition which determines the maximum pressure of the vapour at given temperature is that the line CG cuts off equal areas from the curve above and below".[36]

33. Waals, *J.D. van Der Waals*, 196.
34. James Clerk Maxwell, "Van Der Waals on the Continuity of the Gaseous and Liquid States," *Nature* 10 (1874): 480.
35. Have another look at the letter 674; 604
36. James Clerk Maxwell, *The Scientific Papers of James Clerk Maxwell*, ed. W. D. Niven, 2 vols. (Cambridge:

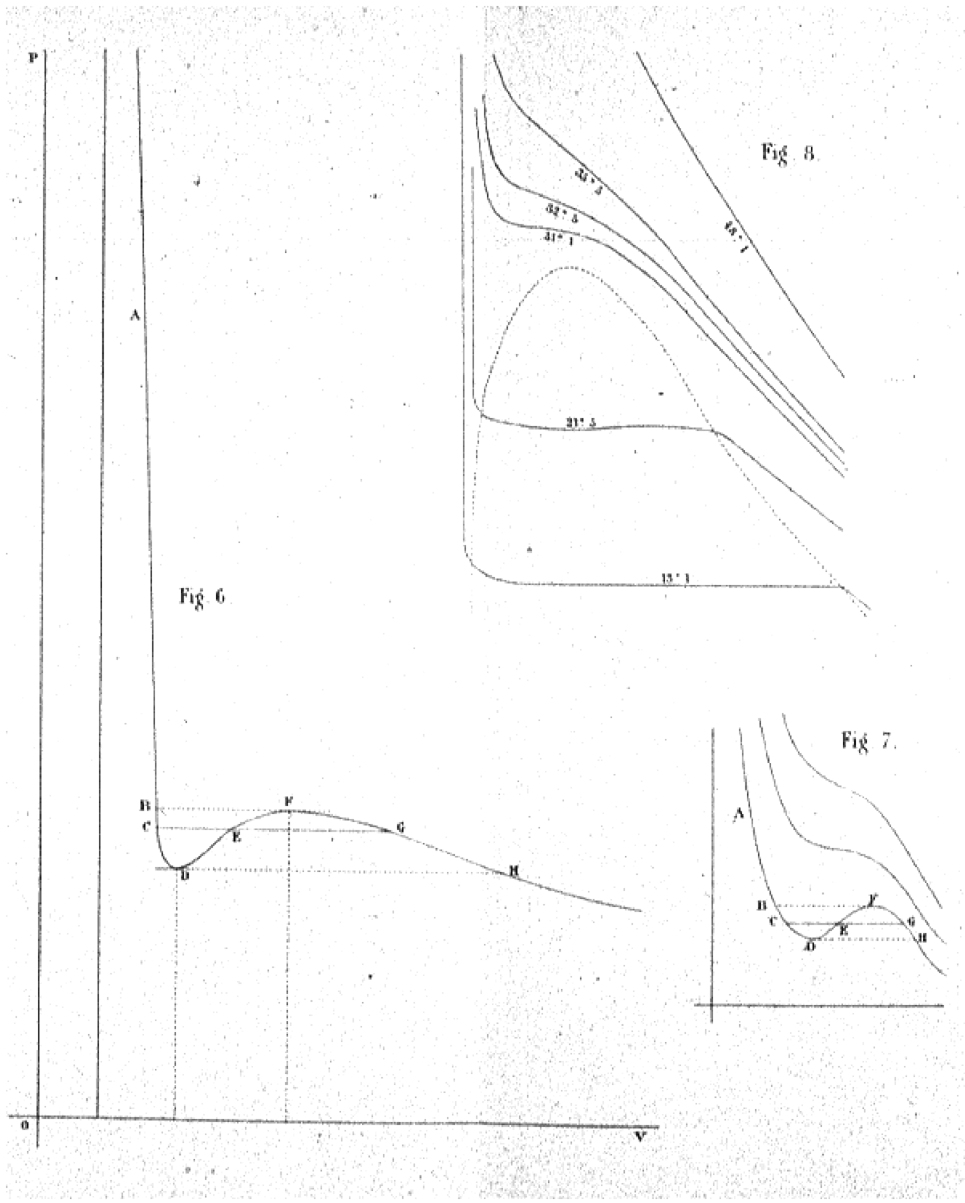


Figure 5.10
"Though, so far as I know, the theoretical proof of the existence of the third volume condition has not been hitherto given, it has been conjectured by James Thomson, who conceived the happy idea of constructing the part of the isothermal belonging to volumes beyond the reach of experiment by means of the part of the isothermal given by experiment; and the curve thus resulting is quite analogous to that deduced from my equation. In order to show this agreement graphically I have taken from Maxwell the curves in figs. 7 and 8 [for Maxwell's curves see Fig. 5.8, Fig. 5.9], and contrasted them with fig. 6, which gives my isothermal. [...] The idea of joining C and G by a straight line, as is done by Maxwell, is not a happy one, but is calculated to throw one off the right track."
Source: J. D. van der Waals, *On the Continuity of the Gaseous and Liquid States*, ed. J. S. Rowlinson (Amsterdam ; New York: North-Holland, 1988), 196.

2 THE TRIPLE POINT

While Maxwell was concerned with the geometrical properties of the isotherms, James Thomson focused on interpreting the physical meaning of the experimental curves. Between 1871 and 1873 he presented several papers at the meetings of the British Association and the Royal Society about the relations between the gaseous, liquid and solid states of matter. To represent these relations, he employed a different graphical construction – instead of Andrews' pressure-volume or his earlier volume-temperature diagrams, he now used temperature (on the vertical) and pressure (on the horizontal); the plane of the diagram represented all possible points of pressure and temperature, while the lines corresponded to transitions from gas to liquid, liquid to solid, and gas to solid states (see Fig. 5.11). The gas-solid line did not extend indefinitely for high pressures and temperatures, but ended in the *critical point* E introduced by Andrews. This allowed Thomson to rephrase in graphical terms Andrews' physical/experimental description of how one could reach a liquid state from a gas state through a continuous process:

> we may see that from any ordinary liquid state to any ordinary gaseous state the transition may be gradually effected by an infinite variety of courses passing round the extreme end of the boiling-line.[37]

But Thomson's attention was concentrated somewhere else. He made the suggestion that the three curves met in one point which he named the *triple point*. As in the case of Andrews' isotherms, Thomson proposed that the curves could be prolonged beyond the transition point – "the dotted lines TP and TQ, may have some theoretical or practical significance not yet fully discovered".[38] This meant that the gas-solid line TN and the liquid-gas line TL were not "continuous" at the triple point T (i.e. T was a singular point).

As in the case of Andrews' isotherms, Thomson's observations were not derived from the

Cambridge University Press, 1890), v.2 425, where BF in the original was changed to CG to match the notation in Fig. 5.9.

37. James Thomson, "Speculations on the Continuity of the Fluid State of Matter, and on Relations between the Gaseous, the Liquid, and the Solid States," *Report of the Forty-First Meeting of the British Association for the Advancement of Science*, 1871, 31.

38. Ibid., 32.

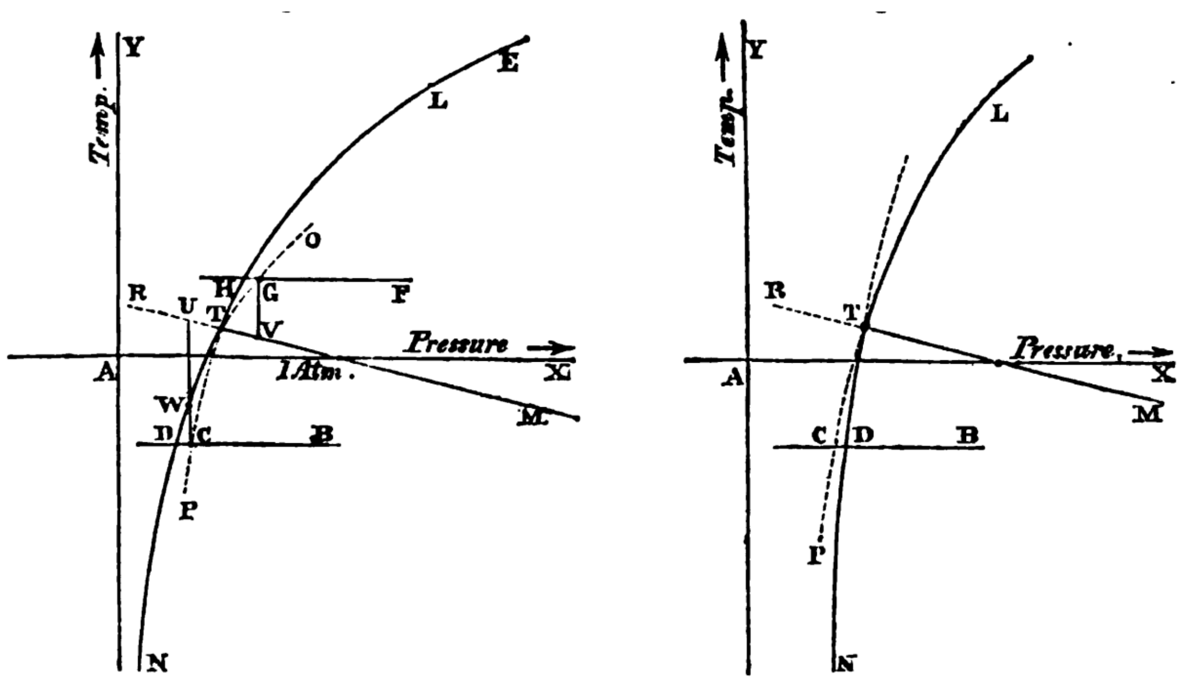


Figure 5.11
TN represents the line between gas and solid, TM the line between liquid and solid and TL the line between gas and liquid. E is the *critical point* first described by Andrews in 1869 beyond which the liquid and gaseous states were experimentally indistinguishable. T is the *triple point* introduced by Thomson. Opposed to previous views (e.g. Regnault), Thomson argued that the TL and TN lines were not "continuous" (i.e. smooth); he represented this by the dotted line TP as the continuation of TL, and the dotted line TQ as the continuation of TN. In his 1871 paper to the British Association, Thomson only considered the diagram on the left in which the gas-liquid and gas-solid lines crossed on the concave side. In 1872, Thomson presented an argument why the second case (the diagram on the right for which the gas-liquid and gas-solid lines crossed on the convex side) could be discounted as unphysical. Source: James Thomson, "On Relations between Gaseous, the Liquid, and the Solid States of Matter," *Report of the Forty-Second Meeting of the British Association for the Advancement of Science*, 1872, 24–30

actual experimental curves, but from the geometrical properties of his sketches – "the figure is intended only as a sketch to illustrate principles, and is not drawn according to measurements for any particular substance, though the main features of the curves shown in it are meant to relate in a general way to the substance of water, steam, and ice".[39] To support his observations, Thomson appealed to the most accurate and complete experimental results available, Victor Regnault's *Memoir* (1847), and "made careful examinations of his [Regnault's] engraved curve, and of his empirical formulae adapted to fit very closely to the results exhibited in that curve,

39. James Thomson, "A Quantitative Investigation of Certain Relations between the Gaseous, the Liquid, and the Solid States of Water-Substance," *Proceedings of the Royal Society of London* 22 (1873): 28.

and of his final Tables of results".[40] Despite his great care, Regnault admitted that he "could not avoid certain small irregularities in the curves"; he pointed out one unusual irregularity in particular because all the experimental determinations were above the curve for the portion between -16°C and 0°C (see Fig. 5.12).[41] It was this part of the curve that attracted Thomson's attention:

> The engraved curve drawn on the copper plate by Regnault himself is offered by him as the definitive expression of his experiments, as being an expression which satisfies as well as possible the aggregate of his observations – subject, however, to a very slight alteration, which he has pointed out as a requisite amendment in the part of the curve immediately below the freezing-point, a part with which the investigations in the present paper are specially concerned.[42]

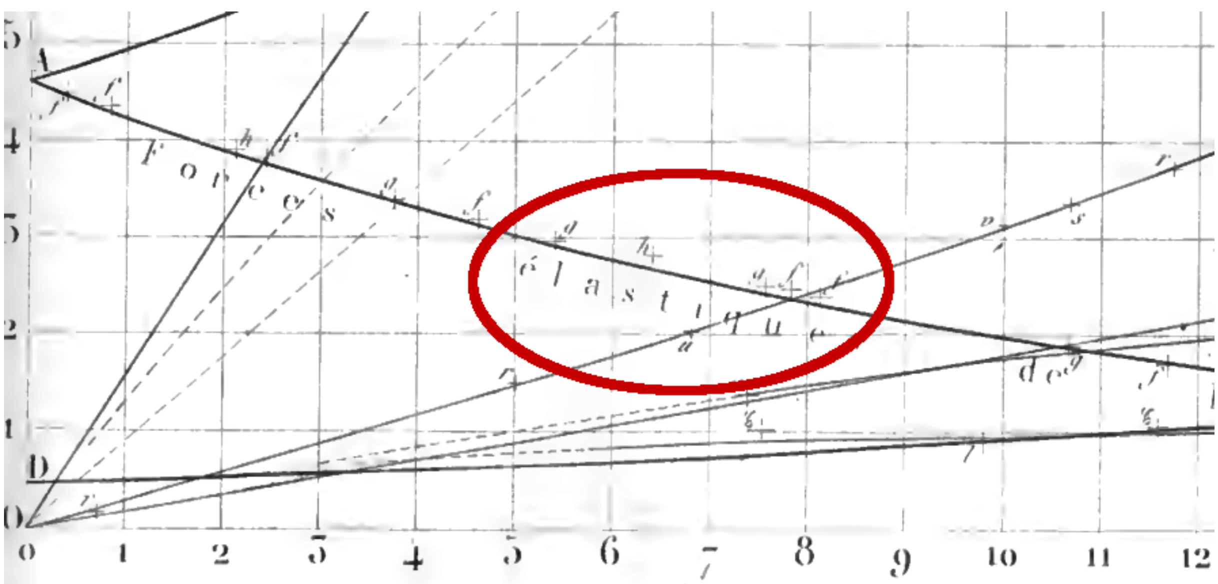


Figure 5.12
The circled area represents the portion of Regnault's curve with "certain small irregularities" which could not be avoided. While Regnault considered that these irregularities arose out of unavoidable experimental errors, for Thomson the irregularity of the curve represented a potential confirmation of his hypothesis. Source: Victor Regnault, "Relation des expériences entreprises par ordre de monsieur le ministre des travaux publics, et sur la proposition de la Commission Centrale des Machine à Vapeur, pour déterminer les principales lois et les données numériques qui entrent dans le calcul des machines à vapeur," *Mémoires de l'Académie Royale des Sciences de l'Institut de France.* 21 (1847): v.1, pl. VIII.

40. Thomson, "A Quantitative Investigation of Certain Relations between the Gaseous, the Liquid, and the Solid States of Water-Substance," 31-32.
41. Regnault, "Relation des expériences sur des Machine a Vapeur," 581.
42. Thomson, "A Quantitative Investigation of Certain Relations between the Gaseous, the Liquid, and the Solid States of Water-Substance," 32.

Two different attitudes towards the continuity of the curve were at play. For the experimentalist Regnault, it was "the curve that truly [réellement] represents the phenomenon" because the graphical method "allows one to distinguish, at a glance, the variations that come from accidental errors of the observations, and the constant errors which depend on the variety of methods that have been employed".[43] The curves were "the final expression of my experiments [l'expression définitive de mes expériences]" from which were obtained, by interpolation, the tables with experimental results.[44] Regnault could correct for errors because the graphical method allowed him to deal with the "ensemble of observations". The "perfect continuity" of the curve assured that "the accidental errors of the observations can only be extremely small".[45] Thomson recognized that the continuity of the curve was a prerequisite of Regnault's graphical method –

> it is not surprising that there should have been a tendency to smooth off this feature on the supposition that any departures of the experimental observations from the course of a continuous or smooth curve were only slight irregularities due to experimental errors or imperfections.[46]

In the first two papers about the "triple point", Thomson used the temperature on the vertical, and the pressure on the horizontal (see Fig. 5.11), most probably because it matched the orientation of the pressure axis for Andrews' isotherms (see Fig. 5.7).[47] However, in the third paper from 1873 presented to the Royal Society, Thomson engaged much more closely with Regnault's experiments and his graphical representation. It should not come as a surprise that Thomson chose to also redraw his diagram with pressure on the vertical, and temperature on the horizontal (see thomson-1873-29). In a letter to his brother about the upcoming talk at the Royal Society, James acknowledged the role played by the diagram in explaining his ideas

43. Regnault, "Relation des expériences sur des Machine a Vapeur," 428.
44. Ibid., 581.
45. "Si l'on fait passer une courbe par tous les points obtenus dans une même série d'expériences, on reconnaît, à sa continuité parfaite, que les erreurs accidentelles des observations ne peuvent être qu'extrêmement petites" (ibid., 580-581).
46. Thomson, "A Quantitative Investigation of Certain Relations between the Gaseous, the Liquid, and the Solid States of Water-Substance," 36.
47. Thomson, "Speculations on the Continuity of the Fluid State of Matter, and on Relations between the Gaseous, the Liquid, and the Solid States."

(see Fig. 5.13):

> I suppose I would need to insert for the Royal Society a diagram of the three curves crossing in the triple point, such as here sketched: because the paper would be rather unreadable without the diagram and a few introductory explanations...[48]

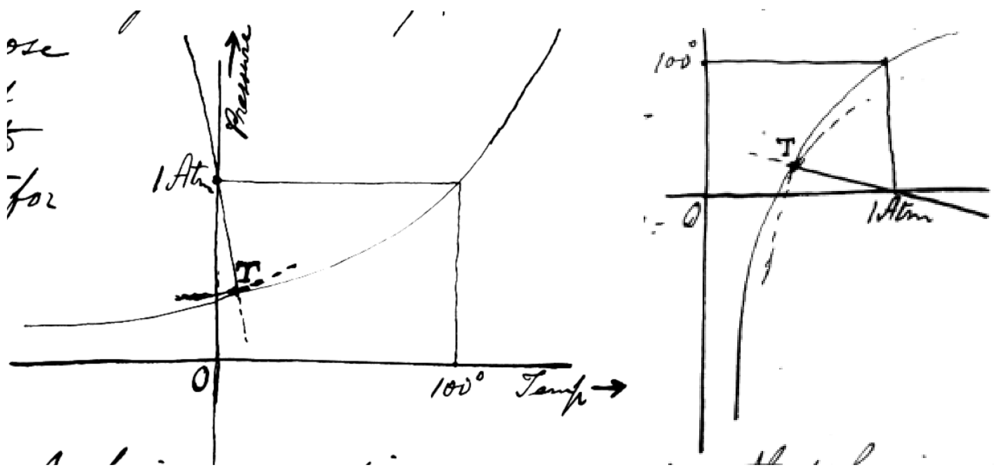


Figure 5.13
"In making it for the R.S. [Royal Society] I would turn the whole diagram round in the plane of the paper through 90° so as to have the axis of temperature running from right to left and the axis of pressure running up and down on the paper. The Brit. Assoc. figures by chance I made at Edinburgh and in Edin volume thus: [see right figure]". Source: James Thomson to William Thomson, 9 January 1873, T487, Add. MS 7342, Kelvin Papers (microfilm edition), Cambridge University Library

In his talks, Thomson also used a model which "helps to afford a clear view of the nature and meaning of continuity of the liquid and gaseous states of matter" (Fig. 5.15).[49] Thomson briefly mentioned how such a model could be constructed from the experimental curves:

> For the practical execution of this, it is well to commence with a rectangular block of wood, and then carefully to pare it down, applying, from time to time, the various curves as templates to it, and proceeding according to the general methods followed in a shipbuilder's modelling-room in cutting out small models of ships according to the curves laid down on paper as cross sections of the required model at various places in its length[50]

Thomson was speaking from experience. In his youth, in the early 1840s, Thomson served as an apprentice in William Fairbairn's iron shipbuilding yard on the Thames where he made

48. James Thomson to William Thomson, 9 January 1873, T487, Add. MS 7342, Kelvin Papers (microfilm edition), Cambridge University Library
49. Thomson, "Considerations on the Abrupt Change at Boiling or Condensing in Reference to the Continuity of the Fluid State of Matter," 5.
50. Ibid.

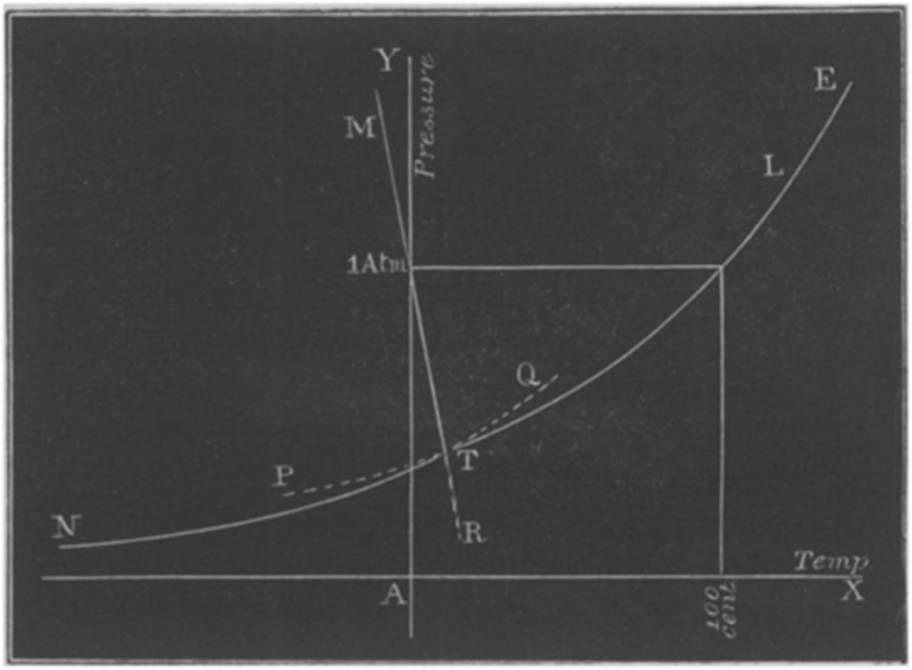


Figure 5.14
"The curve L represents the *boiling-line* terminating in the critical point E. The line TM represents the line between liquid and solid. It is drawn showing in an exaggerated degree the lowering of the freezing temperature of water by pressure, the exaggeration being necessary to allow small changes of temperature to the perceptible diagram. The line TN represents the line between the gaseous and the solid states of water-substance. The line LTN appears to have been generally (in the discussion of experimental results on the pressure of aqueous vapour above and bellow the freezing-point) regarded as one continuous curve; but it was a part of my object in the two British-Association papers referred to, to show that it ought to be considered two distinct curves (LTP and NTQ) crossing each other in the triple point T." Source: James Thomson, "A Quantitative Investigation of Certain Relations between the Gaseous, the Liquid, and the Solid States of Water-Substance," *Proceedings of the Royal Society of London* 22 (1873): 29

drawings of engines and worked in the fitting shop.[51] Nineteenth century ship design made constant use of experimental and theoretical curves – in the 1840s, John Scott Russell, the designer of the *Great Eastern* and one of the founders of the Institution of Naval Architects, proposed the wave-line system for which ship curves imitated the outlines of waves in the water;[52] in the 1860s, William Froude advanced a method of using small ship models to ex-

51. Crosbie Smith and M. Norton Wise, *Energy and Empire: A Biographical Study of Lord Kelvin* (Cambridge [Cambridgeshire] ;New York: Cambridge University Press, 1989), 130,285-292.

52. "The principle on which these wave ships are constructed is, that the hollow lines forming the entrance are to correspond, as nearly as may be consistent with the form of a ship, to the form of a certain wave capable of moving with the same velocity as the vessel", in John Scott Russell, *On the Nature, Properties and Applications of Steam, and on Steam Navigation. From the Seventh Edition of the Encyclopædia Britannica. [With Plates.]* (A. & C. Black, 1841), 304.

Figure 5.15
One of James Thomson's thermodynamic models. Source: © The Hunterian, University of Glasgow 2017.

perimentally determine optimal design.[53] Thomson's model immediately caught the attention of other physicists, and inspired Maxwell, Gibbs and van der Waals. Andrews' also used the model in a talk at the Royal Institution, and in 1876 he presented it at the international exhibition of scientific instruments and equipment at the South Kensington Museum.[54] Under Maxwell's care, the model was also to be acquired and displayed in the glass cabinet of the

53. On Froude's use of models see Gina Hagler, *Modeling Ships and Space Craft: The Science and Art of Mastering the Oceans and Sky* (Springer, 2012), 109-134. In 1873, Froude proposed a better method for constructing models that did not have to rely on the time consuming method of building up the model "in a series of horizontal layers of uniform thickness, consisting of boards of that thickness cut to the form of the intended water lines at the corresponding successive levels as laid down on the drawn plan of the vessel"; see William Froude, "Description of a Machine for Shaping the Models Used in Experiments on Forms of Ships," *Proceedings of the Institution of Mechanical Engineers* 24, no. 1 (1873): 202–215.

54. Thomas Andrews, *The Scientific Papers* (London, New York, Macmillanand Co, 1889), 340; *Catalogue of the Special Loan Collection of Scientific Apparatus at the South Kensington Museum*, 142.

Cavendish Laboratory.[55]

3 GIBBS' THERMODYNAMIC SURFACES

The graphical treatment of thermodynamical problems was radically changed by two articles published by the American physicist Josiah Willard Gibbs (1839-1903) in 1873. In the first paper, "Graphical Methods in the Thermodynamics of Fluids", Gibbs aimed to generalize and extend some of the graphical methods currently in use:

> So far as regards a general graphical method, which can exhibit at once all the thermodynamic properties of a fluid concerned in reversible processes, and serve alike for the demonstration of general theorems and the numerical solution of particular problems, it is the general if not the universal practice to use diagrams in which the rectilinear co-ordinates represent volume and pressure.[56]

Gibbs was right about "the general if not the universal practice" of using solely pressure-volume diagrams. Some of the most popular textbooks – such as Rankine's *A Manual of the Steam Engine and Other Prime Movers* (1859), Clausius' *Abhandlungen über die mechanischen Warmetheorie* (1864-1867), Verdet's *Théorie mécanique de la chaleur* (1868), or Maxwell's *Theory of Heat* (1871) – made exclusive use of this type of representation. Its appeal was both pedagogical and practical – on the one hand, it allowed for an immediate geometrical interpretation of work and efficiency, and on the other hand, Watt's indicator automatically inscribed such diagrams for any type of steam-engine. However, the pressure-volume diagram did not extend to all aspects of theoretical and experimental practice which often employed different independent variables that better suited their purposes.[57]

55. "It will give me a great pleasure to receive on the part of the Cavendish Laboratory a cast of your thermodynamic model with the lines marked on it. We have now got an excellent case with a glass front containing an thermometer by Il Gonfio (before 1640) Wollaston's optical and thermal apparatus etc., and we shall have a special place for models such as yours" (Maxwell, *The Scientific Letters and Papers of James Clerk Maxwell*, v.3, 231).

56. Josiah Willard Gibbs, *The Scientific Papers of J. Willard Gibbs* (Longmans, Green and Company, 1906), v.1, 1.

57. As has been said above, pressure-temperature graphical representations were the standard choice among experimentalists. It is unclear how familiar Gibbs was with experimental work in thermodynamics in 1873. The only authors cited by Gibbs in the two articles from 1873 are Clausius, Rankine, Zeuner, Cazin, Maxwell, Tait,

The pressure-volume diagram allowed one to observe the mechanical action of a steam-engine, but not its underlying thermodynamical principles. In the first half of the 19th century, the theoretical challenge was to understand the relation between the heat absorbed by an engine and the work generated by it. The fundamental principles of thermodynamics would follow from the solution of this problem. Émile Clapeyron – the engineer who made famous the pressure-volume diagram – "translated analytically the geometrical operations" in terms of which he described the stages of a Carnot-cycle, and he chose as independent variables the pressure and volume because this "choice accords best with the graphic representation".[58] He described the other physical quantities, like "absolute heat" or temperature, as functions of these variables. This choice was unfortunate because it further imposed the idea of heat as dependent only on the state of the system (i.e. $Q = Q(p, V)$) and complicated the fundamental role played by temperature in a Carnot-cycle.[59] Other physicists, who still made use of Clapeyron's pressure-volume diagram, preferred different independent variables for their analytical calculations. Rudolf Clausius chose volume and temperature because

> in the theory of heat the temperature t is especially important, and at the same time very suitable for determination by direct measurements, accordingly it is ordinarily regarded as a previously known magnitude upon which depend the several other magnitudes which there enter into consideration.[60]

This approach facilitated Clausius' definition of entropy and the mathematical statement of the "theorem of the equivalence of transformations" as $\int \frac{dQ}{T} \geq 0$.

William Thomson also made brief use of Clapeyron's graphical method, but he preferred to consider temperature as the independent variable when employing an "analytical method"

Andrews, and J. Thomson. The work of the last two was probably brought to his attention by Maxwell's *Theory of Heat*. Regnault is first mentioned in an article by Gibbs in 1879.

58. Émile Clapeyron, "Mémoire sur la puissance motrice de la chaleur," *Journal de l'Ecole Polytechnique* 23 (1834): 163; Rudolf Clausius, *The Mechanical Theory of Heat* (J. Van Voorst, 1867), 23.

59. The historian of science Clifford Truesdell has called Clapeyron choice of variables "awkward" and "unfortunate", though he did not provide a historical justification, see C. Truesdell, *The Tragicomical History of Thermodynamics, 1822-1854* (New York: Springer-Verlag, 1980), 139-143.

60. Clausius, *The Mechanical Theory of Heat*, 23; this choice of variables was first made in Clausius' famous article "Über die bewegende Kraft der Wärme" (1850).

for obtaining the relation between heat and work; this approach was closely connected to the definition of absolute temperature that he proposed.[61] While both Thomson and Clausius referred to Clapeyron's diagram, they did not extend its use but relied solely on analytical methods without offering any new geometrical interpretation. This discrepancy in the choice of variables between the graphical and analytical methods was also pointed out by P. G. Tait in his entry on "Thermodynamics" in the *Encyclopædia Britannica* (1888) – "it is usual, however, to choose v and t as independent variables, while we deal analytically (as distinguished from diagrammatically) with the subject".[62]

Some engineers, who expounded the pedagogical value of graphical methods, further extended the use of Clapeyron's diagram, while remaining faithful to the pressure-volume convention for variables. William Rankine made use of adiabatic curves (a term he coined) to represent the mechanical equivalent of the heat absorbed by a substance that passed from one state to another. If Clapeyron translated his geometrical insights into analytical expressions, Rankine followed the reverse path and translated analytical results back into the diagram. For example, Rankine provided a graphical representation of the second law of thermodynamics, but his approach was particularly cumbersome and did not provide any novel insights. Instead, it was Rankine's analytical methods that opened new theoretical possibilities, for which he also had to give up pressure and volume as independent variables. His "thermodynamic

61. William Thomson, "An Account of Carnot's Theory of the Motive Power of Heat," *Transactions of the Royal Society of Edinburgh* 16 (1849): 549-556; William Thomson, "On the Dynamical Theory of Heat," *Philosophical Magazine Series 4* 4, no. 22 (1852): 18-19. This difference can be seen in the way they have approached Carnot's function, that connected the work produced to the heat transfered:

$$\frac{dW}{dQ} = \frac{dT}{C(T)} \tag{5.2}$$

Clapeyron started from the assumption that Q was a function of pressure and volume, and ended up defining C as, $C = \frac{dQ}{dv}\frac{dT}{dp} - \frac{dQ}{dp}\frac{dT}{dv}$. The actual behavior of $C(T)$ had to be determined experimentally. Thomson instead, considered t to be the independent variable, and wrote $M = H\int_T^S \mu dt$, where in Thomson's notation, M was the mechanical effect or work; H the amount of heat passing through the engine; μ was just the inverse of Carnot's function, $\mu = \frac{1}{C}$.

62. Peter Guthrie Tait, *Scientific Papers* (University Press, 1900), v.2, 479.

function" was an early expression of entropy, but "to find the thermodynamic function for the expansion of a fluid, the pressure p is to be expressed in the form of a function of the volume v, and absolute temperature τ".[63]

It might seem surprising that despite real interest in graphical representations, for almost two decades, the new analytical methods of thermodynamics did not generate new types of representations. Complicated concepts and functions were only given a brief and intricate interpretation in the old pressure-volume diagrams. This surprise can be dispelled if we consider that in the mid-19th century, a graphical representation was part of a graphical method. One did not translate an isolated analytical expression into a geometrical relation. The desire to see was trumped by the need to manipulate. Non-consequential visual representations were not worth the effort. Gibbs was keenly aware of this when he referred to the pressure-volume diagram as the only "general graphical method, which can exhibit *at once all the* thermodynamic properties of a fluid".[64]

Gibbs started his paper by investigating the most general diagram that could be drawn. He first listed the possible quantities that determined the state of the body – which he called "*functions of the state of the body*": the volume, v; the pressure, p; the (absolute) temperature, t; the energy, ϵ; the entropy, η. Two other quantities, the heat and the work, were to be determined by "the whole series of states through which the body is supposed to pass".[65] Out of the five functions of the state only two were independent, while the other three could be determined from three finite equations. This fact allowed one to

> associate a particular point in a plane with every separate state, of which the body is capable, in any continuous manner, so that states differing infinitely little are associated with points which are infinitely near each other, the points associated with states of equal volume will form lines,

63. William John Macquorn Rankine, *A Manual of the Steam Engine and Other Prime Movers.* (London, Glasgow, RGriffin, 1859), 311. Or, in other cases temperature and pressure were to be used as independent variables: "In some investigations it is convenient to take the *pressure* and temperature as independent variables, the volume being expressed as their function" (ibid., 314).
64. My underline.
65. Gibbs, *The Scientific Papers of J. Willard Gibbs*, v.1, 1-3.

> which may be called *lines of equal volume*, the different lines being distinguished by the numerical value of the volume (as lines of volume 10, 20, 30, etc.). In the same way we may conceive of *lines of equal pressure*, *of equal temperature, of equal energy, and of equal entropy*. [...] Suppose the body to change its state, the points associated with the states through which the body passes will form a line, which we may call the *path* of the body.[66]

This procedure would have puzzled most of Gibbs' readers because it did not rely on the standard methods of plotting. Gibbs was not associating points in the plane with pairs of numerical values. There was no system of coordinates. Instead, infinitely close points corresponded to infinitely close states; points sharing some property (of equal volume, temperature, pressure, etc.) were connected by a curve. This procedure of directly associating points on a surface with physical states without the use of any metric closely resembled Riemann's notion of a manifold, especially the often invoked example of a color manifold.[67] Similarly to Riemann, Gibbs restricted all assumptions to a minimum: "Thus far we have made no supposition in regard to the nature of the law, by which we associate the points of a plane with the states of the body, except a certain condition of continuity."[68] The only required assumption was that the association of states and points should be made such that the continuous transition between physical states to be matched to a continuous trace passing through the associated points.

Gibbs identified a series of topological properties that were independent of any system of coordinates, or metric. Imagine that you are given a law of association such that the curves of equal pressure and volume are straight, perpendicular lines. Such a pressure-volume diagram can be continuously transformed into a volume-temperature diagram. The curves of equal temperature can be stretched into straight lines, perpendicular to the lines of equal volume. While the distance between points or the shape of curves might change, there are some relations that will remain invariant under such transformations. For example, two lines of equal

66. Gibbs, *The Scientific Papers of J. Willard Gibbs*, v.1, 3.
67. See the chapter on colors
68. Gibbs, *The Scientific Papers of J. Willard Gibbs*, v.1, 8.

temperature cannot intersect, or a line of equal temperature and a line of equal pressure cannot coincide (though they can intersect more than once). Gibbs was actually able to show that the lines of equal volume, pressure, temperature and entropy passing through a point were always arranged in a particular order which "is not altered by any deformation of the surface on which the diagram is drawn, and is therefore independent of the method by which the diagram is formed" (see Fig. 5.16).[69] Gibbs did not provide a formal, algebraic proof; instead, he based his argument on the intuitive notion of continuous deformations (stretching and bending, but not cutting or piercing):

> The different diagrams which we obtain by different laws of association are all such as may be obtained from one another by a process of *deformation*, and this consideration is sufficient to demonstrate their properties from the well-known properties of the diagram in which the volume and pressure are represented by rectangular coordinates. For the relations indicated by the network of isometrics, isopiestics etc., are evidently not altered by deformation of the surface upon which they are drawn, and if we conceive of mass as belonging to the surface, the mass included within given lines will also not be affected by the process of deformation.[70]

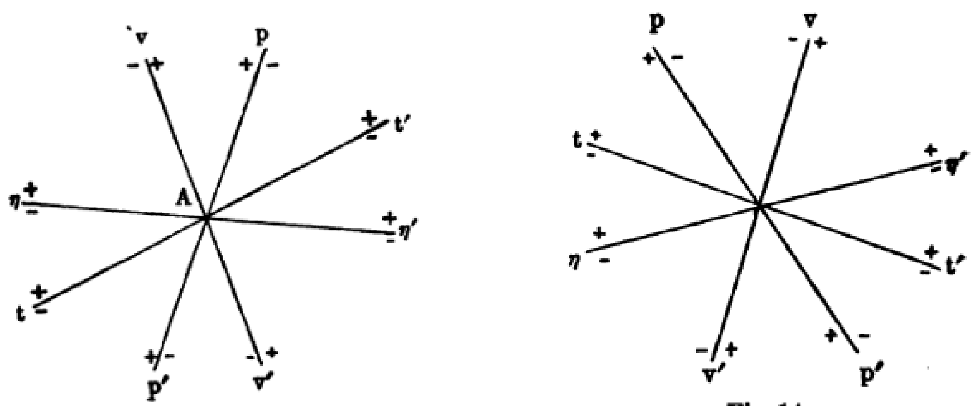


Fig. 13. **Fig. 14.**

Figure 5.16
Gibbs focused exclusively on the topological properties of the lines of equal pressure, volume, temperature and enthropy. He associated the order of these lines with particular thermodynamical properties: Fig. 13 corresponded to $\left(\frac{dp}{d\eta}\right)_v > 0$ and Fig. 14 to $\left(\frac{dp}{d\eta}\right)_v < 0$. Source: Josiah Willard Gibbs, *The Scientific Papers of J. Willard Gibbs* (Longmans, Green and Company, 1906), v.1, 30-31.

While the idea of representing the states of a substance by points was not new (as we

69. Gibbs, *The Scientific Papers of J. Willard Gibbs*, v.1, 29.
70. Ibid., v.1, 8.

have already encountered it in the case of Clapeyron's or James Thomson's diagrams), Gibbs brought epistemological clarity to such representations. He considered that Thomson's graphical representation of the *triple point* by a single point was an unfortunate choice of coordinates – "this must be regarded as a defect in these diagrams, as essentially different states are represented by the same point."[71] Gibbs used an entropy-volume diagram to show how the triple point could be opened into a triangle for which each vertex corresponded to a pure state (solid, liquid and vapor) while the points inside the triangle represented a unique mixture of these states (Fig. 5.17). At the same time, Gibbs acknowledged that not all states could be uniquely represented by a point on the diagram. For example, if some liquid state next to the boiling line MM was heated until it crossed the line, it could either pass into a superheated liquid state or in a vapor-liquid state (Fig. 5.18):

> ...every point on the right of MM and sufficiently near to it represents two different states of the body, in one of which it is partially vaporized, and in the other it is entirely liquid. If we take the points as representing the mixture of vapor and liquid, they form one diagram, and if we take them as representing simple liquid, they form a totally different diagram superposed on the first. There is evidently no continuity between these diagrams except at the line MM; we may regard them as upon separate sheets united only along MM. For the body cannot pass from the state of partial vaporization to the state of liquid except at this line.[72]

The image that Gibbs had in mind was that of "three sheets, which are united along the line MM (one on the left and two on the right)". This construction might seem at first gratuitous. Thomson and Maxwell also represented the boiling-line in their volume-pressure diagrams, however in the case of their diagrams, when the isotherm passed the boiling line from the liquid state it bifurcated into two curves CEG and $CDEFG$. While the physical status and relation between these curves was still uncertain, Gibbs aimed to preserve the consistency of his diagrammatic convention. For Maxwell and Thomson the two curves corresponded to different physical processes, and that was well represented by the shape of the curves. Gibbs attention was not concentrated on the curves – he actually did not draw any experimental

71. Gibbs, *The Scientific Papers of J. Willard Gibbs*, v.1, 25.
72. Ibid., v.1, 27.

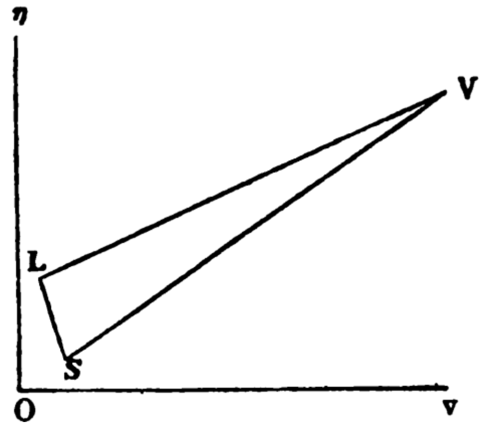


Figure 5.17
Thomson's triple point showed that for some unique temperature and pressure, the three states of matter of a substance can coexist. However, this does not mean these states are indistinguishable. On an entropy-volume diagram, the states would correspond to different points (all at the same pressure and temperature). The state of a mixture of solid, liquid and vapor states corresponds to the center of mass of the triangle, which will always be a point inside the triangle. Because all these states have the same pressure and temperature, "the pressure and temperature are constant for this triangle, i.e., an isopiestic [isobaric] and also an isothermal here expand to cover a space." Gibbs did not further comment on this, but the concept of a curve that expands to cover a space is ill-defined. Gibbs proposed a more cogent geometrical explanation in his second article from 1873. Source: Josiah Willard Gibbs, *The Scientific Papers of J. Willard Gibbs* (Longmans, Green and Company, 1906), v.1, 24.

curves on his diagrams. Instead, he realized that his requirement of continuity would fail if the curves were drawn on the same sheet, because this would mean that one curve could be continuously transformed into the other. However, there was no such continuous physical transformation between the lines of the superheated liquid and the mixture of liquid and vapor. To move between two states on the superposed diagrams one had to first reach the line MM where the two sheets were glued.

This was a particularly intriguing collage of diagrams because no "distortion" could bring these three sheets "into a single plane surface without superposition".[73] Though ingenious, this could not have been a fully satisfactory representation. The shape of the line MM was not physically accurate and it failed to take into account Andrews' critical point. But most importantly, there was no justification why there was a need to glue the three diagrams together. This was a problem because Gibbs' diagrams were not meant to be just another useful and

73. Gibbs, *The Scientific Papers of J. Willard Gibbs*, v.1, 28.

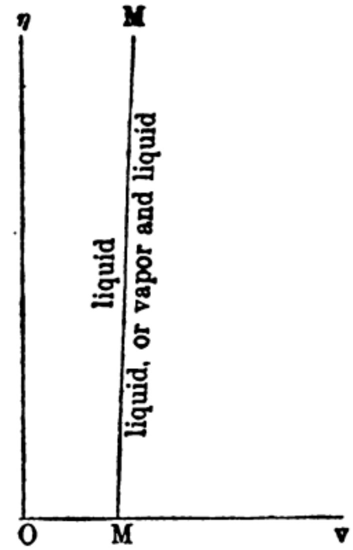


Figure 5.18
While this diagram had almost no content, it still helped the reader imagine Gibbs' explanations. When a substance passed from a state to the left of the line MM (i.e. a liquid state) to the right of the line it would either be found in a superheated liquid state, or a mixture of liquid and vapor. While both of these states could have been represented on the same diagram (as Maxwell and Thomson did), Gibbs argued that such a representation would contravene his requirement of continuity. This forced him to argue that to the right of the line one should imagine two different diagrams corresponding to these different states glued together along the line MM. Source: Josiah Willard Gibbs, *The Scientific Papers of J. Willard Gibbs* (Longmans, Green and Company, 1906), v.1, 27.

practical method of solving problems, but rather a mean of providing physical insights. Gibbs was particularly clear that his graphical method was not just a translation or interpretation of analytical results. While he did employ analytical methods, it was mostly for convenience and he claimed that one could "show the independence and sufficiency of a graphical method", such that

> starting from the first and second laws of thermodynamics as usually enunciated, to arrive at the same results without the aid of analytical formulae, –to arrive, for example, at the conception of energy, of entropy, of absolute temperature, in the construction of the diagram without the analytical definitions of these quantities, and to obtain the various properties of the diagram without the analytical expression of the thermodynamic properties which they involve.[74]

Gibbs' goal of deducing various thermodynamic properties directly from diagrams without the aid of any analytical expressions was also at the center of his second 1873 paper,

74. Gibbs, *The Scientific Papers of J. Willard Gibbs*, v.1, 32.

"A method of geometrical representation of the thermodynamic properties of substances by means of surfaces". Gibbs' approach was very similar to the first paper – he only extended his representation from the two-dimensional plane to a three-dimensional surface. This was also the reason why "the graphical methods" of the first paper were now extended to "a method of geometrical representation". While Gibbs' novel entropy-temperature diagram was particularly useful in illustrating the second principle of thermodynamics, there were some issues that were not yet satisfactorily explained: phase transitions and the coexistence of mixed states.

A possible source of influence for Gibbs could have been James Thomson's thermodynamic model which was referenced in the beginning of the paper. However, if Thomson used pressure, volume and temperature for the axes, Gibbs preferred energy, entropy and volume because he considered that Thomson's choice "affords a less complete knowledge of the properties of the body".[75] The advantage of Gibbs' thermodynamical model was that it allowed him to visualize all the five thermodynamical quantities for a given point – while the coordinates of the point specified its energy, entropy and volume, the pressure and temperature could be obtained from the geometrical interpretation of two thermodynamical relations:[76]

$$p = -\left(\frac{d\epsilon}{dv}\right)_\eta, t = \left(\frac{d\epsilon}{d\eta}\right)_v \tag{5.3}$$

These relations implied that the pressure was equal to the variation of energy with volume at constant entropy, and the temperature was equal to the variation of energy with respect to entropy, at constant volume. This provided a straightforward geometrical interpretation. The intersection of the thermodynamic surface with a plane parallel to the volume axis was a curve of constant entropy; the tangent to a point on this curve was the pressure at that point. One could similarly find the temperature as the tangent to the curve of constant volume. The pressure and temperature tangents determined a tangent plane at that point. That is, if

75. Gibbs, *The Scientific Papers of J. Willard Gibbs*, v.1, 34.
76. These two relations were obtained from the general equation $d\epsilon = td\eta - pdv$.

the thermodynamic surface was given, the inclination of the tangent plane at a certain point determined its pressure and temperature. Thus, all five quantities could be directly observed on the thermodynamic model.

However, Gibbs' interest in graphical or geometrical methods was not to represent what was known, but to offer a mean of understanding properties that were only vaguely explained by analytical means. Gibbs found a coherent and powerful method of explaining *geometrically* phase transitions and the coexistence of mixed states. In what follows, I will present Gibbs' geometrical interpretation in parallel with Maxwell's own account of Gibbs' methods.

While Gibbs' 1873 articles did not spark a lot of general interest, they did attract Maxwell's attention who included a whole section on "Prof. Gibbs' Thermodynamic Model" in the fourth revised edition of the *Theory of Heat* (1875). Gibbs' geometrical ideas fit perfectly with Maxwell's scientific and pedagogical work. While Gibbs' only described his thermodynamic surface in words and provided a simplified diagram of a projection of the surface on the volume-entropy plane, Maxwell actually built a plaster model. In a letter to Andrews, Maxwell confessed that he "made several attempts to model the surface" in the winter of 1874.[77] Because of technical difficulties he chose not to model a real substance using experimental data:

> The numerical data about entropy can only be obtained by integration from data which are for most bodies very insufficient, and besides it would require a very unwieldy model to get all the features, say of CO_2, well represented, so I made no attempt at accuracy, but modelled a fictitious substance, in which the volume is greater when solid than when liquid, and in which, as in water, the saturated vapour becomes superheated by compression. When I had at last got a plaster cast I drew on it lines of equal pressure and equal temperature, so as to get a rough notion of their forms. This I did by placing the model in the sun light, and tracing the curve when the rays just grazed the surface.[78]

Maxwell was particularly proud of the method he developed for tracing curves of equal pres-

77. Maxwell, *The Scientific Letters and Papers of James Clerk Maxwell*, v.3, 236, No. 567; see also 147, No. 537.
78. ibid., v.3, 236-237, No. 567. Maxwell's clay model was described to Gibbs by Alexander Freeman: "You will be gratified to hear that Prof. Maxwell has made a clay model of your Thermodynamic surface wherein entropy, energy & volume are the three coordinates, and is able to explain a great deal by it.", Freeman quoted in ibid., v.3, 148.

sure and temperature on the model; tracing the lines of equal volume, entropy and energy was straightforward.[79] If the model was to have any use in practice, such curves were essential – not only because they "form a complete representation of the relations between the five quantities", but they could also potentially allow for much better extrapolations than any empirical formulae:[80]

> I have not got the model here, but I have been trying to trace the lines of temperature and pressure so far as we can conjecture their forms, I think such graphical methods are better fitted for purely conjectural applications of the principle of continuity beyond the range of experiment than any empirical formulae.[81]

Maxwell's and Thomson's models differed not only in the choice of coordinates, but also in their construction and use. While Thomson carved his model from experimental curves, Maxwell started with a "fictitious" model on which he constructed the thermodynamic curves. Thomson's attention was concentrated on the way the curves changed as they moved along the surface of the model. Maxwell, however, followed Gibbs in concentrating not exclusively on the curves, but also on the curvature of the surface and showed how its geometrical properties could explain the stability or coexistence of different states of matter. If until now physical properties were interpreted mathematically or geometrically, now mathematical/geometrical properties received a physical interpretation.

When in thermodynamic equilibrium, it was assumed that the substance and its surrounding medium were at the same temperature and pressure. This implied that the states which could be in thermodynamic equilibrium with the surrounding medium at fixed pressure and temperature had parallel tangent planes. Energy considerations indicated if these equilibrium states were stable, unstable or neutral. The geometrical condition for a stable equilibrium state

79. To trace the lines of equal pressure and temperature "we have only to place it in the sunshine and to turn it so that the sun's rays are parallel to the plane of volume and energy, and make an angle with the line of volume whose tangent is proportional to the pressure. Then, if we trace on the surface the boundary of light and shadow, the pressure at all points of this line will be the same", James Clerk Maxwell, *Theory of Heat*, 4th ed. (London: Longmans, Green and Company, 1875), 197.

80. Ibid., 198.

81. Maxwell, *The Scientific Letters and Papers of James Clerk Maxwell*, v.3, 237, No. 567.

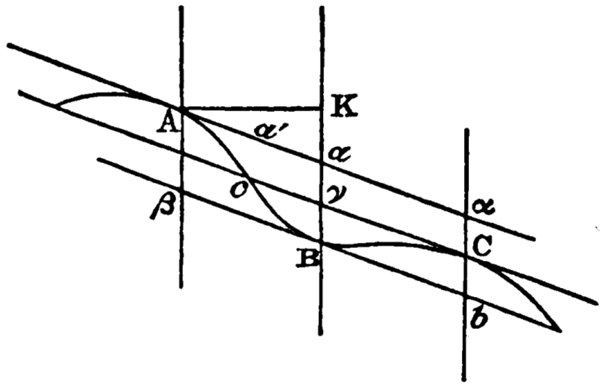


Figure 5.19
The diagram represents a vertical section of the thermodynamic model, where the horizontal direction represents the entropy or volume and the vertical direction represents the energy, which increases downwards for the substance, and upwards for the medium. A B C are three points on the thermodynamic surface for which the tangent planes are parallel such that these points have the same pressure and temperature, and are in thermodynamic equilibrium with the surrounding medium. To know if these states are stable, "we wish to determine whether the substance will tend of itself to pass from one of these states to the other." If the substance could move from state A to state B, it would increase its energy with BK. During this transition, the medium remains at the same pressure and temperature, but the change in entropy or volume will match the change of these quantities for the substance. If the substance could transition to the state B, the medium would be in the state a and it would have lost an energy equal to Ka. The difference between the energy gained by the substance and that lost by the medium, aB, would have to be provided by an external force. "The working substance, therefore, cannot of itself pass from the state A to the state B, if B lies *below* the plane which touches the surface at A." Only the states above the tangent plane are energetically available. State B, "if physically possible for an instant, is essentially unstable, and cannot be permanent" and it would almost immediately move to B or C. State C is stable, because the "substance cannot pass through any continuous series of states from C to A" without crossing below its tangent plane. However, if parts of the substance in state A are in contact with the rest of the substance in state C, the portions would pass from state C to A without passing through the intermediate states. Source: James Clerk Maxwell, *Theory of Heat*, 4th ed. (London: Longmans, Green and Company, 1875), 199-202.

(i.e. "the substance cannot of itself pass into any other state while exposed to the same external influences of pressure and temperature") was that the tangent plane at that point was everywhere above the thermodynamic surface (see Fig. 5.19).[82] This observation provided a simple, but powerful, geometrical mean of determining the coexisting states of a substance for every pressure and temperature –

> when the tangent plane touches the surface at two or more points, and is above the surface everywhere else, portions of the substance in states corresponding to the points of contact can exist in presence of each other, and the substance can pass freely from one state to another in either direction.[83]

82. Maxwell, *Theory of Heat*, 201.
83. Ibid., 202.

The points at which a sheet of paper touched the thermodynamic surface could coexist. If the paper touched the surface only at two points, then the substance had only two coexisting states for that temperature and pressure. The mixed state was

> represented by a point in the straight line joining the centre of gravity of two masses equal respectively to the masses of the substance in the two states, and placed at the points of the model corresponding to these states.[84]

While the coexisting states were points on the thermodynamic surface, the mixed states were points along the line determined by the two points of contact. If one rolled the sheet of paper along the thermodynamics surface always touching it at two points without cutting it, the lines connecting the points of contact determined a "secondary" or "derived" surface.[85] Maxwell called the points of contact node-couples, and the curves traced by these points on the rolling tangent plane node-couple curves.

Maxwell's interest in the secondary surface was not abstract. He immediately provided a procedure by which such a surface could be determined from the thermodynamic model:

> To construct it, spread a film of grease on a sheet of glass and cause the sheet of glass to roll without slipping on the model, always touching it in two points at least.
> The grease will be partly transferred from the glass to the model at the points of contact, and there will be traces on the model of the node-couple curves, and on the glass of corresponding plane curves.
> If we now copy on paper the curve traced out on the glass and cut it out, we may bend the paper so that the cut edges shall coincide with the two node-couple curves, and the paper between these curves will form the derived surface representing the state of the body when part is in one physical state and part in another.[86]

In the *Theory of Heat* (1875), Maxwell included a plane projection of his thermodynamic surface (Figs. 5.21 and 5.22). The few dozens lines crowded together made Maxwell's diagram less comprehensible than Gibbs' simpled drawing. However, both focused on the critical point

84. Maxwell, *Theory of Heat*, 202.
85. Gibbs used the name "derived surface", while Maxwell used "secondary surface" (Gibbs, *The Scientific Papers of J. Willard Gibbs*, v.1, 36; Maxwell, *Theory of Heat*, 203). Mathematically, they defined the "secondary surface" as a "a developable surface, being the envelope of the rolling tangent plane", Maxwell, *Theory of Heat*, 203.
86. Ibid.

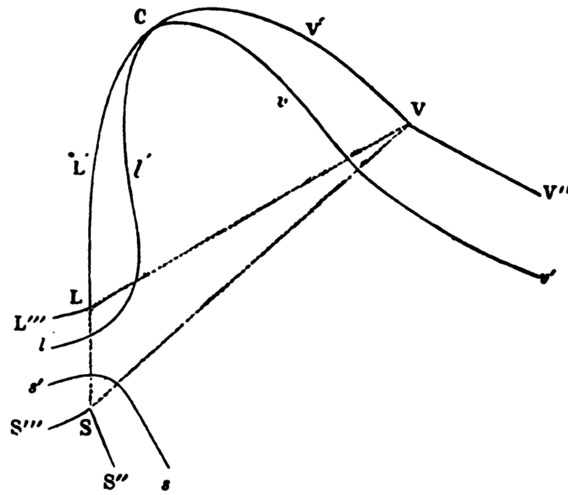


Figure 5.20
"S, L, and V are the points which have a common tangent plane and represent the states of solid, liquid, and vapor which can exist in contact. The plane triangle SLV is the derived surface representing compounds of these states. LL' and VV are the pair of lines traced by the rolling double tangent plane, between which lies the derived surface representing compounds of liquid and vapor. [...]
The geometrical expression of the results which Dr. Andrews, *Phil. Trans.*, vol. 159, p. 575, has obtained by his experiments with carbonic acid is that, in the case of this substance at least, the derived surface which represents a compound of liquid and vapor is terminated as follows: as the tangent plane rolls upon the primitive surface, the two points of contact approach one another and finally fall together. The rolling of the double tangent plane necessarily comes to an end. The point where the two points of contact fall together is the *critical point*." Source: Josiah Willard Gibbs, *The Scientific Papers of J. Willard Gibbs* (Longmans, Green and Company, 1906), v.1, 44-45.

C and the triangle SLG (SLV for Gibbs) that was determined by the coexisting solid, liquid and gaseous states. Because these three states were coexisting, the tangent plane touched the thermodynamic surface at these points at the same time (i.e. all three points were coplanar). As one moved away from the solid state, the tangent plane touched the surface only at two points, along the node-couple curves CL and CG. Up to the point C, the liquid and vapor states could coexist. But at the point C and beyond the tangent plane had only one point of contact with the surface, thus showing that there was only one possible state of equilibrium.

What was particular interesting and consequential was the geometrical interpretation of the critical point C. Imagine that one could cut the thermodynamic surface perpendicular to the line HK. The section that would be obtained is represented in Fig. 5.24, where K corresponds to A and H to D. Though its coordinates are energy and volume (or entropy), this diagram represented the same physical process as Maxwell's and Jameson Thomson's isotherms.

Figure 5.21
Plaster thermodynamic model made by Maxwell and sent to Josiah Willard Gibbs at Yale. Source: © Yale Peabody Museum, Division of Historical Scientific Instruments (YPM HSI 290012).

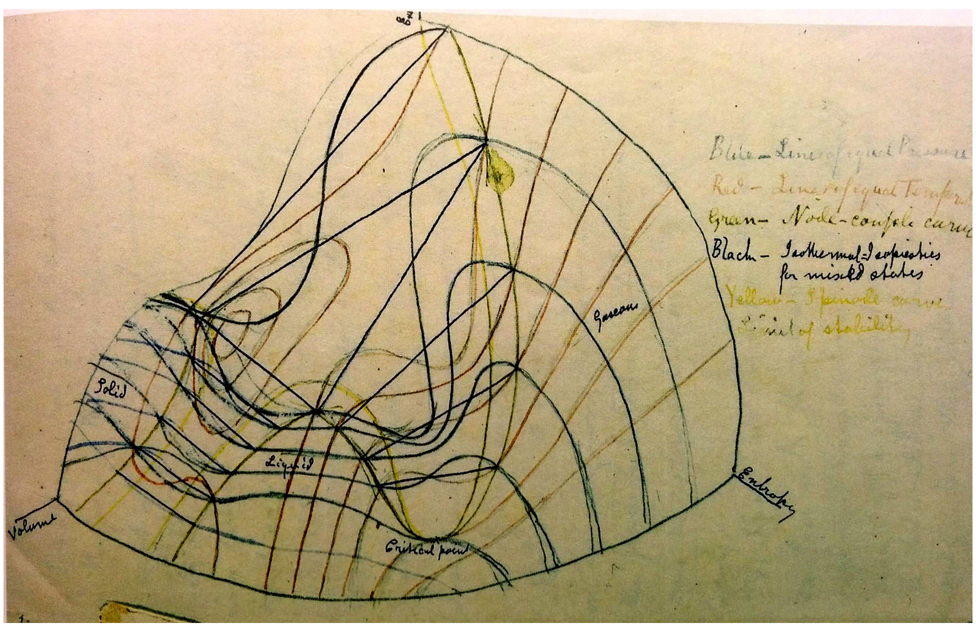


Figure 5.22
Maxwell's sketch of the lines on Gibbs' thermodynamic surface. Notice the *critical point* in the lower-middle of the model and the drawing. Maxwell referred to it as a *tacnodal point* because it aws here that the two node-couple curves meet and the spinode curve is tangent to the node-couple curves. Also, one can spot the triangle of the solid-liquid-gaseous states (this is not represented on the model). Source: Special Collections, Queen's University Belfast, Thomson MS.13/22h.

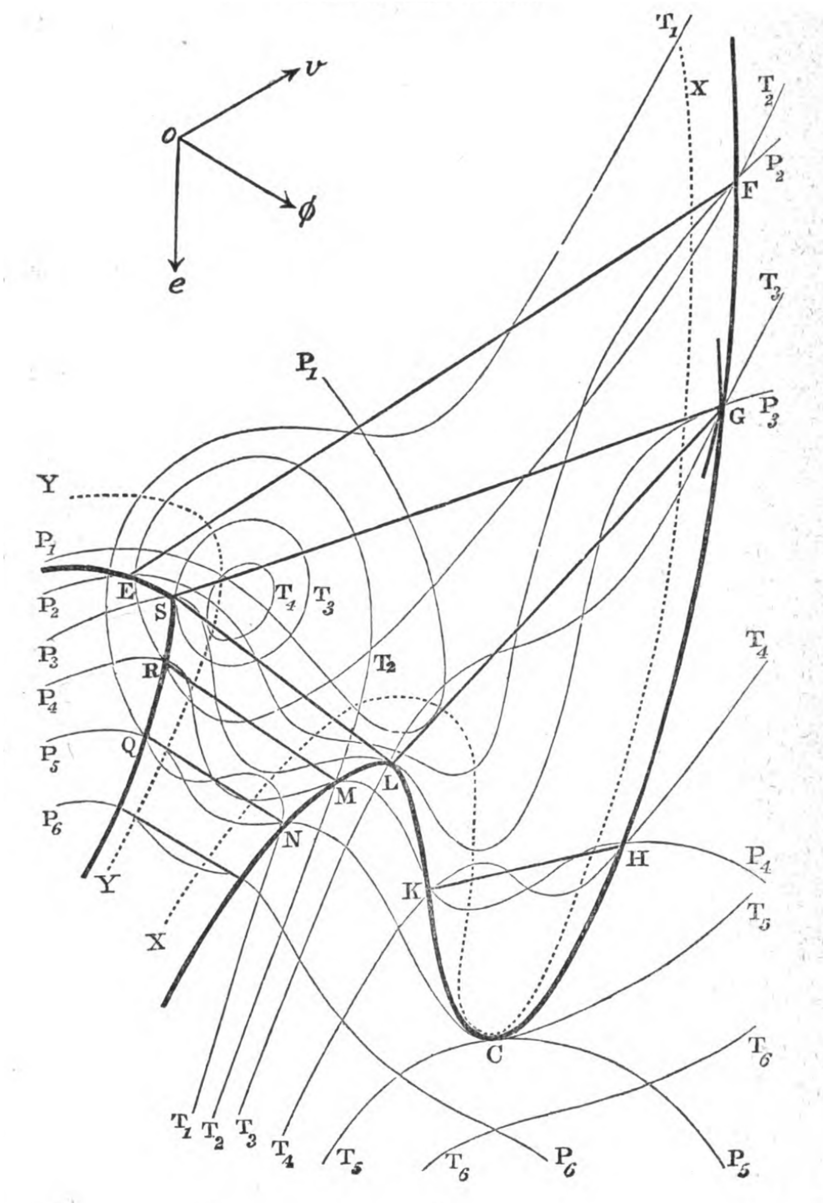


Figure 5.23
Maxwell's diagram is a projection on a plane of his thermodynamic model. While this diagram is useful to depict all the lines, to understand their geometrical meaning one needs to imagine it as a surface embedded in three dimensions. The rolling tangent plane touches S, L, G at the same time (i.e. the points are coplanar). These points correspond to the co-existing solid, liquid and gaseous states. The inside of the SLG triangle corresponds to all possible mixtures of these states. As the rolling tangent plane is moved downwards, it will only have two points of contact with the thermodynamic surface. These points are positioned on the curves CL and CG (the *node-couple curves*) corresponding to co-existing liquid and vapor states. When the tangent plane reaches C, it will touch the thermodynamic surface at only one point. This point C is Andrews' critical point. The dotted line XCX represents the *spinode-curve*. The point C has a particular geometrical property because the spinode-curve (XCX) and the node-couple curve (LCG) are tangent; Maxwell called this the *tacnodal* point. The curves P_i and T_i represent lines of equal pressure and temperature, respectively. For a less crowded diagram, see Gibbs' drawing (Fig. 5.20). James Clerk Maxwell, *Theory of Heat*, 4th ed. (London: Longmans, Green and Company, 1875), 207

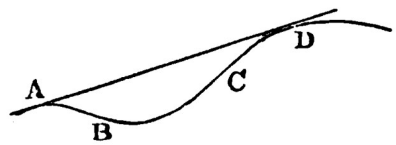


Figure 5.24
A and D are points of stability (because the energy axis is pointing downwards). B and C are inflexion points. The region between B and C is "essentially unstable". The points between A and B (or C and D) are stable when the substance is homogeneous, but "liable to sudden change if a portion of the same substance in another state is present." Source: James Clerk Maxwell, *Theory of Heat*, 4th ed. (London: Longmans, Green and Company, 1875), 205.

The line AD represented the mixed states of liquid and vapor (similarly to the line CG in Fig. 5.9), while the curve $ABCD$ represented a homogeneous state (similarly to $CDEFG$ in Fig. 5.9), and the points B C marked the margins of the instability region (similarly to the points D F). In the pressure-volume diagram, the portion DEF was known to be unstable because pressure and volume could not increase together. In the thermodynamic model, the instability regions received a purely geometrical interpretation. On the thermodynamic surface the instability region was bounded by the dotted line XCX, which Maxwell called the *spinode* curve (or in Gibbs' diagram, the line $ll'Cvv'$ which he called "the *limit of essential instability*"). Inside this region the curvature was concave upwards, while outside the region it was convex. The spinode curve XCX and the node-couple curve LCG at the critical point, that Maxwell called the *tacnodal* point.

The thermodynamic surface was most fruitfully applied to the study of the equilibrium of heterogeneous substances. It was, again, Gibbs who opened this field of investigation with a series of articles published in 1876 and 1878.[87] The geometrical method he employed in these articles was very similar, but now one had to deal with a higher number of variables. If for a homogeneous substance there were only two independent variables, for a heterogeneous substance one could also vary the amount of each individual component. For example, if the heterogeneous substance was formed by two homogeneous substances one could vary their ratio which would correspond to one independent variable; if the substance was formed by three

87. Gibbs, *The Scientific Papers of J. Willard Gibbs*, 55-354.

components, then there would be two independent ratios to vary. Each of the homogeneous components could be in a different state of matter, and multiple states could be coexistent. For example, a binary mixture of two substances S_1 and S_2 could have for a given pressure and temperature the coexistent states liquid(S_1)-liquid(S_2)-vapor(S_2), liquid(S_1)-vapor(S_2)-solid(S_2), liquid(S_1)-vapor(S_1)-liquid(S_2)-vapor(S_2) etc. Which states could actually coexist would depend on both the thermodynamic properties of each individual substance, and the ratios in which they were mixed. The obvious challenge was to construct a theory that could predict the properties of a heterogeneous substance only knowing the properties of the homogeneous compounds.

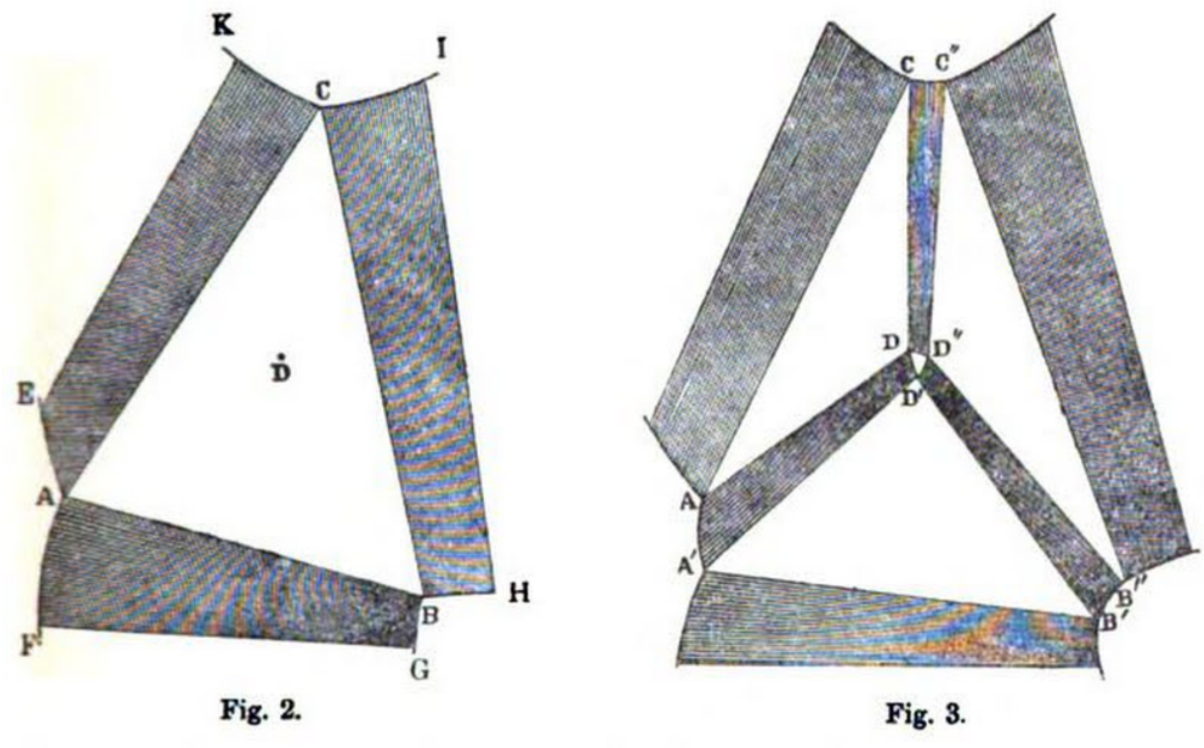


Figure 5.25
Source: Josiah Willard Gibbs, *The Scientific Papers of J. Willard Gibbs* (Longmans, Green and Company, 1906), 123.

If in the 1873 paper Gibbs used volume, energy and entropy as the coordinates for his thermodynamical surface, now he made a different choice of variables employing pressure, temperature and his newly defined ζ potential (i.e., the Gibbs free energy).[88] The choice was purely pragmatic as the temperature and pressure defined the states of equilibrium. Besides these three variables – p, t, ζ – now Gibbs also had to consider the ratio of the components. A mixture of two substances had to be described in a four-dimensional space (p, t, ζ, m_1, m_2), while a mixture of three components needed a five-dimensional space $(p, t, \zeta, m_1, m_2, m_3)$.[89] To be able to visualize such surfaces, Gibbs fixed the temperature and pressure and only focused on describing the two-dimensional $\zeta - m$ space (see Fig. 5.25).

4 ψ-SURFACES AND PLASTER MODELS

Maxwell was the first to react when in 1876 he received from Gibbs a copy of the first part of his paper. In no time, he published a short note and gave a lecture about the article, though he did not mention Gibbs' new thermodynamic surface.[90] This is understandable, as the article was considerably long and rich in novel ideas – such as the chemical potentials, the conditions of equilibrium, etc. – which deserved all the attention. What further use Maxwell would have made of Gibbs' geometrical ideas remains unknown as he shortly died in 1879. However, in 1879 Gibbs also sent van der Waals his recently published paper "On the equilibrium of heterogeneous substances".[91] While van der Waals was among the ninety or so possessors of the paper, he was the only one to consider the new avenue of research opened by Gibbs.[92] He combined Gibbs' thermodynamic relations (or what he broadly referred to as "the mechanical theory of heat") with the properties of molecules, such as their sizes and mutual attractions.

88. The fundamental function ζ was defined as: $\zeta = \epsilon - t\eta + pv$. After taking the differential and substituting $d\epsilon$, one would get: $d\zeta = -\eta dt + v dp + \mu_1 dm_1 + \mu_2 dm_2 + \ldots + \mu_n dm_n$, where μ_i are the chemical potentials defined by the previous relation: $\mu_i = \left(\frac{d\zeta}{dm_i}\right)_{t,p,m_j \neq i}$. By definition, the sum of the masses was one: $m_1 + m_2 + \ldots + m_n = 1$. These two equations showed that all the thermodynamic properties of the substance could be described by a surface embedded in the space $(\zeta, p, t, m_1, m_2, \ldots, m_{n-1})$.

89. The sum of the masses was always defined to be one: $m_1 + m_2 + \ldots + m_n = 1$

90. Maxwell, *The Scientific Letters and Papers of James Clerk Maxwell*, v.3, 281-293, No.587-589; Maxwell, *The Scientific Papers of James Clerk Maxwell*, v.2, 498-500.

91. *Nobel Lectures, Physics 1901-1921*, 263.

92. A. Ya. Kipnis, B. E. Yavelov, and J. S. Rowlinson, *Van Der Waals and Molecular Sciences* (Oxford: Clarendon Press, 1996), 108.

He used his equation of state for a single substance to describe a binary mixture by substituting the parameters of the substances a and b with a_x and b_x "which depend on the properties of the components and on the proportion in which they are mixed"; these new parameters depended on both the individual properties of the substances, and the molecular interactions of the two types of molecules.[93] While Gibbs described the geometrical properties of a general thermodynamic function, van der Waals was able to construct an actual thermodynamic surface based on his equation of state. It was this "happy synthesis" that allowed van der Waals to apply Gibbs' abstract insights and solve a problem that escaped even Maxwell.[94]

Van der Waals closely followed Gibbs' geometrical approach, but he used the ψ-function (the Helmholtz free energy), instead of the ζ-function.[95] Using the equation of state to express $p = f(T, V)$:

$$p = \frac{MRT}{V - b_x} - \frac{a_x}{V^2} \tag{5.4}$$

van der Waals found a closed-form expression for $\psi(x, V, T)$:[96]

$$\psi = -MRT \log(V - b_x) - \frac{a_x}{V} + MRT[x \log x + (1 - x)\log(1 - x)] \tag{5.5}$$

Using this relation for a fixed temperature, van der Waals was now able to construct a ψ-surface embedded in the space (ψ, x, V), where x was the ratio of the two components of the binary mixture. He used this surface and its projections on the three coordinate planes –

93. Waals, *J.D. van Der Waals*, 244. The mixture parameters were defined as:

$$a_x = a_1(1-x)^2 + 2a_{12}x(1-x) + a_2x^2$$
$$b_x = b_1(1-x)^2 + 2b_{12}x(1-x) + b_2x^2.$$

a_1, a_2, b_1, b_2 were the parameters of the individual substances, and a_{12}, b_{12} were the mutual attraction of the two types of molecules. Estimating these parameters was more challenging, and they did not receive a unique definition (see Kipnis, Yavelov, and Rowlinson, *Van Der Waals and Molecular Sciences*, 268).

94. Lorentz quoted in ibid., 111.

95. The ψ-function was defined as $\psi = \epsilon - t\eta$; after differentiation and the substitution of $d\epsilon$, $d\psi = -\eta dt - pdv + \mu_1 dm_1 + \ldots + \mu_n dm_n$.

96. J. D. van der Waals, "Théorie moléculaire d'une Substance composée de deux matiéres différentes," *Archives néerlandaises des sciences exactes et naturelles* 24 (1891): 11.

$(\psi, x),\ (\psi, V),\ (x, V)$ – to describe the coexistence and separation of phases.

The new theory of mixtures was first presented publicly during a lecture in February 1889. Van der Waals made use of a wooden model to show how coexisting phases could be easily determined by a rolling tangent plane, while the regions of instability would be situated in concave-shaped *folds* or *plaits* [*plooi*] (see Fig. 5.26). The complete miscibility of coexisting phases would be found by following the boundaries of these regions to a tacnodal point, i.e. a critical point. The source of inspiration for the model was most probably Maxwell who presented his thermodynamic model of a Gibbs' surface during an address at the International Exhibition of Scientific Apparatus in May 1876, in the South Kensington Museum in London. The exhibition had been attended by eight Dutch physicists, including two close friends of van der Waals – P. L. Rijke and J. Bosscha.[97]

Van der Waals' theory was published in full in the *Archives néerlandaises des sciences exactes et naturelles* in 1891, and his paper was followed by two related articles by the Dutch mathematician Diederik Johannes Korteweg (1848–1941), a friend and colleague of van der Waals at the University of Amsterdam.[98] In these articles, Korteweg provided a more detailed mathematical description of the formation of plaits and the coexistence of phases on the ψ-surface.[99]. Korteweg introduced the name "plait points" (*Faltenpunkte*, *point de plissure*, *plooipunt*) for what Maxwell and Cayley referred to as a "tacnodal points" (see Fig. 5.27). The choice was not hazardous – Korteweg considered that while "the name tacnodal point recalls

97. Kipnis, Yavelov, and Rowlinson, *Van Der Waals and Molecular Sciences*, 111-112. For a list of the Dutch contributors see *Handbook to the Special Loan Collection of Scientific Apparatus 1876*, xliii-xliv.

98. D. J. Korteweg, "La Théorie Générale Des Plis Et la Surface Psi de Van Der Waals Dans Le Cas de Symétrie," *Archives néerlandaises des sciences exactes et naturelles* 24 (1891): 295–368; D. J. Korteweg, "Sur les points de plissement," *Archives néerlandaises des sciences exactes et naturelles* 24 (1891): 57–98. "Sur les points de plissement" was the French translation of an article he published in German two years earlier – D. J. Korteweg, "Über Faltenpunkte," *Sitzungsberichte der Akademie der Wissenschaften Wien, Mathematisch-Naturwissenschafliche Klasse, Abteilung A* 2 (1889): 1154–1191.

99. For a detailed description of Korteweg's contribution to van der Waals' theory of mixtures see Johanna Levelt Sengers, *How Fluids Unmix : Discoveries by the School of Van Der Waals and Kamerlingh Onnes* (Amsterdam: Koninklijke Nederlandse Akademie van Wetenschappen, 2002), 59-85; Johanna Levelt Sengers and Antonius HM Levelt, "Diederik Korteweg, Pioneer of Criticality," *Physics Today* 55, no. 12 (2002): 47–53.

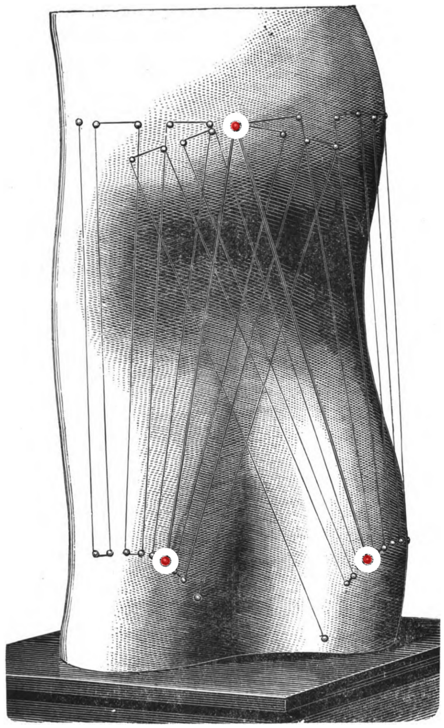


Figure 5.26
Van der Waals' model of a ψ-surface at constant temperature. The surface of the model was determined by the free energy ψ (its axis pointing into the paper), the volume (its axis pointing upwards) and the mixture ratio of two substances (its axis oriented left-right). The upper part of the surface represented the vapor phase, while the lower part represented the liquid phases of the two substances. Coexisting phases were determined by a rolling tangent plane, and represented on this model by the long wires. To make the relations more visible, I have superimposed on the original image the white circles corresponding to a vapor phase in equilibrium with two liquid phases. The coexisting phases are separated by *folds* or *plaits*, in this case by a transversal and a longitudinal fold. The concave part of the fold represents a region of thermodynamic instability. Source: J. D. van der Waals, "Théorie moléculaire d'une Substance composée de deux matiéres différentes," *Archives néerlandaises des sciences exactes et naturelles* 24 (1891): 28.

without question one of their characteristic properties, [...] the name plait point seems to me to indicate even better the nature of these points".[100] For George Salmon and Arthur Cayley, the two mathematicians that Korteweg referred to, a tacnodal point was a double point where two branches of a curve touched. While Cayley extended the notion to surfaces, the tacnode was still understood as a type of singularity point of a curve.[101] Korteweg, instead, regarded the plait points as the origin of the folds (plaits) of the surface – "one thinks, for example, at

100. Korteweg, "Über Faltenpunkte," 1154-1155; Korteweg, "Sur les points de plissement," 57-58.
101. A. Cayley, "On the Singularities of Surfaces," *Cambridge and Dublin Math. J* 7 (1852): 166–171.

the fall of the folds of a curtain".[102] Korteweg focused on describing the formation and disappearance of plait points of a surface under a "continuous deformation". In the second paper, these mathematical ideas were applied to van der Waals' ψ-surface. Korteweg looked at how a tritangent plane – an object of great interest for the physicists because these planes were determined by three coexistent phases (see the white circles in Fig. 5.26) – could appear or disappear under continuous transformations (Fig. 5.28).[103] He then classified all possible folds and plait points for a van der Waals symmetric mixture in terms of the relations between the various physical parameters (see Fig. 5.29).

While the analytical methods used by Korteweg – such as the Taylor expansion of singularity points – were not new, his minutiose use of drawings was unmatched. In the article on van der Waals' ψ-surface, Korteweg used forty-one in-text drawings (similar to Fig. 5.28) and three plates with twenty-eight diagrams (similar to Fig. 5.27). When dealing with tacnodal points Salmon and Cayley employed no diagrams. The graphical reasoning developed by Korteweg is particularly interesting because it was almost fully absent in the works of other mathematicians. Korteweg's work was well known and referenced by the "Dutch School of Thermodynamics" which made constant use of the concept and term of "plaitpoint".[104] In his Nobel Lecture, van der Waals mentioned that "my friend Korteweg, to whom I had communicated in broad outlines the outcome of my examinations, had studied the mathematical properties of these points [plaitpoints] and curves, a study which I have often found of great use".[105]

4.1 A familiar curve

More than a decade passed between the moment when van der Waals started working on the theory of mixtures after he received Gibbs' articles, and the full publication of his work in

102. Korteweg, "Über Faltenpunkte," 1154-1155; Korteweg, "Sur les points de plissement," 57-58.
103. Korteweg, "La Théorie Générale Des Plis Et la Surface Psi de Van Der Waals Dans Le Cas de Symétrie," 310-313.
104. Levelt Sengers, *How Fluids Unmix*, 61.
105. *Nobel Lectures, Physics 1901-1921*, 262.

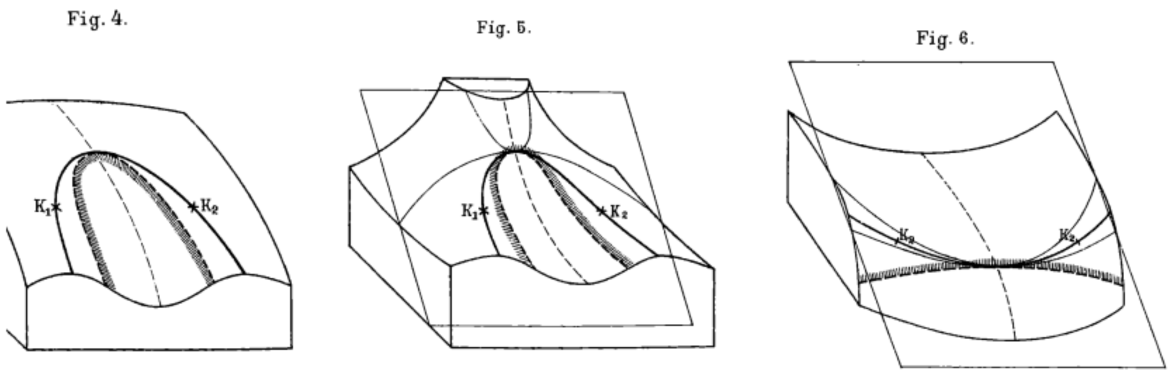


Figure 5.27
Three examples of plait points (tacnodal points in Maxwell and Cayley's terminology). The dashed line represents the spinodal (spinode) curve, and the full line represents the connodal or binnodal (node-couple) curve. Source: D. J. Korteweg, "Über Faltenpunkte," *Sitzungsberichte der Akademie der Wissenschaften Wien, Mathematisch-Naturwissenschafliche Klasse, Abteilung A* 2 (1889): 1154–1191.

1891. His work was not hindered by any technical difficulties, but by the death of his wife in 1881. Grieved by the loss, van der Waals showed no interest towards scientific research and publishing, and he stopped attending the meetings of the Academy of Science which until then he diligently attended.[106] Van der Waals' interest was rekindled by a strange graph presented at the Academy on 27 December 1884.

The communication was made by J. M. van Bemmelen, the chair of chemistry at the University of Leiden, about the recent findings of his doctoral student and assistant, H. W. Bakhuis Roozeboom.[107] Roozeboom had been working on the dissociation of various hydrates, and plotted the relation between the temperature and dissociation pressure. This type of graphical representation had become common among French chemists in the early 1870s, when an analogy was drawn between dissociation and vaporization.[108] Regnault's work was taken as an example of experimental research, and with it his graphical method of representing the

106. Kipnis, Yavelov, and Rowlinson, *Van Der Waals and Molecular Sciences*, 85, 92. If between 1880 and 1881 van der Waals published eleven articles, in 1882 and 1883 he published none, and only one short article in each of the following two years (see the list of publication in ibid., 290-292).
107. Ibid., 157-158.
108. F. Isambert, "Recherches sur la dissociation de certains chlorures ammoniacaux," *Annales scientifiques de l'École Normale Supérieure* 5 (1868): see pl. II.

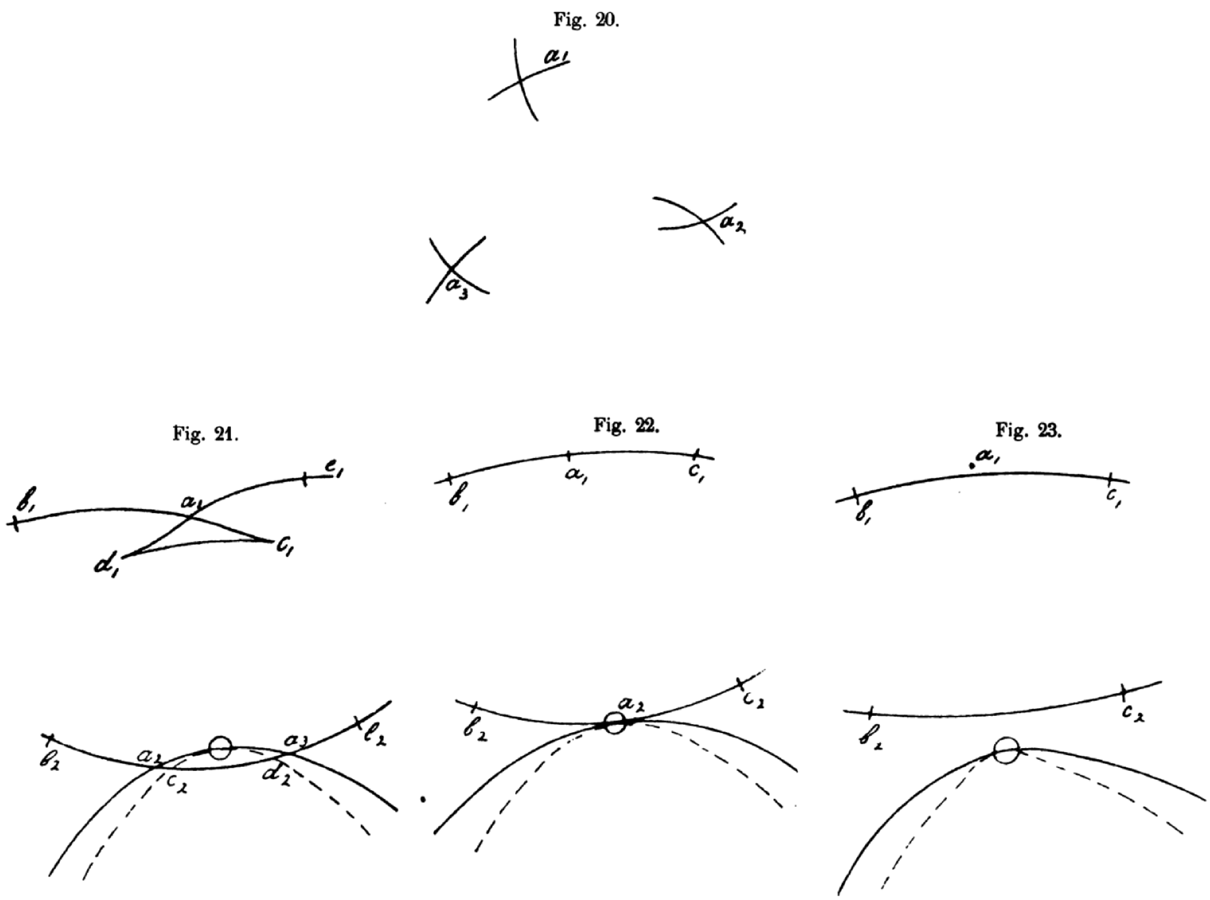


Figure 5.28
Fig. 20 represents three coexistent states a_1, a_2, a_3, similar to the white circles in Fig. 5.26. Korteweg then considered the various ways in which this tritangent plane could disappear under a continuous transformation. Figs. 21-23 present the series of transformations for one such case in which the surface was deformed until the states a_2 and a_3 coincided with the plait point (represented by a circle), after which the points become imaginary (Fig. 23). (The curves $b_1a_1c_1d_1e_1$ and $b_2a_2c_2d_3a_3e_3$ represent connodal curves (or what Maxwell called node-couple curves in Fig. 5.23), where $b_1b_2, c_1c_2 \ldots$ are coexistent states.). Source: D. J. Korteweg, “La Théorie Générale Des Plis Et la Surface Psi de Van Der Waals Dans Le Cas de Symétrie,” *Archives néerlandaises des sciences exactes et naturelles* 24 (1891): 310-313.

relation between pressure and temperature.[109] The interest in this type of representation was also boosted by Andrews’ experiments and his notion of “critical point” that was paralleled by the concept of “point critique de décomposition”.[110] Among the pressure-temperature plots for hydrate dissociation, Roozeboom stumbled upon the dissociation curve of the hydrate of

109. See Chapter 2.
110. For example: “C’est un phénomène un peu analogue à celui du point critique de certains gaz liquéfiables, une sorte de point critique de décomposition” (R. de Forcrand, “Recherches sur les hydrates sulfhydrés,” *Annales de chimie et de physique* 5, no. 38 (1883): 9).

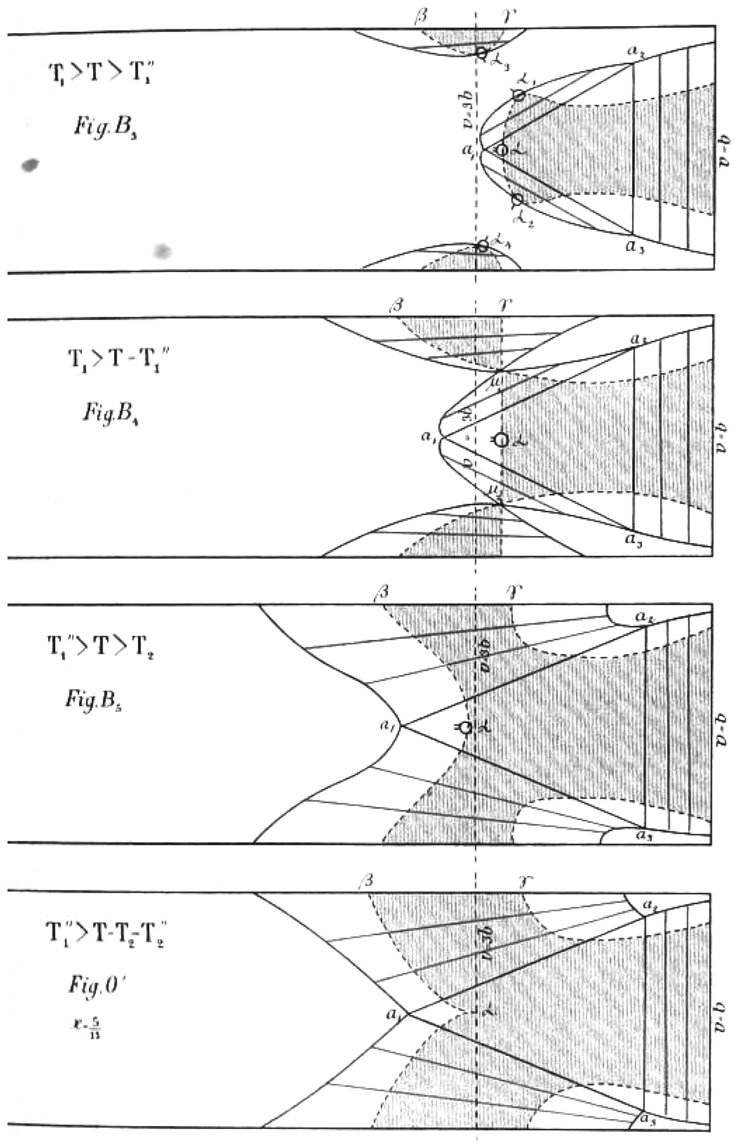


Figure 5.29
The full curves represent the connodal curves, the dotted lines are the spinodal curves, the grayed surface is the instability region, and the circles are the plait points. The straight lines between the connodal curves connect coexistent states. Such diagrams represent a projection on the plane Vx of van der Waals' model (Fig. 5.26). Source: D. J. Korteweg, "La Théorie Générale Des Plis Et la Surface Psi de Van Der Waals Dans Le Cas de Symétrie," *Archives néerlandaises des sciences exactes et naturelles* 24 (1891): 295–368.

hydrobromic acid which had three inflexion points. He carried various experiments to discard any potential interference, but in the end he had to conclude that "the parts FB and BL of the curve had to be considered as true tensions of dissociation" (see Fig. 5.30).[111]

111. H. W. Bakhuis Roozeboom, "Dissociation de l'hydrate HBr 2 H2O," *Recueil des Travaux Chimiques des Pays-*

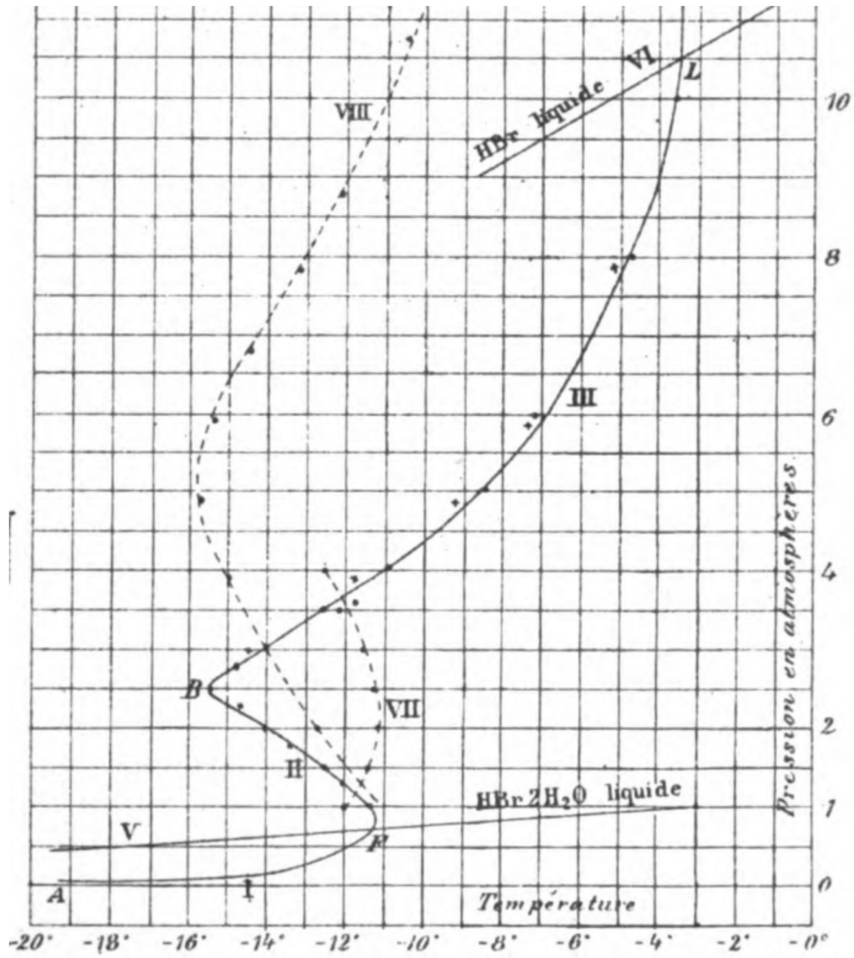


Figure 5.30
The curve that rekindled van der Waals' interest in physics. The unusual meandering of the curve III displayed a similar behavior to Andrews' isotherms (see Fig. 5.10). The tell sign was that a vertical line would have intersected the curve in three points. Source: H. W. Bakhuis Roozeboom, "Dissociation de l'hydrate HBr 2 H2O," *Recueil des Travaux Chimiques des Pays-Bas* 4, no. 4 (1885): 108–124.

During the December meeting at the Academy, van der Waals remained silent and asked no questions, but two months later he published a short explanation for the "surprising results obtained by M. Roozeboom".[112] The equation provided by van der Waals did not prove to be fully satisfactory for Roozeboom's experimental results, and an exchange of letters followed between the two. This interaction culminated in the mid-1886, when van der Waals

Bas 4, no. 4 (1885): 122.

112. Kipnis, Yavelov, and Rowlinson, *Van Der Waals and Molecular Sciences*, 158; J. D. van der Waals, "Influence de la température sur la richesse en gaz d'une solution et sur l'équilibre entre des solutions gazeuses et des hydrates solides," *Recueil des Travaux Chimiques des Pays-Bas* 4, no. 4 (1885): 135; J.M. van Bemmelen, "Het leven en de werken van H. W. Bakhuis Roozeboom in zijnen Leidschen tijd," *Chemisch weekblad*, 1907, 254.

went to Leiden to give a lecture on Gibbs' thermodynamics of phase equilibrium which was attended by van Bemmelen, Lorentz, Kamerlingh Onnes and Bakhuis Roozeboom.[113] The meeting proved to be crucial for the future careers of the last two scientists. The Gibbs' phase rule provided Bakhuis Roozeboom "with a theoretical basis for his experimental work, which he had begun to study in 1882".[114]

Van der Waals' theory of the Gibbs' phase rule played a similarly decisive role in the work of Heike Kamerlingh Onnes (1853-1926), a Dutch physicist remembered especially for the discovery of superconductivity and the liquefaction of helium for which he received the Nobel Prize in 1913. Starting in the 1880s, Onnes had been the professor of experimental physics and the director of the Physical Laboratory at the University of Leiden, and a close friend and collaborator of van der Waals whose theoretical studies inspired and guided his experimental research.[115] Pieter Zeeman, who studied at the University of Leiden and worked with Lorentz and Kamerlingh Onnes, stated that "without the guidance of van der Waals's theory the great undertaking of Kamerlingh Onnes would have failed from a lack of satisfactory knowledge".[116]

Throughout his career Onnes made extensive use of graphical methods based on van der Waals' ψ-surface. For example, because using the equation of state "by analytical processes is certainly exceedingly complicated even when it is feasible", Onnes hopped that "van der Waals' theory could be graphically solved [...] to determine numerically all the phenomena of condensation from the knowledge of a small number of constants".[117] For this purpose, he

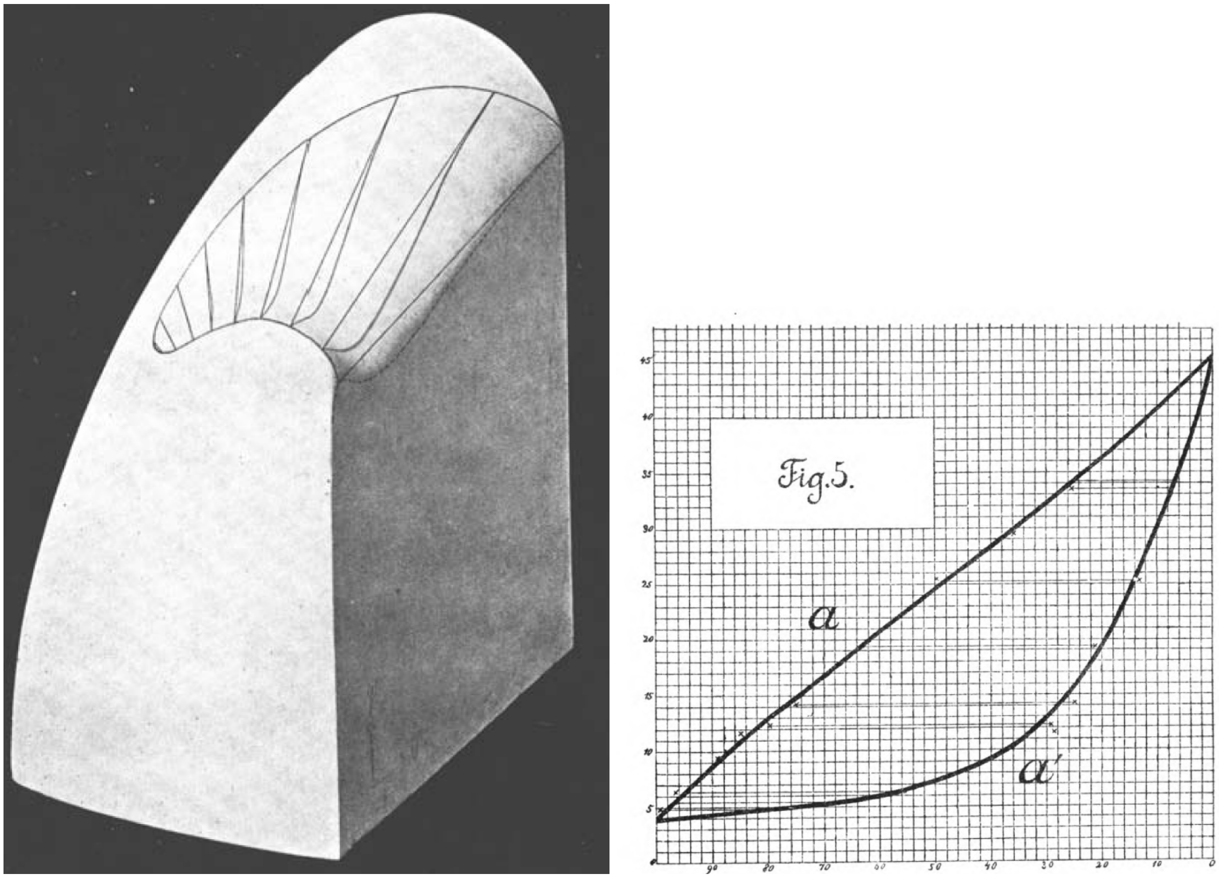
113. W. Stortenbeker, "Henri-Guillaume Bakhuis Roozeboom," *Recueil des Travaux Chimiques des Pays-Bas et de la Belgique* 27, no. 10 (1908): 370; Kipnis, Yavelov, and Rowlinson, *Van Der Waals and Molecular Sciences*, 159.

114. Klaas van Berkel, Albert Van Helden, and L. C. Palm, *A History of Science in the Netherlands : Survey, Themes, and Reference* (Leiden ; Boston: Brill, 1999), 407.

115. For a general overview of Kamerlingh Onnes' work see Simón Reif-Acherman, "Heike Kamerlingh Onnes: Master of Experimental Technique and Quantitative Research," *Physics in Perspective* 6, no. 2 (2004): 197–223; for a more in-depth study see Gavroglu and Goudaroulis, "Heike Kamerlingh Onnes' Researches at Leiden and Their Methodological Implications."

116. Zeeman quoted in Kipnis, Yavelov, and Rowlinson, *Van Der Waals and Molecular Sciences*, 156; Reif-Acherman, "Heike Kamerlingh Onnes," 202.

117. H. Kamerlingh Onnes, "Contributions to the Knowledge of Van Der Waals' Psi-Surface I. Graphical Treatment of the Transvere Plait," *Koninklijke Nederlandse Akademie van Wetenschappen Proceedings Series B Physical Sciences* 3 (1900): 276.



(a) (b)

Figure 5.31
One of Kamerlingh Onnes' thermodynamic plaster models and a diagram constructed from it by one his students. (a): H. Kamerlingh Onnes, "Contributions to the Knowledge of Van Der Waals' Psi-Surface I. Graphical Treatment of the Transvere Plait," *Koninklijke Nederlandse Akademie van Wetenschappen Proceedings Series B Physical Sciences* 3 (1900): 275–288; (b): Charles Marie Antoine Hartman, "Metingen omtrent de dwarsplooi op het Psi-vlak van Van der Waals bij mengsels van chloormethyl en koolzuur" (PhD diss., IJdo, 1899).

developed both a "*graphical method in a plane*" and a "*graphical method by the model*". Onnes aimed to construct accurate plaster models of van der Waals' ψ-surface from empirical data on which he would draw certain curves of interest; these curves would then be projected on different planes from which one could extract further numerical results (see Fig. 5.31). Onnes used the assistance of a local "modeller", Zaalberg van Zelst, to make his first plaster-casts which had a size of 30 x 20 x 40 cm and weighted 80 kg even when hollowed inside.[118] Because

118. Kamerlingh Onnes, "Contributions to the Knowledge of Van Der Waals' Psi-Surface I. Graphical Treatment of the Transvere Plait," 280.

the size "appeared too small for several constructions", a new cast was made with twice these dimensions.[119]

Onnes' models were not fully successful, both because of material difficulties in their construction and because the equations of state did not "give with sufficient accuracy the real behaviour of the pure substances and the mixtures".[120] In the end, Onnes had to settle for a less ambitious goal – instead of computational tools, the models became theoretical tools that allowed him to better understand and predict the consequences of van der Waals' theory:

> it appeared to me, as the number of the applications of van der Waals' theory increased, to become more and more desirable to know in detail the properties of the plait obtained, especially in the neighborhood of the plaitpoint, and to render the graphical construction of the connodal line, the tangent-chords and the condensation phenomena now more useful rather for explaining this theory than for calculating the numerical results of the observations from van der Waals' theory.[121]

The models still played a prominent role in Onnes' laboratory (see Figs. 5.32 and 5.33). Between 1900 and 1907, Kamerlingh Onnes and his students published a long series of sixteen articles on "Contributions to the Knowledge of van der Waals' ψ-surface". The new explanatory role of the diagrams was central for the scientific activity of the Dutch physicists (see Fig. 5.34).

5 CONCLUSION

I have chosen to end my inquiry at the moment when phase diagrams had become institutionalized in the practice and teaching of thermodynamics and physical chemistry because by this point the diagrams behaved as the *paper tools* previously analyzed by historians.[122] By the early 20th century students were inculcated in the use of phase diagrams through direct hands-on practice, such as those used in Onnes' laboratory in Leiden; the diagrams were further disseminated and adapted to solve new problems. However, the context in which the phase diagrams

119. Kamerlingh Onnes, "Contributions to the Knowledge of Van Der Waals' Psi-Surface I. Graphical Treatment of the Transvere Plait," 280.
120. Ibid., 276.
121. Ibid., 281.
122. See Kaiser, *Drawing Theories Apart : The Dispersion of Feynman Diagrams in Postwar Physics.*

Figure 5.32
Kamerlingh Onnes' Laboratorium, Leiden - 1902. Source: © Museum Boerhaave Leiden, Inventarisnummer: P13080.

Figure 5.33
Thermodynamic models from Kamerlingh Onnes's laboratory in Leiden - 1902. Source: © Museum Boerhaave Leiden, Inventarisnummer: P13014, P13015, V07617.

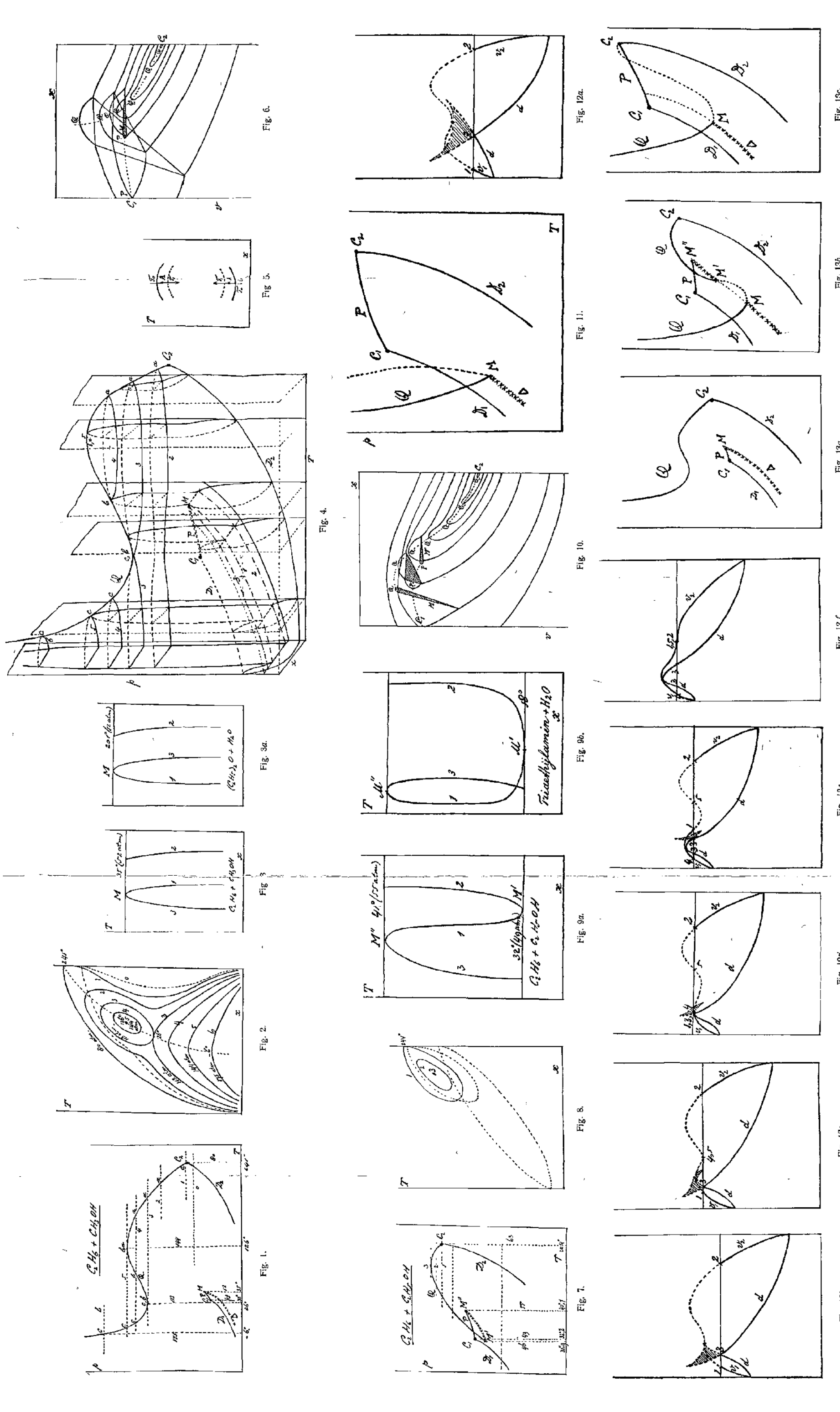


Figure 5.34
The explanatory role of phase diagrams at the "Dutch School of Thermodynamics". Source: J. J. Van Laar, "On the Different Forms and Transformations of the Boundary-Curves in the Case of Partial Miscibility of Two Liquids," *Proceedings of the Royal Netherlands Academy of Arts and Sciences* 7 (1905): 1904–1905.

developed in the second half of the 19th century is remarkably different. Instead of being part of localized and institutionalized pedagogical settings, the phase diagrams were of interest only to a handful of physicists. The dissemination of the diagrams was not limited because their use required special skills that could only be acquired through tacit knowledge and could only be transfered through personal contact. Rather, they were not yet a proper tool.

Because any purposeful manipulation of paper (such as scribbling, drawing, solving equations, etc.) can be described as form of paper tool or theoretical technology, we need a better way of distinguishing between the modes of operation of these manipulations. Instead of an external description (what it looks one is doing), I will provide an internal description (how one is thinking while acting). A paper tool or theoretical technology provides one with a "streamlined method for making calculations".[123] That is, once one has mastered the use of such a tool or technology they can *reliably* and *easily* transform a given input into a needed output through the quasi-mechanical repetition of a series of quasi-standardized steps. The essential feature of a tool or technology is that it needs to act on something which is *given* (a well-defined problem, a number, a curve, etc.). The act of using a tool or technology can be resembled to the *act of translating* or the *act of copying* because for such acts to take place something has to be given. Furthermore, the final products of these acts need to be *consistent* and *reliable*. A numerical table can be *translated* through a graphical representation into a curve. An indicator diagram can be *translated* into a value for the efficiency of an engine by measuring the area within a closed cycle. In such cases, the tool creates a correlation or correspondence (not necessarily one-to-one) between two objects.

In contrast to the paper tools, the use of the phase diagrams presented in this chapter is best resembled by an *act of writing*, that is something which is open-ended and innovative. The distinction is not immediately visible if one focuses exclusively on the manipulation of the paper, as both an act of copying and act of writing produce traces on paper. The act of

123. Kaiser, *Drawing Theories Apart : The Dispersion of Feynman Diagrams in Postwar Physics*, 43.

writing, instead of acting on an object which is given, inserts an object which has no previous correspondent. The act of writing is meaningful not because the written object is the translation of something given, but rather because it can be translated into something. Understood through this lens, the argument of this chapter can be summarized as such:

While Andrews' isotherms were a simple *translation* of the table of measurements into a graphical form, James Thomson's hypothetical curve was drawn through an act of writing. The construction was meaningful not because it corresponded to something real, there were no corresponding experimental measurements, but rather because it *could* correspond to something real. The *written curve* could guide future experiments by postulating a *possible* new state. Thomson's hypothetical curve, though unlikely to have been easily found experimentally because it was unstable, provided a confirmation for van der Waals' molecular theory because it matched the shape of the curve corresponding to his equation of state. Thomson continued using his approach of *writing* hypothetical curves by representing the triple point as the singular intersection of two curves rather than as a smooth point on a curve. These *written curves* allowed him to reinterpret the slight deviation of Regnault's experimental curves.

Inspired by Thomson's attempts, Gibbs aimed to formalize a system of writing out graphically physical states and processes equivalent to the writing of algebraic equations. If previous graphical methods in thermodynamics used diagrams as paper tools by *translating* some numerical values or the indications of an instrument into graphical traces, Gibbs "made no supposition in regard to the nature of the law, by which we associate the points of a plane with the states of the body."[124] His diagrams were not a translation of something given, but rather they were a system of signs which could be translated into equations or into meaningful statements about physical states and processes. Because Gibbs' aimed to specify a system of rules for how his graphical writing could be translated into physical states, Thomson's triple point had to "be regarded as a defect in these diagrams, as essentially different states are rep-

124. Gibbs, *The Scientific Papers of J. Willard Gibbs*, v.1, 8.

resented by the same point."[125] The writing system envisioned by Gibbs was transformed into a proper paper tool (or plaster model tool) by Maxwell, van der Waals and Onnes. These have showed how experimental results could be translated into the models, and then how the models could be manipulated and translated through projections into two-dimensional graphical representations which could be further translated into numerical values.

125. Gibbs, *The Scientific Papers of J. Willard Gibbs*, v.1, 25.

· 6 ·

Curves of Power

> There is a great advantage in using graphical methods over using algebraic methods in some cases. If, you make use of an empirical formula to express a physical law, you get a nice simple formula and it becomes your master, and you cannot believe anything that goes contrary to it; it seems to me, therefore, that a graphical method is always better than a purely empirical formula. (John Hopkinson)[1]

> I believe that empirical formulae are of tremendous use to people who are taking up some of the many investigations connected with dynamo machines. Nearly all the laws that I know of in connection with dynamos, compounding and so on, have been worked out algebraically, by simple algebraic formulae, by empirical formulae which were known to be very wrong indeed. Rules are worked out algebraically, and when we have obtained the rules it is easy to find a graphical method of employing these rules. The ordinary graphical methods which we know of were really discovered algebraically, and when Dr. Hopkinson gives the advice to young engineers to use only graphical methods I think he is giving advice that is a little misleading, and that he does not quite understand his own and their relative positions. (John Perry)[2]

While heated discussions as the one above were not a novelty at the Society of Telegraph Engineers and Electricians, the role embraced by the two protagonists is rather unexpected.[3] In one corner John Hopkinson, a "Senior wrangler" in the Mathematical Tripos examination at Cambridge (a title "synonymous with academic supremacy"); in the other corner John Perry who would condemn the Cambridge Tripos for sacrificing "myriads of people for the purpose of finding the one demigod" and who preferred to be known that "I belong to the very much greater body of men who may be called persons of average intelligence".[4] Because Hopkinson

1. John Hopkinson in Gisbert Kapp, "The Pre-Determination of the Characteristics of Dynamos," *Journal of the society of telegraph-engineers and electricians* 15 (1887): 569.

2. John Perry in ibid., 583-4.

3. When published in the journal of the Society the paper under discussion covered slightly over ten pages while the transcripts of the discussion took up another sixty-five pages.

4. For a description of the Cambridge Tripos as "synonymous with academic supremacy" see Warwick, *Masters of Theory : Cambridge and the Rise of Mathematical Physics*, 205; John Perry, "The Teaching of Mathematics," in *Discussion on the Teaching of Mathematics: British Association Meeting at Glasgow, 1901*, ed. John Perry (London, New York: Macmillan and Co., 1902), 4-7. For Perry's pedagogical program see Chapter 7.

employed the mathematical methods he had mastered in Cambridge to develop an analytical (or algebraic) theory for the alternating current generator, historian Graeme Gooday has described Hopkinson as "unyieldingly committed to the necessity of Wrangler methods":

> As one might have expected of one tutored in the rigor of the Cambridge Mathematics Tripos, Hopkinson emphasized the practice of solving all problems by first formulating differential equations to capture the material conditions, and solving these analytically to arrive at an all-encompassing algebraic expression.[5]

In contrast with Hopkinson, John Perry attended the Model National School in Belfast where "[d]rawing was taught well, but in the Science and Art Department way–descriptive geometry, mechanical drawing, shading in crayons, drawing from models, and even landscape painting".[6] Appointed in 1882 professor of mechanical engineering at Finsbury Technical College, Perry came to be known as an indefatigable advocate for the use of graphical methods and squared paper in the teaching of mathematics.[7]

Despite their backgrounds and pedagogical commitments, in the discussion of Gisbert Kapp's paper on "The pre-determination of the characteristics of dynamos" from November 1886, Hopkinson and Perry found themselves switching sides. Against all expectations, Hopkinson proclaimed that "a graphical method is always better than a purely empirical formula" while Perry retorted that "[r]ules are worked out algebraically" only after which one finds a graphical method for employing these rules: "[t]he ordinary graphical methods which we

5. Graeme Gooday, "Fear, Shunning and Valuelessness," in *Pedagogy and the Practice of Science*, ed. David Kaiser (Cambridge, Mass: MIT Press, 2005), 135-136; For Hopkinson's biography see T. H. Beare, "Hopkinson, John (1849-1898)," in *The Oxford Dictionary of National Biography* (Oxford: Oxford University Press, 2004).

6. Perry in *Report of the Special Committee on the Subjects and Modes of Instruction in the Board's Schools* (London: Hazell, Watson & Viney, 1888), 89. After leaving school, Perry apprenticed for seven years at the Lagan Foundry, and between 1868 and 1870 he attended the engineering classes of James Thomson at Queen's College in Belfast from where he graduated with an engineering degree. For Perry's biography see Graeme Gooday, "Perry, John (1850–1920)," in *The Oxford Dictionary of National Biography* (Oxford: Oxford University Press, 2004).

7. W. H. Brock and M. H. Price, "Squared Paper in the Nineteenth Century: Instrument of Science and Engineering, and Symbol of Reform in Mathematical Education," *Educational Studies in Mathematics* 11, no. 4 (1980): 365–381; for the program of engineering education at Finsbury College see W. H. Brock, "Building England's First Technical College: The Laboratories of Finsbury Technical College, 1878–1926," in *The Development of the Laboratory*, ed. Frank James (London: Macmillan, 1989), 155–170.

know of were really discovered algebraically".[8] Hopkinson's stubborn commitment in this discussion to graphical methods is even more surprising because only two years earlier, when prompted by a fellow engineer why he had not adopted a geometrical method to present his theory of alternators, Hopkinson declared that "for my own part I generally find that I can get along faster with the analysis, and therefore, as a rule, I naturally bring it into use". Ten years later, in a lecture on "the Relation of Cambridge Mathematics to Engineering", Hopkinson voiced almost the same position with Perry:

> my belief is that as a fact algebraic methods have been useful for discovery more frequently than geometrical. …discoveries are often made algebraically, and are afterwards translated into geometry.[9]

Hopkinson's public appraisal of algebraic methods attracted him the criticism of various "practical men" who felt the need to defend graphical methods from the monopoly of Cambridge mathematics: "The Cambridge man looks upon a graphical solution as a kind of foul play, and would as soon think of solving an equation by trial, or by omission of a term, or in any other reasonable way."[10] At the same time, though a staunch defender of algebraic methods, Hopkinson also introduced between 1879-1880 one of the most popular and powerful methods of late-19th century electrical engineering – the "characteristic curve" of a dynamo. The irony behind Hopkinson's commitments was not lost on his contemporaries, as it was pointed out by an editorial from *The Electrician*:

> Dr. Hopkinson himself, as an engineer, rather than a mathematician, gave us that invaluable diagram, which Marcel Deprez afterwards called a "characteristic curve". Let the honour of this discovery atone for all the shortcomings which we have attributed to the Cambridge school of mathematics in its relation to engineering.[11]

8. Perry in Kapp, "The Pre-Determination of the Characteristics of Dynamos," 584.
9. John Hopkinson, "The Theory of Alternating Currents, Particularly in Reference to Two Alternate-Current Machines Connected to the Same Circuit," *Journal of the Society of Telegraph-Engineers and Electricians* 13, no. 54 (1884): 556; John Hopkinson, "The Relation of Cambridge Mathematics to Engineering," *The Electrician* 33 (1894): 85.
10. "The Use and Abuse of Mathematics", Industries 10 (1891): 328; see Gooday, "Fear, Shunning and Valuelessness," 133.
11. Anonymous, "The Relation of Cambridge Mathematics to Engineering," *The Electrician*, 1894, 46.

How can one explain Hopkinson's double commitment to graphical and algebraic methods? Was this a strange case of Dr. Jekyll and Mr. Hyde, of mathematician by day and engineer by night? Graeme Gooday considered that Hopkinson, in his contribution to the alternator theory, tried to use techniques of graphical analysis to make his "Wranglerish" approach more palatable to the vast majority of engineers. It was "the demographic predominance of practitioners who protested against the glibness of his Cambridge methods" which forced Hopkinson towards the "accommodation of the analytical and practical traditions".[12] Analyzing his work on direct current dynamos, Stathis Arapostathis presented Hopkinson as "driven by practical problems and industrial needs towards a more rationalized practice"; these practical concerns led Hopkinson to initiate and rely on graphical methods which were probably used in his on-site activities.[13]

However, as this chapter will show, the value of graphical representations in the 19th-century study of electromagnetism cannot be reduced either to the background of training or to the context of practice. Instead, this chapter will show how graphical representations came to have a higher epistemological status.

1 THE LAW OF THE CURVE

1.1 Optimizing the Dynamo

One of Michael Faraday's experiments on induction from 1831 showed that an electric current was induced in a wire moving through a magnetic field. Though extremely simple in theory, it took almost four decades for this idea to be implemented in a mechanism viable

12. Gooday, "Fear, Shunning and Valuelessness," 139-41.

13. Stathis Arapostathis, "Dynamos, Tests, and Consulting in the Career of John Hopkinson in the 1880s," *Annales historiques de l'électricité* 5, no. 1 (2007): 22-23. However, as Arapostathis acknowledges, because none of his notebooks have been preserved it is difficult to know if and how Hopkinson actually employed the "characteristic curve" of dynamos in his engineering practice.

for industrial purposes.[14] The earliest magneto-electric machines relied on permanent magnets or electromagnets (energized from a battery) which generated magnetic fields that were neither strong nor permanent enough. The breakthrough came in 1866 when Henry Wilde used a "magneto-machine" with magnets to energize an "electro-magneto-machine" which could generate "enormous and unprecedented power".[15] The idea was immediately extended by Werner Siemens (January 1867) and Charles Wheatstone (February 1867) to construct dynamos that were "self-excited" such that part of the current that was produced could be circulated through the electromagnet.[16] It was enough to start with a small magnetic field to produce an initial current that would pass through the electromagnet, increasing the intensity of the magnetic field, and consequently increasing the current. This bootstrap effect could not go on indefinitely because at some point the intensity of the magnetic field in the core would be saturated and stop increasing despite the increasing currents.

Once the basic principle was set, the goal was to find the designs that maximized the energy output and the efficiency. What was the best way to loop the wires to form the armature of the dynamo? Which part of the dynamo should rotate - the magnet or the armature? What type of iron should be used for the core and how should it be shaped? What designs minimized magnetic leakage? How would the output power scale compare to the costs of production and the costs of coal? Any improvement was done at a cost. If one increased the magnetic field by rotating the dynamo faster, more mechanical power would have been needed; a larger iron core required increasing the size of the armature, which also increased the resistance, the manufacturing costs and the mechanical energy needed to rotate a larger dynamo. To know what was worth expanding, one had to know how the efficiency of the dynamo depended on all these factors. While in the case of telegraphic transmissions the task was that of *minimizing*

14. For a history of these first four decades see D. S. L. Cardwell, "On Michael Faraday, Henry Wilde, and the Dynamo," *Annals of Science* 49, no. 5 (1992): 479–487.

15. For a contemporary description see Charles Brooke, *The Elements of Natural Philosophy* (London: J. Churchilll and Sons, 1867), 512; for a description of various dynamos, including Wilde's, see Silvanus P. Thompson, *Dynamo-Electric Machinery: A Manual for Students of Electrotechnics* (London: E. & F.N. Spon, 1886).

16. Cardwell, "On Michael Faraday, Henry Wilde, and the Dynamo," 484-486.

waste and *maximizing* useful work, in the case of dynamo design and operation the task was that of *optimization*. If the minimizing-maximizing conditions had a straighforward moral and mathematical interpretation, the optimization problem remained uncertain on both grounds.[17]

Mechanical and naval engineers had been long confronted with similar optimization problems in the case of steam engines and steamships; the relation between size, output and efficiency was not trivial. Going against all expectations based on common sense, Isambard Brunel's steamship *Great Western* proved that the increase in tonage was not linearly proportional with an increase in water resistance. In some cases, intelligently designed waste (or an opposition to "unwise parsimony") could bring about greater benefits.[18] However, in the case of the dynamo a further problem arose. A dynamo machine could be advertised as capable of reaching a certain output and efficiency, but these quantities depended on the parameters at which it was used. In the case of dynamos, much more than in the case of steam engines and steamships, these parameters were highly variable because they depended on the electric network to which the dynamo was attached. John Hopkinson (1849-1898, FRS 1877), one of the leading British electrical engineers, phrased the problem in these terms:

> It is desirable to know what the various machines can do with varied and known resistances in the circuit, and with varied speeds of rotation: and what amount of power is absorbed in each case. It is a question of interest, whether a machine intended for one light can or cannot produce two in the same circuit, and if not, why not; whether a machine, such as the Wallace-Farmer, intended as it is for many lights, will give economical results when used for one; and so on. It is clear that the attempt to examine all separate combinations of so many variables would be hopeless, and that the work must be systematised.[19]

17. For the moral and mathematical interpretation of maximum-minimum conditions in the first half of the 19th century see Smith and Wise, *Energy and Empire*, 448-452; M. Norton Wise and Crosbie Smith, "Work and Waste: Political Economy and Natural Philosophy in Nineteenth Century Britain (I)," *History of Science* 27, no. 3 (1989): 263–301; M. Norton Wise and Crosbie Smith, "Work and Waste: Political Economy and Natural Philosophy in Nineteenth Century Britain (II)," *History of Science* 27, no. 4 (1989): 391–449; M. Norton Wise and Crosbie Smith, "Work and Waste: Political Economy and Natural Philosophy in Nineteenth Century Britain (III)," *History of Science* 28, no. 3 (1990): 221–261.

18. For the moral and economical competition between the steamships which embodied "reckless extravagance" or avoided "equally extravagance and parsimony" see Crosbie Smith, Ian Higginson, and Phillip Wolstenholme, ""Avoiding Equally Extravagance and Parsimony": The Moral Economy of the Ocean Steamship," *Technology and Culture* 44, no. 3 (2003): 443–469.

19. John Hopkinson, *Original Papers by the Late John Hopkinson*, 2 vols. (Cambridge: Cambridge University

Because early electrical networks were highly unstable and dynamos could not always function at fixed, optimal parameters, one had to optimize the output and efficiency for a wider array of circumstances. Hopkinson considered that

> to know the properties of any machine thoroughly, it is not enough to know its efficiency and the amount of work it is capable of doing; we need to know what it will do under all circumstances of varying resistance or varying electromotive force.[20]

Knowing the efficiency of a dynamo "under all circumstances" was a moral, technical and scientific problem. Manufacturers, consulting engineers, practicing engineers, and users were all interested in the accuracy of the measurements of the efficiency of dynamos.[21] What defined an accurate and trusted measurement depended on the relations between these actors.[22] The technical difficulty of developing on-site testing procedures was further amplified by a scientific challenge: at the time, there was no theory or law of the dynamo, something equivalent to Ohm's law for an electric circuit. The output power of the dynamo was the product of the electromotive force (i.e. the voltage) and the current through the circuit. For a dynamo made with a permanent magnet or an electromagnet that was fueled by a separate electric source, the electromotive force was constant for all resistances in the circuit (as long as the dynamo rotation velocity stayed constant). However, in the case of a self-excited magnet a change in the current produced a change in the electromotive force that would further change the current. The overall change was non-linear and could not be satisfactorily approximated by any theoretical or empirical formula.

John Hopkinson was in a unique position to tackle the challenge of the dynamo in its full complexity. Hopkinson had been a Senior Wrangler in the Mathematical Tripos examination at Cambridge and held a doctorate in science from the University of London.[23] His mathematical

Press, 1901), 32.

20. Hopkinson, *Original Papers by the Late John Hopkinson*, 72.

21. Arapostathis, "Dynamos, Tests, and Consulting in the Career of John Hopkinson in the 1880s," 29.

22. For the problem of accuracy and trust in the [practice of 19th century engineering see Graeme Gooday, *The Morals of Measurement: Accuracy, Irony, and Trust in Late Victorian Electrical Practice* (Cambridge University Press, 2004).

23. Beare, "Hopkinson, John (1849-1898)"; for Hopkinson's career as an engineer see Arapostathis, "Dynamos,

acumen was balanced by a fruitful career as a practicing engineer: Hopkinson worked for ten years at the optical works of Chance Brothers at Birmingham, and as a consulting engineer for various companies including the British Edison Company in London and the Mather and Platt Company in Manchester.

Hopkinson first presented a method for testing dynamos in 1879 in a paper for the Institution of Mechanical Engineers. At the time he had been working at the optical works of Chance Brothers where he had to deal with the design and the production of optical systems, and was interested to know "whether a [electrical] machine intended for one light can or cannot produce two in the same circuit, and if not, why not".[24] To this purpose he carried out a series of experiments on a medium-size Siemens machine for which he measured the voltage for different resistances and rotation velocities. These measurements were presented in a table and in a plot of the electromotive force (the voltage of the dynamo) and the current through the circuit (Fig. 6.1). Hopkinson only remarked in passing that

> The curve really gives a great deal more information than appears at first sight. It will determine what current will flow at any given speed of rotation of the machine, and under any conditions of the circuit, whether of resistances or of opposed electromotive forces. It will also give very approximate indications of the corresponding curve for other machines of the same configuration, but in which the number of times the wire passes round the electromagnet or the armature is different.[25]

As none of Hopkinson's notebooks have survived, it is not known if Hopkinson employed these graphical representations in his on-site activities.[26] However, from the above passage it is clear that Hopkinson recognized its important value as a practical tool for solving problems related to dynamo design and operation. Hopkinson's goal to "to know the properties of **any** machine thoroughly [...] **under all** circumstances of varying resistance or varying electromo-

Tests, and Consulting in the Career of John Hopkinson in the 1880s"; for Hopkinson's theory of the alternator see Gooday, *The Morals of Measurement*; for the role of his Cambridge education on his scientific work see Gooday, "Fear, Shunning and Valuelessness."

24. John Hopkinson, "On Electric Lighting," *Proceedings of the Institution of Mechanical Engineers* 30, no. 1 (1879): 238.

25. Ibid., 247.

26. Arapostathis, "Dynamos, Tests, and Consulting in the Career of John Hopkinson in the 1880s," 25.

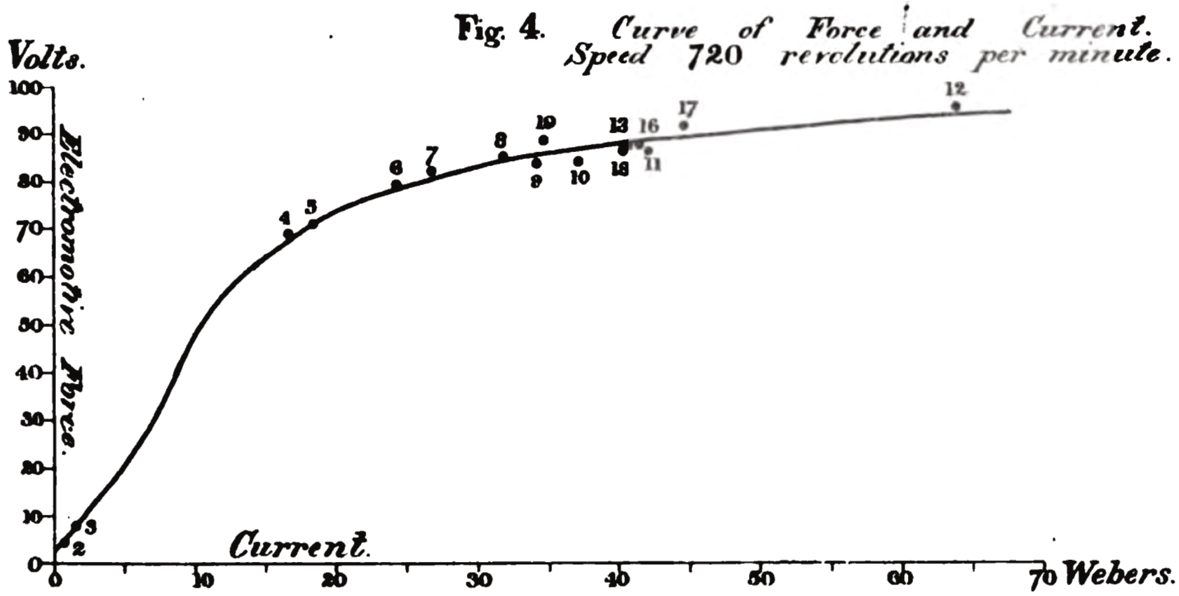


Figure 6.1
Hopkinson's curve of the electromotive force of a Siemens dynamo as a function of the current. Though at first sight the curve looks like a straightforward graphical representation of the experimental results, the curve would become an essential tool in the practice and teaching of engineering, and a bridge between experiment and theory, and engineering and scientific constraints. Source: John Hopkinson, "On Electric Lighting," *Proceedings of the Institution of Mechanical Engineers* 30, no. 1 (1879): pl. 29.

tive force" was satisfied by the ability of the curve to "determine what current will flow **at any** given speed of rotation of the machine, and under **any** conditions of the circuit". In the following years Hopkinson amplified these claims and presented the curve as "capable of solving almost **any** problem relating to a particular machine, and that it was also capable of giving good indications of the results of changes in the winding of the magnets, or of the armatures of such machines".[27] I have underlined "any" and "all" because these terms reveal a defining attitude of some scientific engineers for whom the solution of an engineering problem had to possess a certain *practical generality.*

In a follow-up paper from 1880 Hopkinson expanded on the actual use of the curve in the solution of practical problems. He presented two geometrical constructions – the first allowed one to determine the lowest speed at which a given machine could run and still be

27. Hopkinson, *Original Papers by the Late John Hopkinson*, 72; 44-45 – my underline.

able to produce an electric arc (see Fig. 6.2); the second explained the occasional instability of the electric light as produced by a dynamo.[28] While Hopkinson was clearly interested in the practical applications of his curve and most probably used it in his engineering practice, there is another dimension of Hopkinson's use of the curve that has so far remained unnoticed. To understand Hopkinson's intentions for the cure we must concentrate on his choices and not accept their utility at face value.

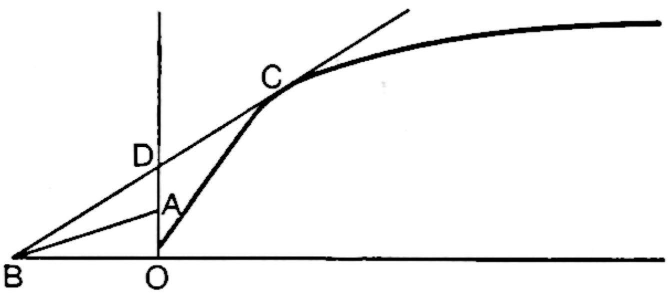


Figure 6.2
Problem: What is the minimum speed at which a machine can run and be capable of producing a short arc that needs a minimum of 20 volts?
Solution: Start by plotting the curve of the electromotive force as a function of the current, for a certain fixed speed. Construct OA such that it corresponds to 20 volts on the vertical axis. Construct OB on the horizontal axis such that $\frac{OA}{OB}$ equals the resistance of the circuit. From B you construct a tangent to the curve and cutting the vertical axis in D. Then the speed of the machine must be reduced in the ration $\frac{OD}{OA}$ (because the electromotive force is proportional to the speed). Source: John Hopkinson, *Original Papers by the Late John Hopkinson*, 2 vols. (Cambridge: Cambridge University Press, 1901), 55.

1.2 The characteristic curve of the dynamo

Both college-trained electrical engineers and "practical men" made extensive use of graphical methods in their everyday practice.[29] However, the publication of these graphical methods usually required some special circumstances.[30] For most engineers, the novelty behind Hop-

28. Hopkinson, *Original Papers by the Late John Hopkinson*, 54-55.
29. For the opposition between "practical men" (or "half-educated electricians") and the new generation of college-trained electrical engineers see Bruce J. Hunt, ""Practice vs. Theory": The British Electrical Debate, 1888-1891," *Isis* 74, no. 3 (1983): 341–355. For the use of graphical methods in the practice of electrical engineering see Ronald Kline, *Steinmetz: Engineer and Socialist* (Johns Hopkins University Press, 1992), 38-39gooday2005.
30. See Chapter 3 for an analysis of the publication of graphical representations in the early 19th century.

kinson's curve was not the plot itself, but the transformation of the plot into a tool suited for engineering practice. William Ayrton and John Perry, two engineers who made a pedagogical cornerstone out of the use of square paper and plots (see Chapter 7), confessed that:

> We had, both, in 1878 used a curve to express this relationship [between the electromotive force and the current], but it was not until 1881, when we met M. Deprez, and learnt of his work, that we had any conception of the many calculations which might be made by graphical methods, using the curve as a fundamental relation.[31]

The French engineer Marcel Deprez (1843-1918) was responsible for popularizing the practical applications of the dynamo curve which he named appropriately "la *caractéristique de la machine*"..[32] Deprez's interest in such graphical methods can be partially explained by his training and practice as an engineer. He had studied at L'École des Mines but never graduated. Throughout his career as an engineer he had been involved with using and perfecting various graphical methods and graphical instruments. Before he started working on electricity in the early 1880s, he had designed dynamometers and indicators for steam engines while developing his own graphical methods for analyzing the indicator diagrams; he also designed an integrometer based on the model of Amsler's famous planimeter that could easily measure the area, the curvature, the center of gravity and the moment of inertia of almost any plane figure.[33] Because of the striking similarity to an indicator diagram, it is easy to understand Deprez's interest in developing such a tool for the study of dynamos.

Deprez presented his graphical method in a communication to the Académie des Sciences from 1881.[34] At the time, Deprez was trying to show that the transport and distribution of elec-

31. W.E. Ayrton and John Perry, "The Magnetic Circuit of Dynamo Machines," *Philosophical Magazine Series 5* 25, no. 157 (1888): 230

32. Marcel Deprez, "Sur un mode de représentation graphique des phénomènes mis en jeu dans les machines dynamo-électriques," *Comptes rendus hebdomadaires des séances de l'Académie des sciences* 92 (1881): 1153.

33. For a comprehensive survey of Deprez's professional and academic activity see Girolamo Ramunni, "Deprez, Marcel (1843-1918). Professeur d'Électricité industrielle (1890-1918)," in *Les professeurs du Conservatoire national des arts et métiers, Dictionnaire biographique 1794 - 1955* (Paris: Institut national de recherche pédagogique, 1994), 405–418.

34. Deprez, "Sur un mode de représentation graphique des phénomènes mis en jeu dans les machines dynamo-électriques."

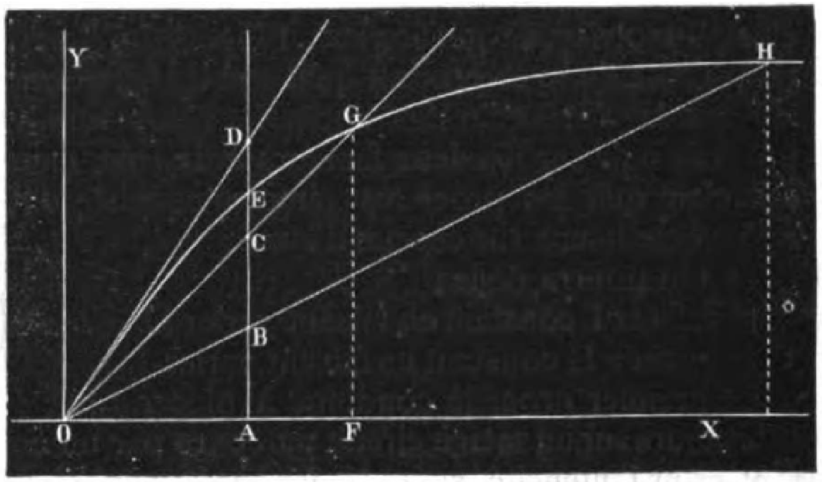


Figure 6.3
Deprez's geometrical analysis of a general characteristic curve. Eight such diagrams were included in the first article, but no experimental curves. Source: Marcel Deprez, "Transport et distribution de l'énergie par l'électricité," *La Lumiére électrique* 5 (1881): 326.

trical energy was a practical affair. With the help of Oskar von Miller, Deprez designed the first long-distance transmission of an electric current from Miesbach to Munich (57 km).[35] Deprez's presentation of his graphical representation employed the same reference to *diagrammatic holism* that we have previously identified in the case of Hopkinson. Deprez proudly claimed that he had found "a graphical technique of great simplicity that would allow the immediate calculation of the values of the current generated by an electro-dynamic machine under **all the possible conditions** for the velocity of the coil and the resistance of the external circuit"; the conclusion referred again to how "this very simple method permits the immediate solution of **all the questions** regarding the dyanmo-electric machines".[36] The short report to the Académie des Sciences was followed by a series of detailed articles in *La Lumière Electrique* in which the functioning of the dynamo was analyzed through a series of geometrical constructions similar to those of Hopkinson (Fig. 6.2) as seen in Fig. 6.3.[37] While initially Deprez only

35. Thomas P. Hughes, *Networks of Power: Electrification in Western Society, 1880-1930* (Baltimore: Johns Hopkins University Press, 1983), 131.
36. Deprez, "Sur un mode de représentation graphique des phénomènes mis en jeu dans les machines dynamo-électriques," my underline.
37. Marcel Deprez, "Représentation graphique des phénomènes qui s'accomplissent dans les machines dynamo-electriques," *La Lumière électrique* 4 (1881).

published his graphical method (which he exemplified on a general curve), he soon decided to also publish his experimental curves for various dynamos after he was insistently asked for the characteristic curves by various engineers (see Fig. 6.4): "Je dois donc penser que ces courbes présentent une utilité sérieuse et immediate pour les hommes spéciaux."[38] Because Deprez considered that "we can use the determined curves to construct tables much more exact than those obtained with interpolation formulas that are purely arbitrary", the tables he published did not contain the actual experimental results but rather the values interpolated from the curves.[39]

Courbe n° 9. *24 janvier 1882.*

CARACTÉRISTIQUE DE LA MACHINE GRAMME, TYPE A
(ORDINAIRE D'ATELIER)
A LA VITESSE DE 1,440 TOURS

Intensités en ampères.	Forces électromotrices en volts.	Intensités en ampères.	Forces électromotrices en volts.	OBSERVATIONS.
5	72	40	127	Poids de la machine, 210 kil.
10	107	45	125	
15	122	50	123	Résistance induit..... 0,410
20	127	55	120	— inducteur. 0,610
25	129	60	116	— totale..... 1,020
30	128	65	110	
35	128	70	101	

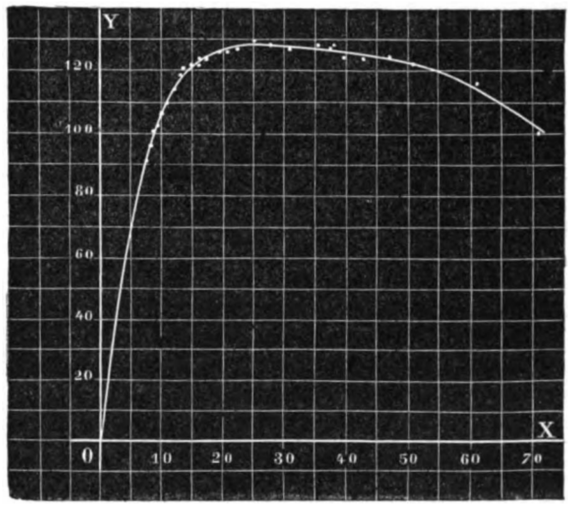


Figure 6.4
Deprez's experimental results for a Gramme dynamo. The table on the left included the values interpolated from the characteristic curve and not the direct measurements. Source: Marcel Deprez, "Recherches expérimentales sur les machines dynamo-électriques," *La Lumiére électrique* 6 (1882): 365.

1.3 The formula of the dynamo

Marcel Deprez's long series of articles in *La Lumière Electrique* was also prompted by the recent publication of some very similar results by the German physicist and engineer Oscar Frölich whose articles from the *Elektrotechnische Zeitschrift* had just been summarized and partially

38. Marcel Deprez, "Recherches expérimentales sur les machines dynamo-électriques," *La Lumiére électrique* 6 (1882): 364.
39. Ibid.

translated in *La Lumière Electrique* in 1881.[40] In these articles Frölich proposed his own theory of the dynamo which shared many features with that of Deprez and Hopkinson.

Compared to the French and British counterparts, German experimentalists often published graphical representations of their experimental results.[41] One of the most thorough experimental studies on dynamos was published by O. E. Meyer and F. Auerbach in 1879, a few months before Hopkinson's first article on the characteristic curve. The German physicists presented some very similar experimental results regarding the dependence of the current generated by a dynamo on the resistance of the circuit and the rotation velocity of the dynamo.[42] They also included four plots, all of which represented on the abscissa the physical quantities that were experimentally controlled (the resistance and the rotation velocity) against the quantities of interest (the current and the voltage) (see Fig. 6.5). However, the plots played a marginal role – they were only mentioned at the end of the paper and they were only meant to show in a more vivid manner the partial agreement between the experimental results and the predictions of an empirical formula.

Meyer and Auerbach did not use their plots to construct their own empirical formula as Frölich will do, but used an already established formula for the relation between the magnetization as a function of the magnetizing force. They first considered using Wilhelm Weber's theoretical formula which was based on his molecular theory magnetism, but their experimental setup did not allow them to determine all the constants present in the formula. In the end they settled on Jacob Müller's empirical formula which closely approximated Weber's theoretical formula and could thus be considered to be "not purely empirical [nicht als eine rein empirische]".[43] While Müller's formula provided a straightforward relation between the

40. Oscar Frölich, "Versuche mit dynamoelektrischen Maschinen und elektrischer Kraftübertragung und theoretische Folgerungen aus denselben," *Elektrotechnische Zeitschrift* 2 (1881): 134–41, 170–5.

41. For a detailed analysis of the use of graphical methods in Franz Neumann's seminar in Königsberg see Olesko, *Physics as a Calling: Discipline and Practice in the Königsberg Seminar for Physics.*

42. O. E. Meyer and F. Auerbach, "Ueber die Ströme der Gramme'schen Maschine," *Annalen der Physik* 244, no. 11 (1879): 494–514.

43. ibid., 502. Müller's and Weber's formulas will be more closely discussed in the next section.

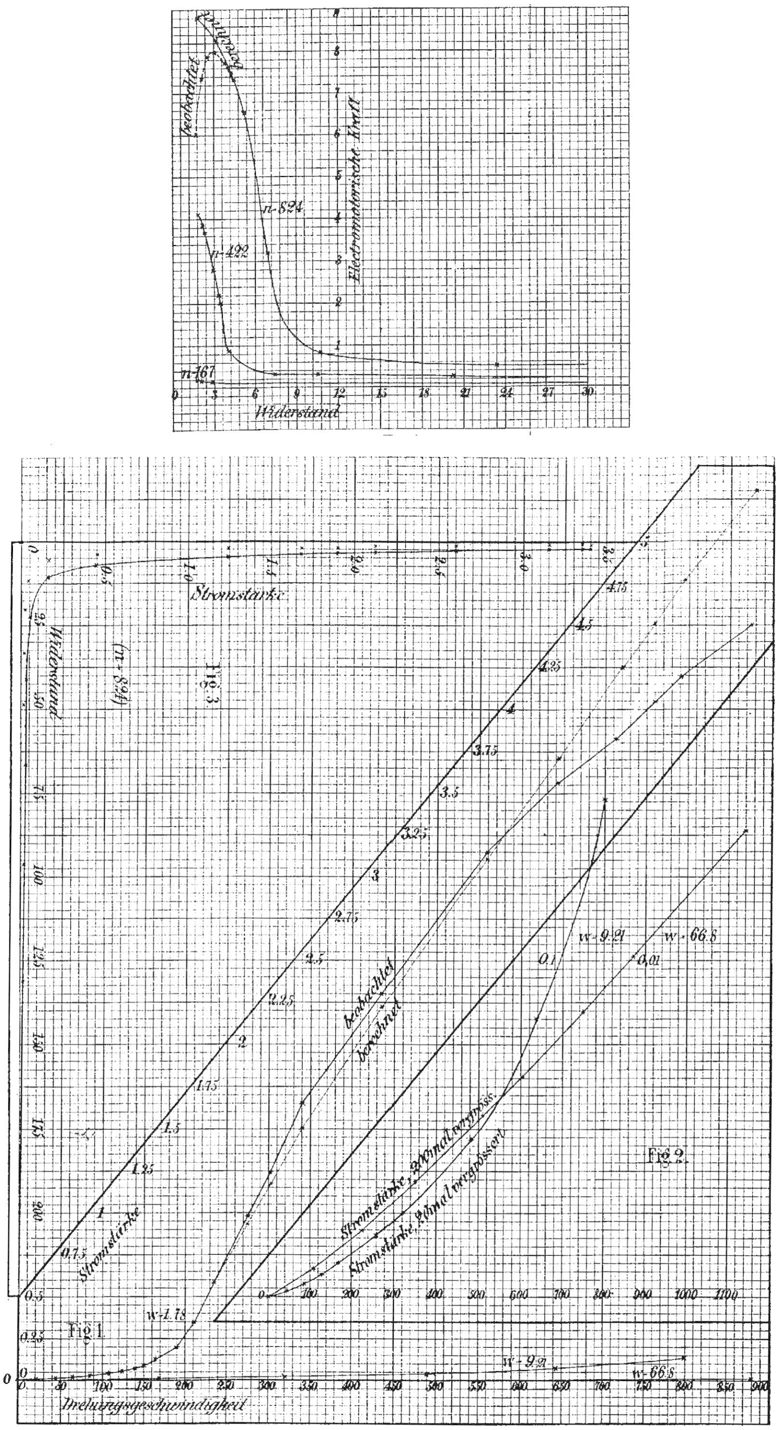


Figure 6.5
Meyer and Auerbach plotted the physical quantities that were experimentally controlled (the resistance and the rotation velocity) on the horizontal and the quantities of interest (the current and the voltage) on the vertical. The plots displayed the partial agreement between their experimental results and Müller's empirical formula. However, the irregular nature of the curves precluded them to propose any correction to the empirical formula. For a different approach of plotting these quantities see Fig. 6.6. Source:O. E. Meyer and F. Auerbach, "Ueber die Ströme der Gramme'schen Maschine," *Annalen der Physik* 244, no. 11 (1879): 494–514.

rotation velocity (v) as a function of the current (I):

$$v = \frac{aI}{m + \arctan I},$$

Meyer and Auerbach chose to plot the current as a function of the rotation velocity, a considerably less precise relation because they had to expand and approximate the *arctan* function. The extra steps taken by Meyer and Auerbach to adapt Müller's formula to their experimental data comes to further confirm that their initial choice of variables (the quantities of interest, the current and voltage, expressed as a function of the experimentally controlled quantities, the resistance and velocity) was not hazardous. For Meyer and Auerbach the plot was a reflection of their experimental results, and a mean of pointing out the limitations of empirical formulas such as that of Müller. While their plots could provide a comparison between their experimental results and Müller's formula, the irregular nature of the experimental curves precluded any further attempts to determine a more accurate empirical formula with which to fit the experimental data.

Oscar Frölich's approach was different. Instead of starting from the "experimental functions" (i.e. the quantities of interest expressed in terms of the experimentally controlled quantities) he reconsidered the theoretical relations between them. The electromotive force was determined by the rotation velocity and the magnetization of the iron core (Mv); Ohm's law connected the electromotive force to the current and resistance to give: $IR = Mv$.[44] Because the magnetization depended on the magnetizing current, $M = f(I)$. Put together the two equations led to $IR = vf(I)$ or:

$$\frac{v}{R} = \frac{I}{f(I)}$$

It was probably this relation that suggested to Frölich to plot the current as a function of v/R.

44. I have changed the notation for consistency. Frölich used J for the current, W for the resistance and v for the rotation velocity.

To his good luck, Meyer and Auerbach's complicated curves for $I(v)$ or $I(R)$ simplified to a curve that could be approximated to a line (see Fig. 6.6).[45] Another advantage, spelled out by Frölich, was that his choice of variables multiplied the number of data points compared to Meyer and Auerbach who had to plot $I(v)$ as separate curves for different resistances. Frölich confidently approximated the curves to a line: $\frac{v}{W} = a + bI$. This gave him an expression for the current, $I = \frac{1}{b}\left(\frac{v}{W} - a\right)$, but more importantly an expression for the magnetization as a function of the magnetizing current (or force):

$$M = \frac{I}{a + bI}.$$

Frölich's new empirical formula, or what he called "interpolation formula [Interpolationsformeln]", was a huge success. It was more precise than the previous formulas of Weber or Müller, but also simpler; its constants could be determined both easily and with a high accuracy. This was a remarkable result. Silvanus P. Thompson considered that:

> It is indeed extraordinary that such able physicists as Mascart and Angot, Mayer and Auerbach, Schwendler, and Herwig sought in vain for the true law to connect the electromotive force of the dynamo with its speed, the resistance of its circuit, and the constants of its construction. [...] The discovery is almost entirely due to Dr. Frölich...[46]

It was the change of variables that allowed Frölich to find a better empirical formula. In this case, the experimental curves were not a direct translation of the table of experimental measurements but a tool for searching and confirming the choice of variables which could simplify to a linear relation. The difference between these two ways of plotting experimental curves cannot be explained by a simple difference in background as both O.E. Meyer and Oskar Frölich had attended Franz Neumann's seminar in Königsberg (Meyer graduated in

45. In the following article Frölich emphasized the choice of variables and expressed the current explicitly as a function of v/R: $I = F\left(\frac{v}{R}\right)$, in Oscar Frölich, "Uieber die Theorie der dynamoelektrischen Maschine und über die elektrische Uebertragung und Vertheilung der Energie.," *Elektrotechnische Zeitschrift* 3 (1882): 69.

46. Silvanus P. Thompson, "On the Law of the Electromagnet and the Law of the Dynamo," *Philosophical Magazine Series 5* 21, no. 128 (1886): 267.

Fig. 1.

Fig. 2.

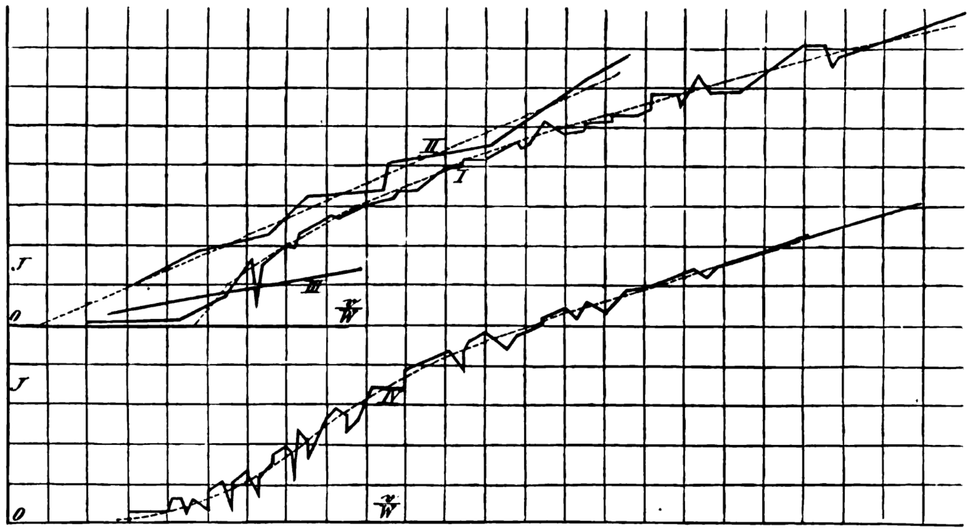


Figure 6.6
Frölich's breakthrough in finding a surprisingly accurate formula for the dynamo was made possible by his choice to compare not the immediate quantities which were experimentally measured (the current, rotation, resistance, magnetization, etc.) but rather a carefully chosen combination of them. Instead of plotting $I(v)$ as Meyer and Auerbach did (see Fig. 6.5), Frölich plotted $I(v/R)$ which reduced the experimental data to an almost linear relation. Source: Oscar Frölich, "Versuche mit dynamoelektrischen Maschinen und elektrischer Kraftübertragung und theoretische Folgerungen aus denselben," *Elektrotechnische Zeitschrift* 2 (1881): 136.

Fig. 18.

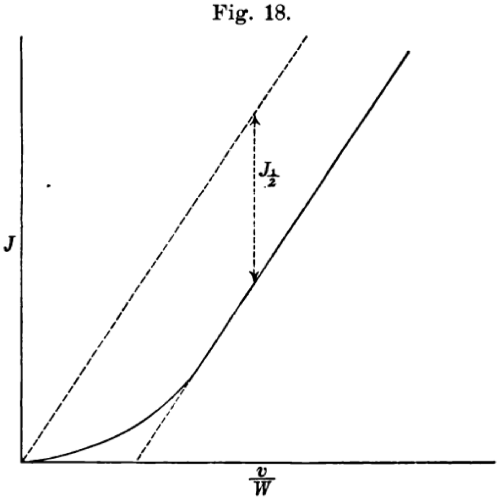


Fig. 25.

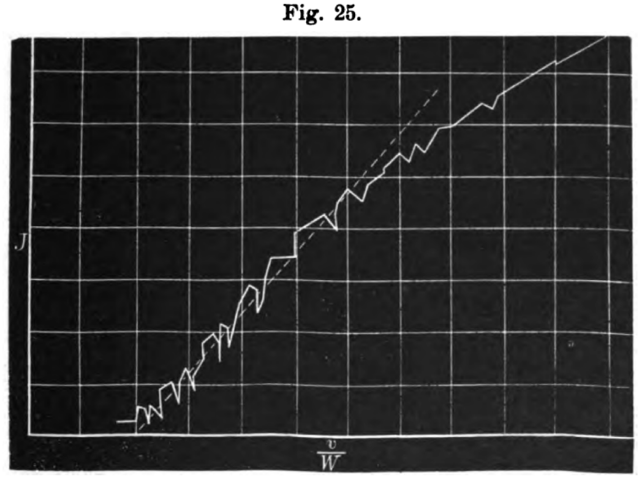


Figure 6.7
Frölich's "current curves" or *Stromkurven*. Though initially the curves were used only to find and confirm Frölich's new empirical formula, Frölich developed them into a pedagogical and practical tool. In his textbook on dynamos students were trained to associate particular physical meanings with certain features of the curves which they could then read on the experimental curves. Source: Oscar Frölich, *Die dynamoelektrische Maschine: eine physikalische Beschreibung für den technischen Gebrauch* (1886), 35-51.

1860, Frölich in 1868).[47] However, while Meyer followed an academic career (he had been appointed professor of physics at Breslau in 1866 where F. Auerbach was a Privatdocent), Frölich had been working as an engineer at the firm Siemens & Halske since 1873; while Meyer and Auerbach published their experimental findings in the *Annalen der Physik*, Frölich published in the *Elektrotechnische Zeitschrift*.

In the following years Frölich worked to consolidate the status of his theory of the dynamo, and with it, his graphical method. While in the first articles Frölich only used the curves to find and confirm his new formula, in his later work he developed the curves into a pedagogical and practical tool (see Fig. 6.7). The main motivation was the competing graphical method of Deprez and Hopkinson. Probably to counteract the influence of the "courbe caractéristique", Frölich chose to also coin the name "current curve" or *Stromkurve* for the curve of the function $I(v/R)$.[48] The importance of the curve in Frölich's theory of the dynamo was noticed by Silvanus Thompson who considered that Frölich's theory "is based upon (1) Faraday's law of induction, (2) Ohm's law, (3) a curve, called by him the current-curve, expressing certain results of experiments made on the series-wound dynamo."[49] Because of the role played by the "current curves", Thompson classified Frölich's approach as a "graphical method" similar to those of Deprez and Hopkinson.[50] However, the "graphical methods" of these three engineers could not have been more different. While Deprez developed a series of geometrical methods to analyze the characteristic curve, Frölich only used his curves to interpret algebraic equations and represent some of the empirical constants which otherwise could have been seen as meaningless or arbitrary (Fig. 6.7). More importantly, Frölich outrightly rejected the epistemological status that Deprez and Hopkinson tried to give to their curves.

47. For Meyer's and Frölich's training in Neumann's seminar see Olesko, *Physics as a Calling: Discipline and Practice in the Königsberg Seminar for Physics*; Jungnickel and McCormmach, *Intellectual Mastery of Nature*.

48. Frölich, "Uieber die Theorie der dynamoelektrischen Maschine und über die elektrische Uebertragung und Vertheilung der Energie.," 70.

49. Silvanus P. Thompson, *Dynamo-Electric Machinery: A Manual for Students of Electrotechnics* (E. & F.N. Spon, 1886), 3.

50. Ibid., 5-6.

Frölich considered that there were two "general methods" for solving the dynamo – a graphical and an algebraic method. However, the algebraic method had the advantage of being "more certain and precise [sicherere und genauere]", while the graphical method was prone to numerical errors because it was based on direct observations and the drawing of curves. Because of this,

> those who often deal with such problems should reject the graphical method and only do computations based on the knowledge of the constants of the machine; this is the shortest and most general method.[51]

Frölich admitted that Deprez's curves allowed one to tackle problems in an "easy" and "simple" manner, however they could not address "all questions".[52] This was an important point for Frölich which he repeated throughout his textbook on the dynamo-machine:

> A complete theory of dynamo machines, which allows one to answer **any arbitrary** [alle beliebigen] questions, can be established only through an analytical method and as its foundation an (empirical) formula must be found which represents the relation of the magnetism to the intensity of the current.[53]

1.4 The law of the dynamo

When Hopkinson's paper was discussed at the Institution of Mechanical Engineers in 1879, W. G. Adams–a professor of applied science at King's College–proposed a different graphical representation in which the resistance of the circuit was used for the abscissa and the electromotive force for the ordinates (a choice also employed by Meyer and Auerbach, see Fig. 6.5). This would have been an immediate choice for an experimentalist because the resistance was the variable in the experiment, though in this case, Adams' choice was motivated by a practical concern. For this graphical representation the tangent to the curve would determine the current through the circuit, and through a simple geometrical construction one could obtain

51. Oscar Frölich, *Die dynamoelektrische Maschine: eine physikalische Beschreibung für den technischen Gebrauch* (1886), 46-47.
52. Ibid., iv.
53. Ibid., 11, my underline.

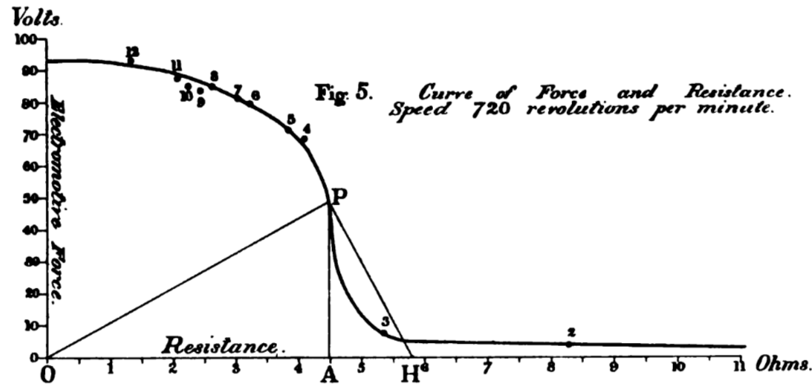


Figure 6.8
The electromotive force as a function of the resistance. The diagram was analyzed geometrically: AH represented the heat produced by a current equal to AP/AO, because H was constructed such that OP and OH would be perpendicular. Source: John Hopkinson, "On Electric Lighting," *Proceedings of the Institution of Mechanical Engineers* 30, no. 1 (1879): Pl. 29.

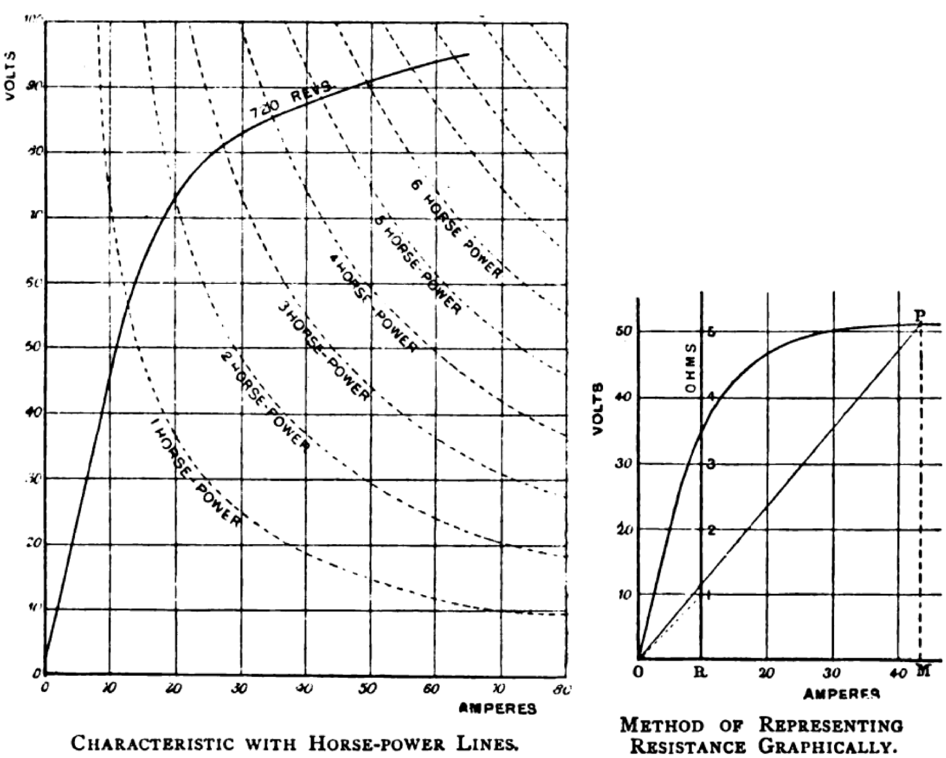


Figure 6.9
Graphical analysis applied to the characteristic curve. In the geometrical analysis of Fig. 6.8, the objects of interest are segments that represent numerically physical quantities without any consideration for the units. In the graphical analysis, the objects of interest are points and curves, and the physical quantity is read by locating the point in a grid of coordinates. Source: Silvanus P. Thompson, *Dynamo-Electric Machinery: A Manual for Students of Electrotechnics* (E. & F.N. Spon, 1886), 361; 369.

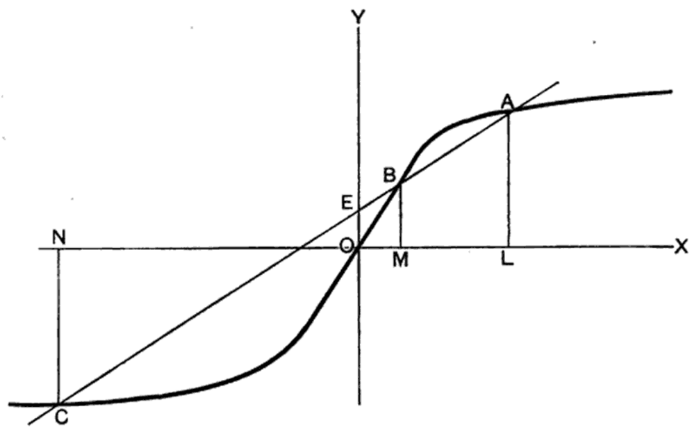


Figure 6.10
Hopkinson showed that the characteristic curve was symmetrical with respect to the origin, in contrast with Frölich's empirical formula $E = aI/(1 + bI)$. Source: John Hopkinson, *Original Papers by the Late John Hopkinson*, 2 vols. (Cambridge: Cambridge University Press, 1901), 73.

the output power and the dissipated heat (Fig. 6.8). In his reply to Adams, Hopkinson defended his choice of coordinates because his diagram could "almost be said to be an indicator diagram, inasmuch work done was represented by area. For this reason, *from an engineering point of view*, there was a certain appropriateness in having the diagram in that shape".[54] No extra geometrical constructions were needed because one could simply multiply the coordinates to get the power output.[55]

Hopkinson mentioned three other reasons for preferring his choice of coordinates. First, his curve better handled the display of the experimental values because the points 4-12 were not crowded in such a small region of space; in Adams diagram point 1 did not even fit in the resistance range. Second, there was a *physical reason*: the electromotive force truly depended on the current circulating around the magnets no matter how this current was regulated. As we will see next, this was an important reason for Hopkinson because it allowed him to connect the behavior of the dynamo to its underlying physics – the dependence of the magnetization of

54. Hopkinson, "On Electric Lighting," 261; my underline.
55. Adams analysis was purely geometrical, and the physical meaning of the actual curves was lost. For example, AH represented the heat produced by the current due to the electromotive force along the abscissa that represented the resistance. Notice that in Adams' diagram the curve and the axes are not even necessary. I say that Adams had a "geometrical" interpretation of the figure (opposed to a graphical/physical interpretation) because he only considered the relation between the lengths of the segments, and not the units. Cf. Fig. 6.9

the iron core on the magnetic field of the magnet. Third, "*from a mathematical point* of view his own curve was most appropriate" because in Adams' curve "the resistance, represented by the abscissa, was a thing which could have only a positive value, not a negative value".[56] However, "the line of the abscissae was an essentially reversible line, and they ought to be able to produce that line backwards, and draw the corresponding curve on the other side of the origin".[57] This objection did not apply to his curve, and Hopkinson constructed such a plot a year later (Fig. 6.10). Hopkinson also used this line of reasoning to dismiss Frölich's empirical formula $E = aI/(1 + bI)$ because he considered that the curve had to be odd (i.e. symmetrical with respect to the origin) and consequently "there should be a point of inflexion in the characteristic curve at the origin".[58]

Besides these four reasons that Hopkinson explicitly articulated there was something else that seemed to motivate his decision to draw such a curve in the first place. Though Hopkinson did not fit any empirical or rational formulas to his curve, he did draw a theoretical connection to Weber's theory of magnetism and Weber's formula (Fig. 6.11). This point was also emphasized at the beginning of the discussion of the paper where Hopkinson specified that

> He also thought a sufficiently accurate formula for the curve in question might probably be based upon Weber's theory of induced magnetism in iron, as he had suggested at the end of the paper. It would be a formula with only two constants in it, so that a full description could be given briefly of all the curve had to tell.[59]

As Meyer and Auerbach before him, Hopkinson did not make any attempt to plot Weber's formula on his experimental plot probably because the coefficients of the formula could not have been easily estimated. What made Hopkinson confident that "[w]e should naturally

56. Hopkinson, "On Electric Lighting," 262; my underline.
57. Ibid., 262.
58. John Hopkinson and Edward Hopkinson, "Dynamo-Electric Machinery," *Philosophical Transactions of the Royal Society of London* 177 (1886): 331-332.
59. Hopkinson, "On Electric Lighting," 250. The passage was in the third person because it was a rendition of the discussion which followed the paper.

According to Weber's theory of induced magnetism, as set forth in Maxwell's *Electricity*, vol. II., if X be the magnetising force and I the intensity of magnetisation,

$$I = \tfrac{2}{3} a \frac{X}{b}, \text{ until } X \text{ rises to the value } b,$$

$$\text{and } I = a\left(1 - \tfrac{1}{3}\frac{b^2}{X^2}\right), \text{ if } X > b,$$

where a and b are constants. We should naturally expect that a similar formula would be approximately applicable to dynamo-electric machines.

Figure 6.11
Hopkinson referencing Weber's theory of magnetism as presented in Maxwell's *Treatise on Electricity and Magnetism.* The reason why "[w]e should naturally expect" that a similar formula to that of Weber would be applicable to the theory of the dynamo was because Hopkinson's curve closely resembled the graphical representation of Weber's formula as reproduced in Maxwell's treatise (see Fig. 6.12). Source: John Hopkinson, "On Electric Lighting," *Proceedings of the Institution of Mechanical Engineers* 30, no. 1 (1879): 248.

expect that a similar formula [to that derived from Weber's theory] would be approximately applicable to dynamo-electric machines"?[60] Hopkinson probably considered that his experiments on dynamos could be connected to Weber's theory of magnetism because both produced remarkably similar curves. Maxwell's *Treatise on Electricity and Magnetism*, which Hopkinson referenced as his source for Weber's theory and formula, also provided a graphical representation for the magnetization as a function of the magnetizing force (Fig. 6.12).[61] While Hopkinson did not directly comment on their similarity, it is probable that the curve in Maxwell's *Treatise* assured him that he might be able to find a theoretical and physical interpretation for his own curve.

Though Hopkinson seemed to be hopeful that a law for the dynamo could be derived from theoretical principles, he never proposed such a formula in any of his papers. This striking omission should come as a surprise. Empirical and rational formulas were the bread and butter of engineers and physicists; almost everyone involved in the study of dynamos put forward a

60. Hopkinson, "On Electric Lighting," 248.
61. Magnetization was usually preferred by the German physicists, while the British mostly used magnetic induction. But, in modern parlance, both $M(H)$ and $B(H)$ had the same behavior and they could be used to derive $E(I)$ because B determined E and I determined H. Notice that in the Maxwell selection (Fig. 6.12) the notation is different - X represents the magnetizing force H, and I or M represent the magnetization.

When X is less than D, $\qquad I = \frac{2}{3}\frac{mn}{D}X.$ (5)

When X is equal to D, $\qquad I = \frac{2}{3}mn.$ (6)

When X is greater than D, $\qquad I = mn\left(1 - \frac{1}{3}\frac{D^2}{X^2}\right);$ (7)

and when X becomes infinite $\qquad I = mn.$ (8)

According to this form of the theory, which is that adopted by Weber*, as the magnetizing force increases from 0 to D, the magnetization increases in the same proportion. When the magnetizing force attains the value D, the magnetization is two-thirds of its limiting value. When the magnetizing force is further increased, the magnetization, instead of increasing indefinitely, tends towards a finite limit.

* There is some mistake in the formula given by Weber (*Trans. Acad. Sax.* i. p. 572 (1852), or Pogg., *Ann.* lxxxvii. p. 167 (1852)) as the result of this integration, the steps of which are not given by him. His formula is

$$I = mn\frac{X}{\sqrt{X^2 + [illegible]}}\,\frac{X^4 + \frac{7}{8}X^2D^2 + \frac{8}{8}D^4}{X^4 + X^2D^2 + D^4}.$$

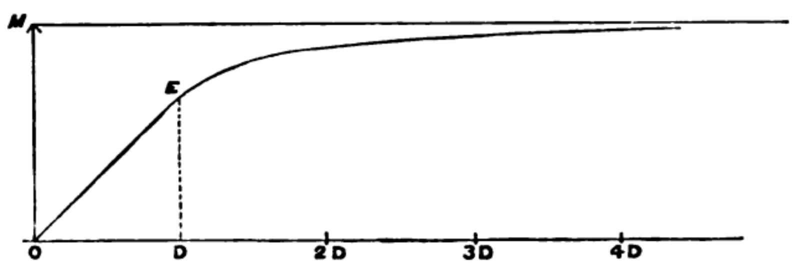


Fig. 7.

The law of magnetization is expressed in Fig. 7, where the magnetizing force is reckoned from O towards the right and the magnetization is expressed by the vertical ordinates. Weber's own experiments give results in satisfactory accordance with this law. It is probable, however, that the value of D is not the same for all the molecules of the same piece of iron, so that the transition from the straight line from O to E to the curve beyond E may not be so abrupt as is here represented.

Figure 6.12
The pages in Maxwell's *Treatise* referenced by Hopkinson (see Fig. 6.11). It was probably Maxwell's curve which made Hopkinson hope that he might connect his characteristic curve of the dynamo to Weber's theory and formula of magnetism. Source: James Clerk Maxwell, *A Treatise on Electricity and Magnetism* (Clarendon Press, 1873), vol.2, 78-79.

formula or made use of one.[62] Silvanus P. Thompson credited Hopkinson with coming close to discovering the "law for the dynamo" but stopping short because "he did not state the law of the machine algebraically, and apparently he accepted as true Weber's formula for the electromagnet". Instead, Thompson believed that "the discovery is almost entirely due to Dr. Frölich".[63] So why did Hopkinson not put forward an algebraic relation?

A potential explanation is offered by some comments made by Hopkinson in 1886 during a discussion of a paper by Gisbert Kapp, one of the leading electrical engineers in Britain. Kapp's paper presented a method of predetermining the characteristic curve of a dynamo. While Kapp's approach was mainly graphical, he did make use of an empirical formula to express the magnetization of iron. This prompted Hopkinson to make the following revealing remarks:

> ...I think that both the graphical and algebraic methods have their proper place. There is a great advantage in using graphical methods over using algebraic methods in some cases. If, you make use of an empirical formula to express a physical law, you get a nice simple formula and it becomes your master, and you cannot believe anything that goes contrary to it; *it seems to me, therefore, that a graphical method is always better than a purely empirical formula.* At Cambridge, the rule is in a part of the mathematical tripos that you are only to use geometrical methods. But candidates soon learn that they can get round that rule by working out the problems by powerful analytical methods, and then translating to geometrical methods. So it is here that, instead of being under any hard and fast rule, we can do as we like and get the advantage of a combination of analytical with graphical methods. *I do feel very strongly that the use of empirical formulae, such for example as that introduced by Frölich for the characteristic curve of a dynamo, is full of danger. One presently begins to think that the empirical formula, which is only intended to be used as an expression of the results which have been obtained by experiment, is really a law of nature.*[64]

It is worth pausing over this quote because it holds several crucial distinctions. In this passage Hopkinson advocated for the dual use of graphical and algebraic methods, without subordinating one method to the other because each had its own advantages for the solution of

62. Some famous examples are: William Ayrton and John Perry, Oskar Frölich, Silvanus P. Thompson, Gisbert Kapp, Marcel Deprez, etc. There were also various empirical and rational formulas for the magnetization of a piece of iron (Weber, Lamont, J. Müller, etc.) that could have been used to derive a (rather imprecise) formula for the dynamo. See Thompson, "On the Law of the Electromagnet and the Law of the Dynamo."

63. Ibid., 3.

64. Hopkinson in Kapp, "The Pre-Determination of the Characteristics of Dynamos," 569, my underline.

certain practical problems. On several occasions Hopkinson did acknowledge his preference for algebraic methods, but mostly as a personal choice. For example, after Hopkinson presented a more mathematical paper with several demanding equations, an engineer expressed his "wonder that Dr. Hopkinson has not adopted the geometrical method of exhibiting some of the results which he has brought out in his paper".[65] Hopkinson replied that

> to many, a geometrical treatment is easier than an analytical one; for my own part I generally find that I can get along faster with the analysis, and therefore, as a rule, I naturally bring it into use; but, for all that, I think to have the thing put from two points of view is a very great advantage.[66]

Almost ten years later, he confessed that his "belief is that as a fact algebraic methods have been useful for discovery more frequently than geometrical. [...] discoveries are often made algebraically, and are afterwards translated into geometry". However, "mathematicians should be ambidextrous – equally ready with algebra and geometry".[67] It should be clear from these passages, that Hopkinson (like most engineers) did not have any strong and dogmatic commitments to one method or technique.[68]

The preference for a method was not unconditioned. For Hopkinson, a physical law was to be preferred to an empirical law, and this trumped the choice of the method. Hopkinson's opposition against empirical formulae was also shared by some of his associates, such as James Swinburne or J. A. Fleming.[69] Hopkinson's disregard for empirical formulae was immediately rebuffed by John Perry, a much more practical engineer:

65. Hopkinson, "The Theory of Alternating Currents, Particularly in Reference to Two Alternate-Current Machines Connected to the Same Circuit," 542.
66. Ibid., 556.
67. Hopkinson, "The Relation of Cambridge Mathematics to Engineering," 85.
68. Cf. Gooday, "Fear, Shunning and Valuelessness."
69. In the same discussion, Swinburne said that: "I have no faith in such formulae as Fröhlich's or the inverse tangent, because they have obviously no sort of connection with the phenomena. [...] It is no answer to say that such formulae fit the results; nothing is easier than to get results to agree with empirical formulae, and a man cannot help choosing the data that fit his theory. In dealing with a man of science verifying his own theory, it must be remembered that his coefficient of involuntary mendacity is about equal to that of an inventor", Swinburne in Kapp, "The Pre-Determination of the Characteristics of Dynamos," 541. Hopkinson's former assistant, J. A. Fleming also accepted that: "it is evident that his graphic method affords a better and more scientific method for the predetermination of the characteristic curves than the use of an empirical formula", Fleming in ibid.

> I believe that empirical formulae are of tremendous use to people who are taking up some of the many investigations connected with dynamo machines. *Nearly all the laws that I know of in connection with dynamos, compounding and so on, have been worked out algebraically, by simple algebraic formulae, by empirical formulae which were known to be very wrong indeed. Rules are worked out algebraically, and when we have obtained the rules it is easy to find a graphical method of employing these rules. The ordinary graphical methods which we know of were really discovered algebraically, and when Dr. Hopkinson gives the advice to young engineers to use only graphical methods I think he is giving advice that is a little misleading, and that he does not quite understand his own and their relative positions.* When a senior wrangler uses a geometrical method he uses along with it, instinctively, unconsciously, a number of other mathematical methods of working; but an ordinary practical electrical engineer will find far more advantage in using an empirical formula, even though that empirical formula is slightly wrong, than in using the graphical method. I should advise the use of that formula which is called Frölich's... If anybody expects to get very much more exactness than the formula will give him in the predetermination of a dynamo, I think that he will be disappointed. .[70]

In this case, the crucial distinction between Perry and Hopkinson was not a disagreement regarding the *practical* methods for solving an engineering problem. It was mainly a difference of purpose. Hopkinson aimed to derive a "law of nature", rather than a practical empirical formula. Kapp's paper from 1885 prompted Hopkinson to publish his own "Theoretical Construction of a Characteristic Curve". Opposed to Kapp who used some heuristic empirical formulae, Hopkinson's approach was based on the "ordinary laws of electro-magnetism and the known properties of iron" and "without any further assumptions". Though Hopkinson's example was based on simplifying approximations, he claimed that his theory was both scientific and practical because "a sufficiently powerful and laborious analysis would be capable of deducing the characteristic of any dynamo to any desired degree of accuracy".[71]

To determine the law of the dynamo equivalent to Ohm's law for an electric circuit, Hopkinson had to specify the dependence of the electromotive force on the current passing through the armature of the dynamo, or $E(I)$. This problem was equivalent with specifying the relation between the magnetizing force (H, a quantity directly determined by the current passing through the armature: $H = 4\pi nI$, where where n is the number of turns) and the magnetic

70. Perry in Kapp, "The Pre-Determination of the Characteristics of Dynamos," 583-4, my underline.
71. Hopkinson and Hopkinson, "Dynamo-Electric Machinery," 332.

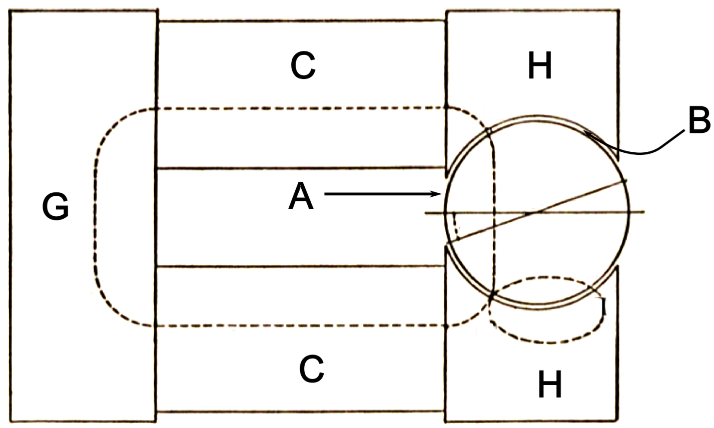


Figure 6.13
A simplified sketch of a dynamo. A (armature); B (air space); C (magnets); G (yoke); H (pole piece). Source: Adapted from John Hopkinson and Edward Hopkinson, "Dynamo-Electric Machinery," *Philosophical Transactions of the Royal Society of London* 177 (1886): 331–358; D. W. Jordan, "The Magnetic Circuit Model, 1850-1890: The Resisted Flow Image in Magnetostatics," *The British Journal for the History of Science* 23, no. 2 (1990): 131–173.

induction (B, a quantity which directly determined the electromotive force), or $B(H)$. Hopkinson considered a closed magnetic circuit passing through the components of the dynamo (see Fig. 6.13). The problem of finding the characteristic of a dynamo $E(I)$ was equivalent with finding $B(H)$, where B was the magnetic induction (or magnetization) and H the magnetizing force. The magnetizing force was directly determined by the current, and the electromotive force by the magnetic induction (and the rotation velocity of the dynamo). Hopkinson's analysis started from two theoretical premises: 1. the line integral for the magnetizing force H around a close loop had to match the current passing through the loop, $4\pi nI$ (where n is the number of turns); 2. the induction through any tube of induction was the same for every section of the dynamo (Fig. 6.13). The line integral for the magnetizing force ($4\pi nI$) was equated to the sum of the contributions from each section of the dynamo:[72]

$$\overbrace{4\pi nI}^{\text{magnetizing force}} = \underbrace{l_1 f\left(\frac{B}{A_1}\right)}_{\text{armature core}} + \underbrace{2l_2\frac{B}{A_2}}_{\text{air spaces}} + \underbrace{l_3 f\left(\frac{\nu B}{A_3}\right)}_{\text{magnet core}} + \underbrace{l_4 f\left(\frac{\nu B}{A_4}\right)}_{\text{yoke}} + \underbrace{2l_5 f\left(\frac{B}{A_5}\right)}_{\text{pole pieces}}$$

A_i represented the area of the sections and l_i their lengths. ν was a leakage coefficient Hopkinson had to introduce because not all the magnetic lines of the magnet also passed through the armature.[73]

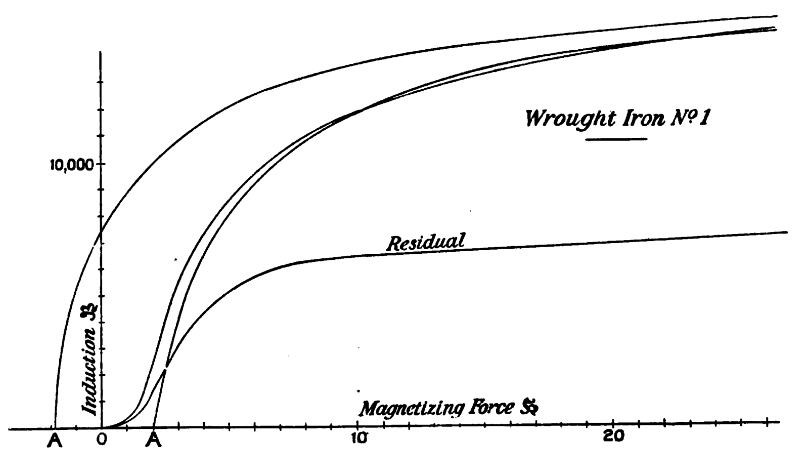


Figure 6.14
Hopkinson's curve of magnetization for wrought iron that represented the induction (B) as a function of the magnetizing force (H). Hopkinson proposed a method of constructing the "characteristic curves" of a dynamo starting from the magnetic curves of their components (see Fig. 6.15). Source: John Hopkinson, "Magnetisation of Iron," *Philosophical Transactions of the Royal Society of London* 176 (1885): 455–469.

The function f was determined experimentally and connected the induction in a piece of iron to the magnetizing force, or $H = f(B)$. The dependence of the magnetization (or magnetic induction) on the magnetizing force had long interested both physicists and engineers. In 1885 Hopkinson dedicated a whole paper to the "Magnetization of iron" in which he provided a series of curves of magnetization for $B(H)$ (see Fig. 6.14). These experimental magnetization curves were used to construct the characteristic curves for each section of the

72. I have slightly changed Hopkinson's notation for consistency.
73. While Hopkinson found the value of ν experimentally, Forbes showed in 1886 how one could compute this coefficient from theoretical considerations, and based solely on the geometry of the machine.

dynamo (Fig. 6.15). Notice that Hopkinson always plotted $B(H)$, i.e. $B = f^{-1}(H)$, despite the fact that he could have as easily plotted $H(B)$. The latter choice would have been more appropriate given his equation; however, by this point $B(H)$ was already the standard choice for the graphical representation. Once one understood how the magnetic force depended on the induction, Hopkinson could represent the "characteristic curve" as:

$$B = F(4\pi nI)$$

This short summary of Hopkinson's work on direct current dynamos has showed that his use of the characteristic curve was different from that of other graphical representations which could be found in the work on the same topic of experimental physicists and engineers. In these other cases, the curves were used to represent the results of an experiment (and its agreement with the theoretical predictions), to better analyze or fit experimental data, to illustrate and make more meaningful algebraic or theoretical concepts, to provide a set tools for solving practical problems.

Hopkinson's use of the characteristic curve is particularly distinctive. In the case of Meyer, Auerbach and Frölich the curves were useful, but not essential; little would have changed in their papers and arguments if all the curves were removed. In the case of Deprez the curves were essential, but only as paper tools; Deprez had only a practical commitment to his curves but not an epistemological one. However, for Hopkinson the curve was both a scientific and conceptual object. As we have seen, his characteristic curves were not immediately reducible to a formula or a set of constants, nor were they just a method of computation or graphical manipulation. The characteristic curve allowed Hopkinson to pull together his commitments to theoretical and experimental physics and engineering practice.

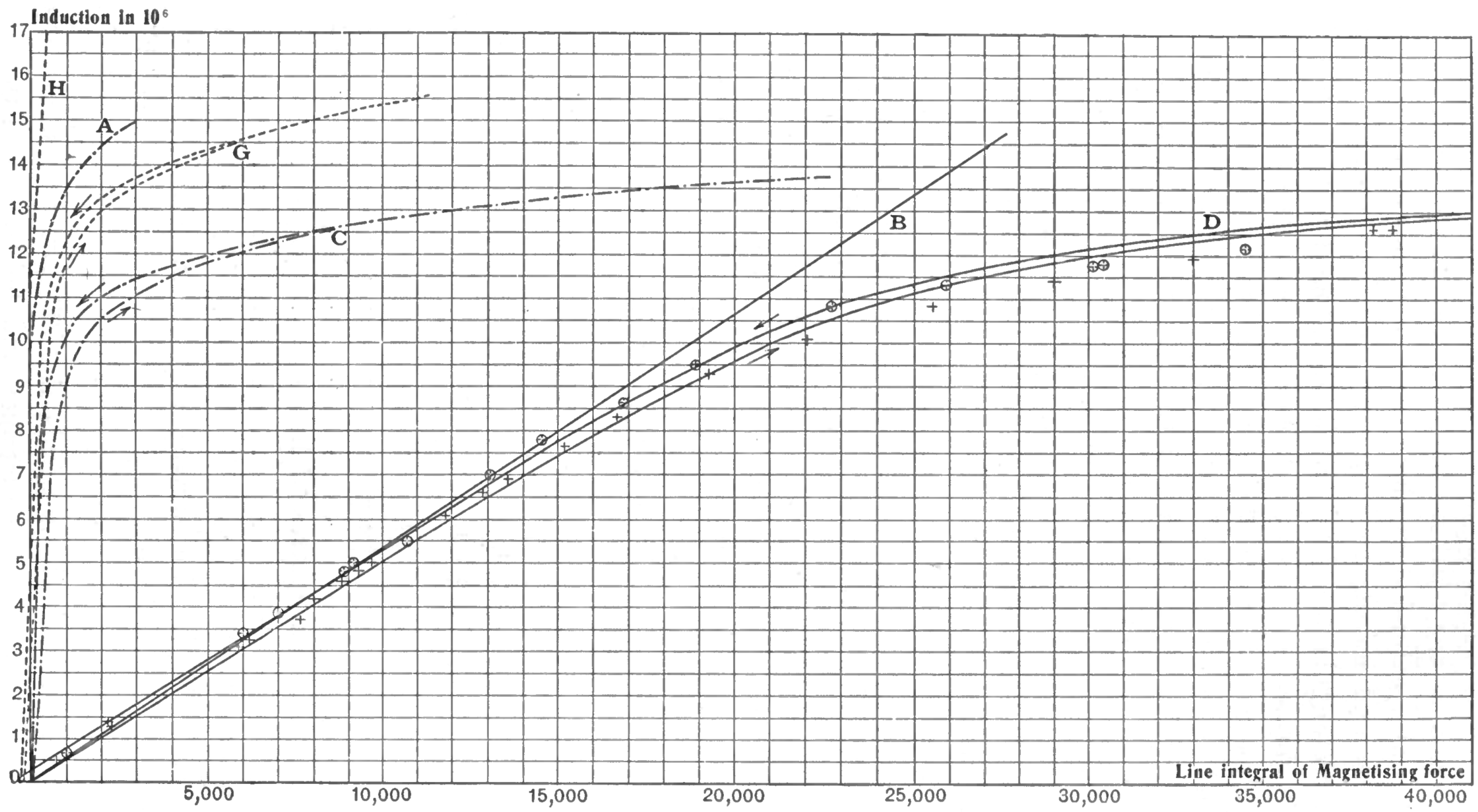


FIG. 4.—CORRECT SYNTHESIS OF CHARACTERISTIC CURVE.

A, armature; *B*, air space; *C*, magnets; *D*, calculated curve; *E*, observations, + ascending, ⊕ descending; *G*, yoke; *H*, pole-piece.

Figure 6.15
The characteristic curves for each section of the dynamo. Instead of providing a formula for the dynamo, Hopkinson proposed that one should graphically add together the magnetic contribution of each component. Section 1.4. Source: John Hopkinson and Edward Hopkinson, "Dynamo-Electric Machinery," *Philosophical Transactions of the Royal Society of London* 177 (1886): 331–358.

2 THE CHARACTER OF THE CURVE

As I have showed above, despite its later use as a practical tool in the solution of engineering problems, Hopkinson first used the "characteristic curve" *indexically* to point to a potential connection between a theory of the dynamo and Weber's theory of magnetism. Instead of providing an empirical formula that could have been derived from Weber's theory of magnetism, Hopkinson's only remark was that "we should naturally expect that a similar formula to that obtained by Weber would be approximately applicable to dynamo-electric machines".[74] Nothing was said about why Weber's formula was "naturally applicable", and no empirical or theoretical formulas were plotted along the experimental curve. When presenting Weber's formula, Hopkinson referenced Maxwell's *Treatise on Electricity and Magnetism* where Weber's theory of magnetism was illustrated by a similar curve. Hopkinson pointed out that the main features of his curve were that it approached an asymptote and that its earlier part was approximately a straight line.[75] For those involved with the study of magnetism, these were the defining features of the magnetic curves that were well explained by Weber's theory. This section will provide a history of how the magnetic curves came to have *defining* features.

2.1 The Asymptote: Maxwell and Weber

Among the 20 plates and 105 in-text illustrations included in Maxwell's *Treatise on Electricity and Magnetism* (1873) there were only two plots, both representing magnetization as a function of the magnetizing force.[76] The plotting of experimental data or of empirical and theoretical formulas was not part of Maxwell's graphical repertoire that otherwise made extensive use of diagrams.[77] As such, it is clear that the two plots played a special role in the *Treatise*.

74. Hopkinson, *Original Papers by the Late John Hopkinson*, 45.
75. Ibid., 48.
76. See fig. 7 and 10 in James Clerk Maxwell, *A Treatise on Electricity and Magnetism* (Clarendon Press, 1873), vol.2, 79;83.
77. The most famous such diagrams studied by historians have been Maxwell's "lines of force and equipotential surfaces". See P. M. Harman, *The Natural Philosophy of James Clerk Maxwell* (Cambridge: Cambridge University Press, 2001); David C. Gooding, "From Phenomenology to Field Theory: Faraday's Visual Reasoning," *Perspectives*

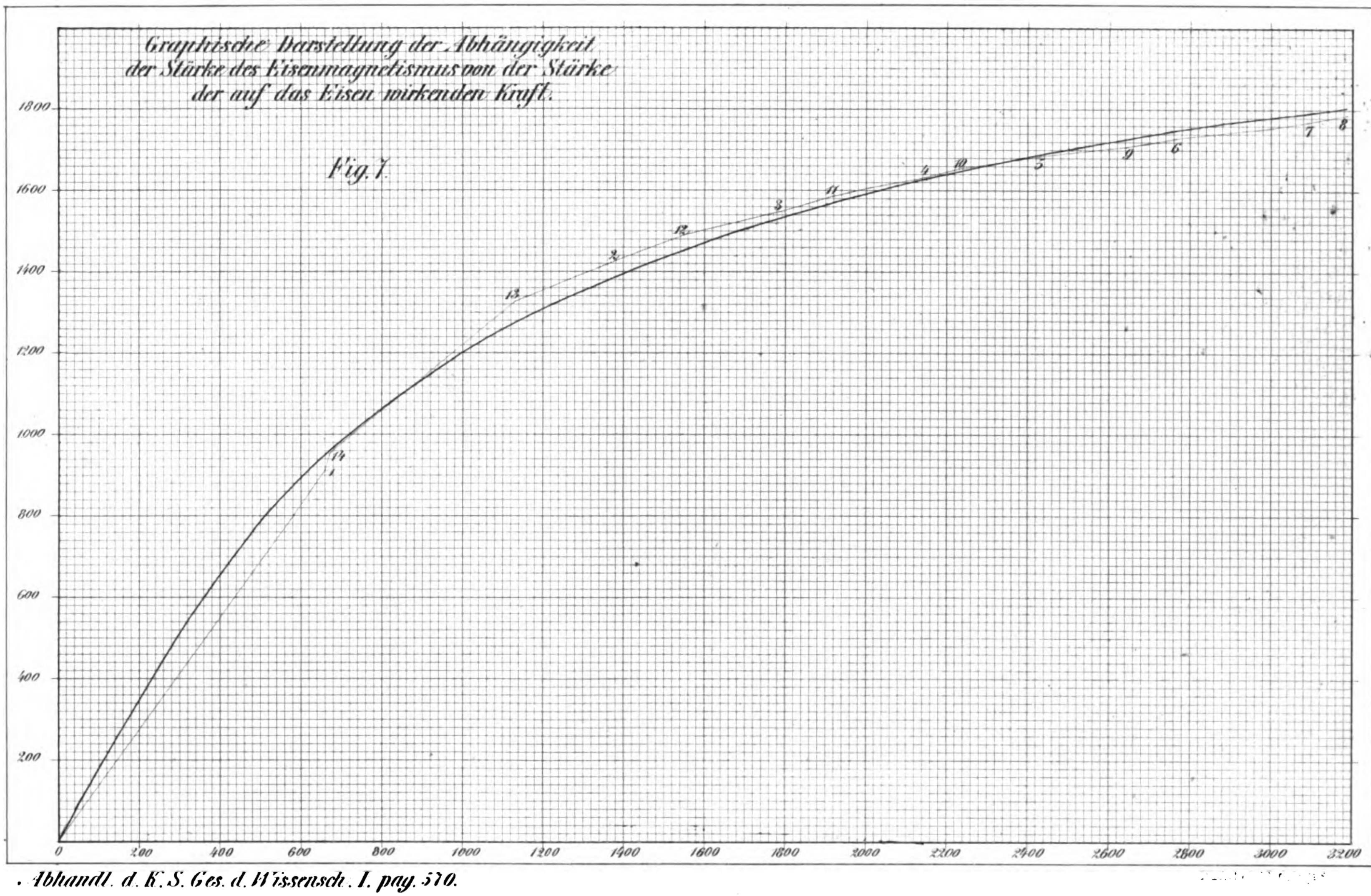


Figure 6.16
Weber's plot displayed the agreement between his experimental results and the formula he derived by modeling the phenomenon of magnetization as the rotation of molecules with an intrinsic magnetic moment. Source: Wilhelm Weber, "Elektrodynamische Maassbestimmungen insbesondere über Diamagnetismus," *Abhandlungen der Sächsischen Akademie der Wissenschaften zu Leipzig* 1 (1852): 483–577.

The two plots were included in the chapter on "Weber's theory of magnetic induction". In the *Elektrodynamische Maassbestimmungen* (1852), Wilhelm Weber had explained the magnetization of iron by assuming that molecules were permanently magnetized such that when a magnetic force acted on the iron it turned the axes of the molecules in one direction causing the iron to become a magnet. Starting from this assumption, he derived a formula for the magnetization as a function of the magnetizing force which he then compared to his experimental measurements using both a table and a plot (see Fig. 6.16). The close agreement between the formula and the experiment, as shown by the plot, led Weber to claim that "because of this, it would seem that the rotation of the iron molecules has been put beyond doubt".[78] Because he found a mistake in Weber's formula, Maxwell provided a corrected version of the relation between the magnetization and the magnetizing force.[79] However, "the law of the magnetization" as arising from Weber's theory of magnetization was expressed not in the corrected analytical formula, but rather in the figure which represented the formula (see Fig. 6.17). The differences between Weber's and Maxwell's graphical representations are indicative of two different programs and epistemological methods.

Despite the fact that he had modified Weber's formula, Maxwell remarked that "Weber's

on Science 14, no. 1 (2006): 40–65.

78. Wilhelm Weber, "Elektrodynamische Maassbestimmungen insbesondere über Diamagnetismus," *Abhandlungen der Sächsischen Akademie der Wissenschaften zu Leipzig* 1 (1852): 574.

79. Weber's original formula was:

$$I = mn\frac{X}{\sqrt{X^2+D^2}}\frac{X^4+\frac{7}{6}X^2D^2+\frac{2}{3}D^4}{X^4+X^2D^2+D^4},$$

where I was the magnetization, X the magnetizing force, m the magnetic moment between the molecules, n the number of molecules, D the force with which each molecule, when deflected, tends to return to the original position. ibid., 572

Maxwell, who carried out his own integration, found a different relation:
For X less than D, $I = \frac{2}{3}\frac{mn}{D}X$;
for X equal to D, $I = \frac{2}{3}mn$;
for X greater than D, $I = mn\left(1-\frac{1}{3}\frac{D^2}{X^2}\right)$;
when X was infinite $I = mn$. Maxwell, *A Treatise on Electricity and Magnetism*, vol.2, 78.

own experiments give results in satisfactory accordance to this law".[80] While Weber actually plotted both the experimental results and his formula to infer their concordance, Maxwell did not bother with this step. This was not an oversight on Maxwell's side, but rather a careful decision. Maxwell considered that "the scientific value of a theory of this kind, in which we make so many assumptions, and introduce so many adjustable constants, cannot be estimated merely by its numerical agreement with certain sets of experiments".[81] This was not meant to be a dismissive criticism of Weber. Though he did not subscribe to Weber's theory of magnetic induction, Maxwell did find value in its physical explanation: "If it has any value it is because it enables us to form a mental image of what takes place in a piece of iron during magnetization."[82] Such an attitude can be found throughout Maxwell's writings. In "On Faraday's Lines of force" (1856) Maxwell described the need for a method of investigation which avoids the "purely mathematical formula" through which "we lose sight of the phenomenon to be explained" and the physical hypothesis which makes one "liable to that blindness to facts and rashness in assumption which a partial explanation encourages". Instead, Maxwell required

> some method of investigation which allows the mind at every step to lay hold of a clear physical conception, without being committed to any theory founded on the physical science from which that conception is borrowed, so that it is neither drawn aside from the subject in pursuit of analytical subtleties, nor carried beyond the truth by a favourite hypothesis.[83]

The danger behind a physical hypothesis was not its incompleteness but the temptation of premature commitment. For this reason, Maxwell considered that "the chief merit of a temporary theory is, that it shall guide experiment, without impeding the progress of the true theory when it appears".[84] When Maxwell introduced his famous model of idle wheels to describe the action of particles and vortices, he pointed out again "that any one who understands the provisional and temporary character of this hypothesis, will find himself rather helped than

80. Maxwell, *A Treatise on Electricity and Magnetism*, vol.2, 79.
81. Ibid., 83.
82. Ibid.
83. Maxwell, *The Scientific Papers of James Clerk Maxwell*, 155-156.
84. Maxwell, *A Treatise on Electricity and Magnetism*, 208.

hindered by it in his search after the true interpretation of the phenomena”.[85]

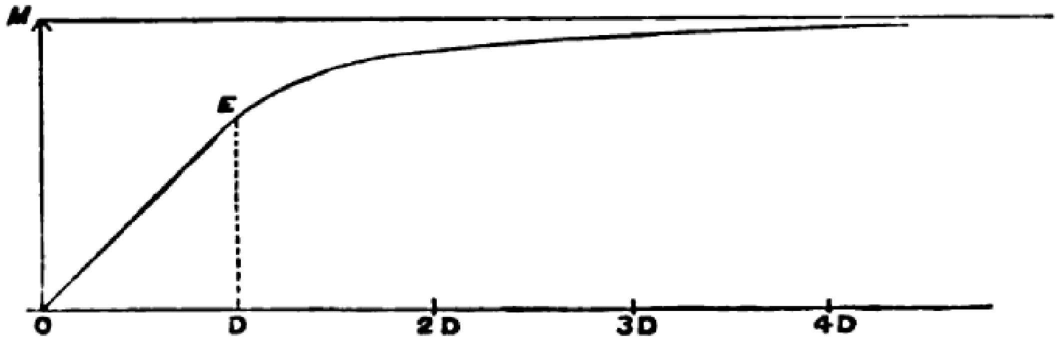


Fig. 7.

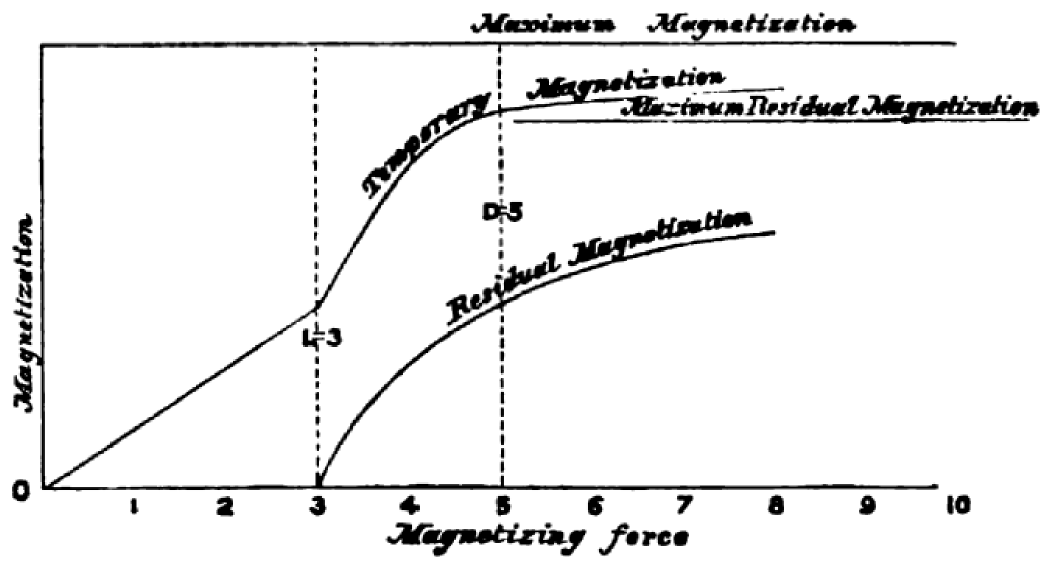


Fig. 10.

Figure 6.17
The first plot corresponded to the graphical representation of Weber’s formula for magnetization; opposed to Weber’s experimental plot (see Fig. 6.16), Maxwell was interested in displaying the asymptotic behavior of the curve. It was this *feature* that made Weber’s formula (and his molecular theory of magnetism) valuable as a “mental image”. The second plot was Maxwell’s extension of Weber’s theory which took into account the effect of residual magnetization. Source: James Clerk Maxwell, *A Treatise on Electricity and Magnetism* (Clarendon Press, 1873), vol.2; 79-83.

Thus, Maxwell’s interest in Weber’s theory of magnetic induction was motivated by its ability to provide a “mental image” or “physical conception” of the underlying phenomenon. The choice to present such a “physical conception” was not justified by a commitment to its

85. Maxwell, *The Scientific Papers of James Clerk Maxwell*, 486.

underlying hypothesis about the rotation of molecules, nor by the close and indiscriminate agreement between formulas and experiments. Because Maxwell assumed the "provisional and temporary character" of Weber's theory, he only focused on the main implication of the theory which showed that the magnetization approached a limiting value. This was the essential feature that could be derived as a consequence of the assumption that molecules were permanently magnetized, and which was "in satisfactory accordance" with the experimental evidence. Because the asymptotic behavior of the magnetization was the main feature of interest for Maxwell, his curve extended until it almost became horizontal. Weber's curve, however, only extended up to the last experimental point because it was used to show the concordance between his experimental results and his formula. Because the magnetizing force was not sufficiently high, the asymptotic behavior could only be inferred by extrapolation.

Because Weber's derivation did not take into account the residual magnetization that was left in the iron rod once the magnetizing force was removed, Maxwell extended Weber's initial assumptions to cover for this case "as an assistance to the imagination in following out the speculations suggested by Weber".[86] As in the previous case, Maxwell was not interested in finding a true and correct formula for the magnetization, but only in understanding the consequences of the extended theory. As such, the relevant result was not the formula but the diagram that showed both the temporary and residual magnetization curves approaching an asymptote (see Fig. 6.17). Maxwell used the two plots to reduce the formulas that were based on "so many assumptions, and [...] so many adjustable constants" to one key feature that displayed the main physical implication of the "mental image" from which he started.

While the use of models and "mental images" was specific to Maxwell's scientific style, the display and interpretation of specific features of an equation or of a model through a plot was mainly derivative and restricted to Maxwell's pedagogical endeavors.[87] Maxwell did not

86. Maxwell, *A Treatise on Electricity and Magnetism*, 79.
87. See Chapter 7 for Maxwell's use of indicator diagrams in his textbook on the *Theory of heat*.

construct the two plots on magnetization because that was his habitual practice, nor because he considered that plots in general have a special pedagogical value. As pointed out above, there are no other examples in the *Treatise on Electricity and Magnetism* in which Maxwell constructed the graphical representation of a function or of experimental results. Not only Maxwell's *Treatise*, but British scientific articles in general did not make great use of plots to present their experimental findings. Michael Faraday, a highly visual experimentalist, or James Joule, one of the first experimentalists who noticed the saturation of the magnetization inside an iron core, never produced any plots. The two plots Maxwell did provide were special not only because they connected explicitly to Weber's theory and graphical representations, but also because it engaged *graphically* with a more well-established tradition that was particularly visible in the writings of German scientists.

2.2 The Asymptote: the first traces

The experimental claim that the magnetization of an iron rod approached a maximum was first established by the German physicist J. J. Müller with the aid of graphical representations. In 1850, Müller published an article in the *Annalen der Physik* in which he claimed to have disproved the Lenz-Jacobi law which stated that the magnetization by a current is proportional to the intensity of the current (i.e. the magnetizing force).[88] Müller used a magnetizing coil (*Magnetisirungsspirale*) through which he passed a current whose intensity he measured using a tangent galvanometer. He used a needle compass to measure the deflection produced by an iron rod which was introduced inside the magnetizing coil. The magnetic force (*magnetisirende Kraft*, p) was computed by multiplying the intensity of the current and the number of windings; the magnetization (*Magnetismus*, m) was obtained by taking the inverse tangent of the deflection angle. He repeated his measurements for different magnetizing coils with dif-

88. Lenz and Jacobi's original article (1839) did not employ any plots, see E. Lenz and M. Jacobi, "Ueber die Gesetze der Elektromagnete," *Annalen der Physik* 123, no. 6 (1839): 225–266.

ferent currents, and iron rods of different diameter. Müller found that while the Lenz-Jacobi law held for small currents (and thus for small magnetic forces), it failed at higher intensities. This was immediately visible from one of his tables of measurements which included the ratio m/p between the magnetization and the magnetic force. The ratio should have been constant (or almost constant) if the quantities were proportional.

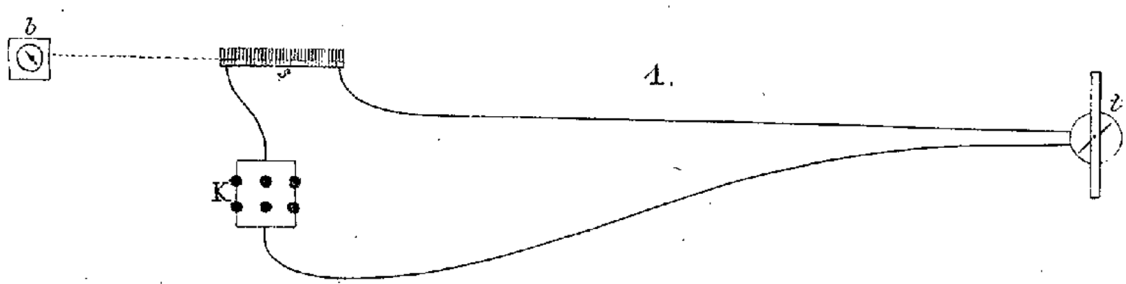


Figure 6.18
s – the magnetizing coil; b – the compass used to measure the magnetic deflection; t – the tangent galvanometer used to measure the current. J. Müller, "Ueber die Magnetisirung von Eisenstäben durch den galvanischen Strom," *Annalen der Physik* 79 (1850): 337–344

p.	m beob-achtet.	m be-rechn.	Unter-schied.	p.	m beob-achtet.	m be-rechn.	Unter-schied.
	Stab No. 1.				Stab No. 3.		
27819	0,2864	0,3156	+0,0292	27223	0,7335	0,7328	— 0,0007
25356	0,2845	0,3113	+0,0268	25267	0,7072	0,7215	+0,0143
23746	0,2842	0,3075	+0,0233	23855	0,6975	0,6968	+0,0014
19555	0,2744	0,2979	+0,0235	16691	0,6228	0,5940	— 0,0288
16397	0,2760	0,2837	+0,0077	13618	0,5648	0,5266	— 0,0382
13963	0,2735	0,2674	— 0,0052	9736	0,4222	0,4217	— 0,0005
13288	0,2627	0,2668	+0,0041	9001	0,3926	0,3974	+ 0,0048
7921	0,2337	0,2151	— 0,0186	6705	0,3092	0,3127	+0,0035
7110	0,2202	0,2030	— 0,0173	5738	0,2642	0,2732	+0,0090
6705	0,2078	0,1962	— 0,0116	3903	0,1768	0,1917	+0,0149
3507	0,1371	0,1237	— 0,0134	3659	0,1786	0,1805	+0,0019
3052	0,1193	0,1002	— 0,0091	3243	0,1541	0,1548	+0,0007
1596	0,0644	0,0605	— 0,0039	2922	0,1404	0,1455	+0,0051
1456	0,0597	0,0558	— 0,0039	2562	0,1283	0,1314	+0,0138

Figure 6.19
Müller's table comparing the observed measurements to those calculated by his empirical formula for the magnetic force p and the magnetization m. Source: J. Müller, "Ueber die Magnetisirung von Eisenstäben durch den galvanischen Strom," *Annalen der Physik* 79 (1850): 343.

Müller then added that after "many failed attempts" he was able to find a formula that covered all the results in his table (see Fig. 6.19):[89]

$$p = 220d^{3/2} \tan \frac{m}{0.00005d^2}$$

This empirical formula played a particularly important role in Müller's paper because it indicated that the magnetization approached asymptotically a maximum limit and it allowed him to estimate its value. Only then, Müller plotted the function "to make more vivid the relation" of the increase in magnetization as a function of the magnetic force (see Fig. 6.20).[90] If the Lenz-Jacobi law would have been always true, all four curves should have been straight lines. However, for the thinnest iron rod one could already see how the curve bent into a horizontal line, suggesting that the magnetization reached a maximum. Lenz and Jacobi found the two quantities to be proportional only because their currents were too weak. The next step in Müller's article was to show the extent to which his empirical formula actually approximated the experimental results. He did that using a using a table, not the graphical representation (see Fig. 6.19). That was by far the most common choice in the 19th century because one could both compare the observed and calculated values, and specify the difference between the two.

Müller's article was immediately dismissed by two experimentalists from Giessen, H. Buff and F. Zamminer, who insisted that the Jacobi-Lenz law was valid and that Müller failed to reproduce it because of defective soft iron cores.[91] What for Müller was a property of the material, for Buff and Zamminer was a defect that had to be removed. In a follow-up article, Müller explained that Buff and Zamminer did not detect the saturation of the magnetization in the iron because their rods were too thick, and the current too small. To support his case, Müller added three different plots for the magnetizing spirals he used; the curves in each plot still

89. J. Müller, "Ueber die Magnetisirung von Eisenstäben durch den galvanischen Strom," *Annalen der Physik* 79 (1850): 340.
90. Ibid., 342.
91. Buff and Zamminer, "Ueber die Magnetisirung von Eisenstäben durch den galvanischen Strom," *Justus Liebigs Annalen der Chemie* 75, no. 1 (1850): 83–94.

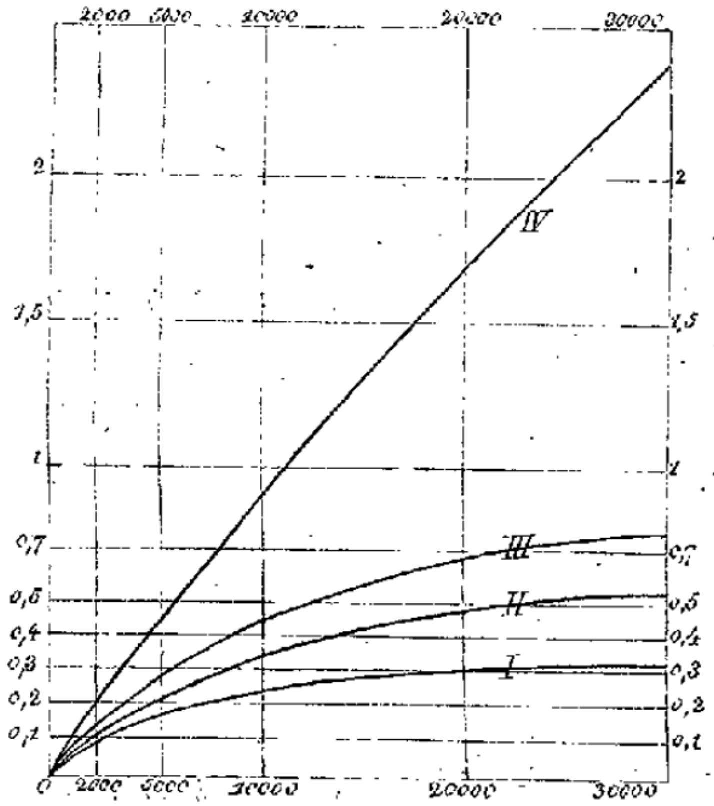


Figure 6.20
Müller's curves played an important role in his criticism of the Lenz-Jacobi law because they clearly showed that only for small magnetic forces (curve IV) was the relation between the magnetic force and the magnetization linear. For high magnetic forces (Curves I, II, III) the magnetization was asymptotically constant or saturated. Source: J. Müller, "Ueber die Magnetisirung von Eisenstäben durch den galvanischen Strom," *Annalen der Physik* 79 (1850): 337–344.

corresponded to iron rods of different length and diameter. In this way he could show that for the same iron rod one magnetizing spiral produced an interval of currents that only exhibited a linear relation between the current and the magnetization (see plot 1, curve I in Fig. 6.21), while a spiral that produced higher currents could show that the magnetization approached a limit (see plot 3, curve III in Fig. 6.21). Müller confidently conclude that his experimental curves closed the dispute: "My claim is so decidedly stated [ausgesprochen] by these curves that they probably do not require any further dispute [Auseinandersetzung]".[92] Though Müller's article represented a strong refutation of Buff and Zamminer's counter-claims, the dispute was settled by Weber's study on *Elektrodynamische Maassbestimmungen* (1852), which was discussed

92. J. Müller, "Ueber den Sättigungspunkt der Elektromagnete," *Annalen der Physik* 82, no. 2 (1851): 181–188.

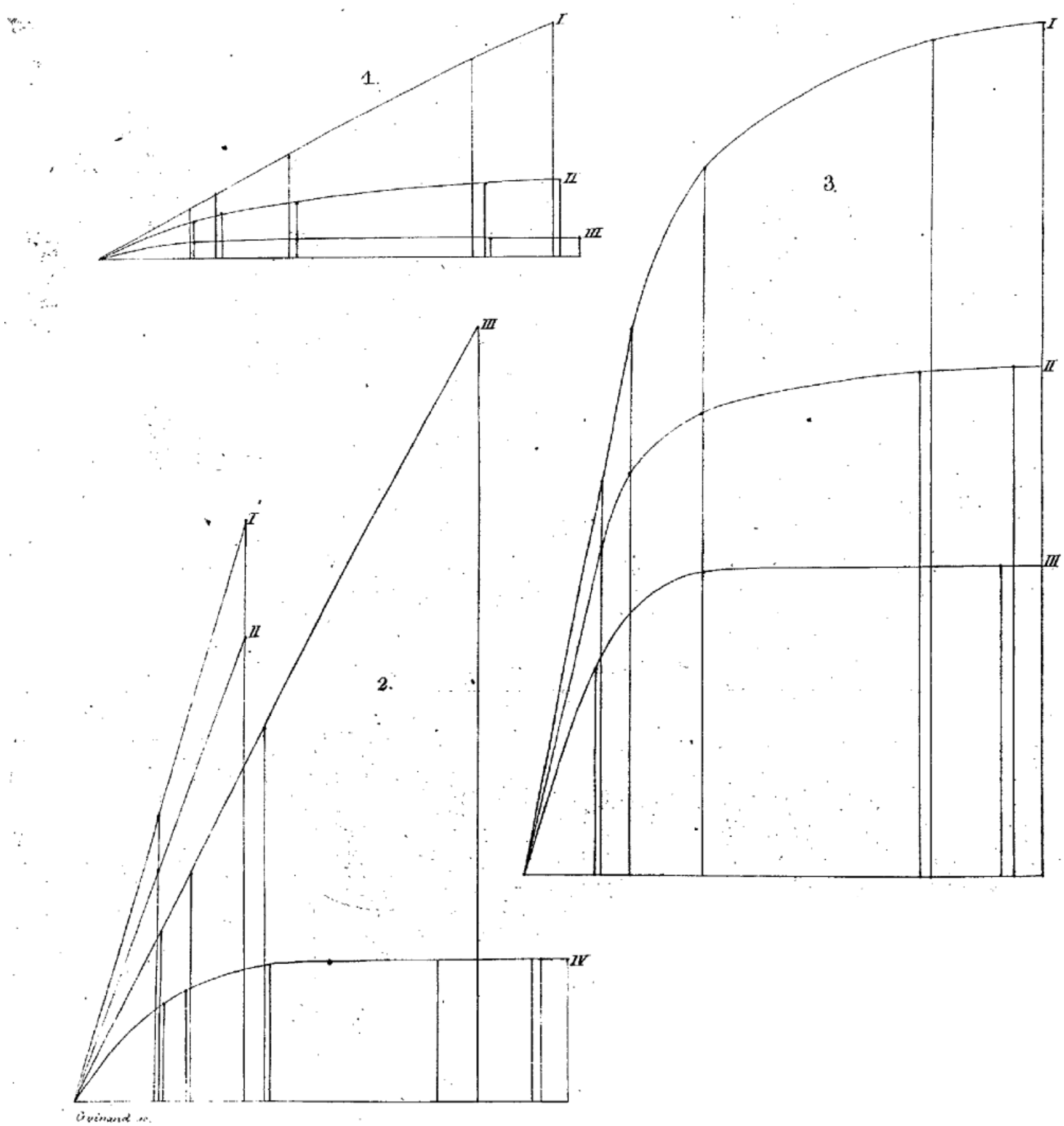


Figure 6.21
Müller's curves for different magnetizing spirals. The curves showed that for low currents (i.e. small magnetizing forces), the magnetization was proportional to the magnetic force. However, for high currents the magnetization became asymptotically horizontal. "My claim is so decidedly stated [ausgesprochen] by these curves that they probably do not require any further dispute [Auseinandersetzung]". Source: J. Müller, "Ueber den Sättigungspunkt der Elektromagnete," *Annalen der Physik* 82, no. 2 (1851): 181–188.

above.[93]

Outside Germany, Müller's study was brought to the attention of the British public by a summary published by John Tyndall in 1851.[94] After learning about Müller's results from Tyndall's summary, James Joule hurried to publish a short letter in the *Philosophical Magazine* with an "Account of Experiments demonstrating a limit to the Magnetizability of Iron" (1851) that collected a series of previous articles regarding his experiments on the magnetization of iron. Joule emphasized that the saturation of the magnetization was a phenomenon he had noticed as early as 1839.[95] However, it should not come as a surprise that Joule's thoughts on the saturation of magnetization had remained unnoticed because in the original paper the focus was on establishing an empirical law between the magnetization and the magnetizing current.[96] Joule chose the most simple law that posited a linear relation, while noticing only in passing that for high currents "the law in this case seems to fail principally because the iron is sooner saturated with magnetism".[97]

Compared to Joule, Müller had put forward an empirical formula that accounted for the saturation and provided a value for the maximum magnetization. Furthermore, he had made the saturation phenomenon one of the cornerstones of his experimental results. The impact of Müller's experiment was further expanded by Weber's study on *Elektrodynamische Maassbestimmungen* (1852) which showed that the saturation phenomenon could be explained by assuming that molecules possessed a permanent magnetic moment. Along with Müller's result and formula spread also his graphical representation. Before 1850, there were no graphical representations of the magnetization as a function of the magnetizing force, mostly because

93. Weber, "Abhandlungen der Sächsischen Akademie der Wissenschaften zu Leipzig," 566-567.

94. John Tyndall, "Reports on the Progress of the Physical Sciences," *Philosophical Magazine Series 4* 1, no. 3 (1851): 194–205.

95. J. P. Joule, "Account of Experiments Demonstrating a Limit to the Magnetizability of Iron," 2 (1851): 313.

96. Compared to the Jacobi-Lenz law which had been published two months earlier in March 1839, Joule's law was stated as the relation between the magnetic attraction of an iron needle and a coil (M), and the current passing through the coil (E): $M = E^2W^2$, where W was the length of the iron wire. However, the implication was still the same – the magnetization of the iron was proportional to the magnetizing current.

97. Joule, "Account of Experiments Demonstrating a Limit to the Magnetizability of Iron," 313.

the function was assumed to be linear. Immediately after the publication of Müller's paper, several other physicists started employing this style of plotting. Julius Plücker, who had already published several articles on magnetism and often employed experimental plots, made use of Müller's magnetic curves to show that "for all magnetic and diamagnetic substances one and the same universal law gives the intensity of the induced magnetism as a function of the excitation force".[98] While Plücker proposed his own empirical formula, he insisted on pointing out that his "curves are the immediate expression of observations, independent of any hypothesis or theoretical opinion".[99]

Müller also included the magnetic curves in his textbook, *Pouillet's Lehrbuch der Physik und Meteorologie* (4th ed; 1852), which was based on Pouillet's *Éléments de physique expérimentale et de métédrologie*.[100] Revised by several authors, the textbook which will come to be known as the *Müller-Pouillet's Lehrbuch der Physik* was published throughout the 19th century up to the 1930s. Starting in the 1860s, the curves were soon reproduced in all the main textbooks on electricity and magnetism, such as Gustav Wiedemann's *Die Lehre vom Galvanismus und Elektromagnetismus* (1861) or Julius Dub's *Der Elektromagnetismus* (1861).[101] Maxwell was familiar with many of the German textbooks and articles that made use of Müller's magnetic curves (such as Wiedemann's *Galvanismus*).[102] In France, Müller's magnetic curves were first popularized by Émile Verdet in an article that summarized the results of the German physicist; Verdet made further use of Müller's 1850 plot in his lessons on electricity at the L'École Normale.[103] Otherwise, the main French textbooks on electricity and magnetism only reported

98. Plücker, "Ueber das Gesetz der Induction bei paramagnetischen und diamagnetischen Substanzen," *Annalen der Physik* 167, no. 1 (1854): 51.

99. Ibid., 35.

100. J. Müller, *Pouillet's Lehrbuch der Physik und Meteorologie* (F. Vieweg & Sohn, 1852), 259.

101. Gustav Heinrich Wiedemann, *Die Lehre vom Galvanismus und Elektromagnetismus* (Braunschweig: F. Vieweg & Sohn, 1861), vol.2, 290; Julius Dub, *Der Elektromagnetismus: Mit 120 in d. Text Eingedruckten Holzschnitten* (Julius Springer, 1861), 89. Of course, there were also notable exceptions such as J. Lamont's *Handbuch des Magnetismus* (1867) which provided a detailed summary of Müller's results but did not include any of the curves – see Johann Lamont, *Handbuch des Magnetismus* (Voss, 1867), 45-49.

102. Maxwell, *A Treatise on Electricity and Magnetism*, vol.1, 330, 356.

103. Emile Verdet, "Mémoires sur la physique publiés à l'étranger," *Annales de chimie et de physique* 48 (1856): 119–128; Emile Verdet, *Conférences de physique faites a l'École normale*, vol. 1 (Impr. Nationale, 1872), 215.

the general implications of Müller's results and his empirical formula, without reproducing the curves.[104] While they did not include Müller's plots, some textbooks noted in passing that "one can confirm [the results] graphically by tracing the curves in which we take the intensity of the current to be the abscissa and the magnetic moment the ordinate".[105] The plots could not be found in any British publications.

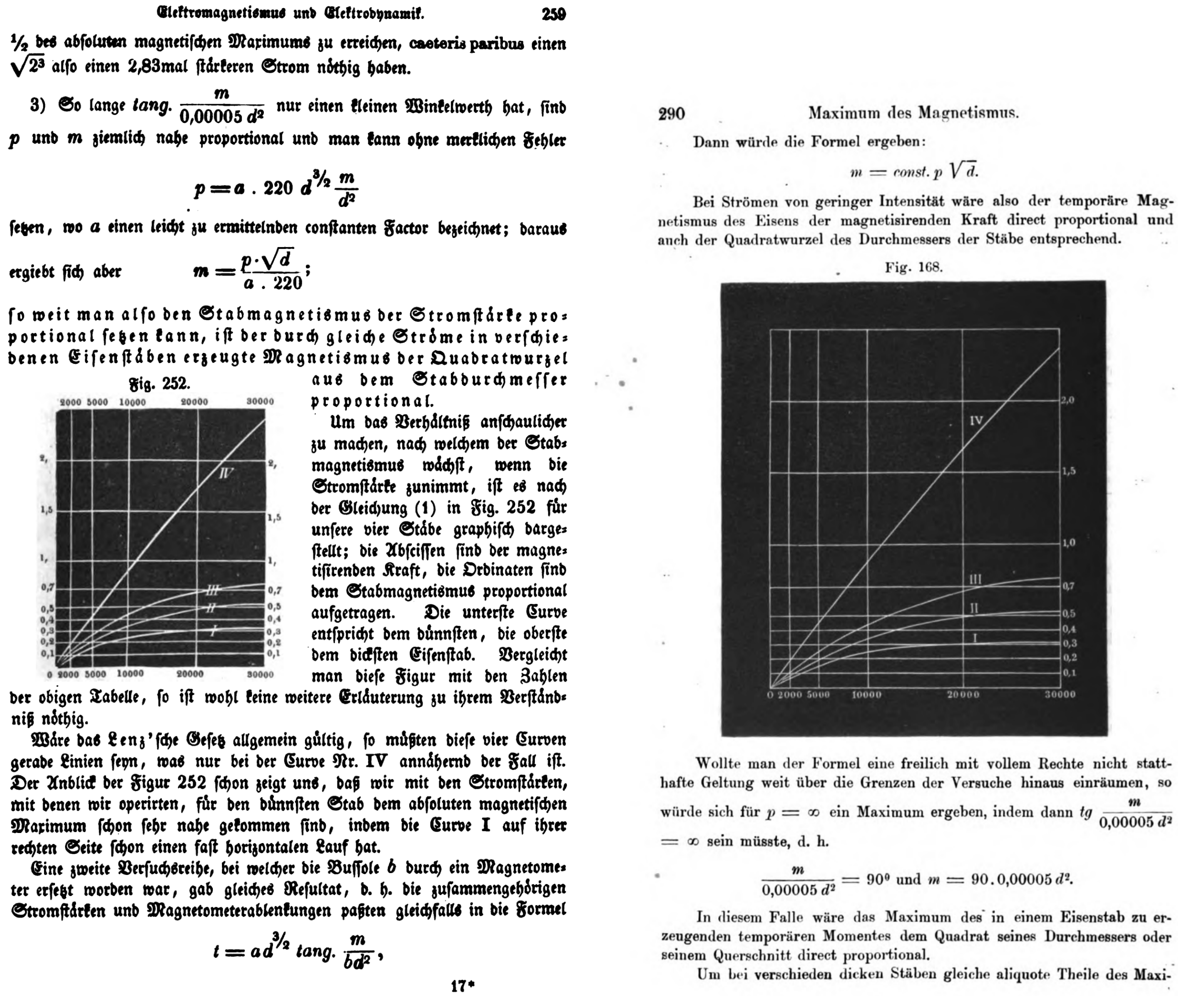
This short review of the German, French and British reactions to Müller's results and his graphical representations allows us to delineate some general national attitudes. These remarks will be further corroborated in the following discussions. While Müller's results did attract some attention among the British, these plots were never reproduced or imitated. This again confirms our claim that graphical representations of experimental results were not common among British scientists. In France, the reactions were mixed – while the plots were reproduced in some cases, the general attitude was to reproduce only the table of measurements and the empirical formula; sometimes the curves or the method of plotting was described.[106] In Germany, Müller's curves had the most direct and powerful impact – several experimentalists borrowed this mode of representation, while the curves were widely reproduced in textbooks.

The sources of these differences are complex and cannot be reduced to a single factor. Whenever Müller's results were presented graphically in a textbook, it was mostly through the reproduction of the original curves; other tables of measurements were never independently plotted. This suggests, that in this case, the curves were associated and perceived as part of the original experimental finding. The curves were not added because of the interven-

104. See Becquerel and Becquerel, *Traité d'électricité et de magnétisme et des applications de ces sciences à la chimie, à la physiologie et aux arts par Mm. Becquerel et Edmond Becquerel*, 181-182; Auguste De la Rive, "Traité d'électricité théorique et appliquée par A. De La Rive," 1858, 324; Jules Jamin, *Cours de physique de l'École polytechnique*, vol. 3 (Mallet-Bachelier, 1866), 255. The only exception was a textbook by Pierre-Adolphe Daguin, a physics professor at the university of Toulouse; like Émile Verdet, Daguin also graduated from École normale in Paris, see P. A. Daguin, *Traité élémentaire de physique théorique et expérimentale*, vol. 3 (Édouard Privat, 1861), 23-24.

105. De la Rive, "Traité d'électricité théorique et appliquée par A. De La Rive," 324.

106. We have encountered a similar attitude towards Gay-Lussac's curves of solubility which were often described in textbooks, but not reproduced until the late 1840s and also under German influence, see Chapter 2.



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½ des absoluten magnetischen Maximums zu erreichen, caeteris paribus einen $\sqrt{2^3}$ also einen 2,83mal stärkeren Strom nöthig haben.

3) So lange $tang. \frac{m}{0{,}00005\, d^2}$ nur einen kleinen Winkelwerth hat, sind p und m ziemlich nahe proportional und man kann ohne merklichen Fehler

$$p = a \,.\, 220\, d^{3/2} \frac{m}{d^2}$$

setzen, wo a einen leicht zu ermittelnden constanten Factor bezeichnet; daraus ergiebt sich aber $m = \frac{p \cdot \sqrt{d}}{a \,.\, 220}$;

so weit man also den Stabmagnetismus der Stromstärke proportional setzen kann, ist der durch gleiche Ströme in verschiedenen Eisenstäben erzeugte Magnetismus der Quadratwurzel aus dem Stabdurchmesser proportional.

Fig. 252.

Um das Verhältniß anschaulicher zu machen, nach welchem der Stabmagnetismus wächst, wenn die Stromstärke zunimmt, ist es nach der Gleichung (1) in Fig. 252 für unsere vier Stäbe graphisch dargestellt; die Abscissen sind der magnetisirenden Kraft, die Ordinaten sind dem Stabmagnetismus proportional aufgetragen. Die unterste Curve entspricht dem dünnsten, die oberste dem dicksten Eisenstab. Vergleicht man diese Figur mit den Zahlen der obigen Tabelle, so ist wohl keine weitere Erläuterung zu ihrem Verständniß nöthig.

Wäre das Lenz'sche Gesetz allgemein gültig, so müßten diese vier Curven gerade Linien seyn, was nur bei der Curve Nr. IV annähernd der Fall ist. Der Anblick der Figur 252 schon zeigt uns, daß wir mit den Stromstärken, mit denen wir operirten, für den dünnsten Stab dem absoluten magnetischen Maximum schon sehr nahe gekommen sind, indem die Curve I auf ihrer rechten Seite schon einen fast horizontalen Lauf hat.

Eine zweite Versuchsreihe, bei welcher die Bussole b durch ein Magnetometer ersetzt worden war, gab gleiches Resultat, d. h. die zusammengehörigen Stromstärken und Magnetometerablenkungen paßten gleichfalls in die Formel

$$t = a d^{3/2}\, tang. \frac{m}{b d^2},$$

17*

290 Maximum des Magnetismus.

Dann würde die Formel ergeben:

$$m = const.\, p \sqrt{d}.$$

Bei Strömen von geringer Intensität wäre also der temporäre Magnetismus des Eisens der magnetisirenden Kraft direct proportional und auch der Quadratwurzel des Durchmessers der Stäbe entsprechend.

Fig. 168.

Wollte man der Formel eine freilich mit vollem Rechte nicht statthafte Geltung weit über die Grenzen der Versuche hinaus einräumen, so würde sich für $p = \infty$ ein Maximum ergeben, indem dann $tg \frac{m}{0{,}00005\, d^2} = \infty$ sein müsste, d. h.

$$\frac{m}{0{,}00005\, d^2} = 90^0 \text{ und } m = 90 \,.\, 0{,}00005\, d^2.$$

In diesem Falle wäre das Maximum des in einem Eisenstab zu erzeugenden temporären Momentes dem Quadrat seines Durchmessers oder seinem Querschnitt direct proportional.

Um bei verschieden dicken Stäben gleiche aliquote Theile des Maxi-

(a) muller 1852 **(b)** Wiedemann 1861

Figure 6.22
Müller's original curves reproduced in some German textbooks. Source: (a): J. Müller, *Pouillet's Lehrbuch der Physik und Meteorologie* (F. Vieweg & Sohn, 1852), 259; (b): Gustav Heinrich Wiedemann, *Die Lehre vom Galvanismus und Elektromagnetismus* (Braunschweig: F. Vieweg & Sohn, 1861), vol.2, 290.

tion of the author in explaining or illustrating a concept, but as a way of better portraying and supporting the original experimental claims. We can further speculate on why Müller's curves received such an important status. As I mentioned earlier, while German physicists often employed plots in their articles, these representations were almost never reproduced in the textbooks that reported their findings (Weber's curve from *Elektrodynamische Maassbestimmungen* (1852) could be such an example of a curve that not was not reproduced). Furthermore, with the exception of Müller's curves, German textbooks on electromagnetism hardly included any other graphical representations of experimental data. In the case of Müller's curves, the plot showed something that neither the tables nor the formula could fully display – *they established a graphical law*. The saturation region was the new finding, and while a table of measurements could show that such a maximum exists in the case of some iron rods, it could not show this for all the rods (because for a certain thickness, the currents had to be too high to reach the saturation region). Instead, the plots allowed one to extrapolate these results independently of Müller's empirical formula.

2.3 The inflexion

While German textbooks focused the attention of students on the asymptotic behavior of the magnetic curves for large magnetizing forces, in the early 1870s the linear behavior of the first part of the curve came to be questioned.

The first challenge came from Aleksandr Stoletow, a young Russian physicist who had attended the lectures of Helmholtz, Kirchhoff and Wilhelm Weber and who had just finished the work on his dissertation in Kirchoff's laboratory in Heidelberg.[107] Stoletow published the results of his dissertation in the *Annalen der Physik* in 1872 and the *Philosophical Magazine* in 1873.[108] Stoletow looked at the magnetic susceptibility of iron as a function of the magnetizing force. While previous experimental results only showed that the susceptibility dropped with the increase of the magnetizing force, Stoletow managed to show that for low magnetizing forces the susceptibility actually increased until it reached a maximum. He first inferred this claim from the table of measurements, which he then plotted. The advantage of the plot was that it allowed him to show that both his measurements and those of other experimentalists

107. "Stoletov, Aleksandr Grigorievich," in *Complete Dictionary of Scientific Biography*, vol. 13 (Detroit: Charles Scribner's Sons, 2008), 79–81.

108. A. Stoletow, "On the Magnetizing-Function of Soft Iron, Especially with Weaker Decomposing-Powers," *Philosophical Magazine Series 4* 45, no. 297 (1873): 40–57; A. Stoletow, "Ueber die Magnetisirungsfunction des weichen Eisens, insbesondere bei schwächeren Scheidungskräften," *Annalen der Physik* 222, no. 7 (1872): 439–463.

displayed the same pattern (a maximum of susceptibility for low magnetizing forces), thus boosting his claim (see Fig. 6.23).

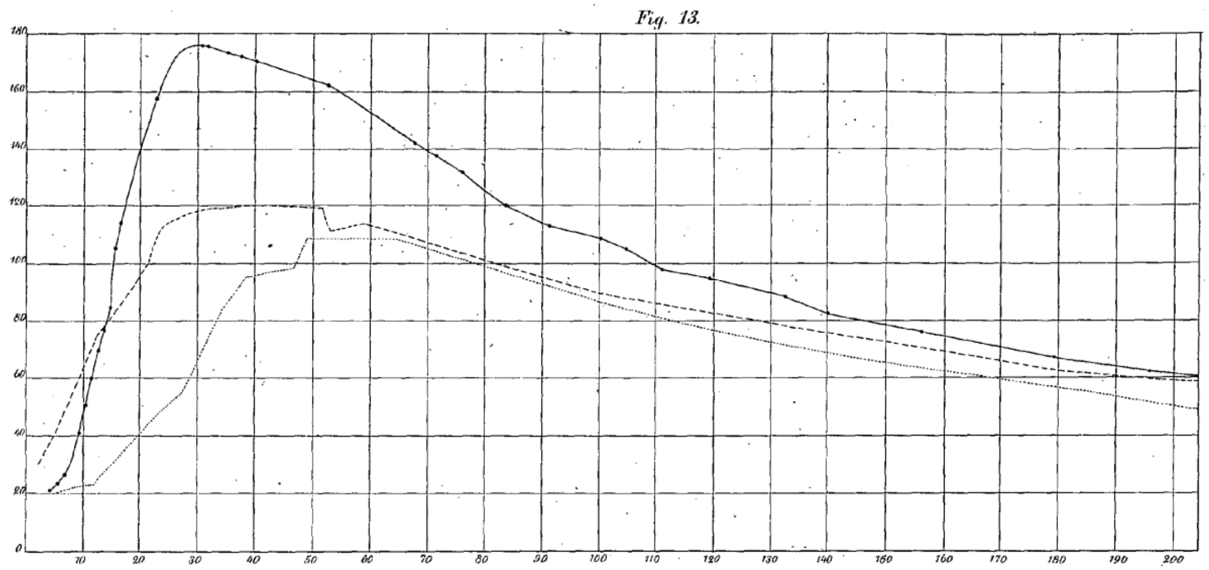


Figure 6.23
The full line corresponded to Stoletow's own experiemental results, while the dotted lines represented experimental data obtained from von Quintus Icilius. As in the case of Müller's magnetic curves, the general purpose of the plot was to establish a general pattern by comparing the behavior of the magnetic curve for different series of measurements. Source: A. Stoletow, "Ueber die Magnetisirungsfunction des weichen Eisens, insbesondere bei schwächeren Scheidungskräften," *Annalen der Physik* 222, no. 7 (1872): 439–463.

Only a few months later, the *Philosophical Magazine* published the very similar results of a young American physicist – Henry A. Rowland.[109] Though Rowland carried out his experiments between 1870-1871, he had to postpone publication because the first draft of his article was rejected by the American Journal of Science. Rowland's article was only published after Maxwell's intervention at the *Philosophical Magazine*.[110] After presenting the tables of experimental measurements Rowland did not directly interpret the numbers, but exclaimed that "the best method of studying these Tables is to plot them".[111] This should come as a striking differ-

109. Henry A. Rowland, "On Magnetic Permeability, and the Maximum of Magnetism of Iron, Steel, and Nickel," *Philosophical Magazine Series 4* 46 (1873): 140–158.

110. Maxwell, *The Scientific Letters and Papers of James Clerk Maxwell*, vol.2, 479, No. 466. For a detailed description of Rowland's work on magnetism see: John David Miller, "Rowland's Magnetic Analogy to Ohm's Law," *Isis* 66, no. 2 (1975): 230–241; D. W. Jordan, "The Magnetic Circuit Model, 1850-1890: The Resisted Flow Image in Magnetostatics," *The British Journal for the History of Science* 23, no. 2 (1990): 131–173.

111. Stoletow, "On the Magnetizing-Function of Soft Iron, Especially with Weaker Decomposing-Powers," 153.

ence from the German style of using tables and plots, as illustrated in the articles of Müller or Stoletow. For example, Stoletow's main insights were directly inferred from the tables of results from which it "hence becomes evident the remarkable fact..." or "we see further from these tables...".[112] Though their experimental method and results agreed, Rowland dismissed Stoletow's graphical representation because it made "the curve infinitely long, it forms a very irregular curve, and it is impossible to get the maximum of magnetism from it".[113] He considered that Stoletow failed to identify a law because of his choice of variables that gave rise to a highly irregular curve: "He plots a curve showing the variation of k; but he plots with reference to R [the magnetic force] as abscissa instead of $R \cdot k$ [the magnetization], and thus fails to determine the law".[114]

Rowland considered more closely the best ways of plotting his experimental results. He first plotted the magnetization as a function of the magnetizing force because the curve was regular and "it is often employed, and gives a pretty good idea of the action" (see Fig. 6.25).[115] As I have showed above, this style of graphical representation had become widely used after it was made famous by J. Müller's articles on the saturation of the magnetization. In the case of Rowland, the main source of inspiration for this graphical representation was most probably Maxwell's *Treatise on Electricity and Magnetism* (1873) which was also cited in his paper.[116] While Stoletow's curve illustrated well his conclusion, it lacked the accepted and widely spread graphical meaning of Müller's magnetic curves. For example, the susceptibility curves failed to clearly show that the magnetization approached a maximum.[117] The correction that Rowland identified in the behavior of the magnetic curves – "the concavity of the curve

112. Stoletow, "On the Magnetizing-Function of Soft Iron, Especially with Weaker Decomposing-Powers," 43.
113. Rowland, "On Magnetic Permeability, and the Maximum of Magnetism of Iron, Steel, and Nickel," 153.
114. Ibid., 141-142.
115. Ibid., 153-154.
116. We also know that Rowland was one of the first Americans to buy Maxwell's *Treatise* in mid-April 1873. As Rowland admitted, he knew only little about the mathematical theory of magnetism in 1870 when he started his experiments; for this reason he invented his own units which he did not bother to modify after he read Maxwell's *Treatise*. See Miller, "Rowland's Magnetic Analogy to Ohm's Law," 240.
117. For that to be visible, the susceptibility curves would have had to be greatly prolonged until they approached the abscissa.

at its commencement, which indicates a rapid increase of permeability" – would also become a defining feature such as Müller's horizontal asymptote.[118]

Despite the advantages, this type of representation still suffered from two of the shortcomings of Stoletow's curve – the curve was infinitely long and one could not precisely determine the maximum of magnetization. Rowland's insight was to choose a different pair of variables – instead of plotting the susceptibility (or permeability) as a function of the magnetizing force, he plotted it as a function of the magnetization. This choice would not have been immediately transparent to an experimentalist who, in general, plotted the quantity under investigation as a function of the quantity that was experimentally controlled. One considered that the quantity that was directly varied caused the change in the investigated quantity. Though counter-intuitive, Rowland choice of variables had the advantage of producing a "perfectly regular" curve of finite dimensions (see Fig. 6.24). The regularity, symmetry and finiteness of the curve allowed Rowland to guess an appropriate fitting function:

$$\mu = \beta \sin\left(\frac{\mathfrak{B} + b\mu + \pi}{D}\right)$$

Furthermore, he could graphically determine the parameters of the function: β was the maximum value of μ and was determined by the height of the curve (BD); b established the inclination of the diameter; π was the line AO; D depended on the line AC. The qualities of the curve led Rowland to believe that his method of plotting proved "in the most unequivocal manner that magnetic permeability is a function of the magnetization of the iron and not of the magnetizing force".[119]

Stoletow's and Rowland's experimental finding of a maximum for the permeability attracted considerable attention. While both their curves illustrated and supported their con-

118. Rowland, "On Magnetic Permeability, and the Maximum of Magnetism of Iron, Steel, and Nickel," 154.
119. Henry A. Rowland, *The Physical Papers of Henry Augustus Rowland* (Baltimore: The Johns Hopkins Press, 1902), 105.

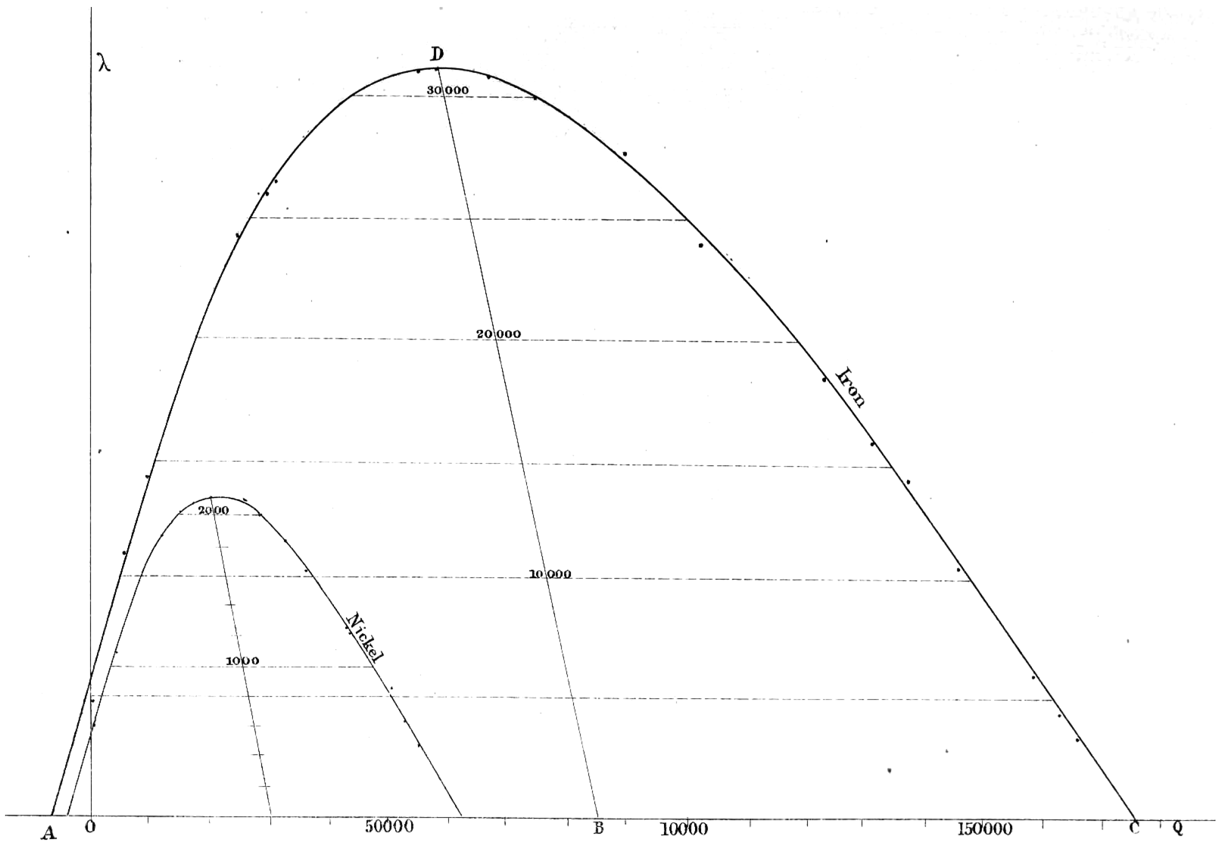


Figure 6.24
Rowland made the unusual choice of plotting the permeability (here, $\lambda = 4\pi\mu$) as a function of the magnetic induction (here, $Q = \mathfrak{B}$), opposed to Stoletow who plotted the permeability as a function of the magnetic force (see Fig. 6.23). Because the plot was highly regular, symmetric and finite, it allowed Rowland both to find an empirical formula and to determine graphically the parameters of the formula. Source: Henry A. Rowland, "On Magnetic Permeability, and the Maximum of Magnetism of Iron, Steel, and Nickel," *Philosophical Magazine Series 4* 46 (1873): 140–158.

clusions, the new feature of Rowland's curve, its concavity or inflexion, also inspired some theoretical considerations. Edmond Bouty, a young French physicist who had been working under the guidance of Jules Jamin, published a series of articles concerning his experiments on the temporary and permanent magnetization of thin needles in which he determined the overall magnetism in terms of a magnetizing current. Though he did not plot his results, he remarked that if one were to draw the experimental curve he would notice that it is "at first concave towards the positive ordinates, the curve then presents an inflexion point [...] and

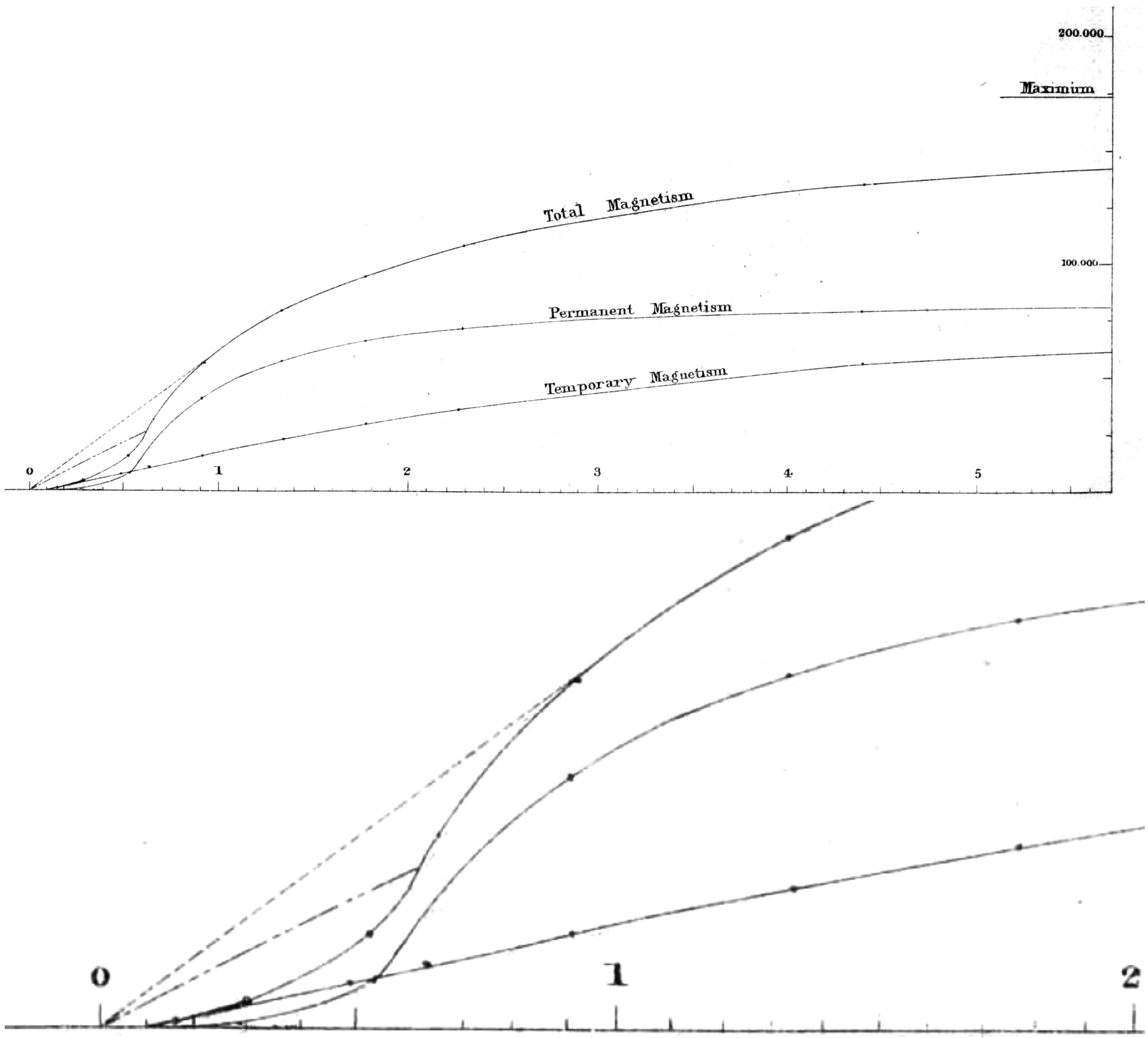


Figure 6.25
Rowland's plot engaged directly with Maxwell's graphical representations of Weber's formula. The dotted line corresponded to Weber's original formula, while the dotted line represented Maxwell's correction of the formula. The full lines represented Rowland's equation which closely fitted the experimental points and showed that for low magnetizing forces the magnetization did not increase linearly but displayed an inflexion point where the curve changed from concave to convex. This feature of the magnetic curve would become one of the main staples of Rowland's contribution. Source: Henry A. Rowland, "On Magnetic Permeability, and the Maximum of Magnetism of Iron, Steel, and Nickel," *Philosophical Magazine Series 4* 46 (1873): 140–158.

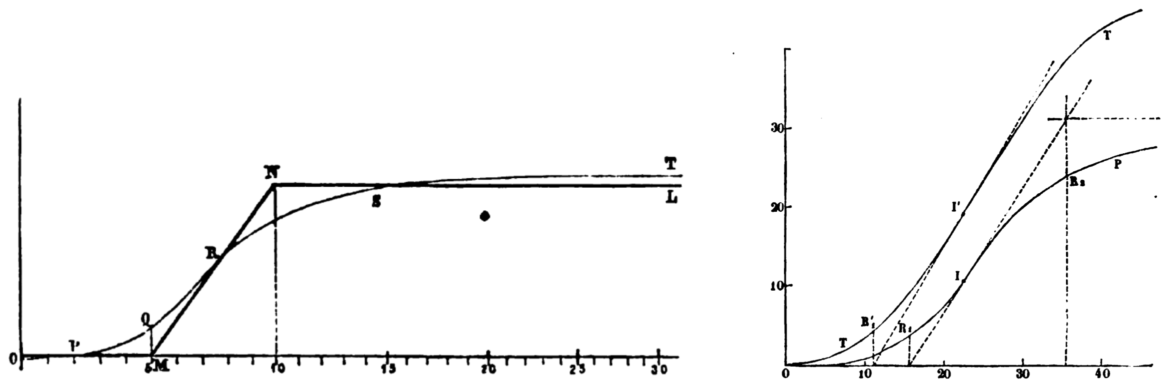
approaches asymptotically a parallel to the axis of the abscissas".[120] Based on these graphical features, Bouty related his results to those of Rowland and Stoletow regarding the magnetization of iron and steel:

> These characters are identical with those of the curves which, according to Rowland and Stoletow, represent the magnetizing function of iron or steel. The general features are everywhere the same; and the resemblance is especially striking when, opposite the preceding curve, we draw that found by Rowland for Bessemer steel.[121]

Bouty did include a redrawing of Rowland's magnetic curve, but for a different purpose – he used a geometrical interpretation of the plot to illustrate Coulomb's concept of "coercive force". Coulomb's theory of magnetism had long fallen out of fashion, and Bouty remarked that "it is interesting to remark that, if the hypothesis of the coercive force is powerless in representing the ensemble of phenomena, it can represent well enough the behavior of permanent magnetization".[122] Bouty employed a geometrical construction to show the correspondence between Rowland's experimental curve and Coulomb's theory. In Fig. 6.26, he approximated Rowland's experimental curve $OPQRST$ with the broken lines OM, MN, NL. Coulomb's theory of magnetism claimed that when the coercive forces (C) surpassed the magnetizing forces (F), $F < C$, the magnetic induction was null (the line OM in Fig. 6.26); when the magnetizing forces surpassed the threshold, $F > C$, the magnetic effect was proportional to $F - C$ and thus increased linearly as represented by the line MN.[123] While Bouty did not find Coulomb's theory satisfactory, he did apply a year later the same geometrical analysis to his experimental results on temporary magnetism because by looking at the curve of temporary magnetism "we can see that it presents the same general character as the curve P of permanent magnetism".[124] A few years later when charged with revising Jules Jamin's famous *Cours de physique de l'École polytechnique* (1883), Bouty also added a short description of Row-

120. E. Bouty, "Études sur le magnétisme (deuxième partie)," *Annales scientifiques de l'École Normale Supérieure* 5 (1876): 130.
121. Ibid., 130.
122. E. Bouty, "Études sur le magnétisme," *Annales scientifiques de l'École Normale Supérieure* 4 (1875): 50.
123. Ibid., 50-51.
124. Bouty, "Études sur le magnétisme (deuxième partie)," 141.

land's experiment and a reproduction of his magnetic curve, along with his own diagram and geometrical interpretation of them.[125]



(a) Bouty 1875 **(b)** Bouty 1876

Figure 6.26
Bouty's geometrical interpretation of Rowland's curve and inflexion point. In Fig-a Bouty used Rowland's curve as an illustration of Coulomb's theory of magnetism which predicted that the magnetization would be null until the magnetizing force surpassed the coercive force (M in the plot), after which it would increase linearly with the magnetizing force (the line MN). This geometrical analysis was later used by Bouty in the analysis of temporary magnetism. In Fig-b he showed that both the permanent and temporary magnetic qualities of a substance could be "characterized very well" by the end points of the line passing through the inflexion point (R_1, R_2 for the curve P, and R_1' for the curve T). Source: (a): E. Bouty, "Études sur le magnétisme," *Annales scientifiques de l'École Normale Supérieure* 4 (1875): 50; (b): E. Bouty, "Études sur le magnétisme (deuxième partie)," *Annales scientifiques de l'École Normale Supérieure* 5 (1876): 141.

2.4 The Tail

In the mid 1880s, Rowland's curve of permeability (Fig. 6.24) came under the scrutiny of experimentalists who measured the magnetization of iron in strong magnetic fields – up to ten times higher than in Rowland's experiments. In his graphical representation of the permeability as a function of magnetization Rowland extrapolated the curve beyond the last data point until it intersected the abscissa to obtain a regular and finite curve. As we have seen in the previous section, for Rowland this was an essential feature because it allowed him to find an empirical formula. However, new studies showed that the permeability did not approach zero for the magnetic fields predicted by Rowland's curve and formula. The point of interest for our study

125. E. Bouty and J. Jamin, *Cours de physique de l'École polytechnique* (Gauthier-Villars, 1883), vol. 4, 298-300.

is that these experimentalists engaged not so much with Rowland's experimental results or empirical formula, but with his graphical representation. It was the neat and regular shape of his plot that had the biggest impact, and it was this graphical claim that needed correction. In these papers the plots were not the means through which one carried a debate about empirical formulas, but rather the plots themselves became the object of debate.

Rowland had already noticed that in his experiments on the magnetization of cobalt, "on plotting the curve I was much surprised to find an entire departure from the regularity which I had before found in all curves taken from iron and nickel when the metal was homogenous".[126] Rowland ascribed these problems to want of homogeneity in the cobalt rings, or as an effect of the increase in temperature with higher magnetic fields. In the end, because of this irregularity, he only published permeability curves for iron and nickel but not for cobalt. But even in the case of iron which produced the most regular curves and were best fitted by his formula, "a sort of tail appears on the curve showing the permeability".[127] Rowland ascribed this troublesome issue with the curve also to a lack of homogeneity in the iron – "it is evident that this tail must always show itself whenever the section of the ring is not homogeneous throughout". However, the curves published by Rowland never included this "tail" which was present only in the table of measurements – "a tail appears to the curve like that in Table III" (see Fig. 6.27).[128]

As the magnetic fields became stronger, other experimentalists were bothered by the tail of the permeability curve. The matter was not without consequence because the intersection of the permeability curve with the abscissa was used as an estimate for the maximum of magnetization. If the permeability was never zero, the very existence of the maximum would have been under dispute. In 1881, Carl Fromme pointed out that when he graphically represented his experimental measurements for the magnetization of iron after "Rowland's method" he found that the permeability curve had an inflexion point [Wendepunkt] after which the con-

126. Rowland, *The Physical Papers*, 70.
127. Ibid., 44.
128. Ibid., 52.

cave curve became convex.[129] No plots were included in this short two-page notice. Despite knowledge of the existence of such a tail, many experimentalists preferred to ignore this feature when plotting the permeability curves. One experimentalist even acknowledged that the intersection of the curve with the horizontal axis was unphysical –

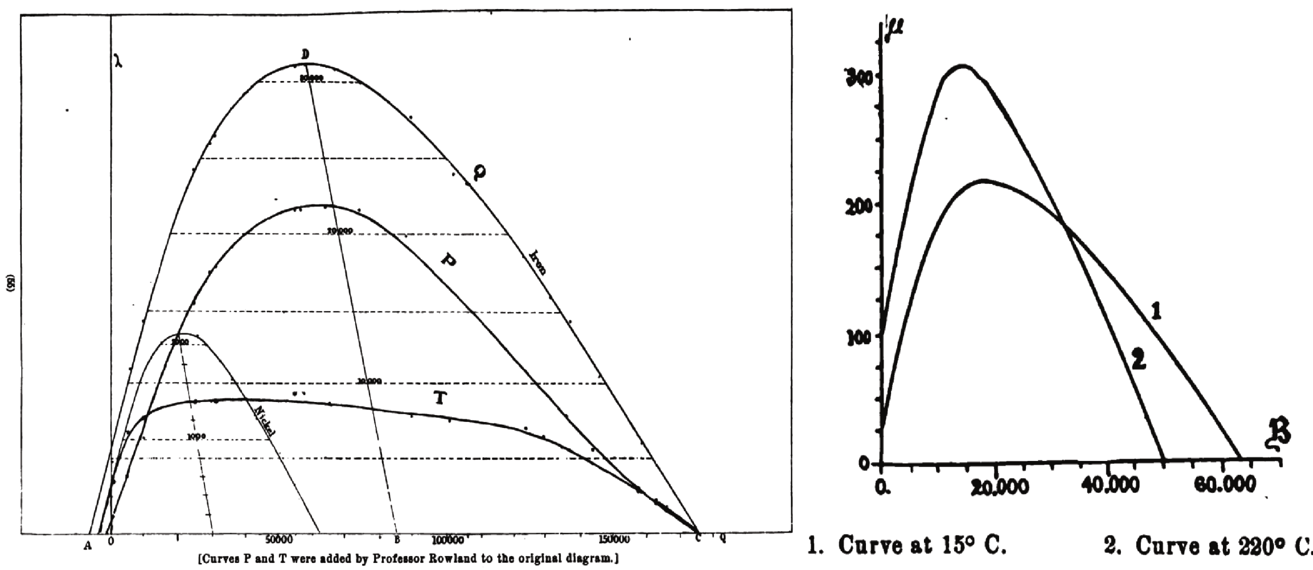
> it is to be expected that the form of the curve will change as it approaches the axis, to which it no doubt becomes finally parallel, since, $\mu = 1$ for non-magnetic bodies, and in no known case is as low as zero. Such an inflection was observed in the curve for iron by Rowland and by Fromme, as before stated.[130]

Though he pointed out the inflexion of the tail, such a feature was not graphically represented (see Fig. 6.27).

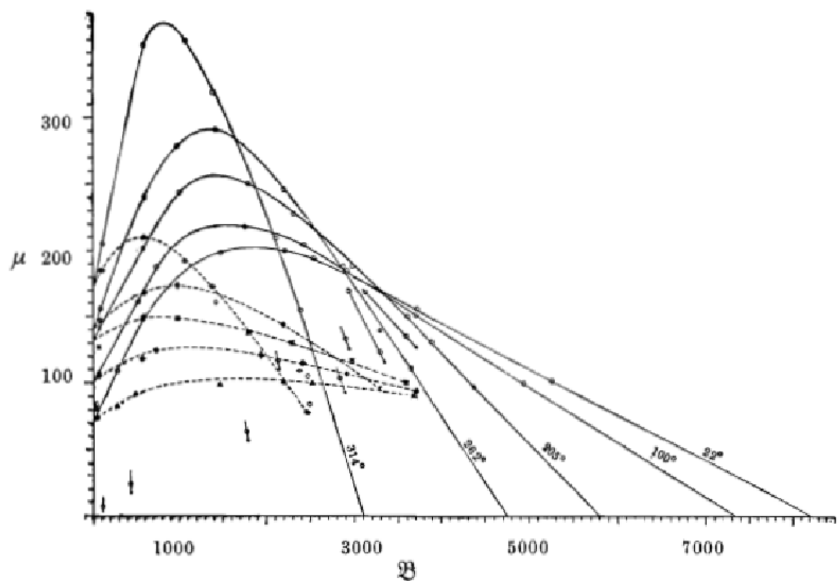
The first graphical challenge of Rowland's curves came from Shelford Bidwell and James Alfred Ewing (1855-1935). Bidwell remarked that "in connexion with the well-known experiments and views of Professor Rowland, the figures thus obtained are of the highest interest".[131] He provided a representation of "Rowland's curve" along with the representation of his experimental data (see Fig. 6.28). The pain taken by Bidwell to fit the range of his measurements along Rowland's curve comes to show the *iconic* status of the curve. What Bidwell set out to disprove with his plot was a *graphical claim*. In 1887, Ewing employed a similar graphical representation that only focused on the tail of the curve. He described the plot as being drawn in "the manner introduced by Rowland for showing the relation of $\mathfrak{B}$ to μ" and pointed to the "inflection that a curve of μ and $\mathfrak{B}$ begins to have when the magnetising force is raised sufficiently high" (see Fig. 6.28). The reader was immediately notified that

> This feature of the curve of μ and $\mathfrak{B}$ was not noticed by Rowland himself, who applied to his curve an empirical formula which fails to take account of it. It has, however, been noticed by several

129. Carl Fromme, "Notiz über das Maximum des temporären Magnetismus beim weichen Eisen," *Annalen der Physik* 249, no. 8 (1881): 695–696.
130. C. A. Perkins, "Variation of the Magnetic Permeability of Nickel at Different Temperatures," *American Journal of Science* s3-30, no. 177 (1885): 229.
131. Shelford Bidwell, "On the Lifting Power of Electromagnets and the Magnetisation of Iron," *Proceedings of the Royal Society of London* 40, no. 242 (1886): 494.



(a) Rowland 1873 & 1874

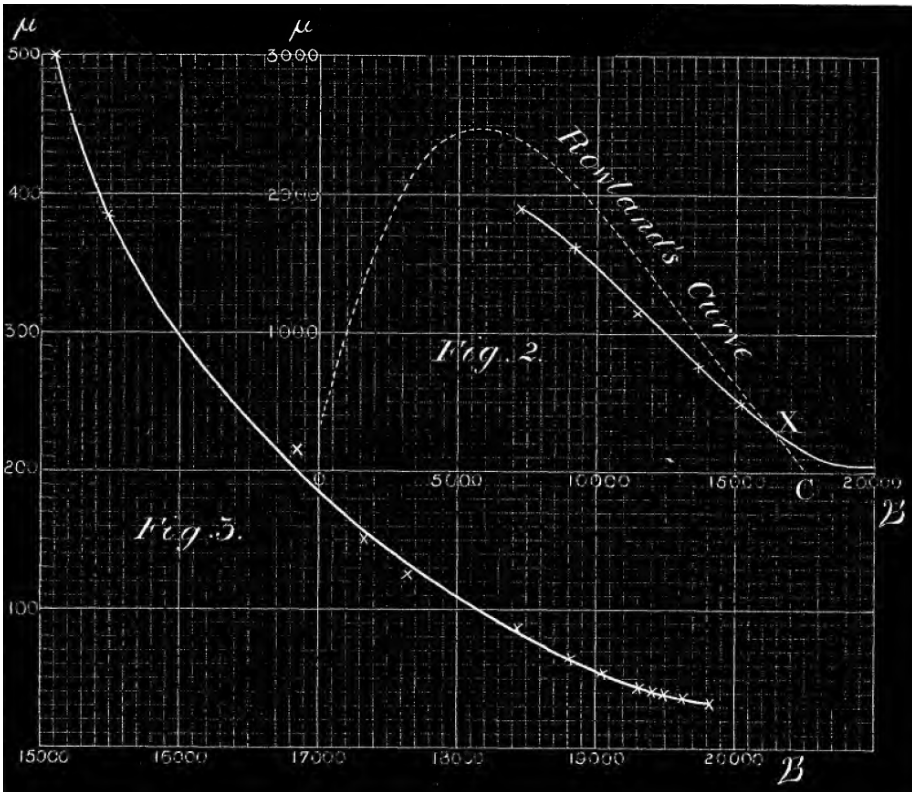


(b) Perkins 1885

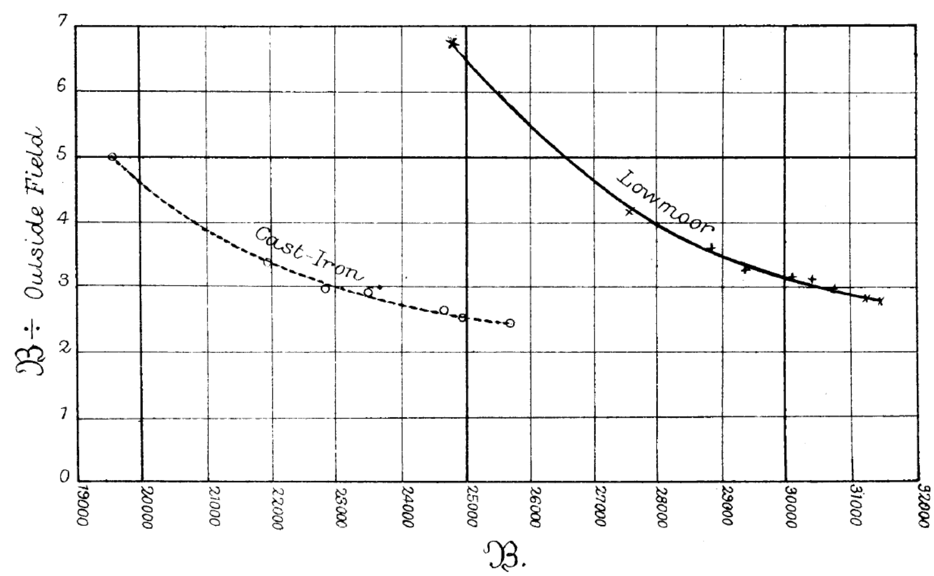
Figure 6.27
Though both authors acknowledged that the experimental results indicated an inflexion of the curve for high magnetic fields, such a feature was not depicted on the actual curves. Source: (a):Henry A. Rowland, *The Physical Papers of Henry Augustus Rowland* (Baltimore: The Johns Hopkins Press, 1902), 55;64; (b):C. A. Perkins, "Variation of the Magnetic Permeability of Nickel at Different Temperatures," *American Journal of Science* s3-30, no. 177 (1885): 218–230.

later observers [Fromme, Bidwell, Ewing].[132]

132. James Alfred Ewing and William Low, "On the Magnetisation of Iron in Strong Fields," *Proceedings of the Royal Society of London* 42 (1887): 208.



(a) Bidwell 1886



(b) Ewing & Low 1887

Figure 6.28
The inflection of the tail of "Rowland's curve". To clearly make visible this feature, a special style of representation was required: the curve was to be plotted only for the relevant range of high magnetizing forces. Source: (a):Shelford Bidwell, "On the Lifting Power of Electromagnets and the Magnetisation of Iron," *Proceedings of the Royal Society of London* 40, no. 242 (1886): 486–496; (b):James Alfred Ewing and William Low, "On the Magnetisation of Iron in Strong Fields," *Proceedings of the Royal Society of London* 42 (1887): 200–210.

If Müller used curves to support his conclusions, for both Bidwell and Ewing the curves were an integral part of their conclusion.

When carefully read, Ewing's articles reveal the new status of the curves not only as objects of credit (notice in the passage above Ewing's care in pointing out the observers who had identified the new feature), but also as irreducible epistemological objects. Ewing considered that the upward inflection of the "descending limb" in Rowland's curve was not only a noteworthy discovery, but also a warning against the hasty reliance on empirical formula fitted to the curve:

> These considerations, if they serve no other useful purpose, show the futility of drawing conclusions as to the initial and ultimate values of the magnetic susceptibility of iron in indefinitely low and indefinitely high fields, from observations made, as all observations must be made, in fields of finite magnitude. For this reason it appears that an empirical formula, such as Rowland applies to the curves of μ and $\mathfrak{B}$, must be misleading when pushed beyond the range of actual experiment. If we do not know whether $\mathfrak{I}$ or $\mathfrak{B}$, or either of them, attains a maximum, it is a truism to say that there is no use in assigning numerical values to that maximum. The results obtained by extending a formula past the limits of experience have scarcely even a speculative interest if the basis of the formula is not an intelligible physical theory and its terms are not capable of physical interpretation.[133]

While the extrapolation of empirical formulas without a physical basis was meaningless, "[c]urves of this kind [see Fig. 6.29] suggest several interesting theoretical questions regarding the results which we should find if we were able to extend indefinitely the range of observations, both towards vanishingly low and towards indefinitely high values of $\mathfrak{H}$ [H in fraktur]".[134] Instead of simply extrapolating the curve, the graphical trace of the curve suggested possible continuations with different physical consequences. In Ewing's major study "Experimental Researches in Magnetism" (1885) which summed his work on the topic, no empirical formulas nor any graphical representations of formulas could be found. Instead the article was lavished with 11 plates and 60 plots! Even when adjusted to the length of the article (of almost

133. James Alfred Ewing, "Experimental Researches in Magnetism," *Philosophical Transactions of the Royal Society of London* 176 (1885): 576-577.
134. Ibid., 574.

120 pages), this profusion of plots is a signal of a new style of presentation which characterized Ewing's scientific publications.[135]

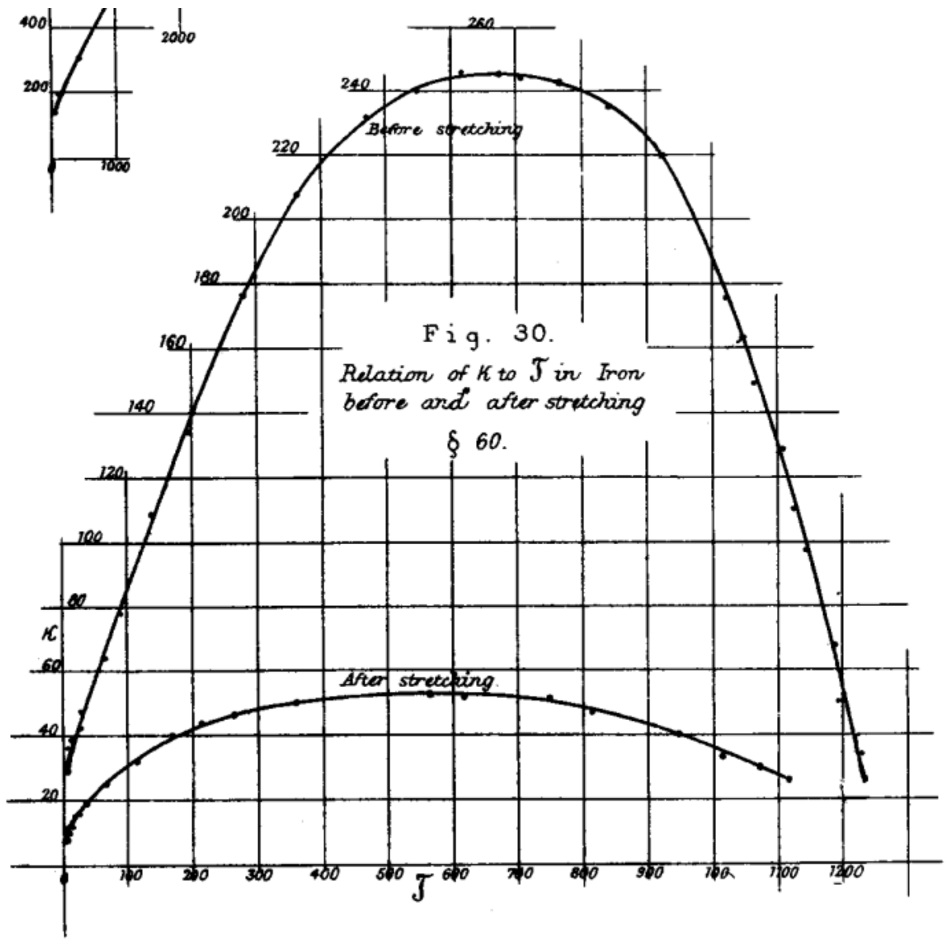


Figure 6.29
Ewing's *graphical restraint* limited the curve only to the experimental points (cf. Fig. 6.27). This graphical choice was correlated with the "theoretical questions" suggested by such curves. Source: James Alfred Ewing, "Experimental Researches in Magnetism," *Philosophical Transactions of the Royal Society of London* 176 (1885): 523–640.

Instead of trying to find empirical formulas, Ewing focused on the curves to find pattern and features. First, the curves were not *singular* representations of the experimental data. Ew-

135. To gain a better sense of the scale, I have listed some of Ewing's articles that used a high number of plots: 60 plots – Ewing, "Experimental Researches in Magnetism"; 34 plots – James Alfred Ewing, "Effects of Stress and Magnetisation on the Thermoelectric Quality of Iron," *Philosophical Transactions of the Royal Society of London* 177 (1886): 361–381; 10 plots – James Alfred Ewing and G. C. Cowan, "Magnetic Qualities of Nickel," *Philosophical Transactions of the Royal Society of London. A* 179 (1888): 325–332; 41 plots – James Alfred Ewing and Helen G. Klaassen, "Magnetic Qualities of Iron," *Philosophical Transactions of the Royal Society of London. A* 184 (1893): 985–1039.

ing specifically tried to identify features that would repeat across several sets of measurements that were exposed to some similar conditions. Then, he would try to set-up the experiment such that those features of the curve would be made visible (see Fig. 6.28). This new epistemological attitude can be seen unfolding if we pay close attention to the language employed by Ewing. When read carefully, Ewing's experimental aims were to reduce "effects of a very complex character" to a behavior that "was clearly marked and perfectly regular" that could be described as "thoroughly characteristic" of a certain phenomenon or material.[136] The 1885 article had a special section on "characteristics of the curve of magnetisation" or "curves of the same character".[137] Ewing emphasized the ability of his curves to display certain phenomena in a "decidedly" fashion, or he referred to the "decided character" of a phenomenon. This led him to create a strong correlation between characters and features of the curve, and physical properties or phenomena. In the following passages I have underlined this correlation between mentions of **the features or characteristics**, **the description of those features** and **their physical interpretation**.

> The curves here have **the square-shouldered form**, which is often found in steel as well as almost always in annealed soft iron. (The absence of **this characteristic** in Ring I., as well as its comparatively low permeability, was an indication of its being in a somewhat hard state.) [...]
> **A characteristic of the curves of fig. 5** is **the remarkably uniform rate** at which B changes with respect to H during a great part of the process of magnetic reversal. After the shoulder of the descending curve has been turned, by applying a sufficiently strong demagnetizing force, the quantity dB/dH takes a large and nearly constant value which it retains until a tolerably strong reversed magnetization has been produced. **The steep and nearly straight portion of the curve** which corresponds to this part of the process has, moreover nearly the same gradient **in all except the smallest cycles. The same characteristic** will be found in examples of annealed iron, to be given later, and the gradient in them is of course even steeper than it is here. [...]
> A reference to fig. 12 will show that **the overlapping of one cyclic curve by the next lower curve** (corresponding to a slightly narrower magnetic range) **is characteristic of low cycles as well as of high ones**.[138]

> The curves connecting these quantities **always form loops as in fig. 2**, and **the characteristic** mentioned in the last sentence of §11 appears to be quite general. [...]

136. Ewing, "Experimental Researches in Magnetism," 582.
137. Ibid., 574.
138. Ewing and Klaassen, "Magnetic Qualities of Iron."

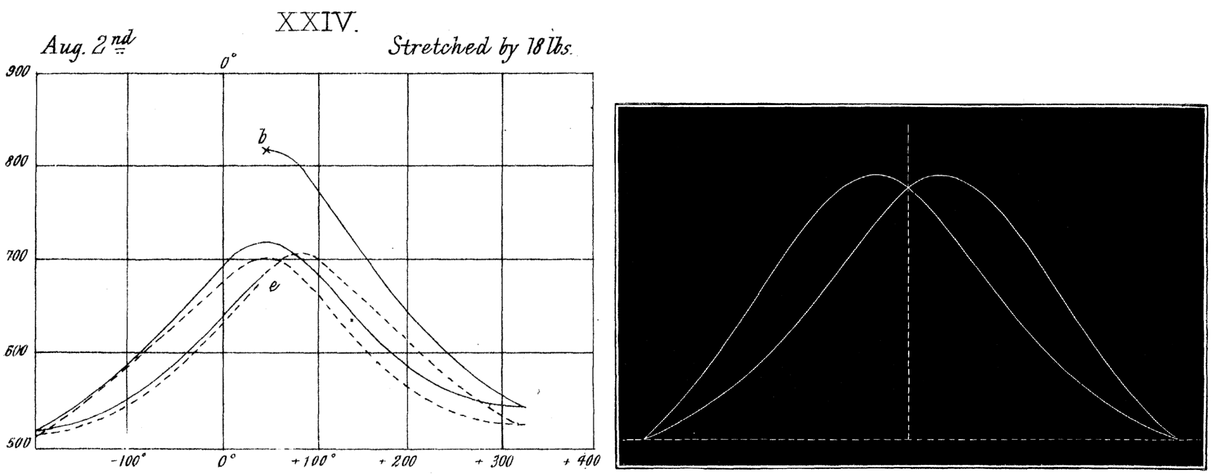
> **A noticeable feature** in this diagram is… **This characteristic is very conspicuous** in the early parts of the operation, but **disappears when the magnetisation becomes strong**. **The same feature** is present in many other diagrams. […]
> **This characteristic of the curves** affords strong confirmation of the idea that retentiveness in soft iron is chiefly due to **a resistance to the rotation of Weber's molecular magnets** of a kind resembling the static friction of solid bodies. […]
> Hysteresis affects both experiments, though quite differently, but by examining the two sets of curves together we can see the general relations of magnetic susceptibility to stress in **the features which are exhibited by both**. […]
> Several other experiments of a similar kind have shown that **the sloping curves of fig. 14** are **thoroughly characteristic of strained iron**. […]
> Earlier and later experiments agree in showing that this behaviour, which will now be described, **is thoroughly characteristic of stretched iron**. […]
> Another **characteristic of the curves**, obviously **attributable to hysteresis**, is **the comparatively easy gradient** at the beginning of the on curve and again at the beginning of the off curve. […]
> **The curve already described as characteristic** of **the effects of stress on a stretched wire** in an inducing field reappears here as **equally characteristic** of **the effects of stress on a stretched wire** when there is no inducing field, and when the magnetism which is varied is wholly residual. […]
> …but **the general character of the curves** is substantially the same in both cases. […]
> **The general characteristics of the curves** are these…
> **The presence of stress** in an annealed wire tends to **round off the outlines of the curve** of magnetisation, so that it resembles somewhat **the curve described in fig. 33 as characteristic of a wire** which has been stretched beyond its limit of elasticity, and this happens although the stress is too small to give the wire any permanent set, or to harden it appreciably. [139]

2.5 The Loop

James Ewing is often credited with discovering the phenomenon of hysteresis. As in the case of most discoveries, the historical record is more complex and its interpretation is affected by hindsight.[140] Several other physicists before Ewing produced what nowadays would be seen as hysteresis-like loops, but which at the time were described as closed curves or cycles. Such curves were published by William Thomson for the variation of the magnetism inside a wire over a cycle of twisting (1879); Emil Cohn for the effects of stress on the current through an

139. Ewing, "Experimental Researches in Magnetism."
140. For a history of hysteresis see Matthias Dörries, "Prior History and Aftereffects: Hysteresis and "Nachwirkung" in 19th-Century Physics," *Historical Studies in the Physical and Biological Sciences* 22, no. 1 (1991): 25–55.

iron wire (1879); Emil Warburg for the magnetization cycles (1881); and James Ewing for the thermoelectric and magnetic effects of stress on and the magnetization cycles (1881-1883).[141] What interests us here are not the full details of these experiments but solely their graphical style of representation.



(a) (b)

Figure 6.30
William Thomson's "exceptional diagrams". The dotted curve in Fig-a showed "the general character of the effect of continued periodic applications of positive and negative torsion, through equal angles on the two sides of zero". The "general character" of all the experimental curves such as the one in Fig-a was *essentialized* and *abstracted* in Fig-b; this curve showed that "if the experiment was continued long enough, the history of the variation of magnetization would in every case be represented by a curve like that in the annexed sketch". Source: William Thomson, "Electrodynamic Qualities of Metals. Part VII. Effects of Stress on the Magnetization of Iron, Nickel, and Cobalt," *Philosophical Transactions of the Royal Society of London* 170 (1879): 72.

William Thomson's study from 1879, which was part of his highly influential and often quoted series of articles on the "Electrodynamic Qualities of Metals", employed an unusual high number of plots because the experimental results were more easily and fully summarized graphically.[142] In total, twenty-nine diagrams were used to summarize each run of the

141. William Thomson, "Electrodynamic Qualities of Metals. Part VII. Effects of Stress on the Magnetization of Iron, Nickel, and Cobalt," *Philosophical Transactions of the Royal Society of London* 170 (1879): 55–85; Emil Cohn, "Ueber das thermo-electrische Verhalten gedehnter Drähte," *Annalen der Physik* 242, no. 3 (1879): 385–403; E. Warburg, "Magnetische Untersuchungen," *Annalen der Physik* 249, no. 5 (1881): 141–164.

142. As readily acknowledged by William Thomson, these experiments had been carried out by Donald Macfarlane, see Thomson, "Electrodynamic Qualities of Metals. Part VII. Effects of Stress on the Magnetization of Iron, Nickel, and Cobalt," 56,68. Because this detailed graphical style had not been used in Thomson's previous articles in the "Electrodynamic Qualities of Metals" series, we are inclined to attribute these diagrams to Macfarlane. For Macfarlane's role as Thomson's assistant see Silvanus P. Thompson, *The Life of William Thomson, Baron Kelvin of Largs* (London: Macmillan, 1910), vol.2, 626, 650-652.

experiment corresponding to different stretching weights, and different initial and final magnetizations. The diagrams were used to point out common "interesting features". Through the "continued periodic applications of positive and negative torsion" one could obtain a curve which represented "the general character of the effect" (see Fig. 6.30).[143] Emil Cohn had completed his doctoral dissertation in 1878 at the University of Strassbourg on a topic inspired by William Thomson's previous work on the thermoelectric properties of wires.[144] Cohn's results were also published in 1879 in an article which included two experimental plots with closed curves for the variation of the current through a wire when cyclically stretched (see Fig. 6.31). While Thomson's curves were highly regular (in part because they were produced after "continued periodic applications" of torsion and not after a single cycle), Cohn's plot connected with a broken line the experimental points (a common approach in German scientific articles).

Two years later Emil Warburg, who was familiar with Thomson's and Cohn's work, published a study on magnetism which was praised by Albert Einstein as "one of the most beautiful fruits of his work".[145] While Cohn and Thomson studied the electric and magnetic effects of stress, Warburg observed the variation in magnetization during a cyclical change in the magnetic force. Besides representing graphically his experimental results, Warburg also provided a physical interpretation of the cycle: the area determined by the closed loop represented the energy lost during a full cycle of magnetization. While this result was a straightforward consequence of the mathematical theory, the experimental proof that such curves existed made it relevant. Warburg ended by presenting an analogy of the phenomenon of magnetization with the friction of solids; the connection was further illustrated by representing the relation between position and force for a block resting on a rough surface and connected to a tensionless spring which pulled the block with a force that could increase continuously; after a point, the force was decreased back to zero (see Fig. 6.32).

143. Thomson, "Electrodynamic Qualities of Metals. Part VII. Effects of Stress on the Magnetization of Iron, Nickel, and Cobalt," 72.
144. Dörries, "Prior History and Aftereffects," 38-39.
145. Einstein quoted in ibid., 40.

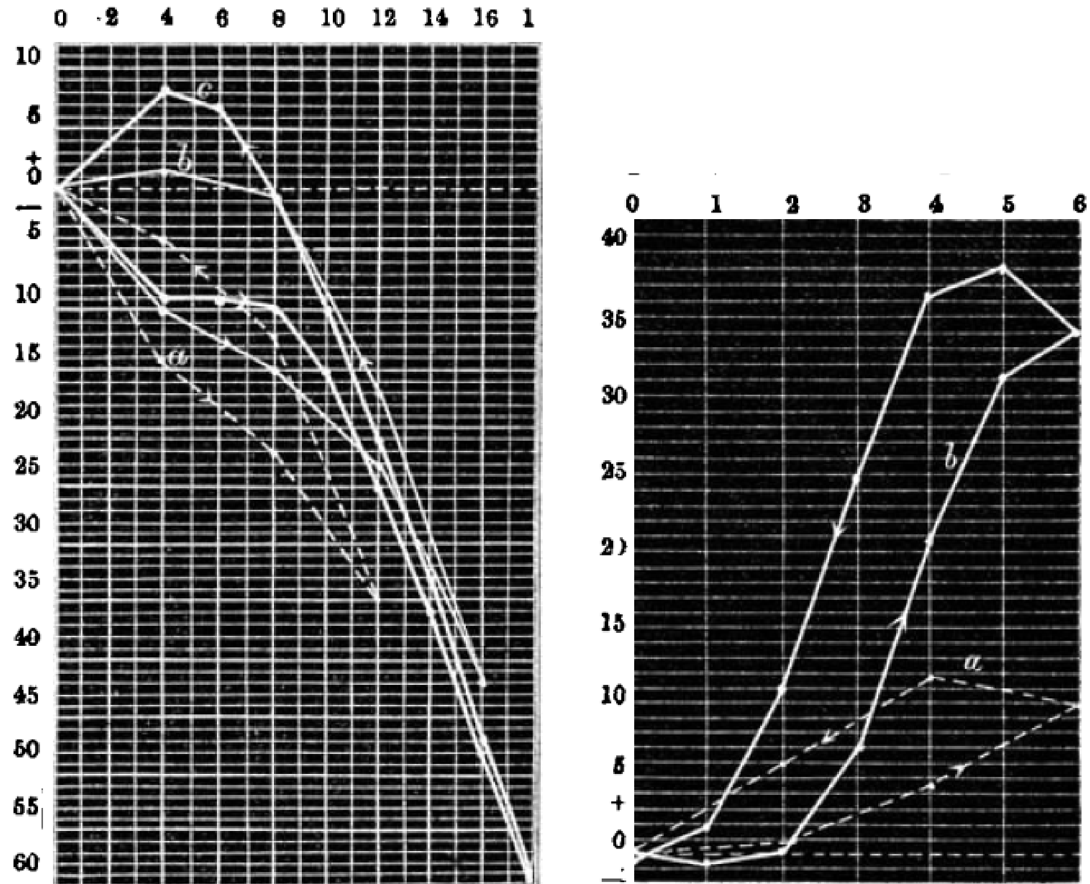


Fig. 1.

Fig. 2.

Figure 6.31
Emil Cohn's "Cyclus" for the change of current against change of stress. Source: Emil Cohn, "Ueber das thermoelectrische Verhalten gedehnter Drähte," *Annalen der Physik* 242, no. 3 (1879): 385–403.

The plots of all three articles represented a cyclical phenomenon and showed that the thermoelectric and magnetic properties of a wire did not depend solely on the given deformation of the wire or the magnetizing force, but also on the previous deformations and magnetizations. While this finding was associated with the loop-like nature of the cycle, only Thomson's article insisted on trying to identify some "general character"; in Cohn's and Warburg's plots little attention was paid to the symmetric nature of the two curves that formed the loop. It was only with James Ewing that the study of hysteresis was centered around the hysteresis loop.

Between 1881 and 1883 Ewing published several studies on the thermoelectric and mag-

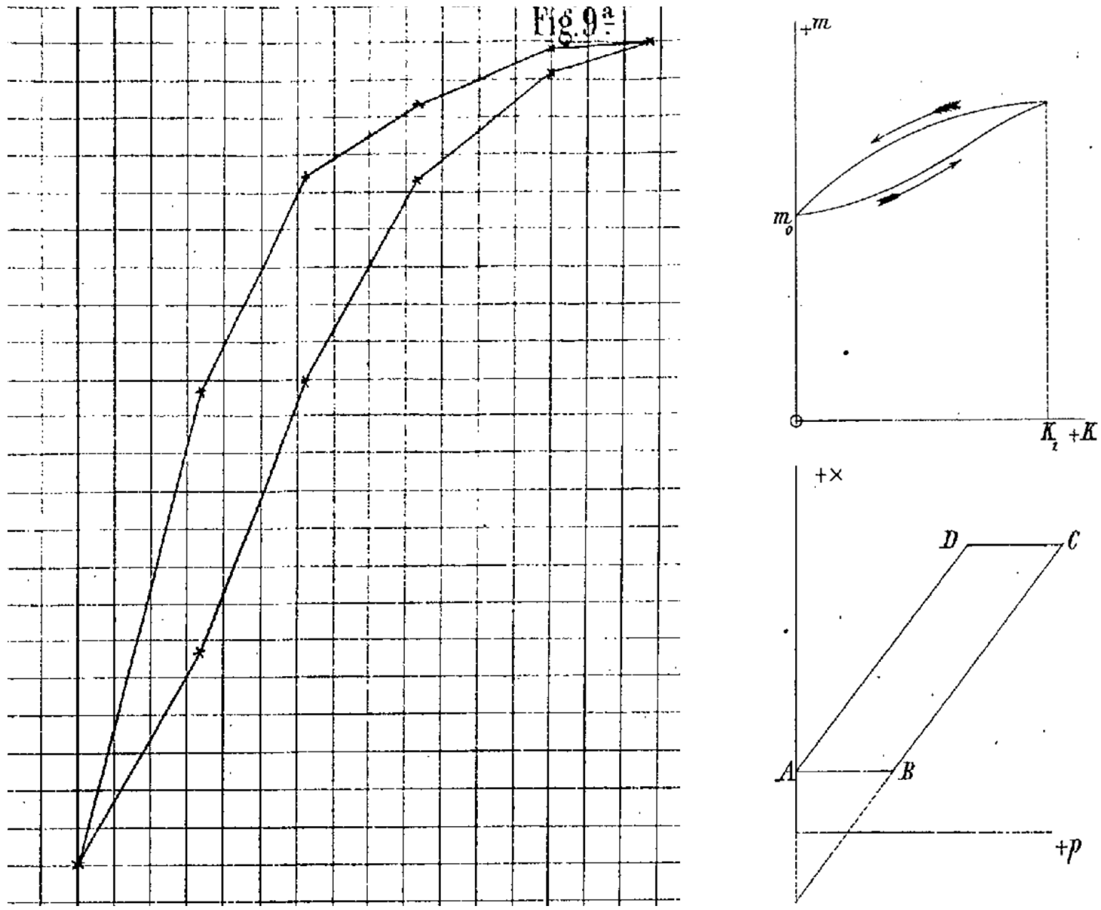


Figure 6.32
The left image showed Warburg's graphical representation of the experimental data for a magnetization cycle. The diagram on the upper right represented a simplified cycle for which the area enclosed by the curve, or $-\oint mdK$, corresponded to the energy during the cycle. The lower right image represented the relation between position and force for a block resting on a rough surface and connected to a tensionless spring which pulled the block with a force that could increase continuously; after a point, the force was decreased back to zero. The closed area of the parallelogram $ABCD$ corresponded to the expended energy. Source: E. Warburg, "Magnetische Untersuchungen," *Annalen der Physik* 249, no. 5 (1881): 141–164.

netic effects of stress, and the magnetization cycle.[146] Opposed to Cohn and Warburg, Ewing took great care to produce highly regular and symmetric experimental curves (see Fig. 6.33).

146. James Alfred Ewing, "Effects of Stress on the Thermoelectric Quality of Metals. Part I.," *Proceedings of the Royal Society of London* 32, no. 212 (1881): 399–402; James Alfred Ewing, "On the Production of Transient Electric Currents in Iron and Steel Conductors by Twisting Them When Magnetised or by Magnetising Them When Twisted.," *Proceedings of the Royal Society of London* 33, no. 216 (1881): 21–23; James Alfred Ewing, "On Effects of Retentiveness in the Magnetisation of Iron and Steel," *Proceedings of the Royal Society of London* 34, no. 220 (1882): 39–45; James Alfred Ewing, "On the Production of Transient Electric Currents in Iron and Steel Conductors by Twisting Them When Magnetised, or by Magnetising Them When Twisted," *Proceedings of the Royal Society of London* 36, no. 228 (1883): 117–135; James Alfred Ewing, "On the Magnetic Susceptibility and Retentiveness of Iron and Steel," *Philosophical Magazine Series 5* 16, no. 101 (1883): 381–383.

As we have shown in the previous section, understanding the features and characteristics of a curve was an essential component of Ewing's experimental method. In 1881 he even coined a name for the new phenomenon: "*Hysteresis* (ὑστέρησις from ὑστερέω, to be behind)".[147] Ewing's "hysteresis" did not apply to a specific physical phenomenon (like those described by Thomson, Cohn, or Warburg) but rather to a general pattern:

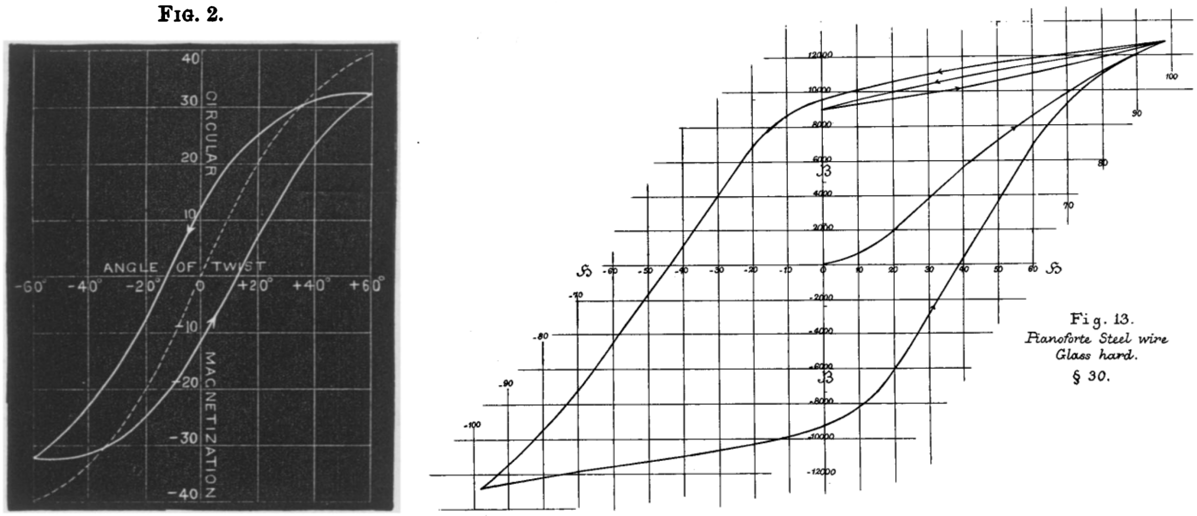
> To define the new term more precisely, let there be given two qualities of matter, M and N, of which M is a function of N; then if when N is changed cyclically the corresponding changes of M lag behind the changes of N, we may say that there is "hysteresis" in the relation of M to N.[148]

The choice was somewhat surprising because such a phenomenon had already possessed a name in the case of magnetization: "retentiveness". However, Ewing defended the new name because it applied to new instances which "at least apparently, [have] no connexion with magnetism".[149] While John Hopkinson saluted Ewing's choice, "The name is a good one, and has been adopted", Warburg was considerably reluctant because he insisted that the phenomenon was purely magnetic and should not be mistaken with the thermodynamic effects of stress.[150] The phenomenon of hysteresis came to be associated with Ewing not only because of his indefatigable studies, but also because of his coinage of the name and its style of representation.

An interesting debate over the law of hysteresis emerged in the early 1890s between James Ewing and the electrical engineer Charles Steinmetz. Steinmetz approached the research on hysteresis with "the eye of a mathematical physicist who was becoming an engineer".[151] Steinmetz found an empirical formula with which he could fit Ewing's experimental data:

$$\int H dI = \eta B^{\epsilon}$$

147. Ewing, "On the Production of Transient Electric Currents in Iron and Steel Conductors by Twisting Them When Magnetised or by Magnetising Them When Twisted.," 22.
148. Ewing, "On the Production of Transient Electric Currents in Iron and Steel Conductors by Twisting Them When Magnetised, or by Magnetising Them When Twisted," 123.
149. Ibid.
150. Hopkinson, *Original Papers by the Late John Hopkinson*, 218; Dörries, "Prior History and Aftereffects," 46.
151. Kline, *Steinmetz*, 48.



(a) **(b)**

Figure 6.33
Ewing's hysteresis loops for the variation of magnetization with rotation (Fig-a) and the variation of magnetization with the magnetizing force (Fig-b). Similarly to Thomson, but different from Warburg and Cohn, the shape of the curves was particularly important for Ewing. Great care had been taken to obtain well-defined experimental curves. Source: (a) James Alfred Ewing, "On the Production of Transient Electric Currents in Iron and Steel Conductors by Twisting Them When Magnetised, or by Magnetising Them When Twisted," *Proceedings of the Royal Society of London* 36, no. 228 (1883): 117–135; (b) James Alfred Ewing, "Experimental Researches in Magnetism," *Philosophical Transactions of the Royal Society of London* 176 (1885): 523–640.

Using Ewing's data, Steinmetz computed that the parameter ϵ had to be equal to 1.6. In 1890, when he first published this relation, Steinmetz referred to it as a "law of hysteresis". The agreement between the observed and calculated values "justifies my considering this coincidence as something more than a mere accident, and, indeed, as an indication of a general law, although certainly this law might be more complicated that the formula".[152] Steinmetz carried a series of lenghty experiments to test his law. He remarked that what "had been undertaken, first, for a strictly practical purpose [...] have since developed in scientific research". His new experiments, and the great concordance between his formula and measurements, made Steinmetz increasingly confident that he had found a true physical law. Steinmetz ended his second article drawing a parallel between his law and the law of gravity – "this law of 1.6th power I believe is not a *differential* law, like for instance the quadratic law of gravitation, but an

152. Charles Steinmetz, "Note on the Law of Hysteresis," *Electrician* 26 (1891): 261–262.

integral law like the law of *probability* with which it seems to be connected in some way".[153]

While some of Steinmetz's colleagues were already speculating about the deeper physical meaning of the law – A. E. Kennelly declared that he will write $\eta B^{1.6}$ as $\eta B^{8/5}$ because "it gives us little more hope of being able to understand what the equation means" – some derided Steinmetz's presentation of his empirical formula as a law of nature.[154] A vitriolic attack came from a British instrument maker who had done research on the magnetization of transformers:[155]

> Could infatuation go any further? What can be done with a man who prefers to accuse American founders of casting bad iron rather than abandon a more or less clumsy empirical formula – which it is ridiculous to call a law – and which was only made to fit the facts as far as they were known. Is it possible that Mr. Steinmetz is unable to distinguish between a physical law and that miserable abortion an empirical law?[156]

While he acknowledged Steinmetz's empirical formula to be "of most practical importance" especially in calculations connected with transformer design, Ewing challenged the idea that the formula had any physical meaning – "a formula of this type cannot be admitted to have any physical significance".[157] Ewing did not reject the physical meaning of the formula because it was not *sufficiently* accurate, but rather because it was not accurate in the right way. While Steinmetz considered his empirical formula to be a "law of hysteresis" because it could be successfully applied to a wide range of materials, what mattered the most for Ewing was the "character" of the divergence between the formula and the experimental curves. The formula was comparable to the limits of experimental accuracy only when the coefficient ϵ was adjusted for different intervals of the magnetic field. However, these changes "correspond to the passage from one to another of the familiar successive stages in the process of magnetization".

153. Charles Steinmetz, "On the Law of Hysteresis (II)," *American Institute of Electrical Engineers, Transactions of the* IX, no. 1 (1892): 711-712.
154. Charles Steinmetz, "On the Law of Hysteresis," *American Institute of Electrical Engineers, Transactions of the* IX, no. 1 (1892): 52-53.
155. Kline, *Steinmetz*, 57.
156. S. Evershed, "A Key to All Magnetism," *The Electrical Journal* 29 (1892): 670.
157. Ewing and Klaassen, "Magnetic Qualities of Iron," 1018.

Each stage might be well approximated by a carefully chosen value of the coefficient ϵ, but the overall pattern was left unexplained, and it was these "well-marked changes of gradient curve which characterize the magnetizing".[158] The formula failed in describing what mattered the most for Ewing – the *defining features* of the magnetic curve.

By 1900 Steinmetz had to acknowledge defeat –

> we know that the hysteresis loss follows the 1.6th power, but we know that is not an inherent, physical law. It must sometimes deviate from the law. It is a somewhat complex phenomenon which can be closely represented by the 1.6th power, but it is not a physical law.[159]

When testing the law, Steinmentz had concentrated not so much on expending the range of the magnetic fields but on varying the type of materials. His initial concern was to prove that "this law does not depend upon a particular constitution of the material, but is of more general meaning".[160] His initial excitement of having had discovered a physical law was based in particular on the application of his law to a wide array of materials. When confronted with the potential tension between his law and Ewing's theory, Steinmetz shrugged it of – "my aim was to gather *facts*, being convinced that based upon a large number of facts, a theory will be found in due time to explain them."[161] While Ewing's molecular theory of magnetization predicted that the hysteresis should initially increase very rapidly and then slowly in the portion of saturation, Steinmetz's formula predicted that "hysteresis seems to follow the same law over the whole range of magnetization".[162]

Though he did not employ any formulas, Ewing did not give up all hope to construct a theory of the hysteresis cycle. In 1890 he published an article on "Contributions to the Molecular Theory of Induced Magnetism" in which he presented his experimentations on "a model molecular structure consisting of a large number of short steel bar magnets, strongly

158. Ewing and Klaassen, "Magnetic Qualities of Iron," 1018.
159. Charles Steinmetz, "Discussion," *American Institute of Electrical Engineers, Transactions of the* XVII (1900): 332.
160. Steinmetz, "On the Law of Hysteresis (II)," 716.
161. Steinmetz, "On the Law of Hysteresis," 58.
162. Ibid., 28.

magnetised, each pivoted like a compass needle upon a sharp vertical centre and balanced to swing horizontally".[163] The bar magnets were then exposed to an external magnetic field, and their different arrangements were observed. Ewing associated their behavior with three different "stages" in the magnetization of iron that he defined based on the defining features of the magnetic curve: its concavity which inflected in a linear portion which was then followed by an asymptotic saturation (see Fig. 6.35).

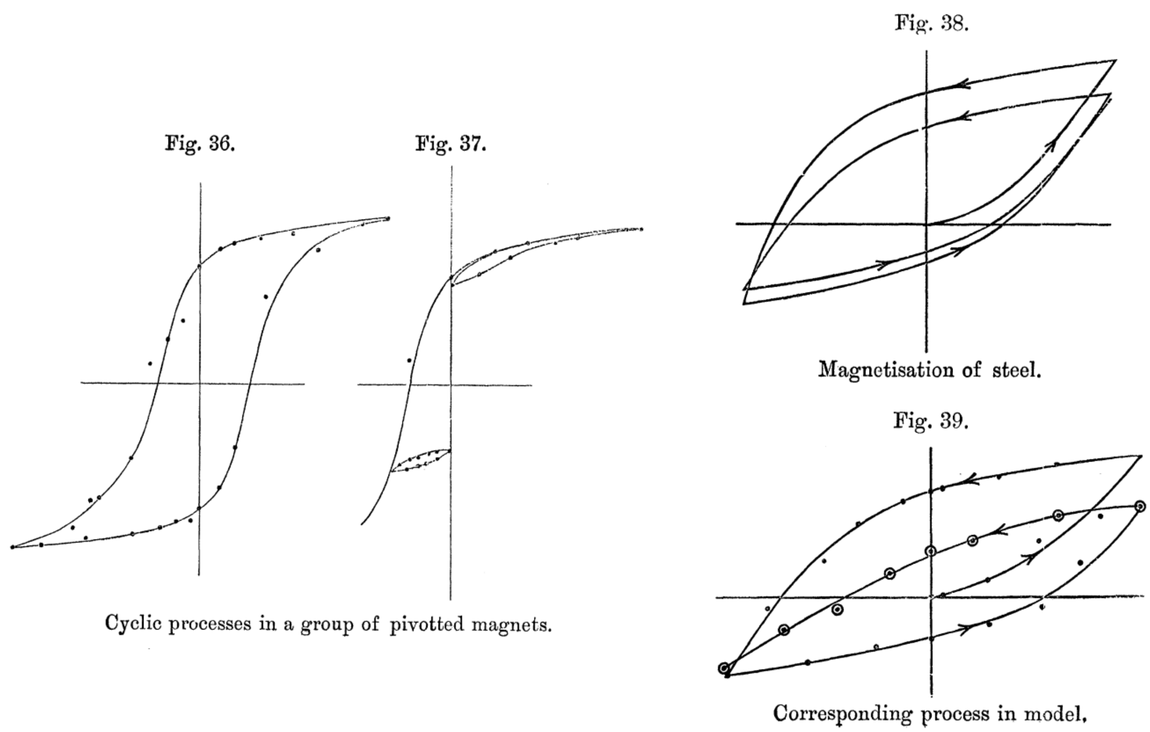


Figure 6.34
Hysteresis curves constructed after the experiments with the model of pivoting magnetic bars, and the curve constructed after the model had "all the general character of the curve of magnetization in actual iron". Source: James Alfred Ewing and Helen G. Klaassen, "Magnetic Qualities of Iron," *Philosophical Transactions of the Royal Society of London. A* 184 (1893): 985–1039.

Shortly after this theoretical article, Ewing made some experiments with the actual model, using sometimes up to 130 little magnets. By measuring the aggregate magnetic moment as a function of the magnetizing current he was able to obtain a curve "which has all the general

163. James Alfred Ewing, "Contributions to the Molecular Theory of Induced Magnetism," *Proceedings of the Royal Society of London* 48, no. 292 (1890): 344.

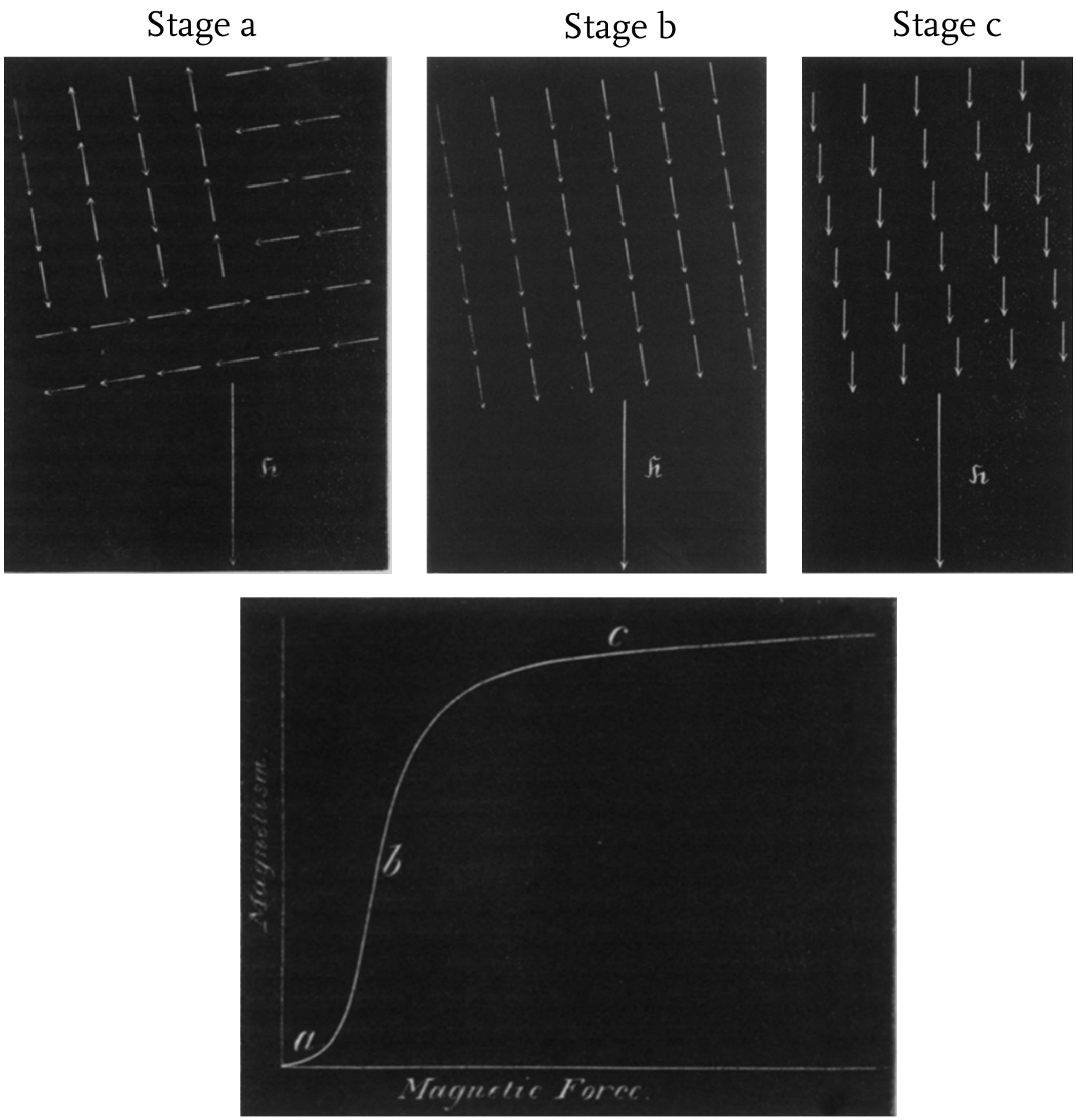


Figure 6.35
Each stage in the magnetic arrangements of the magnetic bars was associated with a different portion and feature of the magnetic curve. *Stage a* corresponded to the inflexion point identified by Rowland and Stoletow; *stage b* corresponded to the linear portion of the curve identified by Lenz-Jacobi; *stage c* corresponded to the saturation part of the curve pointed out by J. Müller. Source: James Alfred Ewing, "Contributions to the Molecular Theory of Induced Magnetism," *Proceedings of the Royal Society of London* 48, no. 292 (1890): 342–358.

character of the curve of magnetization in actual iron".[164] After carrying the pivoted magnets through a complete magnetization cycle he obtained loops that showed that "the behaviour of the model agrees in all respects with that of a magnetic metal" (see Fig. 6.34).[165] For such an interpretation to hold the general features of the curve were essential:

> Observations made on such a model are necessarily somewhat rough, and too much stress must not be laid on particular features of the curves. But in several repetitions of these experiments the same general distinction has always been apparent in the behaviour of the model after one and the other mode of preliminary treatment, a distinction which, as we have seen, forms a striking analogue to what is observed in actual iron or steel.[166]

Ewing's reasoning and use of a model was not far from that of Maxwell. Ewing never claimed that his "molecular theory of magnetization" was fully developed and finished theory, but rather that it allowed one "to explain some characteristic manifestations of magnetic hysteresis".[167]

Ewing's analysis of hysteresis was almost entirely graphical. Ewing employed an automatic "magnetic curve tracer" that he had designed in the early 1890s (the instrument was built by Nalder Bros. and Co.) to produce photographic records of the hysteresis loops (see Fig. 6.36).[168] Ewing used the curve tracer to produce almost cinematic diagrams of the "the superposed magnetizations in soft iron", hysteresis curves for cycles of different time periods, or to show "the projection of the extremities of each cyclic curve over the rising limb of the cycle next above it" (see Fig. 6.37). For Ewing the study of magnetism, and especially that of hysteresis, could not be reduced any longer to simple table of measurements or formulas. No numerical table could make visible the subtle features brought out by Ewing's curves, and no formula could accurately span their entire range. As such, the magnetic curves and the hysteresis loops became the epistemological objects through which Ewing carried out his

164. Ewing and Klaassen, "Magnetic Qualities of Iron," 1036.
165. Ibid., 1037.
166. Ibid., 1039.
167. Ibid., 985.
168. A Heyland, "The Circle Diagram," in *The Electrician* (James Gray, 1903), 708–709.

analysis. Experimental results were now to be interpreted solely in terms of the changes and variations of the shape of the hysteresis loops. This endeavor could be profitable only because a graphical language had emerged in which one could both trace the experimental results and translate them into well established and stable physical meanings.

3 CONCLUSION

The dramatic change in the use of graphical representations in the practice of physics can be observed at a glance if we were to follow the entry on "Magnetism" from the various editions of the *Encyclopaedia Britannica*. In the 8th edition (1853-1860), among the 118 images in David Brewster's article there were no experimental plots. In the 9th edition (1875-1889), among the 49 images of George Chrystal's article there were only four experimental plots, three of which were taken from Rowland's articles (Chrystal superimposed on top of Rowland's curves for magnetization Stoletow's curve of susceptibility); the fourth plot, which also represented magnetic curves, was taken from an article by the German physicist Felix Auerbach. In comparison, Chrystal's article on "Electricity" included 58 images among which there was only one experimental plot drawn by F. Kohlrausch.[169] However, in the 11th edition (1910-1911) Shelford Bidwell's article on "Magnetism" included among its 29 images 12 plots, all from British or Japanese scientists.[170] Similarly, the article on "Electromagnetism" written by John A. Fleming had 6 images among which 3 were plots (all taken from Ewing's articles).

One could be easily deceived by such numbers in believing that they are solely a reflection of a general trend. In the end, all scientific fields relied heavily on graphical representations by the end of the 19th century. Thus, should we not consider this as a consequence of a general at-

169. The article also included one of Tait's thermo-electric diagrams from 1873.

170. As will be discussed in the next chapter, in the second half of the 19th century there was a strong connection between English and Japanese engineers. Ewing had served until 1883 as professor of mechanical engineering at the Tokyo Imperial University. See W. H. Brock, "The Japanese Connexion: Engineering in Tokyo, London, and Glasgow at the End of the Nineteenth Century," *The British Journal for the History of Science* 14, no. 3 (1981): 227–244; Graeme Gooday and Morris F. Low, "Technology Transfer and Cultural Exchange: Western Scientists and Engineers Encounter Late Tokugawa and Meiji Japan," *Osiris* 13 (1998): 99–128.

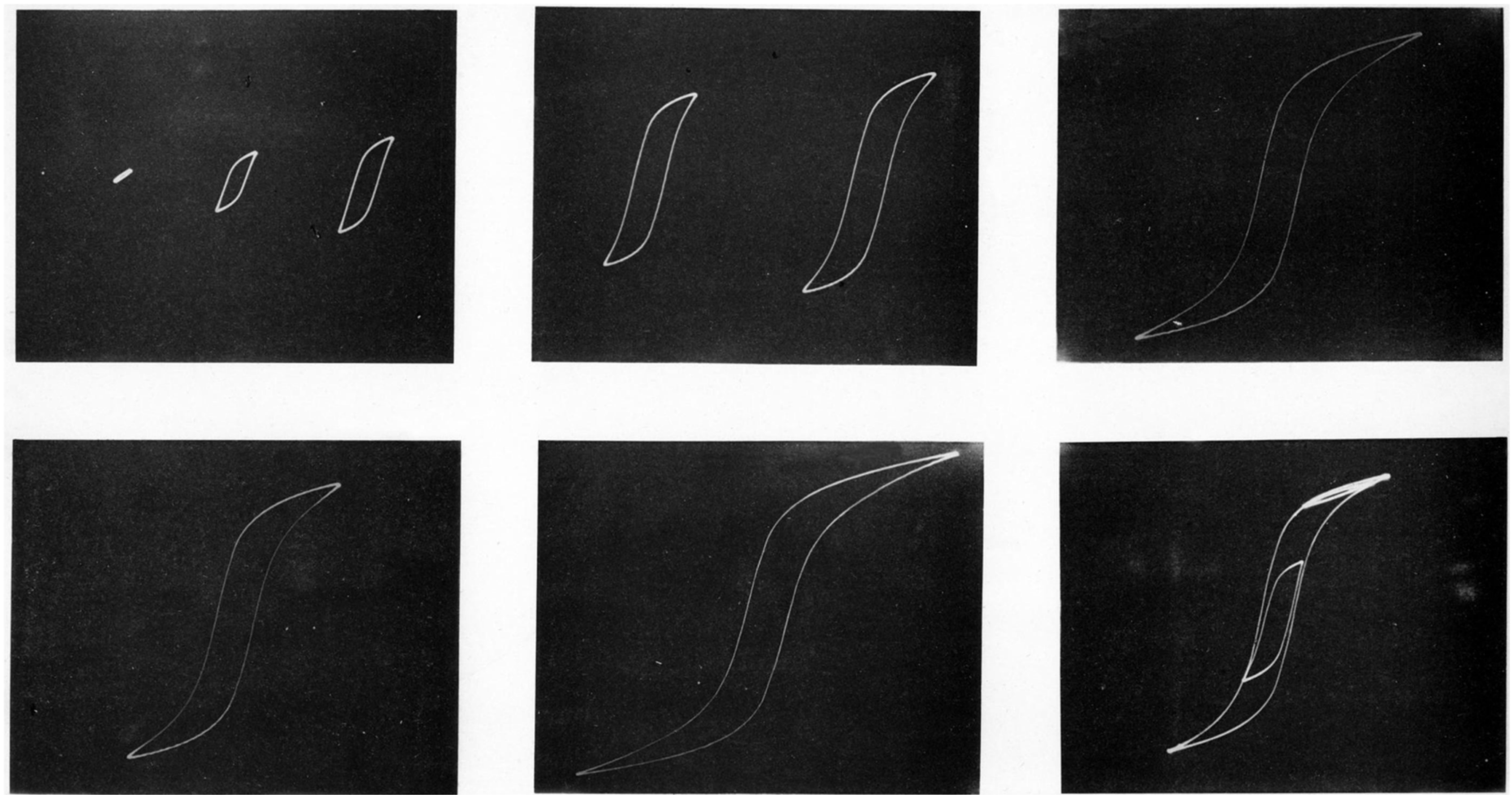
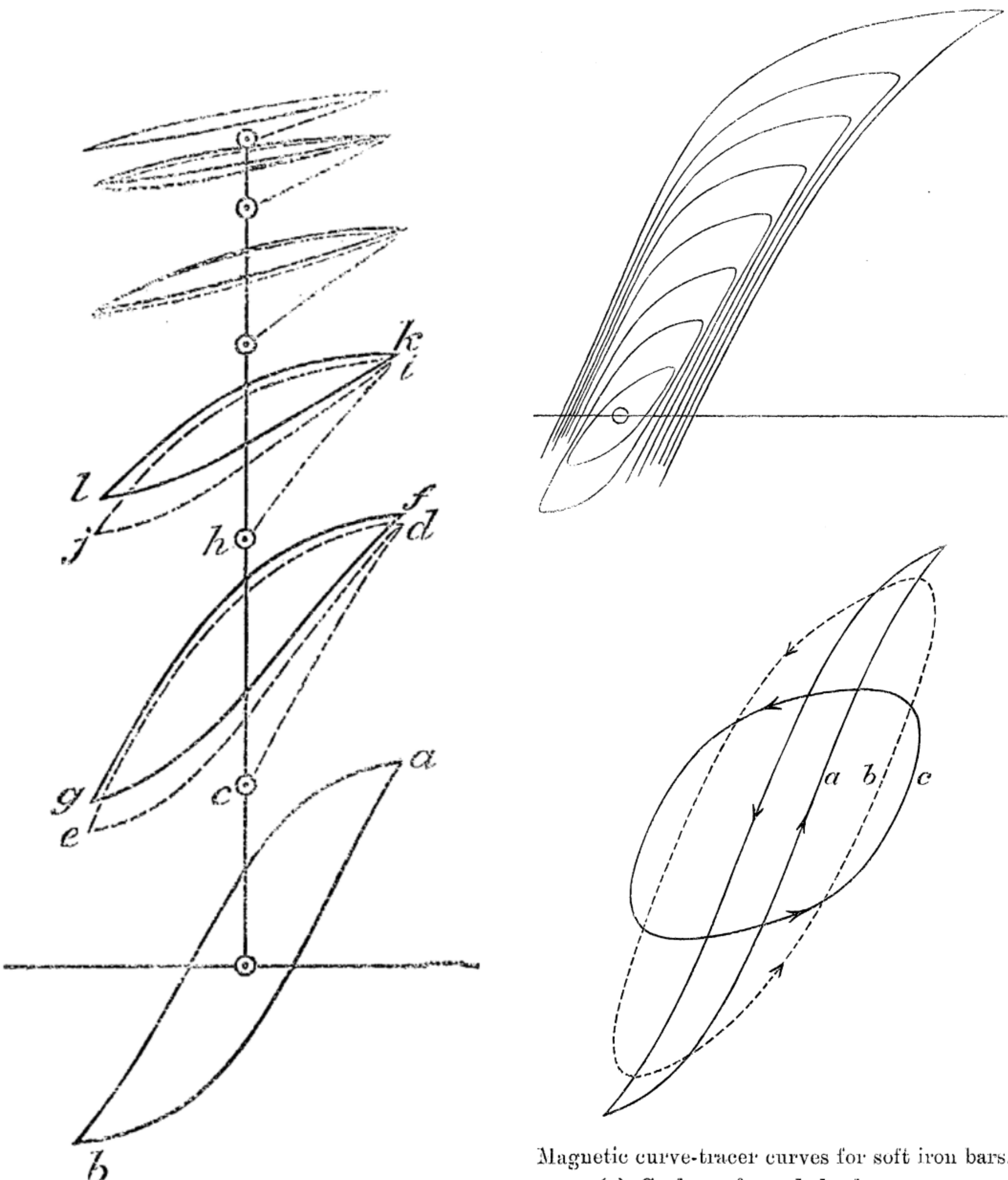


Figure 6.36
Photographic records by the automatic curve tracer. These photographic recordings were used by Ewing to produce his composite-curves (see Fig. 6.37). Source: James Alfred Ewing and Helen G. Klaassen, "Magnetic Qualities of Iron," *Philosophical Transactions of the Royal Society of London. A* 184 (1893): 985–1039.



Magnetic curve-tracer curves for soft iron bars.
(*a*) Cycle performed slowly.
(*b*) Period of cycle 3 seconds.
(*c*) Period of cycle 0·43 second.

Figure 6.37
"Magnetic curve-tracer curves". Ewing used his curve-tracer to study "the superposed magnetizations in soft iron", hysteresis curves for cycles of different time periods, or to show "the projection of the extremities of each cyclic curve over the rising limb of the cycle next above it". Source: James Alfred Ewing and Helen G. Klaassen, "Magnetic Qualities of Iron," *Philosophical Transactions of the Royal Society of London. A* 184 (1893): 985–1039.

titude towards the role and value of graphical representations within scientific practice? This chapter has endeavored to prove the opposite claim: the curves used in the study of electromagnetism were drawn in reaction to previous representations within the field. Rather than being a consequence of a general scientific practice of plotting experimental results, they were one of the main causes for which such a practice spread in the first place. *Graphical representations* came to be *valued* first because of the specific problems to which they were applied; only as the number of such paradigmatic examples multiplied did graphical representations, as a *general* method of presenting experimental results, came to be *valued*.

There are a several important trends that have been established in this chapter:

1. In the mid-19th century, the publication of experimental plots was mainly a German activity. These plots were mainly used to display "at a glance" the evidence for the conclusions of a study; the plots would either compare sets of measurements or show the agreement between a formula and the experimental results. Because they were neither the evidence nor the conclusion, these plots played only a local role within the economy of the article: they were almost never reproduced in publications which cited the findings of the original study. In most cases, the experimentally controlled variables were plotted on the abscissa and the investigated quantities on the ordinates. Though this choice might seem harmless, as I have showed, it did have important consequences in terms of the empirical formulas with which one could fit the experimental curves. Both Frölich and Rowland took an extra step in finding a non-trivial choice of variables. The fact that both of them emphasized the merits and originality of their choice suggests that this was not a common practice – which is to be expected as long as the plot was not used to determine an empirical formula, but only to display the agreement between the experimental results and some already known formula. In comparison to their German colleagues, mid-19th century British physicists made a much more limited use of graphical representation. However, because their interest was not in establishing empirical formulas, when they did appeal to plots, it was not to show the direct concordance between ex-

periments and formulas but to reveal and establish some general characteristics of the curves, as it was shown in the case of Maxwell, Hopkinson and Ewing.

2. Though J. Müller's plot fits well in the trend described above, several important factors played a role in the *multiplication* of his curve. First, the shape of the curve embodied his novel experimental finding that the magnetization of an iron bar could be saturated. The curve was not only a representation of the results, but also a rebuttal of the linear Jacobi-Lenz law. As such, the curve more than his measurements or his empirical formula became the take-away conclusion of his study. Second, Weber also relied on the shape of Müller's curve as evidence for his molecular theory of magnetism. While the formula derived from his theory could have broadly approximated the experimental measurements, what mattered the most (especially for someone like Maxwell) was the ability of the formula to reproduce the saturated region of the curve. These experimental and theoretical meanings assured the fame of Müller's curve and its reproduction in multiple German textbooks.

3. Through Maxwell's *Treatise*, the curve of magnetization was extended from an experimental curve into a *paradigmatic* curve through which a whole theory could be represented and analyzed. For Maxwell, it was not the precision with which Weber's theory could agree with the experimental results that mattered, but rather the ability of the theory to reproduce the *essential features* of the experimental results, i.e. the features defined through the curve. Thus the curve, rather than the formula, became the bridge between experiment and theory. The *paradigmatic* status of the magnetic curve is revealed in particular in the work of Rowland, Hopkinson and Ewing. Rowland, who was familiar with Maxwell's *Treatise*, had decided to represent his experimental results through a magnetization curve exactly because it engaged directly with Weber's theory of magnetism as graphically presented by Maxwell. This represents a significant departure from the trend described above (1.), or from the Rowland's use of the permeability curve to find an empirical formula. In contrast, Stoletow, who had been trained in Kirchoff's laboratory, chose to represent his results employing the usual convention

described above. For Hopkinson, the main appeal of the characteristic curve of a dynamo was its direct and distinct connection to the magnetization curves from Maxwell's *Treatise*.

4. By noticing the *paradigmatic* status of the magnetic curve, we were able to identify a *phenomenology* of the curve (represented especially by Ewing). In the case of most experimental curves, one was focused on the variation of a quantity in terms of another. In terms of gestures, one was following the curve by moving along it. The second common use was to have multiple representations of curves on the same plot; in this case one was gesturing along a vertical to compare the values of the curve for the same abscissa. However, in the case of these types of readings, the curve did not have a long term impact on someone's mind. Ewing's method of reading curves was focused on shapes, patterns, characters, and features. One was not following *local variations* of a value (the motion of the index along the curve), but shapes (kinks). While in the mathematical reading of a curve one was focused *on special points* (points of inflexion, maxima, minima, etc.), in the phenomenological reading one was focused on whole *regions*; instead of following the *variation* of a value one followed the *mutation* of a curve – i.e. the change of one shape into another (see Fig. 6.37).

5. By analyzing the choice of variables, we were able to provide a better understanding of what role an experimental curve could play for a historical actor: 1. in most early cases the experimentally controlled variables were plotted on the abscissa and the investigated quantities on the ordinates (e.g. Meyer & Auerbach, Stoletow, E. Cohn, etc.); 2. in a few cases one employed a clever choice of variables which could produce a regular and finite curve that could be easily fit to an empirical formula (Frölich, Rowland); 3. starting with Müller's plot and through the work of Weber and Maxwell, the magnetization as a function of the magnetizing force came to play a dominating role (see Fig. 6.38). These different roles of experimental curves can suggest a broader *analytical* division of the *graphical*:

Graphical Representation: the *graphical* is understood as a *translation* of the *non-graphical*; the plots or the experimental curves are a translation of a numerical table or algebraic formula.

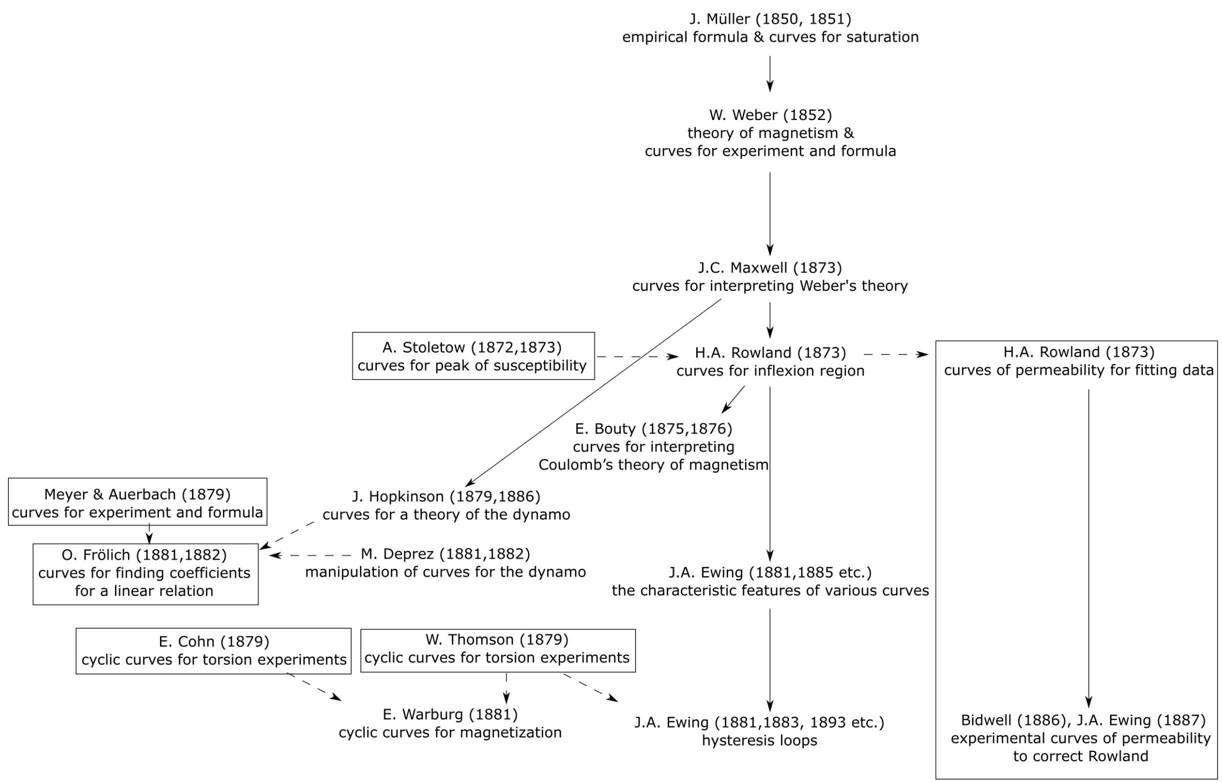


Figure 6.38
A summary of the graphical representations discussed in this chapter. All the unboxed names refer to articles which represented the magnetization as a function of the magnetizing force, while the boxed names refer to articles which employed a different convention. This scheme comes to show, that with the exception of Rowland's novel choice of representing curves of permeability, all the other choices of variables had mainly remained isolated.

It is an equivalent mode of presentation which has the advantage of showing "at a glance" the evidence or conclusions of a study. Understood solely as a *translation*, the *graphical representation* has a low epistemological status having the purpose of facilitating an easy understanding for the reader.

Graphical Method: the *graphical* is understood as an *operation*; through a series of constructions and measurements one can determine the coefficients of a formula or the value of certain quantities. For example, by graphically manipulating the characteristic curve one could answer all practical questions about a dynamo (see the concept of *diagrammatic holism* introduced above). Understood under these terms, the *graphical method* could be used by engineers to challenge the epistemological claims of theoretical science. To the generality and rigor of algebra, the engineer opposed the *practical generality* of the graphical methods which could take into account complex configurations without requiring the sort of approximations and simplifications necessary to manipulate an algebraic relation.

Graphical Feature: the *graphical* is understood as an association between graphical representations which reveals graphical features or graphical characteristics. While the graphical representation, as defined above, is a translation of a different type of representations (a numerical table or formula), the graphical feature comes to be associated with a physical state or process. While the graphical representation is *exhausted* in displaying the agreement between experiment and theory (and thus, it is never further reproduced), the graphical feature *essentializes* the physical phenomenon and becomes the mean through which experimental and theoretical results are interpreted and represented.

· 7 ·

Squared Paper

> Graphic methods of representing functions have become universal in the last generation. […] Graphic methods of one form or another are now found in the courses in mathematics, at least in the Realanstalten, in all countries, having gradually made their way from engineering, through thermodynamics and general physics, to pure mathematics.[1]

> The use of graphical methods in elementary algebra teaching is universal and entirely a 20th-century development.[2]

A momentous reform in the teaching of mathematics at the secondary school level took place in the first decade of the 1900s. The teaching of mathematics as a rigorous, deductive system was challenged by a new approach based on intuition and experimentation. Some of the reformers went so far as suggesting that mathematics itself was an experimental science which should be taught in a similar fashion to the physical sciences and should make use of "mathematical laboratories". The reform took place almost concomitantly in Britain, France, Germany and the US, and spread to several other countries through the activity of the International Commission on Mathematical Instruction. The spirit of the reform was best embodied by and most often exemplified through the use of graphical methods, especially the graphical representation of a function on squared paper. Such a graphical method was seen as correlating the three main parts of school mathematics – arithmetic, geometry and algebra – which until then were commonly taught as independent and non-intersecting topics; as connecting the teaching of mathematics to the physical sciences because graphs were commonly employed

1. David Eugene Smith, "Intuition and Experiment in Mathematical Teaching in the Secondary Schools," in *Proceedings of the Fifth International Congress of Mathematicians*, ed. Ernest William Hobson and A. E. H Love, vol. 1 (Cambridge: University Press, 1913), 614, 622.

2. Charles Godfrey, "Methods of Intuition and Experiment in Secondary Schools," in *Proceedings of the Fifth International Congress of Mathematicians*, ed. Ernest William Hobson and A. E. H Love, vol. 1 (Cambridge: University Press, 1913), 641.

in experimental work and in determining empirical laws; as making mathematics more accessible to the average student because it appealed to his intuition and it was connected to the graphs that students encountered on a daily basis in newspapers; and not the least, as preparing students for technical careers by allowing them to learn more easily advanced topics such as calculus. These various roles distinguished the late 19th and early 20th century *graphical representation of a function on squared paper* from the *geometrical representations*, *interpretations* or *illustrations* of algebraic expressions employed in the previous centuries. Though backed by the same underlying mathematical idea, *graphical* and *geometrical* representations were part of different *paper worlds*.[3]

1 REFORMING MATHEMATICS

The reform of mathematical education in the first decade of the 20th century was spearheaded by an address delivered by John Perry in 1901 at the Glasgow meeting of the British Association for the Advancement of Science. Perry faulted current teaching practices which valued more than anything logical rigor and trained students mainly to excel in their examinations. He envisioned instead a program which was aimed at students with "average intelligence" and which cultivated their love for mathematics by providing them with "mental tools" to tackle real-life problems. While many of Perry's recommendations were considered to be too radical, a few of them were pushed forward by a special committee and a year later they were endorsed by the examination syndicates of the Universities of Cambridge, Oxford and London. Among the momentous changes was the replacement of Euclid based geometry that dominated British schools throughout the 19th century. In the words of the early 20th century historian Florian Cajori, as a result of Perry's address "Euclid was dethroned in England...".[4]

3. The term is borrowed from Warwick's study of the "paper world" of Cambridge mathematics in Warwick, *Masters of Theory : Cambridge and the Rise of Mathematical Physics*, esp. 168-169.

4. Florian Cajori, *A History of Elementary Mathematics: With Hints on Methods of Teaching* (Macmillan, 1917), 292.

The teaching of geometry based on Euclid's *Elements* had previously come under scrutiny in the 1870s after a Schools Inquiry Commission from 1868 inquired if "Euclid is a good text book for beginners" and "whether boys should not commence with something easier and less abstract".[5] The Commission's inquiry prompted James Wilson, the mathematical master at Rugby, to publish an alternative textbook to Euclid which he attacked for "his artificiality, the invariably syllogistic form of his reasoning, the length of his demonstrations, and his unsuggestiveness".[6] Wilson, together with other masters and mathematicians, formed an Association for the Improvement of Geometrical Teaching (AIGT) which by 1873 numbered more than a hundred members. Over the next two decades the Association put forward a revised syllabus and textbook. Though they generated a heated debate, these early attempts failed to produce concrete changes – most examination bodies, including those of Oxford and Cambridge, accepted original proofs only if they were based on Euclid's axioms and order of proof.[7] Furthermore, textbooks that closely followed Euclid remained the norm.

While the 1870s debate over Euclid remained mostly academic and focused on questions regarding the system of axioms and order of proofs, the later debate engaged with broad pedagogical arguments about the role of mathematics.[8] The impact of John Perry's address is more than unusual given the fact that he was only a professor of mechanical engineering and applied mathematics. How could an engineer successfully persuade a room full of mathematicians?

5. *Schools Inquiry Commission*, vol. 1 (GE. Eyre & W. Spottiswoode, 1868), 30-31. For a comprehensive history of the debates over the use Euclid in 19th century British see Joan L. Richards, *Mathematical Visions: The Pursuit of Geometry in Victorian England* (Boston: Academic Press, 1988), 161-200; W. H. Brock, "Geometry and the Universities: Euclid and His Modem Rivals 1860–1901," *History of Education* 4, no. 2 (1975): 21–35; Michael Haydn Price, "The Reform of English Mathematical Education in the Late Nineteenth and Early Twentieth Centuries" (Thesis, University of Leicester, 1981). For the role of the Commission in starting the debate over Euclid see Brock, "Geometry and the Universities," 25; Price, "The Reform of English Mathematical Education in the Late Nineteenth and Early Twentieth Centuries," 84-85.

6. James Maurice Wilson, *Elementary Geometry* (Macmillan, 1869), vii.

7. Brock, "Geometry and the Universities," 28.

8. Most of the AIGT members were interested mainly in improving the formal training in deductive geometry and showed little interest in practical geometry or geometrical drawing, see Brock, "Geometry and the Universities," 26; Price, "The Reform of English Mathematical Education in the Late Nineteenth and Early Twentieth Centuries," 91, 342; Richards, *Mathematical Visions*, 173. Brock has identified only two members of the AIGT who could "stand out for technical geometry and, equally significantly, for the admixture of arithmetical and algebraic matter", Brock, "Geometry and the Universities," 34n51.

1.1 The Education of an Engineer

As stated in various autobiographical notes, John Perry attended the Model National School in Belfast until the age of fourteen where he was taught well how to draw "in the Science and Art Department way – descriptive geometry, mechanical drawing, shading in crayons, drawing from models, and even landscape painting".[9] After leaving school he apprenticed for seven years at the Lagan Foundry, and between 1868 and 1870 he attended the engineering classes of James Thomson at Queen's College in Belfast from where he graduated with an engineering degree.[10] From 1870 to 1873 he taught at Clifton College in Bristol, where he established "a school workshop and a physics and mechanics laboratory", "the first workshop in connection with a public school".[11] In 1874 he became William Thomson's assistant in Glasgow who recommended him for the professorship of mechanical and civil engineering at the Imperial College of Engineering in Tokyo.[12] Here he joined the physical laboratory of William E. Ayrton (1847-1908), the professor of natural philosophy and telegraphy since 1873 and a former pupil of William Thomson. For the next two decades Perry and Ayrton became close collaborators on scientific topics in electrical engineering and on the reform of engineering education.[13]

While the Tokyo laboratory was closely modeled after Kelvin's and Rankine's Glasgow laboratories, Ayrton and Perry experimented with the curriculum and their teaching meth-

9. *Report of the Special Committee on the Subjects and Modes of Instruction in the Board's Schools*, 89; for Perry's biography see Gooday, "Perry, John (1850–1920)."

10. See John Perry's kind obituary of James Thomson in *The Engineer* (May 13, 1892), 413.

11. John Perry, *England's Neglect of Science* (Unwin, 1900), 73.

12. Brock, "The Japanese Connexion"; Y. Takahashi, "William Edward Ayrton at the Imperial College of Engineering in Tokyo-the First Professor of Electrical Engineering in the World," *IEEE Transactions on Education* 33, no. 2 (1990): 198–205. In 1894-1895 Perry criticized William Thomson's estimate of the sun's age; for the exchange between the two see Smith and Wise, *Energy and Empire*, 544-548.

13. For a comprehensive study on the reform of engineering education see R. A. Buchanan, "The Rise of Scientific Engineering in Britain," *The British Journal for the History of Science* 18, no. 2 (1985): 218–233; for Perry and Ayrton's reform of engineering education see D. W. Jordan, "The Cry for Useless Knowledge: Education for a New Victorian Technology," *Physical Science, Measurement and Instrumentation, Management and Education - Reviews, IEE Proceedings A* 132, no. 8 (1985): 587–601; Brock, "Building England's First Technical College"; for the examples of the collaboration between Perry and Ayrton see Gooday, *The Morals of Measurement*, 153-160.

ods.[14] The Tokyo laboratory was hailed in Britain as "the only college in which telegraphy is systematically taught" and "the only English-speaking technical university".[15] If mid-19th century British engineering schools were less systematized and centralized in comparison to their counterparts in France, Germany or Switzerland, Japan provided a "quasi-colonial experiment in a radically new scheme for state-funded technical education and research".[16] The pedagogical methods and organization "rehearsed" in Japan were brought back to Britain by Ayrton and Perry.[17]

Returned to Britain in 1878, Ayrton became professor of applied physics at the newly founded City and Guilds of London Institute which provided evening lectures for artisans. In 1883 the Institute was reorganized into Finsbury College which now provided day and evening classes in electrical, chemical, mechanical engineering and applied art. Ayrton was joined at Finsbury College by John Perry as professor of applied mechanics and by Henry Edward Armstrong as professor of chemistry.[18] Together the three men developed a particular pedagogical approach – "the Finsbury method", as it has been called by the historian William Brock. The method consisted of five features: 1. teaching was supposed to be analytic (or "heuristic") rather than deductive; 2. the teaching was carried out in particular through the laboratory and the workshop rather than just the lectures; 3. it developed a "practical" mathematical syllabus based on the specific needs of scientific and engineering practice rather than abstract mathematics; 4. all students in the first-year took a common course which included chemistry, mathematics, mechanical drawing, electrical and mechanical engineering, French or German;

14. Graeme Gooday, "Teaching Telegraphy and Electrotechnics in the Physics Laboratory: William Ayrton and the Creation of an Academic Space for Electrical Engineering in Britain 1873-1884," *History of Technology*, no. 13 (1991): 85-90.

15. Gooday, "Teaching Telegraphy and Electrotechnics in the Physics Laboratory," 93; on Rankine's teaching methods see David F. Channell, "The Harmony of Theory and Practice: The Engineering Science of W. J. M. Rankine," *Technology and Culture* 23, no. 1 (1982): 39–52.

16. Gooday and Low, "Technology Transfer and Cultural Exchange," 102.

17. Brock, "The Japanese Connexion," 239.

18. Brock, "Building England's First Technical College"; Jordan, "The Cry for Useless Knowledge"; Gooday, "Teaching Telegraphy and Electrotechnics in the Physics Laboratory"; Graeme Gooday, "Precision Measurement and the Genesis of Physics Teaching Laboratories in Victorian Britain," *The British Journal for the History of Science* 23, no. 1 (1990): 25–51.

5. Finsbury College administered its own examination rather than use outside examinations based on a less than useful school curricula.[19] Some of these key principles are best illustrated in the textbooks published by John Perry.

In 1883 Perry published his first textbook, *Practical Mechanics*, that aimed "to put before non-mathematical readers a method of studying mechanics" which had been developed and tested "at the Imperial College of Engineering in Japan, and in other places". The core idea was that "all experimenting must be quantitative" and the technical instruction should be based not on first principles and proofs, but on experience and experimentation. The theoretical generality of "elementary principles" was to be supplanted by *practical generality* as students were taught "a method of studying whatever phenomena happen to come before his eyes".[20] Perry invoked the background of his students to justify his method:

> Now, the standpoint of an experienced workman in the nineteenth century is very different from that of an Alexandrian philosopher or of an English schoolboy, and many men who energetically begin the study of Euclid give it up after a year or two in disgust, because at the end they have only arrived at results which they knew experimentally long ago.[21]

Instead of starting his teaching from "elementary principles" as if his students were "schoolboys of no experience", Perry began from his students' "natural standpoint, the standpoint given him by all his experience". In this way the teacher could take "advantage of the fact that his pupil may already possess an excellent foundation on which a superstructure of knowledge may be built". Thus, "the most illiterate men may be rapidly taught practical mechanics if we take the right way to teach them".[22] The "Preface" to the *Practical Mechanics* illustrated so well Perry's core pedagogical ideas that it was later included in his collection of essays on *England's Neglect of Science* (1900).[23]

Though Perry's *Practical Mechanics* received positive reviews in the *English Mechanic* and

19. Brock, "Building England's First Technical College," 166-168.
20. For the concept of *practical generality* see also Chapter 6.
21. John Perry, *Practical Mechanics* (1883), viii
22. Ibid., v-ix.
23. Perry, *England's Neglect of Science*, 110-113.

the *Scientific American* it was scathingly attacked in *Nature* by Dr. J. F. Main (a Trinity College alumnus and tenth wrangler in the Cambridge Mathematical Tripos of 1876) who considered the book to have failed in "logical arrangement and clearness and exactness of expression".[24] The reviewer targeted Perry's core pedagogy – proofs based on quantitative experiments instead of deductive reasoning. Main derided that

> the empire of the Greeks in geometry must give place to the supremacy of the intelligence of the working man, and even Euclid himself must fall from his high estate to be compared and contrasted with the modern schoolboy.[25]

While his first textbook from 1883 was met with some ridicule (as Perry himself acknowledged), fifteen years later Perry published a new textbook on *Applied Mechanics* (1898) which received a laudative review in *Nature* from James Alfred Ewing who titled his piece "Applied Mechanics, and the way to teach it".[26] The textbook was based on the course taught at Finsbury College and was aimed "for the use of students who have time to work experimental, numerical and graphical exercises illustrating the subject".[27]

The use of graphical exercises on squared paper became a core feature of the Finsbury method.[28] In 1882, before the new building of Finsbury College was opened, Ayrton and Perry presented their pedagogical method of using squared paper:

> Students must be early taught to graphically express the results of their work on squared paper; indeed, it is wonderful what an insight into analytical geometry even a non-mathematical student can obtain from a judicious use of squared paper. Hence it has been necessary to have prepared for the students at the Finsbury Technical College a special form of squared paper, for drawing curves on, which combines accuracy with about the sixth of the price of ordinary squared paper

24. See *English Mechanic and World of Science* 36 (Feb 23, 1883): 560; *Scientific American* 48 (Apr 21, 1883): 250.
25. J. F. Main, "Perry's Practical Mechanics," *Nature* 27 (1883): 456.
26. For Perry's reaction to the reception of his first textbook see John Perry, *Applied Mechanics* (New York: Van Nostrand, 1898), iv; James Alfred Ewing, "Applied Mechanics, and the Way to Teach It," *Nature* 57 (1898): 313–314. Ewing shared a similar background with Perry: he had studied physics under Peter Guthrie Tait at the University of Edinburgh, and worked on telegraph cables for William Thomson and Fleeming Jenkin. In 1878 Ewing replaced Perry as professor of mechanical engineering at the Imperial College of Engineering in Tokyo. Since 1890 Ewing was professor of mechanism and applied mechanics at the University of Cambridge.
27. Perry, *Applied Mechanics*.
28. Brock, "The Japanese Connexion"; Brock and Price, "Squared Paper in the Nineteenth Century."

> such as is used by engineers.[29]

Because it allowed even the "non-mathematical student" to gain "wonderful" insights, the squared paper was an important ally for Perry's brand of practical mathematics. The engineer deplored that "so many people should be ignorant of the great uses to which a sheet of squared paper may be put".[30]

On numerous occasions, Perry and Ayrton tried to take full credit for transforming the use of squared paper from a tool "for the recording of results of original experiments" into a pedagogical tool for training future practitioners:

> Prior to the commencement of the courses at the Finsbury Technical College, in 1879 squared paper was practically used in England only for the recording of results of original experiments. And as these results, rather than the training of the experimenter, were the most important part of the investigation, the paper was very accurately divided, and sold at a high price totally out of the reach of students. It became, therefore, necessary to have squared paper specially made cheap, and at the same time sufficiently accurately divided for students' purposes; and such paper, machine-ruled, can now be obtained at less than a farthing per sheet, or at about one-thirtieth of the cost of the older squared paper.[31]

A similar account was presented by Perry, who traced the use of squared paper back to the Tokyo laboratory:

> Before 1876 sheets of squared paper were very expensive; they were only used by a few people in important work. In that year Prof. Ayrton and I began to use it extensively in Japan, and when we returned to London and introduced at the Finsbury Technical College our methods of teaching Mechanical and Electrical Engineering and laboratory work which have now become so common, we saw that one essential thing was the manufacture of cheap squared paper. It can now be bought for 7d. a quire instead of 8d. per sheet. Our students treat it almost like scribbling paper. This year the candidates in three important subjects of the Science and Art Department will, for the first time, write their answers upon books of squared paper. It is of importance that the student should use many sheets of squared paper, use them lavishly. It used to be that many men knew how squared paper might be used, but they really never used it, or if they did use it, they used it

29. William Edward Ayrton and John Perry, "Some Remarks on the Technical Education of an Electrical Engineer," *Journal of the Society of Telegraph Engineers and of Electricians* 11, no. 43 (1882): 397-398.
30. Perry, *Practical Mechanics*, 7.
31. William Edward Ayrton, *Practical Electricity: A Laboratory and Lecture-Course* (London: Cassell, 1887), 30.

> not for solving problems but for illustrating methods of solving problems.[32]

Indeed, a visitor of the Tokyo laboratory also remarked that the majority of students in the drawing office "were reducing observations and drawing curves on squared paper".[33]

1.2 The Mind of the Student

Between 1896 and 1913, John Perry acted as the professor of mathematics and mechanics at the Royal College of Science (which became part of the Imperial College from 1907).[34] During this period he delivered a series of lectures on "practical mechanics" to working men and published a popular textbook on the *Calculus for Engineers* (1896). Most significantly, Perry started extending his views on engineering education to general education; he even put forward the bold claim that "the proper method of teaching any subject is through some kind of experimental work".[35] But how could an engineer persuade fellow mathematicians or secondary school teachers that his views on "practical mathematics" were applicable not only to technical students, but also to students that would pursue some liberal profession? Simply invoking the practical utility of such an education would not have been enough to make it acceptable for a general education. What made Perry's rhetoric so effective was that he did not advocate only that all students should acquire a practical or technical knowledge, but rather that such knowledge was best suited for all (or at least, most) students.

For most of the 19th century, the purpose of a liberal or general education was that of *training*, *cultivating*, *disciplining* or *forming* the mind. This "formal discipline" was often based on a "muscular model of mental capacity" (as referred to by the historian Fritz K. Ringer) because it described the mind as a muscle which can be trained and strengthened through

32. John Perry, *Practical Mathematics: Summary of Six Lectures Delivered to Working Men* (London: Printed for H.M. Stationery, 1899), 27.
33. "The Physical Laboratory of the Imperial College of Engineering, Tokei," *Japan Weekly Mail*, 1878, 1130.
34. Gooday, "Perry, John (1850–1920)."
35. Perry, "The Teaching of Mathematics," 19.

"mental gymnastics".[36] The form of education ruled over its content because what mattered was not the acquisition of some particular or specialized "useful knowledge", but rather the overall formation and strengthening of the intellect.

> The true view of education, on the contrary, is to regard it as a course of training. The youth in a gymnasium practises upon the horizontal bar, in order to develop his muscular powers generally; he does not intend to go on posturing upon horizontal bars all through life. School is a place where the mental fibres are to be exercised, trained, expanded, developed, and strengthened, not 'crammed' or loaded with ' useful knowledge' (Jevons 1877, 197).[37]

The acquisition of knowledge was similarly opposed to mental development because the goal of a general education was "to form mental muscle and not mental fat".[38]

The central metaphor of a "formal discipline" based on "mental gymnastics" *valued* pedagogical approaches which emphasized the role of struggle and hardship in the acquisition of knowledge. For example, William Whewell, one of the main early 19th century defenders of the place of mathematics within a liberal education, rejected the use of Continental algebraic and analytical tools which facilitated the solution of certain problems because "brief and easy methods of arriving at each result" could provide "no exercise of intellectual vigour and power". "Struggle" was "the very condition and essential point of intellectual discipline".[39] Augustus de Morgan replied in a similar vein when he acknowledged that students would prefer

36. "A related characteristc of the French case for the classcal curriculum was the almost universal recourse to the metaphor of mental gymnastics. No single definition of education acquired as much currency among French educational traditionalists, and indeed in reformist circles as well, as the notion of a 'general cultivation of the mind' or of 'the intelligence'. Croiset dramatized this aspect of his argument by picturing the child gaining strength in a 'contest' with though more 'vigurous' than his", in Fritz K. Ringer, *Fields of Knowledge : French Academic Culture in Comparative Perspective, 1890-1920* (Cambridge England ; New York: Cambridge University Press, 1992). This coordinating metaphor was active in both Britain, the US, France and Germany. See Walter Bernard Kolesnik, *Mental Discipline in Modern Education* (University of Wisconsin Press, 1958), esp. 10-29; Sheldon Rothblatt, *Tradition and Change in English Liberal Education : An Essay in History and Culture* (London: Faber and Faber, 1976), 126-129; Ringer, *Fields of Knowledge*, 145; Katja Krüger, *Erziehung Zum Funktionalen Denken: Zur Begriffsgeschichte Eines Didaktischen Prinzips* (Berlin: Logos-Verl., 2000), 76-77; Lewis Pyenson, *Neohumanism and the Persistence of Pure Mathematics in Wilhelmian Germany* (Philadelphia: American Philosophical Society, 1983), 29. For mathematics as a "mental gymnastics" required in the "formation of the spirit" see Alexandre Ribot, ed., *Enquête sur l'enseignement secondaire: procès-verbaux des dépositions*, vol. 1 (Motteroz, 1899), 340.

37. W. Stanley Jevons, "Cram," *Mind* os-2, no. 6 (1877): 197.

38. William Kingdon Clifford, *Lectures and Essays* (Macmillan and co., 1886), 71.

39. William Whewell, *Of a Liberal Education* (London: J. W. Parker, 1845), 51.

J.M. Wilson's *Elementary Geometry* over Euclid's *Elements* because

> they all like systems to be shaved clean of difficulties by the razor of unlimited assumption. But the way to accurate thought is hard and stony, requires the mind to be turned upon the sources of error, and braced to their discovery.[40]

Morgan considered that beginners should be first

> accustomed to deduction by reasoning, without having recourse to the mechanism of algebra, which, as a quaint editor of Euclid observed, "is the paradise of the mind, where it may enjoy the fruits of all its former labours, without the fatigue of thinking".[41]

A similar emphasis on struggle and hardship in education to increase the "mental power" of students was also common in the US:

> [if a student] wants mental power, let him write out in choice language a difficult page of Greek or Latin, or study out a theorem of geometry, or the reaction of a chemical experiment. Now, what is the result, as seen in society, of easy-going methods which do not demand the most strenuous effort on the part of the pupil? By mitigating hard tasks we are raising a pack of noodles who fumble but do not skilfully manipulate the conditions of success. We have a large class of kid-gloved, milk-and-water fellows who are lazily looking after a soft job and thus bringing reproach on the public school.[42]

Especially in places such as Cambridge, physical prowess demonstrated through rowing competitions paralleled the intellectual prowess demonstrated through examinations such as the Tripos Examinations.[43]

To the mid-19th century image of intellectual and physical vigor, John Perry opposed the image of "a spiritless boy of nineteen, with rounded shoulder" who has "has done much science, and *all* algebra, and *all* trigonometry".[44] Average students were "killed with torments" because of the "mental harm" produced by the "academic machinery":

40. Augustus De Morgan, "Review of Elementary Geometry," *The Athenaeum*, no. 2125 (1868): 72; see also Richards, *Mathematical Visions*, 165-167.
41. Augustus De Morgan, *An Essay on Probabilities, and Their Application to Life Contingencies and Insurance Offices* (Longman, Orme, Brown, Green & Longmans, 1838), xii.
42. *Annual Report of the State Superintendent of Common Schools. State of Maine* (Augusta: Burleigh & Flynt, 1888), 106.
43. See Warwick, *Masters of Theory : Cambridge and the Rise of Mathematical Physics.*
44. John Perry, "The Preliminary Education of the Engineer," *School Science and Mathematics* 2, no. 5 (1902): 10.

> The average English boy takes unkindly to abstract reasoning, and if compelled to such study when unwilling, is hurt mentally for life; loses his self-respect first, then his respect for all philosophy; gets to hate mathematics.[45]

Perry targeted not only the secondary school mathematical education, but also mathematical examinations such as the Cambridge Tripos which he saw not as a celebration of the heroic performance of gifted students, but rather as a sacrifice of the average student in which "10 million destroyed for the sake of producing one great mathematician", or:

> In the heroic times every traveller was asked an enigma; if he did not answer he was killed with torments; if he answered, he was declared a demigod and given to rule over nations. In those times it was thought good to sacrifice myriads of people for the purpose of finding the one demigod.[46]

Perry's rhetoric would have found some resonance not only with the outside critics of the Tripos, but even with the students who did pass through the "academic machinery".[47] In a letter to his father from 1842, after considering a long list of men who broke down under the pressure of hard study, Francis Galton reached a similar conclusion to Perry:

> I feel more convinced every day that if there is a thing more to be repressed than another it is certainly the system of competition for the satisfaction enjoyed by the gainers is very far from counterbalancing the pain it produces among the others.[48]

Galton himself broke down during his third year at Cambridge and had to lose a term:

> I suffered from intermittent pulse and a variety of brain symptoms of an alarming kind. A mill seemed to be working inside my head; I could not banish obsessing ideas; at times I could hardly read a book, and found it painful even to look at a printed page. Fortunately, I did not suffer from sleeplessness, and my digestion failed but little. Even a brief interval of complete mental rest did me good, and it seemed as if a long dose of it might wholly restore me. It would have been madness to continue the kind of studious life that I had been leading. I had been much too zealous, had

45. John Perry, ed., *Discussion on the Teaching of Mathematics: British Association Meeting at Glasgow, 1901* (London: Macmillan and Co., 1902), 22.
46. Perry, "The Teaching of Mathematics," 6; Perry, "The Teaching of Mathematics," 7; see also John Perry, "Oxford and Science," *Nature* 69, no. 31 (1903): 207–14; John Perry, "The Mathematical Tripos at Cambridge," *Nature* 75 (1907): 273–274.
47. See Warwick, *Masters of Theory : Cambridge and the Rise of Mathematical Physics*, 182-191.
48. Karl Pearson, *The Life, Letters and Labours of Francis Galton*, vol. 1 (Cambridge: University press, 1914), vol. 1, 171.

> worked too irregularly and in too many directions, and had done myself serious harm. It was as though I had tried to make a steam-engine perform more work than it was constructed for, by tampering with its safety valve and thereby straining its mechanism. Happily, the human body may sometimes repair itself, which the steam-engine cannot.[49]

Perry's accusations of "mental harm" would have been cogent beyond such personal anecdotes. In the late 19th century, the principles of the economy of work started also being applied to the "mental economy" which paid particular attention to "mental fatigue" and "mental effort". An 1886 report on "le surmenage intellectuel" of students described how the "surcharge intellectuelle" could produce "une fatigue, un affaiblissement plus ou moins durable de l'intelligence, qui perdant toute initiative, toute force de volonté, toute énergie morale, devient et parfois demeure durant le reste de l'existence remarquablement lente, lourde, hébétée".[50] Some classified "mental fatigue" as a "malady of the will [malaldies de la volonté]".[51] The main recommendation was to decrease the amount of study, and increase physical exercise. The concern with "mental fatigue" reflected a much more general trend within 19th century society.[52] In Germany, the pedagogical value of "das Schwierige in der Mathematik" came to be questioned because such a training would transform the student in a "gefühllosen, traurigen Sklaven". The effort of abstraction and abstract manipulations was to be supplanted by a practical and intuitive education centered on cultivating a student's interest, understanding, creativity, joy for success, curiosity, or ambition.[53]

Perry's reform of mathematics was successful not because he advocated for useful knowl-

49. Sir Francis Galton, *Memories of My Life* (London: Methuen, 1908), 79; Warwick, *Masters of Theory : Cambridge and the Rise of Mathematical Physics*, 183.

50. *Revue d'hygiène et de police sanitaire* 8 (1886): 431.

51. See for example Th. Ribot, *Les Maladies de La Vononté* (Paris: G. Balliere, 1883).

52. On mental fatigue see Anson Rabinbach, *The Human Motor: Energy, Fatigue, and the Origins of Modernity* (University of California Press, 1992), 146-178. Henri Bergson tried to identify the nature of "l'effort intellectuel" required by understanding in Henri Bergson, "L'effort intellectuel," *Revue philosophique de la France et de l'étranger* 53 (1902): 1–27; John Dewey also tackled the "psychology of effort" in John Dewey, "The Psychology of Effort," *The Philosophical Review* 6, no. 1 (1897): 43–56; see also Edward Thorndike, "Mental Fatigue. I," *Psychological Review* 7, no. 5 (1900): 466–482; Edward Thorndike, "Mental Fatigue. II," *Psychological Review* 7, no. 6 (1900): 547–579

53. B. Habenicht, "Erleichterungen im geometrischen Unterrichte, besonders des ersten Jahres," *Unterrichtsblätter für mathematik und naturwissenschaften* 5 (1899): 92, 109.

edge, but rather because he construed utility as the prerequisite of true knowledge acquisition. This step was essential if he wanted to make his brand of practical mathematics appropriate for a general education. Some mathematical educators like Sir Philip Magnus attacked Perry because he did not properly specify the aims of mathematical teaching: was it "a tool to be used for obtaining certain practical results", or was the aim "brain development, accuracy in calculation and in measurement, the acquisition of correct methods of reasoning"?[54] In the end, Magnus thought that "for the purposes of general education, a boy cannot be said to be learning mathematics by merely acquiring dexterity in the use of mathematical tables".[55]

Perry had anticipated this line of attack. In the address he presented the goals of his mathematical education as being not that of teaching a student how to use practical mathematical tools, but rather that of making mathematical and scientific principles "become part of his mental machinery" such that "he can no more forget them".[56] However, such a knowledge could not be imparted through "academic absorption from a book".[57] Only what a student "discovers for himself, that is of real value to him, that becomes permanently part of his mental machinery".[58] In the mechanical laboratory even "the dullest student" could make the fundamental principles of mechanics "become part of his mental machinery", "if he is not too much spoon-fed; and if his difficulties are not cleared away by some wretched routine system of laboratory work being adopted by cheap laboratory instructors".[59] Properly taught mathematics "ought to be as much a part of [a student's] mental machinery as the power to walk is part of his physical function", such that "the pupil is certain to apply it in all sorts of practical problems, and will no more allow it to become rusty than his power to read or write or walk".[60] Perry contrasted

54. Magnus in Perry, *Discussion on the Teaching of Mathematics: British Association Meeting at Glasgow, 1901*, 83.
55. Magnus in ibid., 84.
56. Ibid., 11.
57. John Perry, *The Steam Engine and Gas and Oil Engines* (London: Macmillan and Co., 1909), 402.
58. Perry, *Discussion on the Teaching of Mathematics: British Association Meeting at Glasgow, 1901*, 8.
59. John Perry, "On the Teaching of Elementary Mechanics," in *Discussion on the Teaching of Mechanics: British Association Meeting at Johannesburg, 1905*, ed. John Perry (London, New York, Macmillan, 1906), 56.
60. Perry, *Discussion on the Teaching of Mathematics: British Association Meeting at Glasgow, 1901*, 5, 29.

his experience of learning calculus – "to me every example was a labour, an interesting labour; but truly a difficult job" – with that of other students who were taught "the knack ... of rapidly picking up just such instruction as enabled them to do the examples". Though he "lagged behind in exercise work", Perry felt that because he was "really using the idea of calculus in all sorts of problems outside the academic ones" it became "part of my mental machinery".[61] Opposed to "mental gymnastics", Perry's aim was "giving men mental tools as easy to use as their legs or arms".[62]

1.3 The New Geometry

Following the 1901 Glasgow meeting of the British Association where John Perry read his paper on "The Teaching of Mathematics", a committee was appointed to report on possible improvements in the teaching of mathematics, and especially on elementary geometry. The chairman of the committee was Prof. Forsyth, while the secretary was John Perry. In 1902, at the next meeting of the BAAS in Belfast, a report was presented that made several recommendations. The impact of this report was boosted after it was endorsed by the examination syndicates of the Universities of Cambridge, Oxford and London. Although the report was quite conservative compared to Perry's initial stance, it still pushed forward several of his ideas. Most remarkably, the teaching of demonstrative geometry was to be preceded by

> the teaching of practical and experimental geometry, together with a considerable amount of accurate drawing and measurement. Simple instruments and experimental methods should be employed exclusively in the earliest stages, until the learner has become familiarised with some of the notions of geometry and some of the properties of geometrical figures, plane and solid.[63]

There was no unique textbook, order or method of proof. If until then, geometry and algebra had been taught as distinct water-tight compartments , now "[a]lgebraical processes" were to

61. Perry, *Discussion on the Teaching of Mathematics: British Association Meeting at Glasgow, 1901*, 9.
62. Ibid., 5.
63. John Perry, "Teaching of Elementary Mathematics - Report of the Committee," in *Report of the British Association Meeting at Belfast, 1902* (London, New York: Macmillan and Co., 1903), 475.

be combined "with the methods of practical geometry" because "some association of arithmetic and algebra with geometry is desirable in all cases where this may be found possible"; this was particularly the case in the teaching of proportions.[64] Furthermore, propositions in Euclid were to be experimentally "tested by squared paper" either by "counting squares on squared paper to verify rules", while areas were to be measured by "weighing a piece of cardboard and comparing with the weight of a square".[65]

These recommendations were followed by the new geometry textbooks which were soon published, such as Charles Godfrey and Arthur Siddons's *Elementary Geometry: Practical and Theoretical* (1903) or Alfred Warren's *Experimental and Theoretical Course of Geometry* (1903).[66] Both textbooks opened with an experimental part followed by a theoretical part. Their prefaces emphasized that the textbooks were aimed at making geometry "an attractive subject to the average British boy or girl" for whom "the experimental course... [was] found to stimulate interest".[67] Opposed to previous textbooks, the new textbooks employed a wide array of exercises: "some are experimental and lead up to future propositions, some are graphical and numerical illustrations of known propositions, some are 'riders' of the ordinary type".[68] The central role was occupied by the manipulation of paper: "the fullest possible use is made of squared paper, set squares, rule and compasses", while "geometrical properties are illustrated by paper folding".[69]

Several aspects set apart the old "practical geometry" from the new "experimental geometry" advocated by Perry. First, the order of teaching was reversed. Practical geometry, descriptive geometry or graphical drawing were supposed to be an application of the principles

64. Perry, "Teaching of Elementary Mathematics - Report of the Committee," 476.
65. Ibid., 479.
66. Taro Fujita, "The Study of "Elementary Geometry" (1903) by Godfrey and Siddons (1): Roles of Experimental Tasks in the Teaching of Geometry.," *Hiroshima Journal of Mathematics Education* 9 (2001): 11–19.
67. C Godfrey and A. W Siddons, *Elementary Geometry: Practical and Theoretical* (Cambridge: University Press, 1903), v.
68. Ibid., vi.
69. A. T Warren, *Experimental and Theoretical Course of Geometry* (Oxford: Clarendon Press, 1903), Preface.

of rigorous, deductive geometry. In the case of “experimental geometry”, students were “led to discover many geometrical truths which are proved later” and “the pupil is led to draw for himself conclusions”.[70] If “practical geometry” was often taught as a “bundle of rules”, “experimental geometry” was modeled after an experimental science.[71] Students were taught how “to use instruments, to measure accurately lines and angles (this will in future be regarded as an indispensable part of geometrical work), to construct and recognize the simpler plane and solid figures, to solve problems by drawing to scale”. Instead of constructing abstract proofs or following practical rules, students were “encouraged to put into words and make notes of any such discoveries”.[72] Only after the experimental proofs did the theoretical proofs follow. If for Monge and his followers graphical constructions embodied the language of geometry, for Perry and his associates graphical methods were the language of experimental science.

2 THE PERRY MOVEMENT ABROAD

2.1 US

Despite the fact that much of Perry’s criticism was specific to Britain, his address generated a surprising amount of interest among German, French and American pure mathematicians. In December 1902 Eliakim Hasting Moore (1862-1932), professor of mathematics at the University of Chicago and president of the American Mathematical Society, delivered a presidential address on the “The Foundations of Mathematics” which mentioned “the surprisingly vigorous and effective agitation with respect to the teaching of elementary mathematics which is at present in progress in England”.[73] Moore declared that

> As a pure mathematician, I hold as the most important suggestion of the English movement the

70. Warren, *Experimental and Theoretical Course of Geometry*, Preface.
71. John Perry, “Reform of Mathematical Education,” *Engineer* 93 (1902): 203.
72. Godfrey and Siddons, *Elementary Geometry: Practical and Theoretical*, v.
73. On Moore’s pedagogical aims see David Lindsay Roberts, “E. H. Moore’s Early Twentieth-Century Program for Reform in Mathematics Education,” *The American Mathematical Monthly* 108, no. 8 (2001): 689–696; David Lindsay Roberts, “Mathematics and Pedagogy: Professional Mathematicians and American Educational Reform, 1893-1923” (PhD diss., The Johns Hopkins University, 1998).

> suggestion of Perry's, just cited, that by emphasizing steadily the practical sides of mathematics, that is, arithmetic computations, mechanical drawing and graphical methods generally, in continuous relation with problems of physics and chemistry and engineering, it would be possible to give very young students a great body of the essential notions of trigonometry, analytic geometry, and the calculus.[74]

Moore defined the fundamental problem of teaching mathematics as "*the unification of the pure and applied mathematics*" and "*the correlation of different subjects* of the curriculum".[75] Such a unification was to be carried out through the "laboratory method" which was centered on "graphical depiction":

> This program of reform calls for the development of a thoroughgoing laboratory system of instruction in mathematics and physics, a principal purpose being as far as possible to develop on the part of every student the true spirit of research, and an appreciation, practical as well as theoretic, of the fundamental methods of science.
>
> …
>
> As the world of phenomena receives attention by the individual, the phenomena are described both graphically and in terms of number and measure; the number and measure relations of the phenomena enter fundamentally into the graphical depiction, and furthermore the graphical depiction of the phenomena serves powerfully to illuminate the relations of number and measure. This is the fundamental scientific point of view. Here under the term graphical depiction I include representation by models.[76]

In Moore's laboratory students were supposed to collaborate with each other and use "laboratory record books, cross-section paper, computational and graphical methods in general, including the use of colored inks and chalks".[77] The point was taken up again in 1906 when Moore read at the Mathematical Club of the University of Chicago a paper on "The cross-section paper as a mathematical instrument".[78] Moore proposed "the systematic use of cross-section paper

74. E. H. Moore, "On the Foundations of Mathematics," *Bulletin of the American Mathematical Society* 9, no. 8 (1903): 411.
75. Ibid., 413.
76. Ibid., 417-418.
77. Ibid., 420.
78. Moore engaged with the broader category of "cross-section paper" which included along with square-ruled paper other styles of ruling – rectangles, parallelograms, triangles, circles or diverging radii. Moore pointed out that "the interaction of the various papers is especially important" (Eliakim Hastings Moore, "The Cross-Section Paper as a Mathematical Instrument," *The School Review* 14, no. 5 (1906): 317 n2).

as a unifying element in mathematics". Because "cross-section paper" led directly to the to the concept of *functionality* it could both *correlate* mathematics and the physical sciences, and *unify* pure and applied mathematics.[79] As Perry before him, Moore emphasized the ideological value of such an approach:

> In general, we agree to develop arithmetical, algebraic, geometric technique in a physical and intellectual environment logically and psychologically rich, full of movement, force, color, full of connotations and implications of and for real life of all kinds, including most certainly the real life of mathematics and the sciences. [80]

The paper ended with the hyperbolic slogan – "Canonize the Cross-section Paper".[81]

2.2 France

In France, secondary school education (for boys) was substantially reformed in 1902 after a special committee had been previously established in 1898 under the supervision of Alexandre Ribot. The mathematical commission in charge of the reform was presided by Gaston Darboux, while the subcommissions were formed by Jules Tannery, Paul Appell, Gabriel Koenigs, etc.[82] Most of the members of the mathematical commission were coming from institutions of higher education, mainly l'École Normale Supérieure and Sorbonne, and aimed to replace the teaching that was "routinier, dogmatique, et abstrait" with an education accessible to most students.[83] The teachers of physics for all the sections (including the classical education centered on Greek and Latin) were asked to "use frequently graphical representations, not only for better showing to students the shape of phenomena, but to make them understand [pour

79. Moore, "The Cross-Section Paper as a Mathematical Instrument," 317-318.
80. Ibid., 338.
81. Ibid.
82. In the 1890s Gaston Darboux had coordinated a popular series of mathematical textbooks under the title *Cours complet de mathématiques élémentaire*: Jules Tannery,*Leçons d'arithmétique, théorique et pratique* (1894); M. Tisserand and H. Andoyer, *Leçons de cosmographie* (1895); Carlo Bourlet, *Leçons d'Algèbre élémentaire* (1896); Jacques Hadamard, *Leçons de géométrie élémentaire* (1898); Carlo Bourlet, *Leçons de trigonométrie rectiligne* (1898).
83. Bruno Belhoste, "L'enseignement secondaire français et les sciences au début du XXe siècle: La réforme de 1902 des plans d'études et des programmes," *Revue d'histoire des sciences* 43, no. 4 (1990): 372.

faire pénétrer dans leur esprit] the so important ideas of function and continuity".[84] Because the notion of function played a key role in the physics curriculum, it was also extended to the mathematical lessons.[85] The study of mathematics in the "classe de philosophie" (the final year of study in the Greek-Latin section) required "l'usage du papier quadrillé" in solving equations; "représentation graphique de la variation d'un phénomène qui dépend d'une seule variable; courbes des températures, des poids; application à la statistique"; "graphique des chemins de fer"; "courbes fournies par les appareils enregistreurs".[86] For the science based sections, the graphical representations of simple equations was introduced much earlier, at the end of the first cycle.[87] A further modification was brought to the mathematical curriculum in 1905 which defined the teaching of geometry as "essentiellement concret".[88]

The use of "papier quadrillé" in mathematical instruction had been strongly advocated by the French mathematician Charles-Ange Laisant, who along with the Swiss mathematician Henri Fehr co-founded in 1899 the journal *L'Enseignement mathématique*. Between 1899 and 1903 Laisant delivered a series of lectures at the Psycho-physiological Institute in Paris on "L'éducation fondée sur la science". In a lecture from 1899 on "l'initiation mathématique", Laisant presented "le papier quadrillé" as an

> instrument merveilleux qui devrait être entre les mains de quiconque fait des mathématiques (depuis la famille ou la salle d'asile jusqu'à l'école Polytechnique, et même au delà), et, d'une manière générale, de quiconque fait de la science. Mais c'est surtout un instrument merveilleux, au point de vue pédagogique, pour donner aux petits enfants ces premières notions de la forme, de la grandeur et de la position, sans lesquelles l'initiation n'est qu'un leurre.[89]

84. *Plan d'études et programmes d'enseignement dans les lycées et collèges de garçons arrêtés du 31 mai 1902* (Paris: Delalain frères, 1902), 74,118,126.

85. "L'étude des fonctions, en mathématiques, reste marquée par ses origines physiciennes. Elle est pratique, quasi-expérimentale : pas de définitions générales et abstraites, mais un crayon et du papier millimétré pour construire les graphes des quelques fonctions simples que le programme prévoit d'étudier." in Belhoste, "L'enseignement secondaire français et les sciences au début du XX e siècle," 394.

86. *Plan d'études et programmes d'enseignement dans les lycées et collèges de garçons arrêtés du 31 mai 1902*, 163-164.

87. Ibid., 88-89, 122, 178.

88. *L'Enseignement mathématique* 7 (1905): 493.

89. Charles-Ange Laisant, "L'initiation mathématique," *Revue scientifique* 11 (1899): 365. The lecture was later republished in Charles-Ange Laisant, *L'éducation fondée sur la science* (F. Alcan, 1905).

Similarly, in *La mathématique: philosophie-enseignement* (1st ed. 1898, 2nd ed. 1907), Laisant emphasized the universal use of squared paper in the teaching of mathematics which "est aussi nécessaire à l'auditeur des cours les plus élevés de nos facultés qu'au petit enfant formant pour la première fois ses chiffres".[90] The "method of graphical representations" was also recommended as presenting "d'immenses avantages".[91]

While the 1902 reform was mainly concerned with algebra, the 1905 changes focused mainly on geometry. The shift was bolstered by a 1904 series of conferences at the Musée pédagogique regarding the teaching of the mathematical and physical sciences. Though John Perry's name was not mentioned, the central ideas and principles closely resembled those of Perry and Moore. The young mathematician Émile Borel, a former student of Gaston Darboux and *maître de conférences* at l'École normale supérieure, gave a talk on "les exercices pratiques de mathématiques dans l'enseignement secondaire" which could bring "plus de vie et de sens du réel" in the teaching of mathematics.[92] Borel also embraced the idea of creating "de vrais laboratoires de mathématiques" imagined as "mensuration workshops".[93] The laboratory was an opportunity to bring closer together the teaching of mathematics and physics. Like Felix Klein in Germany, Borel did not support the abolishment of "theoretical mathematics" but only its supplementation by practical mathematics. Ultimately such an education would "create free men" for whom "reason only bends in front of facts".[94] Borel defended the idea that "mathematics is not a pure abstraction" and advocated the incorporation of geometrical drawing within geometry classes, and its connection to numerical computations.[95] A few years later, Borel and Jules Tannery (who had been a member of Gaston Darboux's mathematical commission for the 1902 reform) started at l'École Normale Superieure a "laboratoire

90. Charles-Ange Laisant, *La mathématique: philosophie, enseignement* (Paris: Carre et Naud, 1898), 202.
91. Ibid., 153.
92. Emile Borel, "Les exercices pratiques de mathématiques dans l'enseignement secondaire," in *L'enseignement des sciences mathématiques et des sciences physiques* (Paris: Imperimerie nationale, 1904), 121.
93. Ibid., 123.
94. Ibid., 131.
95. Ibid., 122.

d'enseignement mathématique".[96] Thew new "laboratory" taught future teachers how to build mathematical models and mechanical apparatus out of wood and cardboard.[97]

Borel further advocated the new pedagogical principles in his textbook on *Algèbre* (1903) which was written with the main purpose of "intéresser les élèves" by paying attention to "les nécessités de la vie pratique" and "la réalité journalière".[98] If such realities were ignored one ran the risk of "dégoûter un grand nombre d'excellents esprits".[99] The idea was taken up again in an article from 1914 in which Borel warned about the dangers of an education which was increasingly divorced from "life and reality".[100] Only by employing familiar objects, such as the graphs encountered in newspapers, could mathematical instruction actually interest the student. However, when "an education is too scholastic, it repels [dégoûte] a great number of students and deforms rather than forms the spirit".[101] The same point had been raised by Charles Laisant who proposed that one should educate a student "by amusing instead of boring him, by substituting the play for the fatigue, by making him interested instead of disgusted".[102]

Jacques Hadamard, a former student of Jules Tannery at l'École Normale Supérieure, also expressed his support for the "mathematiques experimentales". Hadamard considered that if geometry was taught as "une science physique — ce qu'elle est véritablement", then "on fera

96. Albert Châtelet, "Le laboratoire d'enseignement mathématique de l'École Normale Supérieure de Paris," *L'Enseignement mathématique* 11 (1909): 206–210.

97. In the case of Italy, the reform of the teaching of geometry was translated into the term "geometria intuitiva", as in Aureliano Faifofer's *Trattato di geometria intuitiva ad uso dei ginnasi e scuole tecniche* (1882); G. Veronese's *Nozioni elementari di geometria intuitiva* (1901); G. Frattini's *Geometria intuitiva per uso delle scuole complementari e del ginnasio inferiore* (1901). See Livia Giacardi, "From Euclid as Textbook to the Giovanni Gentile Reform (1867–1923): Problems, Methods and Debates in Mathematics Teaching in Italy," *Paedagogica Historica* 42, no. 4 (2006): 587–613; Marta Menghini, "From Practical Geometry to the Laboratory Method: The Search for an Alternative to Euclid in the History of Teaching Geometry," in *Selected Regular Lectures from the 12th International Congress on Mathematical Education*, ed. Sung Je Cho (Springer International Publishing, 2015), 561–587.

98. Between 1903 and 1905, Émile Borel ended up publishing a complete series of courses on arithmetic, algebra, geometry and trigonometry. Borel's textbooks were given as an example by Felix Klein, and were readily translated in German.

99. Émile Borel, *Algèbre* (Paris: A. Colin, 1903), 3-5.

100. Émile Borel, "L'adaptation de l'enseignement secondaire aux progrès de la science," *L'Enseignement Mathématique* 16 (1914): 204.

101. Ibid.

102. "…en l'amusant au lieu de l'ennuyer, en substituant le jeu à la fatigue, en arrivant à l'attrait à la place du dégoût", in Charles-Ange Laisant, "La première éducation scientifique," *Revue scientifique (Revue rose)*, 1908, 451.

disparaître ce que son enseignement a présenté jusqu'ici d'artificiel et de rebutant".[103] The idea of a "géométrie empirique" or "géométrie expérimentale" was dismissed by a few school teachers who considered that what was taught under this name was not actually geometry, and it thus had no pedagogical value. The new trend was seen as arising from "vouloir supprimer chez l'élève tout effort intellectuel. Cette tendance est mauvaise. Il faut au contraire faire chercher l'élève par lui-même, l'habituer au travail personnel."[104]

2.3 Germany

A similar reform of secondary school eduction took place in Germany in 1905, and came to be known as the "Meran reform" or "Meran program". The reform committee defined "the most important tasks of the teaching of mathematics" as being "the strengthening of spatial perception and the education of the habit of functional thought [die Stärkung des räumlichen Anschauungsvermögens und die Erziehung zur Gewohnheit des funktionalen Denkens]".[105] Felix Klein, the main proponent of the program, continued to insist that "the soul of mathematical school instruction" should generally be "[t]he concept of function in geometric form [der Funktionsbegriff in geometrischer Form]".[106]

Similarly to Perry and Moore, Klein emphasized that the "arithmetic and geometry merge [verschmelzen] in the central idea: the function-concept in geometrical form [Der Funktionsbegriff in geometrischer Form]".[107] Opposed to the notion of an equation, the notion of function was presented as something much more general that connected the whole of mathematical

103. *L'enseignement des sciences mathématiques et des sciences physiques* (Paris: Imperimerie nationale, 1904), 163-164.

104. J. Richard, "Contre la géométrie expérimentale," *Revue de l'enseignement des sciences*, 1910, 152.

105. *Die Tätigkeit der Unterrichtskommission der Gesellschaft Deutscher Naturforscher und Ärzte* (B. G. Teubner, 1908), 96, 104; See also Krüger, *Erziehung Zum Funktionalen Denken.*

106. Felix Klein and Rudolf Schimmack, *Vorträge über den mathematischen unterricht an den höheren schulen* (B. G. Teubner, 1907), 34; Gert Schubring, "Der Aufbruch Zum „funktionalen Denken": Geschichte Des Mathematikunterrichts Im Kaiserreich," *NTM International Journal of History and Ethics of Natural Sciences, Technology and Medicine* 15, no. 1 (2007): 6.

107. Klein and Schimmack, *Vorträge über den mathematischen unterricht an den höheren schulen*, 38.

teaching.[108] The "conceptual formation [Begriffsbildung]" imparted by this type of education based on "mathematical mental activity [mathematischen Geistestätigkeit]" was supposed to accompany the student throughout his life.[109] In all of this, graphical methods were to be at the center of the new education based on functions. As Klein remarked, "[w]e cannot open a newspaper from which no such figure springs out [Wir können keine Zeitung mehr aufmachen, aus der uns nicht eine solche Figur entgegenspringt!]"[110] Graphical representations were not only ubiquitous in the "modern literature of the exact sciences", but they were also "going through the whole life of the present!"[111] For such reasons, the student had to learn to draw on "Koordinatenpapier".[112] In a few years, the pedagogical principles of Klein and the "Meran reform" were incorporated in a new generation of textbooks such as D. Behrendsen and E. Götting's *Lehrbuch der mathematik nach modernen grundsätzen* (1909), or K. Schwab and O. Lesser *Mathematisches Unterrichtswerk zum Gebrauche an höheren Schulen* (1909).[113]

Given the common goals of their reform programs it is not surprising that, especially after the Glasgow address, John Perry became a figure of interest for Felix Klein and his associates. Perry's textbooks and ideas were popularized in Germany especially through the activity of Robert Fricke, Klein's nephew and one of his main collaborators.[114] Fricke, together with Fritz Süchting, prepared a German translation of Perry's *Calculus for Engineers* under the title

108. "Die Erziehung zum funktionalen Denken ist eben etwas viel Allgemeineres! [The education to functional thinking is something much more general!]" in Rudolf Schimmack, *Die Entwicklung der mathematischen Unterrichtsreform in Deutschland* (Leipzig und Berlin: Teubner, 1911), 98.

109. Klein and Schimmack, *Vorträge über den mathematischen unterricht an den höheren schulen*, 40, 6.

110. Ibid., 34.

111. Ibid., 21.

112. Ibid., 34.

113. Krüger, *Erziehung Zum Funktionalen Denken*, 180-1.

114. Several of Klein's mathematical works were written in collaboration with Fricke, such as *Vorlesungen über die Theorie der elliptischen Modulfunktionen* (1890) or *Vorlesungen über die Theorie der automorphen Functionen* (1897-1912). For Klein's goals in the reform of technical education and secondary school education see Gert Schubring, "Mathematics Education in Germany (Modern Times)," in *Handbook on the History of Mathematics Education*, ed. Alexander Karp and Gert Schubring (Springer New York, 2014), 241–255; Schubring, "Der Aufbruch Zum „funktionalen Denken“"; Gert Schubring, "Pure and Applied Mathematics in Divergent Institutional Settings in Germany: The Role and Impact of Felix Klein," *The history of modern mathematics* 2 (1989): 171–220; Karl Heinz Manegold, *Universität, Technische Hochschule und Industrie* (Berlin: Duncker & Humblot, 1970); Krüger, *Erziehung Zum Funktionalen Denken*; Pyenson, *Neohumanism and the Persistence of Pure Mathematics in Wilhelmian Germany*, 63-65.

Höhere Analysis für Ingenieure (1902). The book was justified as a mean of "abolishing the alienation between the technical and the mathematical science".[115] "Perry's method [Perrys Weg]" was only recommended as a mean of reaching a "golden middle way" between the "wissenschaftlichen Grundcharakter" of the mathematical lectures that were given in German schools, and the practical use and application of such knowledge.[116] The goals of the "Perry movement" and the British textbooks which were shaped by it were discussed in great detail in an 1904 article by Fricke.[117] Fricke criticized some of the textbooks based on Perry's methods (such as Castle's *Elementary practical mathematics* (1899) or *Practical mathematics for beginners* (1901)) for being too "dogamtic" in presenting only rules and theorems without developing a true understanding or justification. By implementing Perry's method, "the opposite extreme has been adopted" and Euclid's teaching had been "simply thrown overboard".[118] Fricke's views on Perry were also voiced by Klein who praised the British engineer for having launched "a reform of, one might even say, a revolutionary character". However, Klein considered Perry's recommendations suitable only for "*in-service schools, and lower and middle vocational schools*". For secondary schools, Klein recommended a "*middle course* between the two possible extremes: where along with the intuitive development of geometry, starting from practical experiences, the logical demonstrations will not be neglected".[119] The idea of mathematical instruction as a middle-path between application and abstraction had been one of the guiding principle of Klein's educational reforms. Already in 1900, Klein remarked that

> we want to stimulate mathematical instruction by using the applications, but we do not wish that the pendulum, which in earlier decades has perhaps pointed too much to the abstract side, to now pass into the other extreme; rather, we want it to stay in the center.[120]

115. John Perry, *Höhere Analysis für Ingenieure*, ed. Robert Fricke and Fritz Süchting (B.G. Teubner, 1902), v.
116. Ibid., vi.
117. Robert Fricke, "Über Reorganisationsbestrebungen des mathematischen Elementarunterrichts in England.," *Jahresbericht der Deutschen Mathematiker-Vereinigung* 13 (1904): 283–296.
118. Ibid., 293.
119. Felix Klein, *Elementary Mathematics from a Higher Standpoint: Geometry* (Berlin: Springer, 2016), 245-246.
120. *Verhandlungen über Fragen des höheren Unterrichts: Berlin, 6. bis 8. Juni 1900* (Halle: Buchhandlung des Waisenhauses, 1901), 153-154.

3 THE SQUARED PAPER IN ACTION

3.1 The Textbooks

The previous section has focused on school curricula and reform programs to delineate the place of graphical methods within mathematical training. While the early 1900s reform of secondary school mathematics gave rise to a new generation of mathematics textbooks which were centered on graphical methods, some of the earlier textbooks also employed graphical methods. By comparing the *use* and *place* of graphical methods within pre- and post-reform textbooks, this section will reveal the central role given to the *graphical*.

By the late 19th century, graphical methods were mainly and most consistently encountered in mathematical textbooks aimed for technical and engineering schools. While such methods were initially associated with the field of graphical statics, they were soon generalized into a topic commonly referred to as "graphical calculus", "calcul graphique" or "graphische Rechnen". This topic was brought to prominence by Carl Culmann's *Die Graphische statik* (1866) which had almost a hundred pages dedicated to "graphische Rechnen".[121] Following Culmann, many nineteenth century textbooks on graphical statics opened with a consistent discussion of graphical calculus. Karl von Ott, a professor at an *Oberrealschule* and a graduate of the Prague Polytechnicum, published *Der Grundzüge des graphischen Rechnens und der graphischen Statik* (1870) an extremely popular textbook which from the fourth edition (1879) included a whole volume on "graphische Rechnen".[122] While Culmann's course on graphical statics was aimed at engineer students who were familiar with descriptive and projective geometry, the teaching of graphical calculus was soon extended to students in technical schools who only possessed a rudimentary grasp of basic geometry.[123] For example, Julius Wenck,

121. Tournes, "Pour une histoire du calcul graphique"; Chatzis, "La Réception de La Statique Graphique En France Durant Le Dernier Tiers Du XIXe Siècle."

122. Ott's textbook was also translated in English as *The Elements of Graphic Statics* (1876). In 1865, Ott also published a short pamphlet on *Graphische Darstellung der Funktionen.* See also *Andeutung über die graphische Darstellung von Funktionen* (1848) by Edmund Oberreit, from the Gewerbeschule in Zittau.

123. Scholz, *Symmetrie, Gruppe, Dualität*; Scholz, "Graphical Statics."

the director of the "Baugewerbe- und Gewerbeschule" in Gotha, published a textbook on *Die graphische Arithmetik und ihre Anwendungen auf die Geometrie* (1879) – which though was initially conceived as part of a larger treatise on graphical statics, it was published as separate part because it could be of use not only in technical schools but also in *Realschulen* and *höheren Bürgerschulen*.[124]

While graphical calculus was mainly restricted to engineering schools (though, some introductory lessons and basic methods trickled down into the technical schools), secondary schools often introduced some basic concepts of analytical geometry (mainly restricted to the concept of coordinates which was only introduced in the highest classes) or the graphical representations of polynomials or the law of movement. In France, *l'enseignement secondaire spécial* required the "graphical representation of the law of movement", while *le concours d'agrégation* also included the topic of the "locus represented by the first degree equation with two unknowns. The graphs [graphiques] of railroads".[125] As for the *lycée*, the graphical representation of simple equations was required only for the classes of *mathématiques élémentaires*, the last class of the section which sustained the *Baccalauréat classique* in *lettres-mathématiques* opposed to *lettres-philosophie*.[126] In Germany, the Bavarian curriculum from the 1870s required for the highest class in the *Realgymnasium* (the equivalent of the French *lycée d'enseignement secondaire spécial*) the teaching of the "Function; ihre geometrische Darstellung".[127] The Austrian curriculum from 1884 for the highest class of the *Gymnasium* (the equivalent of the French *lycée*) expected students to be familiar with "Cartesischen Coordinaten" from their lessons in trigonometry; the lessons on analytical geometry used "die Coordinatengeometrie"

124. In Britain, "graphic arithmetic" was added as a special chapter in a textbook on *Practical plane and solid geometry* (1890) aimed at draughtsmen preparing for the South Kensington Examinations.

125. *Programmes officiels de l'enseignement secondaire spécial* (Librairie de L. Hachette, 1866), 170; *Bulletin administratif du Ministère de l'Instruction Publique*, vol. 44 (Paris: Impr. Nationale, 1888), 628.

126. *Plan d'études et programmes de l'enseignement secondaire classique dans les lycées et collèges, suivis des programmes des classes de mathématiques élémentaires et spéciales* (Paris: Delalain frères, 1897), 89. For the division of French secondary school education at the end of the 19th century see Belhoste, "L'enseignement secondaire français et les sciences au début du XX e siècle," 375.

127. Herwig Säckl, "Die Rezeption Des Funktionsbegriffs in Der Wissenschaftlichen Basis an Hochschule Und Schule Im Neunzehnten Jahrhundert" (PhD diss., Universität Regensburg, 1984), 110.

to show "der geometrischen Abbildung linearer Gleichungen". To save time, the graphical representations were to be drawn "on paper divided by lines into very small squares".[128] "Der Koordinatenbegriff und einige Grundlehren von den Kegelschnitten" was also added from 1892 in the Prussian curriculum for the highest class of the *Gymnasium*; this requirement was immediately followed by the curricula of Hesse and Saxony.[129]

The textbooks reflected very well the narrow place occupied by graphical representations within the late 19th century school curriculum. Most textbooks only included an *isolated* chapter or section on the graphical representation of a function or the geometrical locus of an equation.[130] Because their purpose was only that of introducing some notions of analytical geometry or calculus, such a chapter or section was often placed at the end of the book or in an annex; sometimes such material could even be published as a very short pamphlet. Ernst Bardey's *Arithmetische Aufgaben* (1881), a textbook written not for the general education of a *Gymnasium* but rather "für Realschulen zweiter Ordnung, Gewerbeschulen und höhere Bürgerschulen", stated that "no pupil should be released from these schools who does not know what a graphic representation is".[131] However, Bardey's textbook for the secondary schools (*höheren Schulen* which included "Gymnasien, Realgymnasien und Oberrealschulen"), *Methodisch geordnete Aufgabensammlung* (1st ed. 1871), did not include any graphical representations until the 11th edition from 1883.[132] In both textbooks, the section on graphical representations was the very last section of the book. In the early 1900s, Felix Klein criticized such approaches which only made a limited use of functions as a special topic. Schimmack, one

128. *Verordnungsblatt für den Dienstbereich des Ministeriums für Cultus und Unterricht* (Wien: Ministerium für Cultus und Unterricht, 1884), 233-234.

129. See Schimmack, *Die Entwicklung der mathematischen Unterrichtsreform in Deutschland*, 27. Several short pamphlets on "Der Koordinatenbegriff und einige Grundlehren von den Kegelschnitten" were published by Albrecht Emmerich (1893), Wilhelm Krimphoff (1893), Ignaz Praetorius (1894), Heinrich Schotten (1895), or Karl Koppe (1897). Schotten would later play an important role in the "Meran reform", see Krüger, *Erziehung Zum Funktionalen Denken*, 156-158. "Der Koordinatenbegriff" was part of the "allgemeines Lehrziel" for the *Gymnasium* in the 1902 Prussian curriculum, see *Lehrpläne und lehraufgaben für die höheren Schulen in Preussen von 1901* (Halle: Buchhandlung des Waisenhauses, 1913), 52, 54, 59.

130. J. E. Oliver, L. A. Wait, and G. W. Jones, *A Treatise on Algebra* (Ithaca: D. F. Finch, 1887), 181-193.

131. Ernst Bardey, *Arithmetische Aufgaben* (Leipzig: Teubner, 1881), 4.

132. Schimmack, *Die Entwicklung der mathematischen Unterrichtsreform in Deutschland*, 20.

of Klein's close associates on education reform, considered that the presentation of the function concept in older textbooks was as good as nothing.[133] The only book that made extensive use of the concept of function was A. Schülke's *Aufgaben-Sammlung aus der Arithmetik, Geometrie, Trigonometrie und Stereometrie* (1902), which however, was not used until 1906 in any Prussian classrooms.[134]

A similar trend was to be found in the US. The popular *Elementary Algebra for Schools* (1st ed. 1885) and *Higher Algebra* (1st. ed 1887) by H.S. Hall and S.R. Knight, both former scholars at Cambridge, or *A Treatise on Algebra* (1st ed. 1888, aimed for higher classes of the schools and junior students in the universities) by Charles Smith, master of Sidney Sussex College in Cambridge, did not contain any discussion of graphical representations until the end of the 19th century. When Smith added a chapter on this topic in the fifth edition from 1896, it was inserted as an off-sequence "Chapter X*" following "Chapter X" on "Simultaneous Equations"; the chapter employed its own pagination not to offset the rest of the chapters (see Fig. 7.1). After the examination curricula changed in 1902, Hall hurried to publish a booklet, *A Short Introduction to Graphical Algebra* (1902), that was later included as the last chapter in his algebra textbook which was now advertised as *Elementary algebra for schools containing a full treatment of graphs* (1907). In the US, James Taylor's *College Algebra* included in 1889 a final chapter on the "Graphic Solutions of Equations and of Systems".

A similar pattern can be identified if one examines George Albert Wentworth's textbooks on algebra. Initially, the graphical representations of functions was only discussed in Wentworth's college textbooks but no diagrams were included in his high school textbooks.[135] Only the *New School Algebra* (1898) had a special chapter on "Graphs". The chapter, positioned at the end of the textbook, had been included "at the request of many teachers and superintendents"

133. Schimmack, *Die Entwicklung der mathematischen Unterrichtsreform in Deutschland*, 35.
134. Ibid.
135. Such a discussion was absent from the high school edition of *Elements of Algebra* (1881) or *School Algebra* (1890).

Figure 7.1
A special "Chapter X*" on the "graphical representation of functions" was added off-sequence after the Chapter X on "Simultaneous Equations". The chapter employed its own pagination not to offset the rest of the chapters. Source: Charles Smith, *A Treatise on Algebra* (London: Macmillan, 1896)

because:

> Many colleges now require for entrance examination a very elementary knowledge of the rudiments of the subject of Graphs. It is the opinion of many good teachers that an insight into Graphs is of considerable value to the pupil in finding the roots of equations, especially equations of the second degree and of degree higher than the second. All agree that the study of Graphs tends to stimulate the interest of the pupil in the work of finding the roots of equations.[136]

While Wentworth's first college textbook on algebra from 1881 included a chapter on the "loqi of equations", the next reiteration from 1888 referred to "the graphical representations of functions" or "the graph of a function" (see Figs. 7.2 and 7.3).[137] The banal shift in terminology underlines a more profound redefinition of epistemological and disciplinary objects. While the "geometrical locus of an equation" was the standard term of analytical geometry, "the graphical representation of a function" was a new catchphrase (or *Schlagwort*) that was common to both mathematical analysis and experimental science. Wentworth clearly struggled to move from the intuitive concept of "line" to the general concept of "*curve*", and then

136. George Albert Wentworth, *New School Algebra* (Boston: Ginn, 1898), 408.
137. George Albert Wentworth, *Elements of Algebra* (Boston: Ginn, 1881), 447; George Albert Wentworth, *A College Algebra* (Boston: Ginn, 1888), 401. *Elements of Algebra* came out in two editions – a short edition for high school and academies, and a "complete edition" for colleges; only the latter edition included the chapter on "loqi of equations".

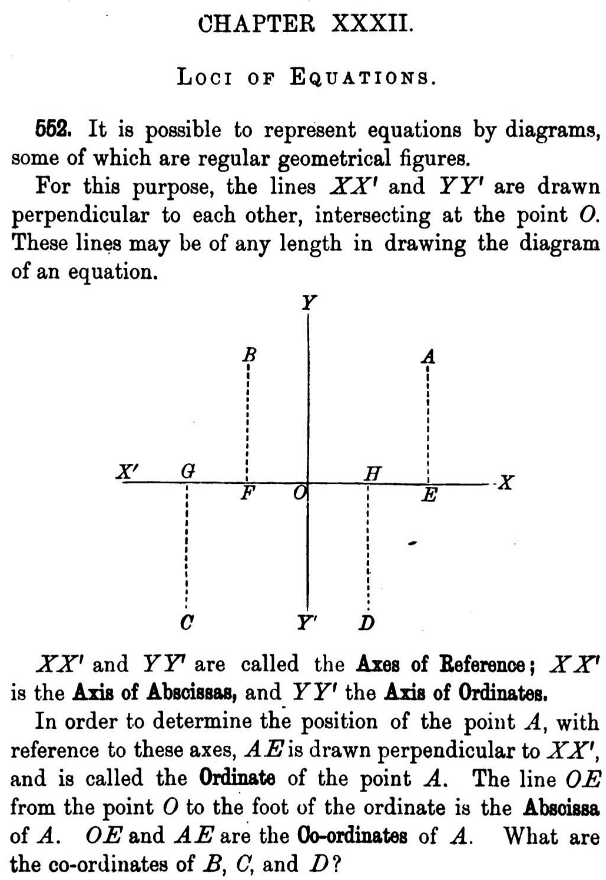
to the technical terms "**graph**" and "**locus**" (these gradations were also embodied in the font style):

> If different values of x be laid off as abscissas, and the corresponding values of $f(x)$ as ordinates, the points thus obtained will all lie on a line; this line will generally be a curved line, or, as it is briefly called, a *curve*. This curve is called the **graph** of the function $f(x)$; it is also called the **locus** of the equation $y = f(x)$.[138]

While the 1888 edition of Wentworth's *College Algebra* only changed the terminology, the high school algebra textbook of 1898 also employed a different visual language. Now, the graph was not introduced as a purely geometrical object (or figure) that was to be geometrically constructed. The chapter on graphs opened not with general definitions and the construction of the axes (as in the previous editions), but rather with a reminder and a motivation: "Diagrams, called graphs, are often used to show in a concise manner variations in temperature, in population, in prices etc. etc."[139] Graphs were considered to be either familiar to many of the students, or intuitive enough to be first introduced by the concrete example of the changes in temperature for a day (see Fig. 7.4). Only afterwards, did the general definitions and constructions followed. Though the general construction was very similar to that of the previous editions, now the paper was also rendered visible (see Fig. 7.5). This was a conscious choice used in all the diagrams. The utility of the squared paper was underlined in the chapter: "in plotting points and graphs the student will find coordinate paper of much help in giving accuracy and in saving time."[140] We should not be mislead by this point. The squared paper was not simply more useful, but it was more useful for its new purpose. Now students were required to plot more, and more precisely.

As illustrated by the analysis of Wentworth's textbooks, in the US, the use of plots in the teaching of algebra was first introduced in higher education to only later be extended to secondary school education. This trend is somewhat surprising because one would commonly

138. Wentworth, *A College Algebra*, 402.
139. Wentworth, *New School Algebra*, 409.
140. Ibid., 413.



CHAPTER XXXII.

LOCI OF EQUATIONS.

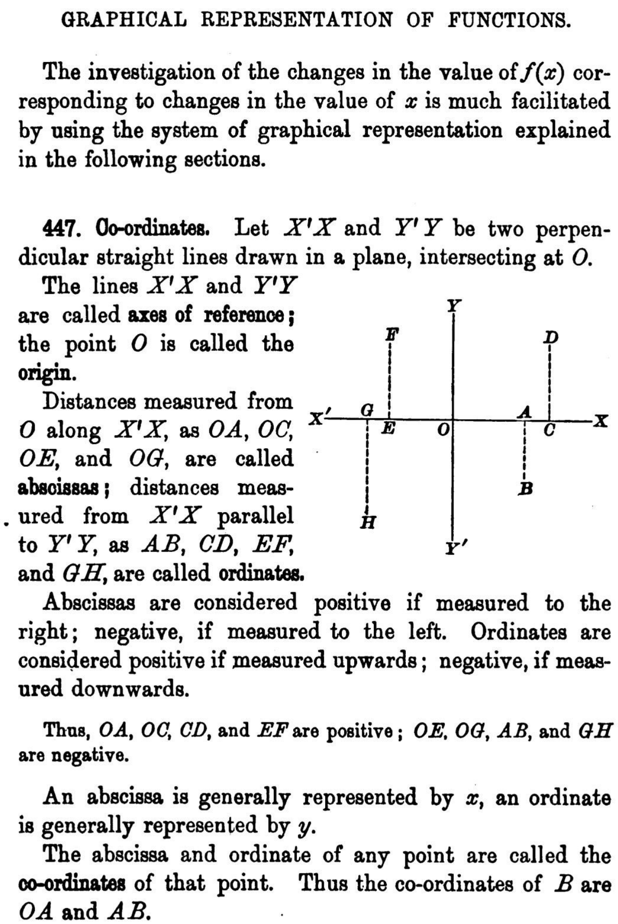
552. It is possible to represent equations by diagrams, some of which are regular geometrical figures.

For this purpose, the lines XX' and YY' are drawn perpendicular to each other, intersecting at the point O. These lines may be of any length in drawing the diagram of an equation.

XX' and YY' are called the **Axes of Reference**; XX' is the **Axis of Abscissas**, and YY' the **Axis of Ordinates.**

In order to determine the position of the point A, with reference to these axes, AE is drawn perpendicular to XX', and is called the **Ordinate** of the point A. The line OE from the point O to the foot of the ordinate is the **Abscissa** of A. OE and AE are the **Co-ordinates** of A. What are the co-ordinates of B, C, and D?

(a) Wentworth 1881



GRAPHICAL REPRESENTATION OF FUNCTIONS.

The investigation of the changes in the value of $f(x)$ corresponding to changes in the value of x is much facilitated by using the system of graphical representation explained in the following sections.

447. Co-ordinates. Let $X'X$ and $Y'Y$ be two perpendicular straight lines drawn in a plane, intersecting at O.

The lines $X'X$ and $Y'Y$ are called **axes of reference**; the point O is called the **origin.**

Distances measured from O along $X'X$, as OA, OC, OE, and OG, are called **abscissas**; distances measured from $X'X$ parallel to $Y'Y$, as AB, CD, EF, and GH, are called **ordinates.**

Abscissas are considered positive if measured to the right; negative, if measured to the left. Ordinates are considered positive if measured upwards; negative, if measured downwards.

Thus, OA, OC, CD, and EF are positive; OE, OG, AB, and GH are negative.

An abscissa is generally represented by x, an ordinate is generally represented by y.

The abscissa and ordinate of any point are called the **co-ordinates** of that point. Thus the co-ordinates of B are OA and AB.

(b) Wentworth 1888

Figure 7.2

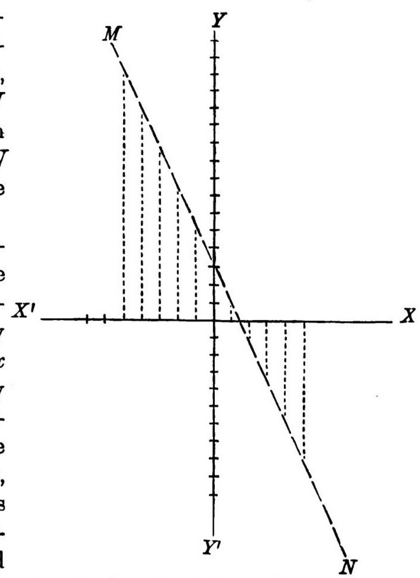
While the 1881 edition described the representation of equations by diagrams as the "loci of equations" (a term specific to analytical geometry), the 1888 edition referred to the "graphical representation of functions" (a modern term which connected to both calculus and scientific graphs). Source: (a) George Albert Wentworth, *Elements of Algebra* (Boston: Ginn, 1881), 447; (b) George Albert Wentworth, *A College Algebra* (Boston: Ginn, 1888), 401

LOCI OF EQUATIONS. 449

554. The equation $2x+y=3$ may be satisfied by an infinite number of corresponding values of x and y. By changing the equation to the form $y=3-2x$, the following table is readily computed:

If $x=1$,	$y=1$.	If $x=-1$,	$y=5$.
" $x=2$,	$y=-1$.	" $x=-2$,	$y=7$.
" $x=3$,	$y=-3$.	" $x=-3$,	$y=9$.
" $x=4$,	$y=-5$.	" $x=-4$,	$y=11$.
" $x=5$,	$y=-7$.	" $x=-5$,	$y=13$.

This table contains the co-ordinates of ten points. If the points are located with reference to the axes, and the line *MN* is drawn through them, the line *MN* is the **Locus** of the given equation.



If *MN* be prolonged, the value of y which corresponds to any given value of x may be found by laying off an abscissa equal to the given value of x, erecting at its extremity an ordinate terminated by *MN*, and measuring the length of the ordinate.

If $x=0$, the diagram shows that $y=3$; and if $x=1.5$, $y=0$. **If $x=-3.5$, what is the value of y?**

(a) Wentworth 1881

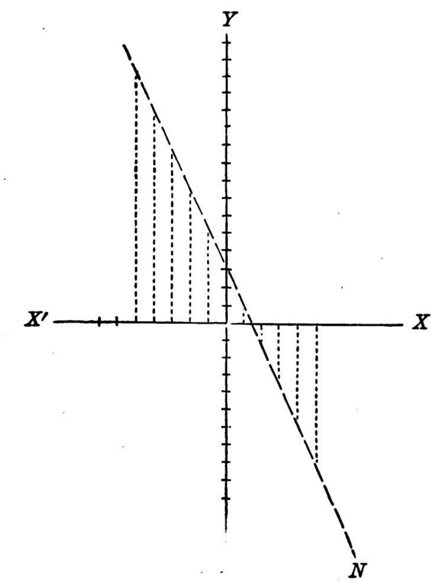
GRAPHICAL REPRESENTATION OF FUNCTIONS. 403

(1) Construct the graph of $3-2x$.

Put $y=3-2x$. The following table is readily computed:

If $x=1$,	$y=1$.	If $x=-1$,	$y=5$.
" $x=2$,	$y=-1$.	" $x=-2$,	$y=7$.
" $x=3$,	$y=-3$.	" $x=-3$,	$y=9$.
" $x=4$,	$y=-5$.	" $x=-4$,	$y=11$.
" $x=5$,	$y=-7$.	" $x=-5$,	$y=13$.

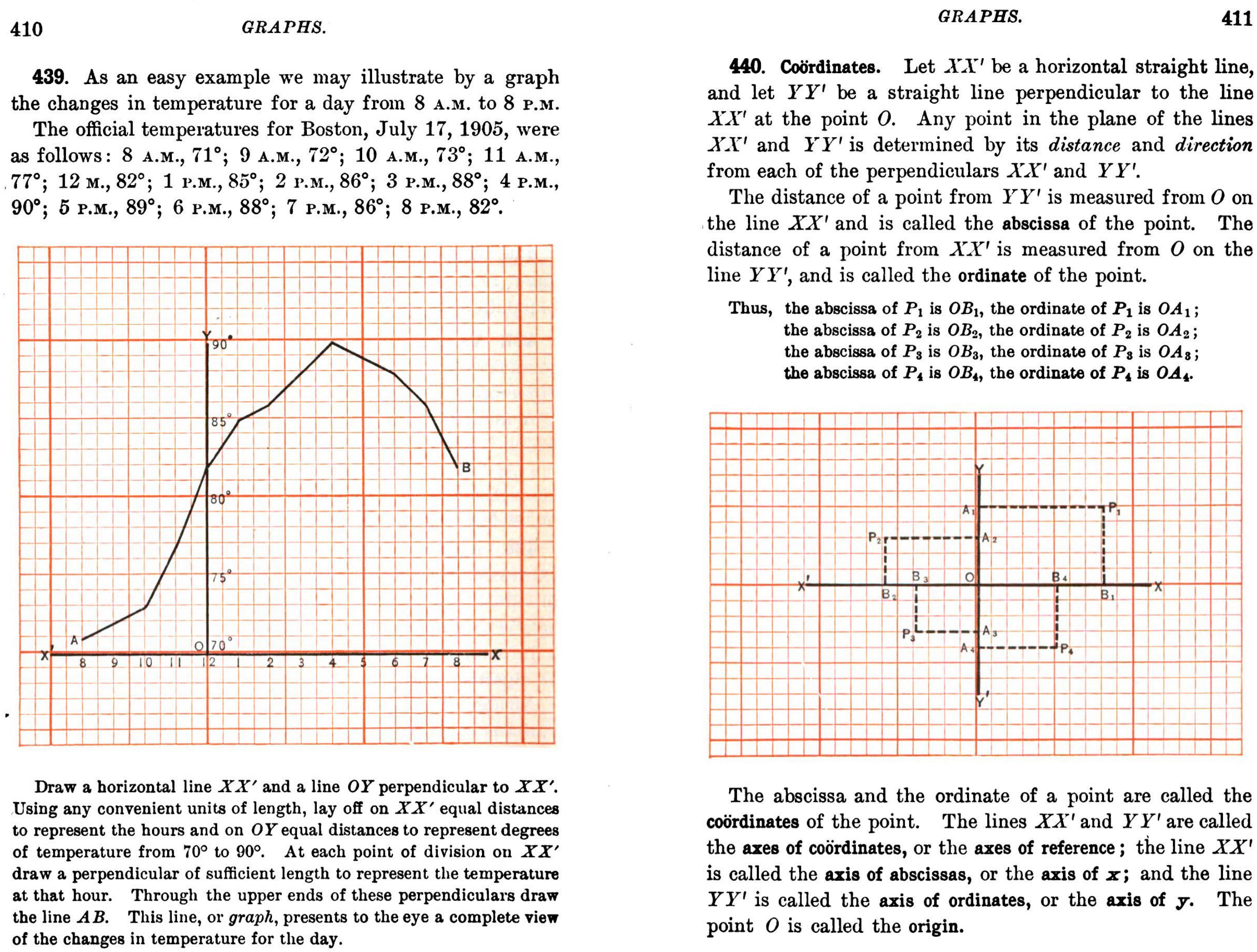
Constructing the above points, it appears that the graph of the function $3-2x$ is the straight line *MN*.



In general, where the equation $y=f(x)$ contains only the first powers of x and y, the locus will be a straight line.

(b) Wentworth 1888

Figure 7.3
In the 1881 edition the line MN was described as the "locus of the equation", while in the 1888 edition the line was described as the "graph of the function".Source: (a) George Albert Wentworth, *Elements of Algebra* (Boston: Ginn, 1881), 449; (b) George Albert Wentworth, *A College Algebra* (Boston: Ginn, 1888), 403



410 GRAPHS.

439. As an easy example we may illustrate by a graph the changes in temperature for a day from 8 A.M. to 8 P.M.

The official temperatures for Boston, July 17, 1905, were as follows: 8 A.M., 71°; 9 A.M., 72°; 10 A.M., 73°; 11 A.M., 77°; 12 M., 82°; 1 P.M., 85°; 2 P.M., 86°; 3 P.M., 88°; 4 P.M., 90°; 5 P.M., 89°; 6 P.M., 88°; 7 P.M., 86°; 8 P.M., 82°.

Draw a horizontal line XX' and a line OY perpendicular to XX'. Using any convenient units of length, lay off on XX' equal distances to represent the hours and on OY equal distances to represent degrees of temperature from 70° to 90°. At each point of division on XX' draw a perpendicular of sufficient length to represent the temperature at that hour. Through the upper ends of these perpendiculars draw the line AB. This line, or *graph*, presents to the eye a complete view of the changes in temperature for the day.

GRAPHS. 411

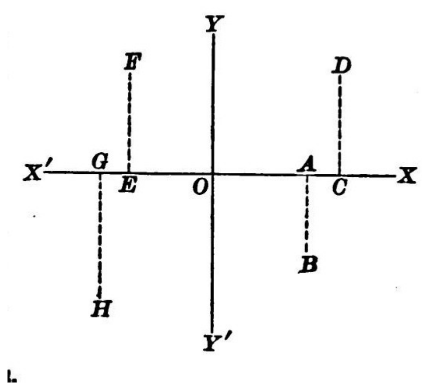
440. Coördinates. Let XX' be a horizontal straight line, and let YY' be a straight line perpendicular to the line XX' at the point O. Any point in the plane of the lines XX' and YY' is determined by its *distance* and *direction* from each of the perpendiculars XX' and YY'.

The distance of a point from YY' is measured from O on the line XX' and is called the **abscissa** of the point. The distance of a point from XX' is measured from O on the line YY', and is called the **ordinate** of the point.

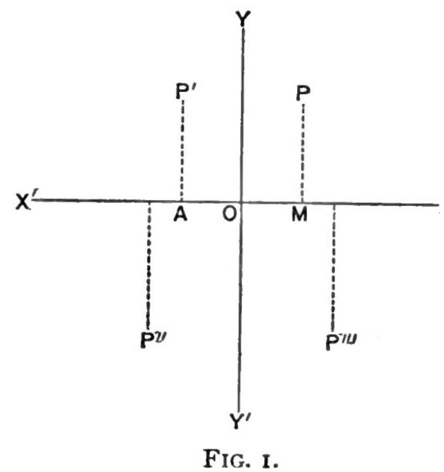
Thus, the abscissa of P_1 is OB_1, the ordinate of P_1 is OA_1;
the abscissa of P_2 is OB_2, the ordinate of P_2 is OA_2;
the abscissa of P_3 is OB_3, the ordinate of P_3 is OA_3;
the abscissa of P_4 is OB_4, the ordinate of P_4 is OA_4.

The abscissa and the ordinate of a point are called the **coördinates** of the point. The lines XX' and YY' are called the **axes of coördinates,** or the **axes of reference;** the line XX' is called the **axis of abscissas,** or the **axis of x;** and the line YY' is called the **axis of ordinates,** or the **axis of y.** The point O is called the **origin.**

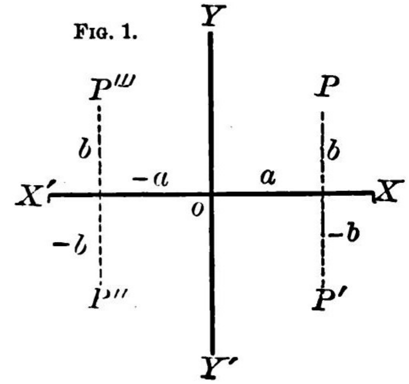
Figure 7.4
In the new edition of Wentworth's high school algebra, *graphs* were first introduced through a concrete example such as the changes of temperature for a day; only afterwards, did the general definitions and constructions followed. In this edition, the diagrams also represented the squared paper on which the students were supposed to draw their plots. Cf. Fig. 7.3. Source: George Albert Wentworth, *New School Algebra* (Boston: Ginn, 1898), 410-411



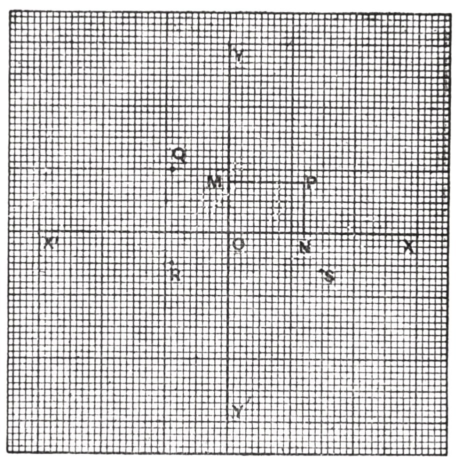
(a) Wentworth 1881



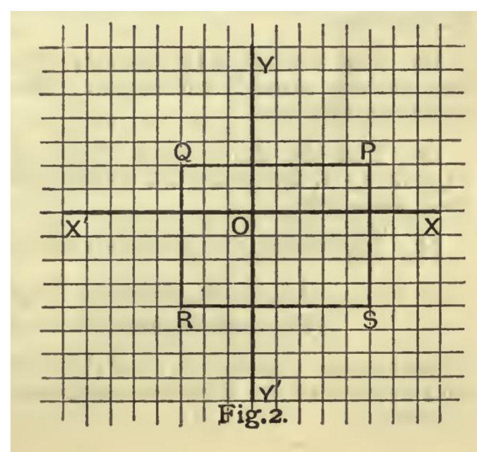
(b) Taylor 1895



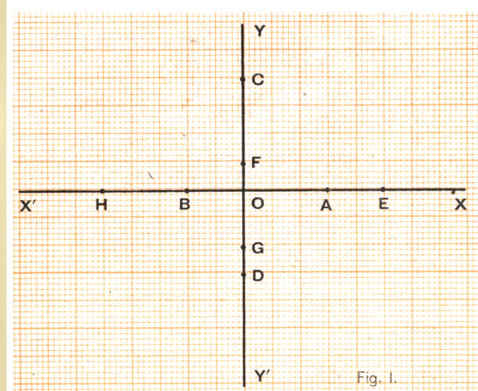
(c) Hall & Knight 1895



(d) Smith 1896



(e) Hall 1903



(f) Morgan 1904

Figure 7.5

The visual shift from geometrical illustration to graphical representations. Source: (a) George Albert Wentworth, *A College Algebra* (Boston: Ginn, 1888), 401; (b) James M. Taylor, *A College Algebra* (Boston: Allyn & Bacon, 1895), 347; (c) Henry Sinclair Hall and Samuel Ratcliffe Knight, *Elementary Algebra* (Macmillan and Company, 1895), 444; (d) Charles Smith, *A Treatise on Algebra* (London: Macmillan, 1896), 172b; (e) Henry Sinclair Hall, *A Short Introduction to Graphical Algebra* (London Macmillan, 1903), 3; (f) Robert Burns Morgan, *Elementary Graphs* (London: Blackie & Son, 1904), pl. 1

expect higher mathematics to be increasingly abstract, and thus intuitive means such as diagrams to be increasingly scarce. Several factors contributed to this unusual trend. First the concept of coordinate geometry was usually taught in conjunction with analytical geometry, an advanced topic in both the US and Europe. Second, the wide use of graphical representations within scientific practice provided the most important incentive to teach students how to construct graphs. Third, the emphasis on graphical representations was part of a new mathematical trend which focused on functions rather than equations.

The pattern for such texts seems to have been set by "a novel and successful treatise" on *Graphic Algebra or Geometrical Interpretation of the Theory of Equations of one unknown quantity* (1882) written by A. W. Phillips and W. Beebe, assistant professors of mathematics at Yale.[141] Though the book was "based on some years' experience in teaching the Theory of Equations to the Freshman Class in Yale College", Phillips and Beebe chose not to write a textbook on the theory of equations with a special chapter on graphical representations. Instead, all the topics of the book were discussed and illustrated through graphical representations. The appeal of this approach went beyond narrow curricular interests. Graphical methods, which "have been used with great profit in many departments of science", were now used to "give a clear notion of the geometrical meaning of the theorems and operations of the theory of equations", and also to furnish a suitable introduction to analytical geometry and calculus.[142] However, opposed to regular treatises on analytical geometry, Phillips and Beebe introduced their graphs not as geometrical constructions but rather as concrete constructions (on squared paper) based on empirical measurements.

A similar approach was followed in the UK by George Chrystal, the professor of mathematics at the University of Edinburgh, who published an *Introduction to algebra* (1898) for the

141. The description of the textbook is from Florian Cajori, *The Teaching and History of Mathematics in the United States* (Washington: U.S. Government Printing Office, 1890), 157.
142. A. W. Phillips and W. Beebe, *Graphic Algebra, Or Geometrical Interpretation of the Theory of Equations of One Unknown Quantity* (New York: H. Holt and Company, 1882), 2.

use of secondary schools and technical colleges. In the "Preface", Chrystal pointed out that his textbook made "constant use of graphical illustrations" to which about fifty pages were exclusively dedicated:

> This proportion may startle some; but will not astonish those who are familiar with the tendency of the best modern teaching. The graphic method furnishes the most valuable antidote to the tendency of school algebra to degenerate into puzzle-solving and legerdemain. By the constant exercise of graph-tracing the beginner acquires through his fingers three fundamental mathematical notions, viz. the Idea of a Continuously Varying Function, the Conception of a Limit, and the Method of Successive Approximation. These notions he will find to be more valuable in the higher mathematics and in applications to practice than all the rest of his algebraic accomplishments put together.[143]

However, to get "the full educative benefit of graph-tracing", the sole reading of the book was not enough!

> The teacher must trace the graphs before his pupils; and also cause them to work the curves out independently. To facilitate this kind of work, I strongly recommend that a blackboard, permanently ruled into small squares, like a sheet of plotting paper, should be part of the furniture of every mathematical class-room.[144]

When working out the chapter on rational function, the student was supposed to "trace out all the fundamental curves for himself, using either plotting paper or square and scale. In no other way can he arrive at that kind of 'lively conviction' which will enable him to discuss new cases for himself without hesitation or liability to error".[145] And again, "the proper way to acquire skill and conviction" was by drawing a "considerable variety of graphs to scale by means of plotting paper".[146]

By the end of the 19th century, a whole array of short textbooks (or pamphlets) were dedicated exclusively to "graphs" and "graphic algebra" (see Table 7.1). This new topic was started in the US by college instructors such as Arthur L. Baker, a professor of mathematics

143. George Chrystal, *Introduction to Algebra: For the Use of Secondary Schools and Technical Colleges* (A & C Black, 1898), x.
144. Ibid., x-xi.
145. Ibid., 365.
146. Ibid., 393.

at the University of Rochester, who published a *Graphic Algebra. Graphs* (1892), "an elementary textbook for college students" based on "the notes used in the author's class room from time to time";[147] or F.E. Nipher, a professor of physics and electrical engineering at Washington University, who published an *Introduction to Graphical Algebra* (1898) for the use of high schools. In the UK such textbooks appeared in the wake of the 1902 reform and were aimed at secondary schools. A notable exception was *Plotting, or, Graphic mathematics* (1888) by Richard Wormell, a prolific author of of mathematics and physics textbooks and "head master of the city of London middle class schools". Though the book was considered to be a "new departure" from standard presentations of analytical geometry, it was seen as an insufficient introduction of the topic.[148] An unpersuaded reviewer pointed out that Wormell's textbook could not serve "any but the science student whose sole wish in acquiring co-ordinate geometry". But even this purpose was somewhat inappropriate because a book need not have been devoted "to so small an object which an hour's talking by a good teacher might have attained for better". Finally, even the title of the book "Graphic Mathematics" was seen to be a "a too high-sounding title, inasmuch as it must lead anyone who knows anything about the subject to anticipate some discussion, at all events, of Graphic Statics and other methods, which he will certainly fail to find".[149]

The reaction to Wormell's textbook comes to show that "graphs" and "graphic algebra" were not yet an accepted topic before the 1900s reforms. Somewhat paradoxically, the systematic teaching of analytical geometry or calculus often suppressed the diagrams that could have been found in the introductory chapters. The shift that took place is particularly visible if we compare the first two editions of Jules Tannery's *Introduction à la théorie des fonctions d'une variable* (1st ed. 1886, 2nd ed. 1904-1910). While in the first edition Tannery abstained "de tout langage et de toute figure géométriques", in the second edition he took advantage

147. Arthur Latham Baker, *Graphic Algebra: An Elementary Text Book for College Students. Graphs* (Scrantom, Wetmore & co., 1892), iii.
148. *The Practical Teacher* 8 (1889): 419.
149. Ibid.

Table 7.1
The earliest British and American textbooks and pamphlets dedicated exclusively to graphs and graphic algebra.

1875	*A Graphic Method for Solving Certain Algebraic Problems*	George L. Vose	US
1882	*Graphic Algebra or Geometrical Interpretation of the Theory of Equations of one unknown quantity*	A. W. Phillips and W. Beebe	US
1888	*Plotting, or, Graphic mathematics*	Richard Wormell	UK
1892	*Graphic algebra: An elementary text book for college students. Graphs*	Arthur L. Baker	US
1898	*An Introduction to Graphical Algebra: For the Use of High Schools*	F. E. Nipher	US
1902	*Graphs*	Robert J. Aley	US
1902	*A Short Introduction to Graphical Algebra*	H. S. Hall	UK
1903	*Graphs or the graphical representation of algebraic functions*	C. H. French and G. Osborn	UK
1904	*Graphs and Imaginaries*	J. G. Hamilton and F. Kettle	UK
1904	*Elementary Graphs*	R. B. Morgan	UK
1904	*An Introduction to the Calculus, Based on Graphical Methods*	G. A. Gibson	UK
1905	*An Elementary Treatise on Graphs*	G. A. Gibson	UK
1905	*Graphic Algebra for Elementary and Intermediate Students*	John Lightfoot	UK
1905	*Graphic Algebra for Secondary Schools*	H. B. Newson	US
1908	*Graphic Algebra*	A. Schultze	UK

"des facilités qu'offre le langage géométrique". Tannery attributed his "timidity" in employing diagrams to a fear that "if the reader saw a figure would not be well persuaded that this figure was only an aid and did not conceal some hole impossible to fill only with the resources of logic."[150] Tannery's *Notions de mathématiques* (1903), an introductory textbook for the class of philosophy published after the 1902 reform, included over 170 figures with an introduction to the use of "papier millimétrique", the plotting of empirical curves such as temperature curves, and the "graphique des chemins de fer". The graphical representation of functions and the "courbes empiriques" of automatic registers were also covered in Tannery's *Leçons d'algèbre et d'analyse* (1906).

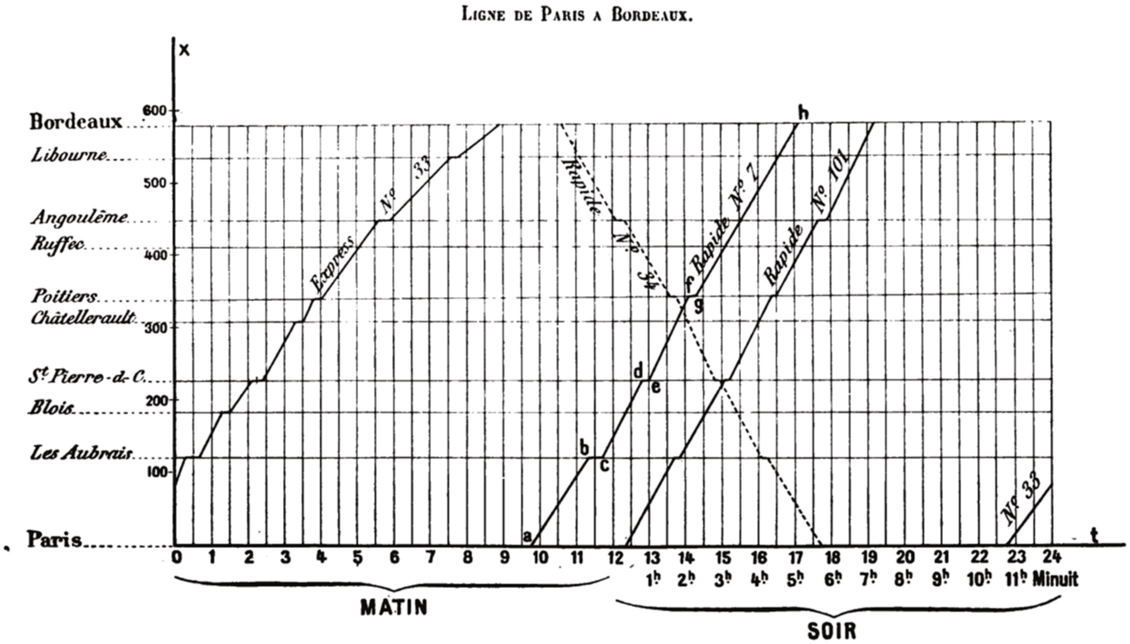
One graphical representation in particular became *paradigmatic* in the study of algebra because it perfectly embodied the spirit of the new reforms: "the graphic railroad time-table", or "le graphique des chemins de fer", or "graphische Eisenbahnfahrpläne". This mode of rep-

150. Jules Tannery, "Introduction à La Théorie Des Fonctions d'une Variable," 1904, vol. 1, vi.

resentation was first developed by the French engineer Ibry in 1840.[151] Though eminently a practical object mainly encountered in rail-road offices, the graphic time-table was also a pedagogical tool for some engineers. For example, George L. Vose, a professor of engineering at Bowdoin College and the author of a *Manual for railroad engineers* (1873), published in 1875 "A Graphic Method for Solving Certain Algebraic Problems" which relied on graphical railroad time-tables.[152]

This graphical construction was specifically required by the 1902 French curriculum to be studied in the "classe de philosophie".[153] All the main French textbooks published after the reform (Tannery's *Notions de mathématiques*, Borel's *Algèbre* (1903), Bourlet's *Éléments d'algèbre* (1904), Laisant's *Initiation mathématique* (1906), etc.) included such a diagram. Felix Klein, who praised these textbooks as models to be followed by the German reformers, also included his own "graphischer Eisenbahnfahrplan" for the line Northeim-Göttingen-Münden for the Winter 1906-1907 (see Fig. 7.6).[154] Such diagrams were readily included in the German textbooks which followed the Meraner program, such as Behrendsen/Götting's *Lehrbuch der mathematik nach modernen grundsätzen* (1909) or Schwab/Lesser's *Mathematisches Unterrichtswerk* (1909).[155] The representation was less common in elementary algebra books written in English.[156] However, some teachers such as the American mathematician Florian Cajori strongly advocated for the use of such a "real applied problem" and provided a local timetable chart taken from the *American Railway*.[157] Opposed to other graphical representations, the graphic railroad time-tables were quite unique because the plots were given for real trains.

151. Funkhouser, "Historical Development of the Graphical Representation of Statistical Data," 308.
152. The method was first published as an article in Van Nostrand's Engineering Magazine, and republished the same year as a separate pamphlet.
153. *Plan d'études et programmes d'enseignement dans les lycées et collèges de garçons arrêtés du 31 mai 1902*, 164.
154. Klein and Schimmack, *Vorträge über den mathematischen unterricht an den höheren schulen*, 34-36.
155. See also Krüger, *Erziehung Zum Funktionalen Denken*, 180-1.
156. Though the "the graphic railroad time-table" was not as visible in the English elementary algebra textbooks, it could still be found sometimes as an exercise which required students to construct a diagram from a table, see W.G. Borchardt's *Elementary Algebra* (1905).
157. Florian Cajori, "Graphic Railroad Time-Tables," *School Science and Mathematics* 10, no. 3 (1910): 204–205. In a reply to Cajori, Beman pointed out the French textbooks in which one could encounter such diagrams, but no English textbooks were mentioned, see beman1910.



(a) Bourlet 1904

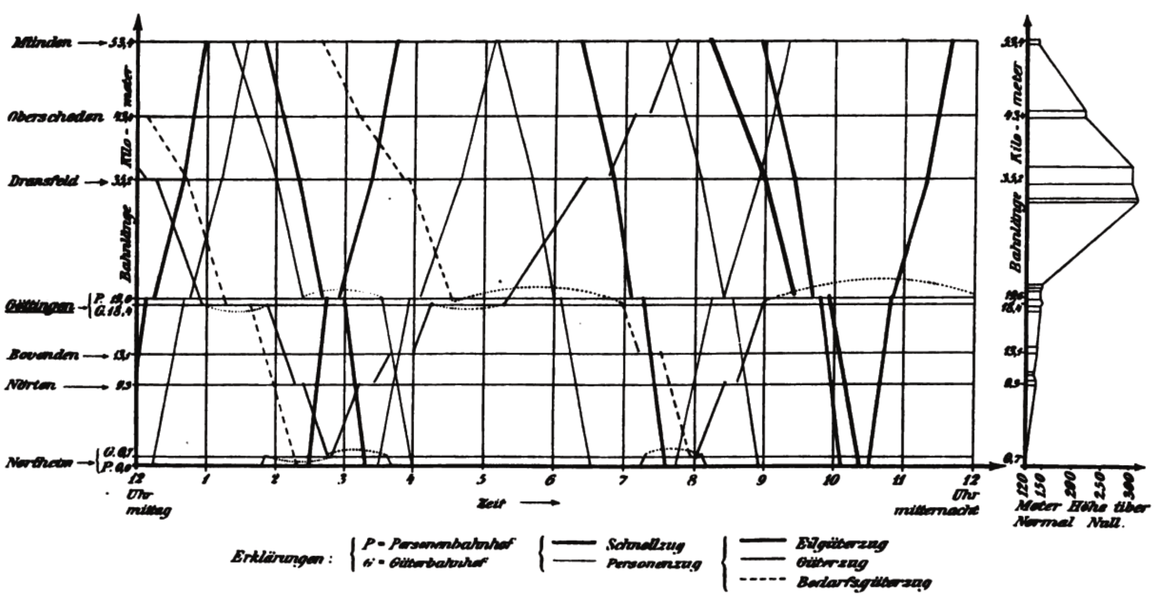


Fig. 8. Graphischer Eisenbahnfahrplan für die Strecke Northeim—Göttingen—Dransfeld—Münden; Winter 1906/07.

(b) Klein & Schimmack 1907

Figure 7.6
"Le graphique des chemins de fer" and "der graphische Eisenbahnfahrplan". Source: (a) C. Bourlet, *Éléments d'algèbre* (Paris: Hachette, 1904), 176; (b) Felix Klein and Rudolf Schimmack, *Vorträge über den mathematischen unterricht an den höheren schulen* (B. G. Teubner, 1907), 34-36

Laisant provided a chart for the beginning of the year 1905 for the *rapide* 1 between Paris and Marseille, and the *rapide* 16 between Marseille and Paris; Bourlet chose the *rapide* 7 and 101 and the *express* 33 between Paris and Bordeaux; Klein chose to compare the *Schnellzüge*, *Personenzüge* and *Güterzüge* for the line Northeim-Göttingen-Münden for the Winter 1906-1907; Behrendsen and Götting chose the line Göttingen-Bebra for the Winter 1907-1908.[158]

3.2 Child's Play

As discussed above, the early 1900s graphical revolution depended on an essential rhetorical move: graphical methods were not merely practical (which would have made them ideal for technical and engineering schools but unsuited for a general or liberal education), but pedagogical: they kept students interested and allowed them to make discoveries on their own. Henry Armstrong, the professor of chemistry at Finsbury College and a colleague of John Perry, was the main proponent of the "heuristic method" in education.[159] Armstrong believed that a child should learn through self-discovery, instead of being passively taught something. Knowledge could only be gained through personal observation and experimentation.

Many of Armstrong's pedagogical ideas were supported by his own teaching experience at Finsbury College. However, in the late 1890s, Armstrong carried his own pedagogical experiments on three of his children – Harold, Nora and Robbin – who were 7, 10 and 12 years old (see Fig. 7.7).[160] Armstrong tried to motivate his children to undertake their own scientific investigations. He started by reading them a children's story, *The Monkey that would not Kill*,

158. Charles-Ange Laisant, *Initiation mathématique* (Paris: Hachette, 1906), 122; 34-36 C. Bourlet, *Éléments d'algèbre* (Paris: Hachette, 1904), 176; Klein and Schimmack, *Vorträge über den mathematischen unterricht an den höheren schulen*, 34-36; D. Behrendsen and E. Götting, *Lehrbuch der mathematik nach modernen grundsätzen* (Leipzig und Berlin: Teubner, 1911), 218.

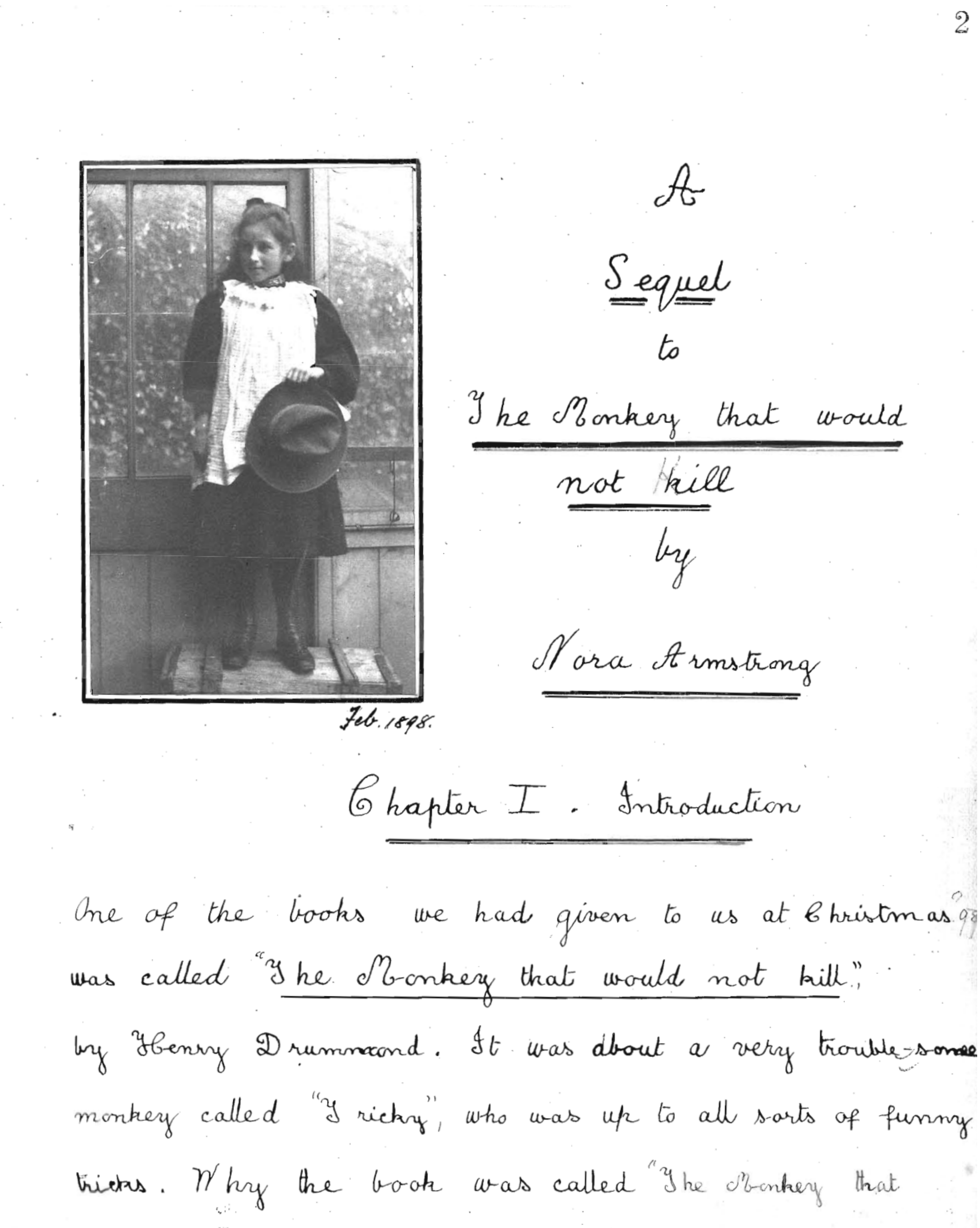
159. Henry Edward Armstrong, *H. E. Armstrong and Science Education : Selections from The Teaching of Scientific Method and Other Papers on Education*, ed. G. Van Praagh (London: Murray, 1973), See; Henry Edward Armstrong, *H.E. Armstrong and the Teaching of Science, 1880-1930*, ed. William H. Brock (London: Cambridge University Press, 1973).

160. Armstrong's investigations are described in Henry Edward Armstrong, *The Teaching of Scientific Method and Other Papers on Education*, 2d (London: Macmillan and co., 1910), 393-399

Figure 7.7
Armstrong's children: Harold (7), Nora (10), Robbin (12). Source: Nora's Notebook in the Papers of H.E. Armstrong, Archives Imperial College London.

where a monkey was thrown into the sea, tied to a stone which he could not lift; while under water the monkey was able to lift the stone and walk to the shore, because the stone was lighter in the water than in air (see Fig. 7.8). Instead of satisfying their curiosity, Armstrong gave his children a weight balance and guided their experiments. Each of the children kept a notebook, where they had to thoroughly describe all their experiments, and the insights and conclusions they would draw from them. These notebooks, carefully revised by the father, combined the style of a personal diary with that of a laboratory report.

Armstrong's daugher, Nora, studied the relative weight of different materials in water. At



2

A

Sequel

to

The Monkey that would not kill

by

Nora Armstrong

Feb. 1898.

Chapter I. Introduction

One of the books we had given to us at Christmas 98 was called "The Monkey that would not kill", by Henry Drummond. It was about a very trouble-some monkey called "Tricky", who was up to all sorts of funny tricks. Why the book was called "The Monkey that

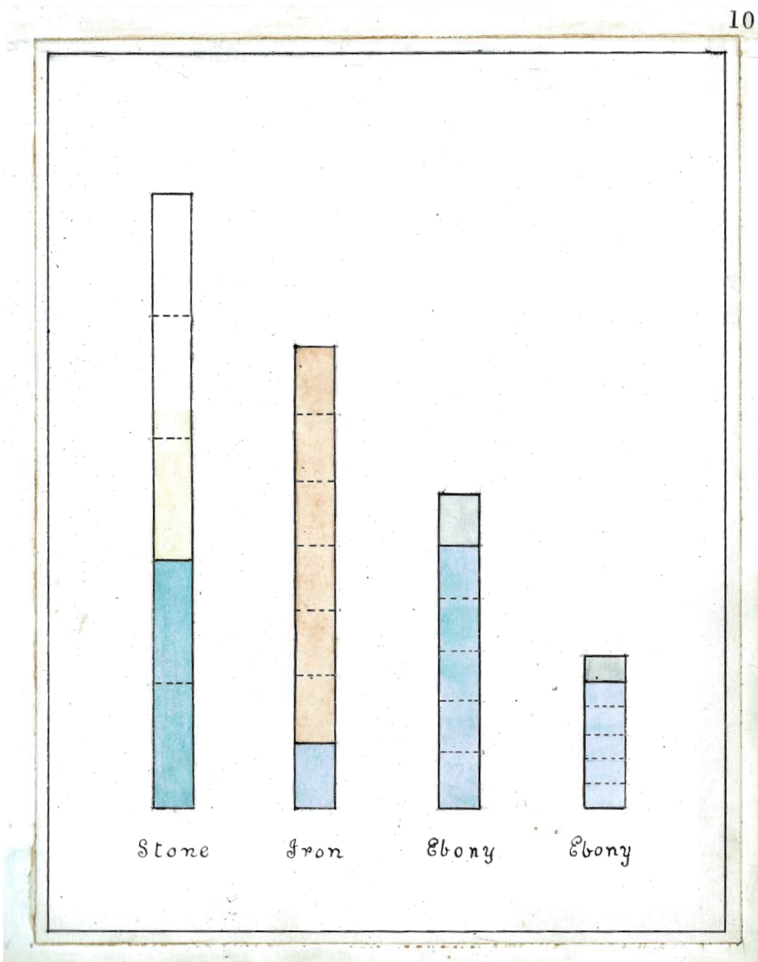
Figure 7.8
The opening page of Nora's diary. Source: Source: Nora's Notebook in the Papers of H.E. Armstrong, Archives Imperial College London.

"father's suggestion I have represented the four things weighed" because her conclusions were "better shown by the picture" (see Fig. 7.9). Nora described in great detail how her first diagram was constructed:

> I first drew four upright strips each 10 millimeters wide, and marked off on each of these lengths corresponding to the loss of weight in water, and the weight of each of the four things; representing 1 gram by the length of 2 millimeters. The parts coloured blue, represent the loss of weight in water; the upper part of the stone is coloured yellow, that of the iron, brown, and that of the ebony, grey. I have marked off by dotted lines on the stone and iron strops the lengths corresponding to the loss in water; but in the case of ebony as I could not do this I have divided the length representing the loss by the difference between this and the total weight. Such pictures are called diagrams, Father [f corrected to F] said, and he told me to see what this word meant, so I looked in the dictionary, and found that...The diagrams show very clearly how *much* the light ebony, and how *little* the heavy iron loses.

After Nora learned what diagrams were, she used such representations again "in order to make this [Law of the Duchess] clear to everyone" (see Fig. 7.10). Because the children were encouraged to write their notebooks independently, each employed their own language. Harold, pointed out how his diagram "saves a lot of time as you need only to look at it once to see if the results are good or bad; it would take you a long time to look all over the figures", and "[y]ou can help yourself to understand the results a great deal by making a curve to show how they differ". Each time a new diagram was drawn, the children wrote down how the images helped them and their virtual readers – "So as to show more clearly what the size of a thing had to do with its loss of weight in water, I drew the diagram on page 18" (Harold). All diagrams had to be clearly labeled and described: "[a]s some people may not know what the red and blue mean, I will tell you..." (Harold).

Armstrong carefully pushed his children to solve increasingly more difficult problems. If the children initially dealt with cubes for which they could easily calculate the volumes, they then had to deal with cylinders. To fully prove his key pedagogical principle Armstrong did not provide his children with any formulas, but pushed them to find independently a way of computing the volume. At Armstrong's incentive, the children had to describe each step of



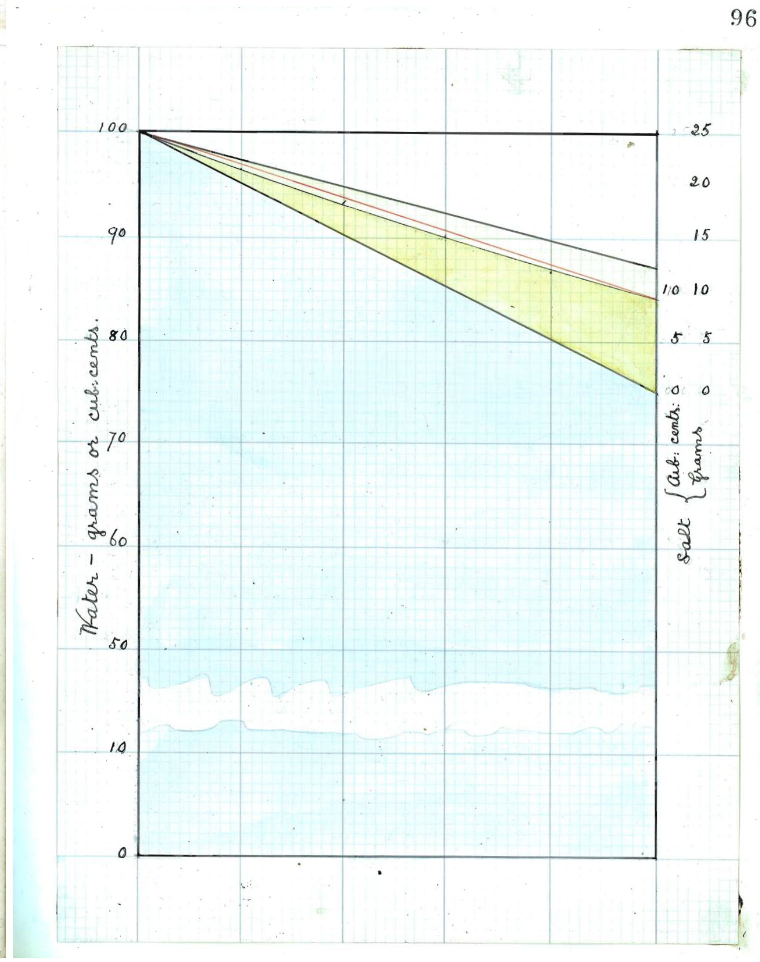
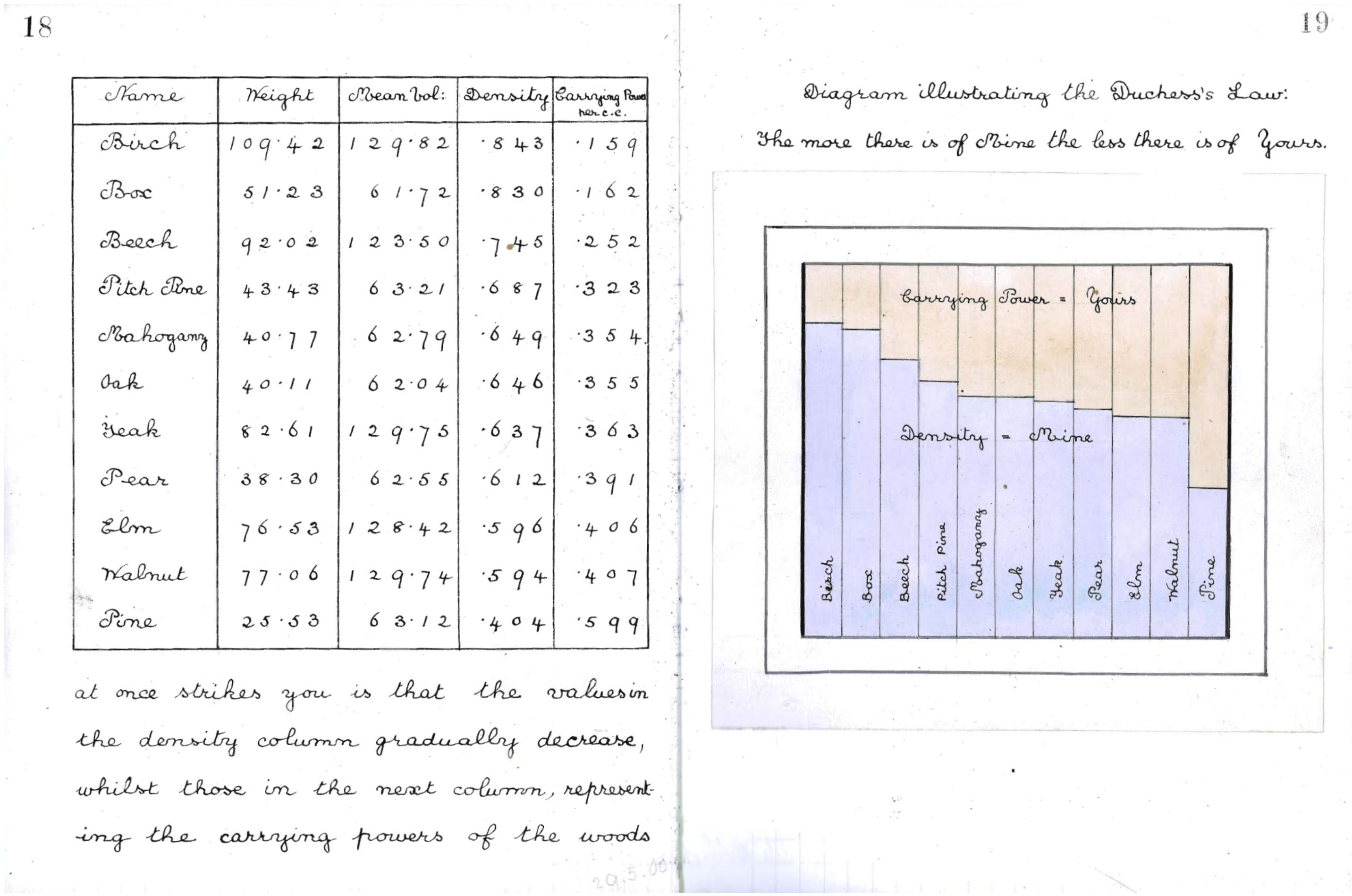


Figure 7.9
The relative weight of the different materials in water. "Such pictures are called diagrams, Father [f corrected to F] said, and he told me to see what this word meant, so I looked in the dictionary, and found that…The diagrams show very clearly how *much* the light ebony, and how *little* the heavy iron loses." Source: Nora's Notebook in the Papers of H.E. Armstrong, Archives Imperial College London.



Name	Weight	Mean Vol:	Density	Carrying Power per c.c.
Birch	109·42	129·82	·843	·159
Box	51·23	61·72	·830	·162
Beech	92·02	123·50	·745	·252
Pitch Pine	43·43	63·21	·687	·323
Mahogany	40·77	62·79	·649	·354
Oak	40·11	62·04	·646	·355
Teak	82·61	129·75	·637	·363
Pear	38·30	62·55	·612	·391
Elm	76·53	128·42	·596	·406
Walnut	77·06	129·74	·594	·407
Pine	25·53	63·12	·404	·599

Figure 7.10
"The truth of the law laid down by the Duchess – The more there is of Mine the less there is of Yours – can not be doubted, but in order to make this clear to everyone I have drawn and colored the diagram on the previous page". Source: Nora's Notebook in the Papers of H.E. Armstrong, Archives Imperial College London.

their reasoning. Harold, the youngest of the three children, described his reasoning as such (see Fig. 7.11):

> As the volume of a cube is found by multiplying the area of one face by the thickness, I thought that the volume of a log might also easily be found in the same manner. But on examining a small cylinder of Pine wood, which Father gave me, I was in a difficulty as I did not know how to find the area of a circular face. It took me a long time to think how to calculate this out, but, I at last decided that the best way would be to draw, on squared paper, a square with a circle inside it of the same diameter. I could then count the number of the small squares within the circle and those in the square, and see what was the relation between the square and the circle. When I had drawn the circle inside the square, I at once saw that a portion in each corner of the square was cut off by the circle, and that it was only necessary to count the number of squares in a quater circle and a quater [sic] square and to multiply by four.
>
> The diagram which I drew will be seen in the margin. I have rulled [sic] off a quater square, and painted yellow the little squares through which the circle is drawn, in order to be able to count them more easily.
>
> On counting the number of squares in the quarter [sic] circle and quater [sic] square I discovered that the former was 172/225 = 0.76 times the size of the latter. On multiplying 0.76 by 4, as I had only dealt with a quarter circle, I find that the circle is 3.05 times the size of the quater square; which, as I saw on looking at it, is better described as the square on the radius of the circle. Thus in order to find the area of a circle I must multiply the radius squared by 3.05.

In a different problem, curves were used to interpolate intermediate values. Once, this method was mastered, the children (with or without immediate guidance from their father) extended the use of this method to find the cube root of 1096:

> There was no way in which I could at once find the number of which 1096 was the cube, and as it would be a very troublesome business to do so by guessing, I thought that as curves had helped me before to get intermediate values I might draw a curve showing how the number from 1-12 are related to their cubes. The number I wanted must be between 10 and 11. As H[arold] had made such a curve, I was saved the trouble and as he has fully described how the curve was drawn I need not do so again. On looking at the curve, it appeared that 1096 was the cube of 10.3.

Harold was equally careful to point out when one graphical solution was inspired by a previous one:

> I did this because from the curve on page 41 showing how density and carrying power were related we were able to calculate the density if we knew the carrying power of a wood and vice versa, so

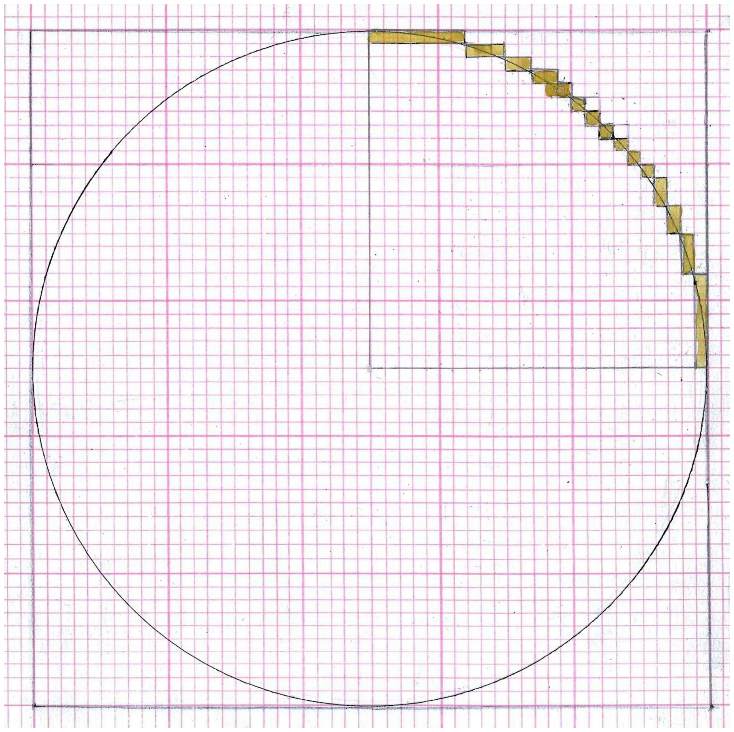


Figure 7.11
Harold's use squared paper to find the area of a circle. Source: Harold's Notebook in the Papers of H.E. Armstrong, Archives Imperial College London.

> I thought that if I made a curve of the same sort for cubes I should be able to get at the length of their faces very easily.

These notebooks were the experimental proof for Armstrong's "heuristic method". As in the case of Perry's program, the point was not that graphical methods were useful (a trivial point) but rather that such methods were more than practical tools – they were mental tools whose use could be mastered by anyone. The independent use of these tools was the real test of true understanding.

4 CONCLUSION

The introduction of graphical methods in mathematics has taken a tortuous path. By the end of the 19th century, graphical methods were commonly used in three disjoint pedagogical settings. First, graphical calculus was taught in technical and engineering schools as a way of familiarizing students with the principles and constructions of descriptive and projective geometry which were to be applied to graphical statics. Second, the concept of a coordinate system and the geometrical representation of an equation was taught in conjunction with analytical geometry in universities or the terminal years of secondary education. Third, the graphical representation of experimental measurements though commonly used in scientific publications was consistently employed as a pedagogical tool only in a few laboratories such as those at Finsbury College.

Because it was the exemplar of rigorous reasoning and the model for abstract theoretical science, for much of the 19th century deductive geometry had been the most cherished mathematical topic in secondary schools.[161] Furthermore, only the abstraction of deductive geometry could exercise the mind of students to its highest extent because it did not divert their attention to secondary practical problems. Graphical drawing and practical geometry were of subordinate importance and were often ignored altogether. However, as modern experimental science came to occupy an increasingly important role in classrooms and laboratories, several reformers felt compelled to militate for a reversal of the old order.

If geometry had been the model for deductive science, inductive science was now to become the model for geometry. Such an inversion was supported not so much by epistemological arguments about the true nature of mathematics or rigorous proofs, but rather by psychological arguments about what could encourage genuine understanding. The logical necessity of a proof was now to be preceded by the psychological necessity of the method of proof. The "laboratory method", the "heuristic method", or the "scientific method" would make "the pupil be led to feel the need of the mathematical tool through some material experiment he has made or things he has done".[162] Thus, "[t]he logical proof is regarded rather as the climax

161. For the role of geometry in French scientific education before the 1902 reform, see Belhoste, "L'enseignement secondaire français et les sciences au début du XX e siècle," 380; For "geometry as an exemplar of reasoning" in Britain, see Richards, *Mathematical Visions*, 175, 196.

162. J. W. A. Young, *The Teaching of Mathematics in the Elementary and the Secondary School*, in collab. with University of Connecticut Libraries (New York [etc.] Longmans, Green, and co., 1907), 104.

of the work than as its foundation".[163] This opposition between the logical and psychological can also be understood as an opposition between the (geometrical) system and the (graphical) method: "Der systematische Lehrbetrieb ordnet den Unterrichtsstoff nach sachlich-logischen, der methodische nach psychologischen Gesichtspunkten."[164]

If in the late 18th century (as seen in Chapter 1) the *graphical* was always part of a dichotomy – opposed to trigonometry, algebra or analysis – and was used to separate the boundaries between various disciplines, by the early 20th century the *graphical* was invoked as a place of intersection for geometry and algebra, mathematics and experimental science, induction and deduction. Furthermore, if in the 17th and 18th century the *graphical* was a subdomain of the *mechanical*, in the early 20th century *graphical* pedagogies came to be opposed to mechanical pedagogies based on rote memorization and mechanical drilling. In this new context, graphical methods did not only train the hands but also the mind by cultivating the students' interest and providing them with mental weapons. The *place* of the *graphical* had shifted not only in relation to the school disciplines, but also within the economy of the classroom. While in the school of Monge the graphical work was an application of the principles of descriptive geometry (and as such, followed after the theoretical instruction), in the reformed education envisioned by John Perry and Henry Armstrong graphical work was the foundation on which future theoretical instruction was to be based. For Perry and E.H. Moore the ideal classroom resembled not so much the drawing rooms of Monge's École Polytechnique, but rather the mechanical and electrical laboratories of Tokyo and London.

163. J. W. A. Young, "A Course in Practical Mathematics. By F.M. Saxelby," *The School Review* 14 (1906): 460–461.
164. Friedrich Pietzker, "System und Methode im exact wissenschaftlichen Unterricht," *Unterrichtsblätter für mathematik und naturwissenschaften* 5 (1899): 46.

· 8 ·

Conclusion

The purpose of this study has been that of revealing the internal structure or topology of the historical category of the *graphical.* Instead of approaching graphical objects as unproblematic and transparent entities which are simply given to us – "you know a graphical object when you see one" – this study has showed that the *graphical* was a *value-laden* category which reflected and reinforced various epistemological and pedagogical programs.

Though graphical methods (as seen by a *contemporary eye*) were used throughout the early modern period, they only emerged as a historical category in 18th century France to refer to an array of operations, methods or constructions. Two factors were essential. First, a distinction was drawn between geometry (seen as the model of rigorous reasoning which defined the principles and objects of its science) and the arithmetical, trigonometrical, mechanical or graphical operations through which it was manipulated. For those practitioners for whom geometry was not first of all a model of rigorous reasoning, the category of "geometrical methods" would have sufficed. Second, a few particular problems had to be tackled by multiple methods. While everyday practitioners would have indiscriminately used any method which they deemed to be the most expedient, 18th century academic authors employed a uniform and general style of presentation to write their mathematical treatises. The price to be paid for such stylistic unity was a certain number of cumbersome solutions for which more practical methods had to be considered.

By the end of the 18th century, the status of geometry was challenged by mathematical analysis generating a conceptual opposition between the two – each field appealing to its own principles and objects.[1] In this context the *graphical* became the language of geometry,

1. See Lorraine Daston, "The Physicalist Tradition in Early Nineteenth Century French Geometry," *Studies In*

an interpretation which was employed in particular by Monge and his students to unify the teaching at l'École Polytechnique. Thus, graphical methods gave way to graphical disciplines and graphical work. This interpretation of the *graphical* category was employed in the mid-19th century by Karl Culmann who introduced through his seminal textbook *Die graphische Statik* (1865) the new discipline of "graphical statics" as an attempt "to use the newer geometry [i.e. the projective geometry of Jean Victor Poncelet] to solve those tasks of engineering suitable for geometric treatment" by "replacing as much as possible the algebra with the geometry of position".[2] The category of the *graphical* was useful for Culmann because his discipline was an application of the principles of projective geometry and was presented as an alternative to the already established analytical methods. In the first edition of *Die graphische Statik* (1865) Culmann even banned all analytical proofs, a decision he had to revoke in the second edition.[3] Some engineers even drew a direct parallel between descriptive geometry and graphical statics: while from geometry "we obtain a method of construction, or *descriptive* geometry", from "geometrical statics we obtain also a construction method or routine–viz., *graphical* statics".[4]

The category of the *graphical* could only play such a role in a particular pedagogical setting. While British physicists and engineers developed important methods for what will become the discipline of graphical statics, their preferred category was not the *graphical* but the *diagram.* William Rankine's "method of drawing diagrams of forces" inspired Maxwell to introduce his famous "reciprocal figures" or "reciprocal diagrams", which were later extended in a "general theory of diagrams of stress".[5] Similarly, Maxwell expressed his appreciation for Robert Henry Bow's "method of drawing diagrams", while Culmann's *Die graphische Statik* was only

History and Philosophy of Science Part A 17, no. 3 (1986): 269–295; Joan L. Richards, "Rigor and Clarity: Foundations of Mathematics in France and England, 1800–1840," *Science in Context* 4, no. 2 (1991): 297–319.

2. Karl Culmann, *Die Graphische Statik* (Mayer & Zeller, 1866), vi-xi; see also Karl-Eugen Kurrer, *The History of the Theory of Structures : From Arch Analysis to Computational Mechanics* (Berlin: Ernst & Sohn, 2008), 318; Scholz, *Symmetrie, Gruppe, Dualität*.

3. Karl Culmann, *Die Graphische Statik* (Meyer & Zeller, 1875).

4. Augustus Jay Du Bois, *The Elements of Graphical Statics and Their Application to Framed Structures* (New York: J. Wiley and son, 1875), xlv.

5. Maxwell, *The Scientific Papers of James Clerk Maxwell*, v.1 514-525; v.2 161-209; see Kurrer, *The History of the Theory of Structures*, 320-322.

described briefly as “makes great use of diagrams of forces, some of which, however, are not reciprocal”.[6] Though the category of *graphical* also became fashionable in Britain, it did not carry with it its Continental associations. The “graphical methods” were not an application of the principles of descriptive or projective geometry, but rather, as Maxwell defined them in his article on “Diagrams” from the *Encyclopaedia Britannica* (9th ed., 1875), they were those “[m]ethods in which diagrams are used for purposes of measurement”.[7] It was their ability of manipulating and representing numerical values, rather than being an application of geometrical principles, which brought them to the forefront of engineering practice in the second half of the 19th century.[8]

While at the beginning of the 19th century the *graphical* was the language of geometry and the method through which the principles of geometry were applied, by the end of the century the *graphical* became the language of inductive science and the method through which the experimental method was applied. The new associations of the *graphical* were so strong that they allowed the reinterpretation of school geometry from a model of deductive reasoning into an application of inductive science.

6. Maxwell, *The Scientific Papers of James Clerk Maxwell*, v. 2, 494.

7. See ibid., v. 2, 647-659.

8. For Culmann’s failed program see Scholz, *Symmetrie, Gruppe, Dualität*; Scholz, “Graphical Statics.” For the connection between graphical methods and computational techniques see Joseph Lipka, *Graphical and Mechanical Computation - Including Nomographs and Mechanical Integration* (Watchmaker Publishing, 2003); Dominique Tournes, “Du compas aux intégraphes : les instruments du calcul graphique,” *Repères*, no. 50 (2003): 63–84; Tournes, “Pour une histoire du calcul graphique.” For the description of graphical methods by engineers in the late 19th century see H. S. Hele Shaw, “First Report of the Committee … Appointed to Report on the Development of Graphic Methods in Mechanical Science,” in *Report of the Fifty-Ninth Meeting of the British Association for the Advancement of Science* (J. Murray, 1890), 322–327; H. S. Hele Shaw, “The Graphical Method of Solving Engineering Problems,” *Transactions of the Liverpool Engineering Society* 14 (1893): 173–188; H. S. Hele Shaw, “The Teaching of Graphical Methods in Engineering Colleges and Schools,” *Proceedings of the Society for the Promotion of Engineering Education* 1 (1894): 184–206; H. S. Hele Shaw, “Third Report on the Development of Graphic Methods in Mechanical Science,” in *Report of the Sixty-Third Meeting of the British Association for the Advancement of Science* (J. Murray, 1894), 573–613.

Geometrical Illustrations, Empirical Curves and Graphical Objects

Opposed to empirical curves, geometrical illustrations (i.e. objects constructed through some well-defined geometrical operations such as constructions by rule and compass) were of limited epistemological value within scientific practice. The geometrical illustrations could only be an approximation of a numerical table (by the 19th century the precision of measurements greatly exceeded the precision of a graphical construction) or of an equation (while all geometrical constructions could be translated into an equation, some equations could only be approximatively represented through iterative geometrical constructions). Though they were not absent from scientific publications or mathematical textbooks, geometrical illustrations only played a secondary, intermediate role: they were not used to establish conclusions, but only to illustrate them.

The first consistent and coherent category of empirical curves were the weather charts of 18th century meteorology. Such charts were seen as the most efficient manner of condensing an ever increasing number of barometric measurements (towards the end of the 18th century some observers took up to three measurements per day). Condensing the measurements within an average was not a satisfactory solution in the study of the weather because one had a special interest in trying to understand and predict pressure variations. Weather charts became particularly popular among those meteorologists who tried to correlate the changes in atmospheric pressure with the phases of the moon. Two factors allowed weather charts to become the paradigm for empirical curves: 1. the barometric curves were not reducible to any simple geometrical curve (when some meteorologists tried to identify a pattern of variation, they drew geometrical curves on top of the barometric curves); 2. the great number of measurements allowed one to construct well-defined curves by only drawing individual points. Thus, even before self-registering instruments started being employed, the variation of the pressure drew its own curve. When in the early 19th century empirical curves started being employed to represent other quantities, there was almost always a direct connection to the practice of constructing a weather chart.

By the beginning of the 19th century empirical curves were also drawn in the case of observations with a small number of measurements. While most of the time one only displayed the agreement between the experimental measurements and empirical formula (in these cases no actual empirical curve was drawn, but only the curve corresponding to the formula), in a few particular instances empirical curves were drawn when no empirical formula could satisfactorily describe the whole range of measurements (see the curves of solubility in Chapter 3). While initially such empirical curves were only considered to be more accurate than the empirical formulas, they also came to challenge the role of the individual measurements. Because the empirical curve had to be drawn between the experimental points, one was confronted with a moral choice of deciding which individual measurements were more trustworthy.[9] Furthermore, because the empirical curve was an object which reflected the whole set of measurements, each individual measurement could be corrected in relation to the curve. While individual measurements were used to evaluate the adequacy of an empirical formula, an empirical curve could be used to evaluate the adequacy of the individual measurements.

Because the empirical curves were drawn *between* the points *libera manu* (free-handed) or by bending a metal rule, some authors carefully distinguished between such graphical constructions and geometrical constructions. For example, in 1833 William Herschel presented a method for carrying out double-star measurements which was "essentially graphical" and "not a mere substitution of geometrical construction and measurement for numerical calculation" because instead of being a system of calculation, it relied on "the eye and hand to guide the judgment".[10] Herschel considered that "such charts and graphical representations" should not

9. See Hankins, "A "Large and Graceful Sinuosity". John Herschel's Graphical Method."
10. John FW Herschel, "On the Investigation of the Orbits of Revolving Double Stars; Being a Supplement to a Paper Entitled" Micrometrical Measures of 364 Double Stars"," *Memoirs of the Royal Astronomical Society* 5 (1833): 178; See Hankins, "A "Large and Graceful Sinuosity". John Herschel's Graphical Method."

be considered as "mere helps" but rather "the only means we possess, or ever can possess, of purifying great masses of observational data from the effects of local influence and personal or casual error".[11][625]hankins2006 Similarly, for Victor Regnault the graphical method when used with sufficient care could produce "la courbe qui représente réellement le phénomène".[12]

Though some empirical curves could claim a higher epistemological status compared to a table of measurements or an empirical formula, it was only the latter which could easily travel. This was not so much a limitation of "optical consistency" as much as one of "material consistency".[13] As seen in the graphical work of Victor Regnault, paper could not be always trusted as it could easily change when stretched or exposed to humidity. Because one could only trust the graphical constructions and not the graphical representations (i.e. the object which was left at the end of the graphical process), empirical curves could only play a significant scientific role at a local level. When graphical representations did travel or were reproduced in a publication, they were a mere illustration of the graphical constructions. This situation applied throughout the mid-19th century when self-registering and self-recording instruments started being used extensively in observatories and physiology laboratories; as showed in Chapter 2, initially the inscriptions and traces of these instruments did not travel either; the graphical traces were usually reduced on the spot into numerical tables.

As long as the epistemological value of the empirical curves depended on the precision with which they were drawn, such objects could not easily travel while preserving their epistemological status. This changed when some empirical curves acquired a new role – instead of being just objects of precision they also became *paradigmatic* objects. A *paradigmatic* graphical object (either a curve, a diagram, a model etc.) was not defined any longer in terms of the precision with which it was drawn (as was the case with Regnault's graphical table), but

11. John Herschel, "Terrestrial Magnetism," *Quarterly Review* 66 (1840): 89-90.
12. Regnault, "Relation des expériences sur des Machine a Vapeur," 428.
13. For the importance of "optical consistency" in the making of "immutable mobiles" see Latour, "Visualization and Cognition."

rather in terms of its *graphical features*. Thus, the windings of a curve could be associated with unseen hypothetical states of matter and used as evidence for the agreement between van der Waals' molecular theory of heat and Thomas Andrews' experimental isotherms for carbonic acid; or, the topological order of lines in a plane and of folds on a solid could show the state of equilibrium of mixed states of matter; the asymptote of a magnetic curve could be interpreted either macroscopically as corresponding to a region of magnetic saturation, or microscopically (in terms of a fictitious model) as corresponding to a perfect molecular alignment.

It is important to notice that these graphical features were not simply read off the curves and diagrams, despite the utopic vision of some graphical practitioners like Étienne-Jules Marey who presented his inscription devices as capable of writing in an universal pictorial language, the language of the phenomena themselves. A visual object could be read only if one knew what to look for – which patterns, shapes or variations were meaningful. While one learned to read well-established or institutionalized graphical objects through a special regimen of training and practice, the graphical features of these objects could not have emerged through inculcation alone. Graphical features emerged through a historical process of continuity and identity. A new graphical feature was not revealed by an isolated graphical object – *this* curve – but rather by a curve which could be identified as a variation (a correction or a rewriting) of a previous curve.

The concept of *historical family resemblances* or *historical category* has been essential because it has forced us to consider not only the variation of graphical objects, but rather a unified variation which reinforced itself. This approach showed that thinking with a graphical object did not imply solely looking at an image or manipulating a tool, but it also meant thinking about objects as being the same and yet different. While a tool acted on what was given, a unified variation allowed one to think new possibilities.

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