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Who was J. Robert Oppenheimer? Charisma and Complex Organization

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ABSTRACT Charismatic authority flourishes in places where some social scientists evidently do not expect to find it – in late modernity and in highly complex and instrumentally orientated technoscientific organizations. This paper documents and interprets participants' testimony about the workings of wartime Los Alamos in relation to the charisma of its Scientific Director, J. Robert Oppenheimer. We treat charisma as an interactional accomplishment, and examine its rôle in technoscientific organizations. Los Alamos was a hybrid place, positioned at the intersection of military, industrial and academic forms. Everyday life there was marked by a high degree of normative uncertainty. Structures of authority, communication and the division of labour were contested and unclear. The interactional constitution of Oppenheimer as charismatic enabled him to articulate, vouch for and, finally, come to embody a conception of legitimate organizational order as collegial, egalitarian and communicatively open. We offer concluding speculations about the continuing importance of charismatic authority in contemporary technoscientific organizations. Just as normative uncertainty is endemic in late modernity, so too, we argue, is charisma.

Keywords charismatic authority, leadership, military, science, technoscientific

Who Was J. Robert Oppenheimer?

Charisma and Complex Organization

Charles Thorpe and Steven Shapin

A Magic Mountain

Late in 1942 there began to grow up in the New Mexico high desert a site whose like the world had never seen before. It was the size of a small town, encompassing many of the institutions and mundane relations of similar small towns, yet its small-town neighbours miles away were not supposed to know it even existed. Officially, at least, some of the most illustrious people coming to live there changed their names and, supposedly, their public identities, upon entry. It was a place of both power and paradox. Subject to military security, surrounded by a chain-link fence and a moat of secrecy, it was quite unlike the academic world from which many of its inhabitants had come. It was a topsy-turvy world, a technoscientific carnival, in which hairy-chested generals fought a sometimes losing battle for authority against pointy-headed intellectuals. It was a cosmopolitan community with very special properties. It was made up of young people:

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the average age of its scientific inhabitants was in some calculations 29 years – in others 24 – and its director was just 38 when he took up his appointment. There were a lot of babies, but there were 'no invalids, no in-laws, no unemployed, no idle rich and no poor'.[1]

It was at once a workplace and a place of residence. Men used to travelling miles from home to work now walked yards. Many of those who had wives and children had them on site. Much of what was done there was in the nature of basic inquiry, but the institutional purpose of the place was rigidly defined by an instrumental goal, the making of a technological artefact whose broad specifications were laid down in advance of most of the inquiries relevant to bringing that artefact into being.

In realizing that goal, scientists and engineers accustomed to limited financial resources and limitless time now quickly got used to limitless resources and extremely limited time. It was arguably the most expensive scientific project in the history of the world. Since the work was on a scale and of a complexity unknown in the academy and rare in civilian industry, organizational patterns appropriate to managing people and problems were largely unavailable. Scale, complexity and security argued for the pertinence of rigid bureaucratic separations and military hierarchy, while the scientific nature of the task impelled it towards some version of an open community. When, after several years' work, members of that community successfully attained their instrumental goal, they had brought into being both a new form of modern technoscientific organization and a new precariousness of global human existence. Some of these members were horrified by what they had done – 'Now we're all sons of bitches';[2] others were deeply satisfied – 'I feel great pride in that accomplishment';[3] still others were morally ambivalent or just reckoned they had done their duty.

And when the Manhattan Project at Los Alamos was wound down, many who then returned with relief to their normal university employments recalled that they had never had so much fun and that science had never been freer. Some called it 'an island in the sky';[4] still others had so much fun that they regarded the fence around the place not as something that kept its inhabitants in, but as keeping others from coming in. The wife of a Los Alamos mathematician called it 'a mountain resort as well as an Army camp' – 'just like a camp out'.[5] A historian who lived at Los Alamos as the wife of a metallurgist calls it 'a magic place', and, indeed, the words 'magic', 'mystical' and 'enchanted' appear recurrently in descriptions both of the laboratory and of its natural environment.[6] Having just finished reading Thomas Mann's *The Magic Mountain* when Oppenheimer asked him to join the project, the experimental physicist Robert Wilson found himself particularly attuned to the romance of Los Alamos.[7] At a party one evening, the theoretical physicist Edward U. Condon – briefly Associate Director of Los Alamos – 'picked up a copy of *The Tempest* and sat in a corner reading aloud passages appropriate to intellectuals in exotic isolation'.[8] Edward Teller brought his own piano to Los Alamos and played it with great gusto.[9]

'I don't think', said one scientist's spouse, 'I shall ever live in a community that had such deep roots of cooperation and friendship'.[10] Teller giggled, 'Aren't we all a big happy family here?'.[11] Unlike the scientific world before or outside, here there was said to be 'no class distinction' between the scientific big shots and the small fry.[12] The wife of a cosmic-ray physicist – herself a 'computer' – wrote that all the scientists 'seemed to be enjoying themselves, as scientists always do when they ponder their problems together'. They worked hard and, as the expression is, they partied hard: '[T]he mere thought of returning to a sane and prosaic civilian life sounded flat and dull. We were having the time of our lives'.[13] The English physicist James Tuck said that it was a 'golden time'; the experimental physicist Isidor I. Rabi announced that it was 'the most significant period in [our] lives'.[14] Speaking in 1967 at a memorial service for the lab's wartime Director, the theoretical physicist Hans Bethe reckoned that, for all of its scientific inhabitants, Los Alamos 'was really the great time of their lives'.[15] The project that for many years was thought likely to put an end to 'Western civilization' – and that still might – was judged by some participants to have been a heroic 'culmination' of that civilization. 'Here at Los Alamos', one physicist said, 'I found a spirit of Athens, of Plato, of an ideal republic'.[16]

Normative Uncertainty and a Sense of Place

It is not just the historian or the sociologist who has trouble in deciding what kind of place this was; the residents of the Manhattan Project's Los Alamos installation were also confronted – on a day-to-day basis – with consequential practical decisions about proper behaviour and legitimate social order at the place where they lived and worked. Was it a military base, and, if so, of what kind? Was it some social form belonging to the academy, and, if so, was it to be understood as a type of disciplinary department, as an interdisciplinary research institute, as some summer workshop, momentarily cut off from normal social patterns and work rhythms? Was it a large-scale engineering project, such as those familiar from contemporary American industry or the Tennessee Valley Authority? Was it home or work or holiday, or some combination of these? Many understandings were available to participants about what kind of place, and what kind of social situation, Los Alamos was.

Different institutional maps of Los Alamos were constantly at issue between different types of participant as, for example, between scientists and the military; between different types of scientist, say, between theoretical physicists and metallurgists; and between scientists of the same sort who happened to have different visions of what Los Alamos was and ought to be. As they acted on the basis of those maps, and as they interacted with those who had different maps, so participants created, sustained and changed the social orders of Los Alamos and its various constituent parts. If you knew what kind of place Los Alamos was, then you knew how it was proper to behave there, what to expect of the behaviour of others and how

you might call them to account. And contests over institutional order and legitimacy often presented themselves as contested maps of place and situation. As one participant said, 'Everyone had his own Los Alamos'.[17]

Participants' maps were not only different, they were often set in conflict with each other. The tensions and paradoxes of Los Alamos were constitutive of its identity as a place. They were never resolved such that all participants came to share a clear systematic understanding of legitimate and normal local social order. Normative uncertainty was just endemic. And, despite some structural-functionalist idylls, such globally shared understandings and senses of legitimacy are, in all probability, never attained in any complex organization or chunk of society. But in an organization such as Los Alamos normative uncertainty, and contests over institutional maps, took highly developed and consequential forms: the question of what kind of place Los Alamos was became a matter of intense, systematic and ongoing reflection. The upshot of formal and informal negotiations about its identity ultimately took the form of organizational charts and associated legal and moral sanctions on who could talk to whom, when, where and about what; about who could give orders to whom; about time-scales; about where different types of people could go and what material and human resources they could effectively command.

Charismatic Authority and Technoscientific Organization

It is with reference to the normative uncertainty of Los Alamos that one paradox came to have special pertinence – that between the complex organization required to build the bomb and the unique individual to whose virtues and capacities many scientific participants attributed so much of the project's success. His scientific colleagues and subordinates at Los Alamos recurrently attributed the triumph of the Manhattan Project to the individual virtues and capacities of its Scientific Director, J. Robert Oppenheimer (1904–67).[18] Such attributions were, as we later point out, neither universal at Los Alamos nor uncontested by other participants. They tended to be made by other scientists and, more specifically, by physicists. And, while the scientists' version of what happened at Los Alamos, and why it happened, is undoubtedly important, it is not the only version, and in other versions Oppenheimer's rôle looms less large.

The success of a technoscientific project as large and as complex as Los Alamos, an enterprise that brought into being new and elaborate forms of institutional differentiation and institutional coordination, one that was obliged on a day-to-day basis to wrestle with questions about the precise relations between bodies of specialized expertise, each of which was deemed necessary for the building of the bomb, was widely ascribed to the rôle of a unique, irreplaceable individual: a 'born leader', a 'natural leader'.[19] Enrico Fermi told Emilio Segrè that 'When anyone mentions laboratory directors, I think of directors and directors and Oppenheimer, who is unique'.[20] Edward Teller – one of the most awkward characters with whom Oppenheimer had to deal, and 'a disaster to any organization' – said

that 'Oppenheimer was probably the best lab director I have ever seen'.[21] Bethe ascribed the high morale and efficiency of Los Alamos substantially to the personal rôle of Oppenheimer: 'He was a leader'.[22] Wilson called Oppenheimer 'our leader in every respect', and referred Oppenheimer's ability to solve day-to-day problems of the laboratory 'to his combination of skill, wisdom, and moral stature'.[23] When, in 1963, Oppenheimer returned to Los Alamos for the first time since the War, he was introduced by the current Director, Norris Bradbury, as 'Mr Los Alamos', adding that Oppenheimer had 'built Los Alamos by the sheer force of personality and character'.[24]

Moreover, the language used to describe this unique individual has cultural resonances that pitch it directly against the very idea of complex, bureaucratized modern forms of organization and how such organizations are often said to work. Rabi wrote that Oppenheimer succeeded Einstein 'as the great charismatic figure of the scientific world', linking Oppenheimer's charisma to his 'spiritual quality'.[25] Wilson reported how he was 'soon caught up by the Oppenheimer charisma' at Los Alamos.[26] The chemist Glenn T. Seaborg dwelt on Oppenheimer's 'magnetic, really electric, personality, [and] his charismatic presence'.[27] And Teller spoke of the 'brilliance, enthusiasm and charisma' with which Oppenheimer led Los Alamos.[28] So participants tell us that if we want to understand the social and technical order of one of the most important technoscientific sites of modern times, we should get to grips with the rôle of embodied personal authority in general, and charisma in particular.

The prompt is unsettling just because the notion of 'charisma' came into social scientists' vocabulary early in the 20th century to pick out a form of authority that was being lost from the modern 'disenchanted' world, and whose loss was attributed to the rise of bureaucratic modes of organization. Los Alamos, or, indeed, any modern technoscientific organization, might be thought a surprising place to see charismatic authority at work.[29] So, among many examples, Magali Larson writes that science is characterized by 'the supreme objectivity of technique', and that it is the submergence of the personal in the technical that 'appears to endow [scientists] personally with superior objectivity':

> Thus, despite the deep and bitter rivalries that pervade all scientific fields, the scientific-technical caste excludes purely charismatic claims to power perhaps more absolutely than any other intellectual caste in history.[30]

In Max Weber's usage, charisma has both pure and hybrid forms. In its pure form, charisma – an attributed personal 'gift of grace', the possession of *pneuma* by a recipient of divine inspiration – and associated charismatic forms of authority and organization, are explicitly contrasted to any form of institutional routine and, especially, of bureaucratic organization. Charisma meets social needs 'that go beyond those of everyday routine' and, in so doing, acts as 'the specific creative revolutionary force in history'. In times of 'psychic, physical, economic, ethical, religious, [and] political distress'

> ... the 'natural' leaders ... have been neither officeholders nor incumbents of an 'occupation' ... The natural leaders in distress have been holders of specific gifts of the body and spirit; and these gifts have been believed to be supernatural, not accessible to everybody.

It is the exceptional, out-of-the-ordinary force of charisma that sets it against 'any kind of bureaucratic organization':

> [T]he charismatic structure knows nothing of a form or an ordered procedure of appointment or dismissal. It knows no regulated 'career', 'advancement', 'salary', or regulated and expert training ...[31]

A 'disenchanted' world is also a world substantially drained of charisma, and bureaucratic forms of organization are implicated in both losses. Weber flirted with the conclusion that the career of charisma – thus understood – is a marker of modernity, charismatic authority definitively belonging to The World We Have Lost.[32]

However, Weber also thought that this pure or personal charisma was inherently unstable, and that charisma – or something he reckoned was very like it – could somehow become institutionalized and even routinized, as in the *ex officio* supernatural authority passed on in apostolic or monarchical succession. This routinized charisma nevertheless shows signs of dilution: the great potency of pure charisma diminishes as it is institutionalized. The social force which is originally defined as standing outside of social institutions is now treated as an integral aspect of institutions. (It is in this connection that Clifford Geertz pays evidently non-ironic tribute to Weber's 'extraordinary ability to hold together warring ideas'.)[33] This led later commentators and apologists towards eclectic positions which further fuzz any coherent juxtaposition of charisma and institutionalized action. 'Charisma and bureaucracy', Amitai Etzioni writes, 'can be combined to varying degrees.... A structure might be purely bureaucratic, or have some charismatic positions, or have a large number of such positions, or be almost completely staffed by charismatics ...'.[34] Other sociologists acknowledge the rôle of charismatic authority within modern bureaucratic structures, while arguing that this makes for institutional dysfunctionality.[35] Institutions devoted to the rational achievement of instrumental ends – and technoscientific organizations would presumably head any such list – can only be damaged by the intrusion of charismatic authority.[36]

Sociologists sometimes react with dismay when terms of professional art are let loose in the wider culture. In such cases, it is said, sociological rigour is 'vulgarized' in the mouths of the ignorant laity. So Reinhard Bendix, for instance, deplores current 'debased' usages of the notion of charisma:

> Charismatic leadership depends upon a widespread belief in the existence of extraordinary or supernatural capacities, but such beliefs are at a discount in secular contexts.[37]

Rigorous expert use has become vague and vulgar misuse. Of course, Bendix is quite right to be sceptical about any very systematic genealogical

or semantic link between Weber's and Everyman's charisma. Without Weber's contributions to academic culture, it is, of course, unlikely that atomic scientists would have had the word 'charisma' available to them and, to that extent at least, their vernacular stands in a genealogical relationship with the vocabulary of social-scientific expertise. But to claim more than that is both problematic and, for our purposes, unnecessary. Los Alamos scientists were performing charisma, not reflecting on it; they were building a bomb, not doing social theory. Quite possibly, 'charisma' was, for them, just a fancy or fashionable way of expressing sentiments for which alternative, and more traditional, vocabularies already existed.[38] We want not to worry whether Los Alamos usage was properly Weberian. We will show that when atomic scientists referred to Oppenheimer's charisma – or, indeed, when they used broadly similar vocabulary – they were picking out his exceptional attributes of body and mind, and saying that, in specific contexts, these attributes were powerful solutions to organizational problems. So what special features of body and mind did Los Alamos inhabitants attribute to Oppenheimer? How did they see the relationships between those extraordinary features of Oppenheimer's body and mind and specific aspects of Los Alamos organizational life?

Following attributions of charisma delivers us to a series of questions now increasingly central to science studies in particular and social science in general: how should we conceive of individuals – their attributes and the consequences of their actions – in relation to social structures? What is the rôle of the embodied, the local, the familiar and the face-to-face in the technoscientific systems and institutions of late modernity? Given the interpretative flexibility of organizational rules, how should we account for local order and, indeed, how should we conceive such order? How ought we to describe the moral economy of technoscientific places? What are the sources of authority and legitimacy within them, and, by extension, of their products?[39]

The Face of Charisma

The *topoi* through which Oppenheimer's associates described his special attributes turn out to be remarkably stable: they are stable in their recurrence from one commentator to another and, as general descriptors, they are also interestingly stable in their form and function over a great span of time in Western culture. Indeed, commentators themselves sometimes showed awareness of prior uses of these *topoi* in the historical past, thus constructing for themselves a link between premodern culture and the technoscientifically modern. These *topoi* are found in torrents of commentary on what many associates experienced as Oppenheimer's extraordinary features of body, face and gesture, as well as on his manner of living in the world (suave, cosmopolitan, generous and detached), and on his special social, moral and intellectual *presence*, a presence which was, in turn, often associated with his bodily endowments. Here, to indicate the texture of

much of this commentary, we let physiognomical *topoi* mark the general tone and substance.

Almost no account of the force of Oppenheimer's personal presence failed to remark upon his tall and thin bodily frame and, especially, upon the colour of his eyes and the intensity of his gaze. Before the War, his Berkeley students were used to what they called the 'blue glare treatment' – 'when aroused', Goodchild reports, 'Oppenheimer's eyes seemed to turn from a gray-blue to a vivid blue'.[40] Bernice Brode wrote about his blue eyes, which 'had that special intensity, peculiar to him';[41] the young son of a Los Alamos machine-operator remembered 'his blue eyes more than anything else. A very gentle man. He had a great smile that would melt me'.[42] Eleanor Jette, the wife of a Los Alamos metallurgist, referred to his 'electric blue eyes' in identifying Oppenheimer as the individual 'who guided the work and wove the threads together';[43] Rabi spoke about 'the penetrating gaze of his blue eyes' which helped make him 'a center of attention in almost any company';[44] Roger Robb, the Atomic Energy Commission's counsel in the Security Board hearings, expressed his dislike of Oppenheimer in physiognomic terms: 'he had the iciest pair of blue eyes I ever saw';[45] and his one-time friend Haakon Chevalier described him as

> ... tall, nervous and intent.... But it was the head that was the most striking: the halo of wispy black curly hair, the fine, sharp nose, and especially the eyes, surprisingly blue, having a strange depth and intensity, and yet expressive of a candor that was altogether disarming. He looked like a young Einstein, and at the same time like an over-grown choir-boy. There was something both subtly wise and terribly innocent about his face. It was an extraordinarily sensitive face, which seemed capable of registering and conveying every shade of emotion.

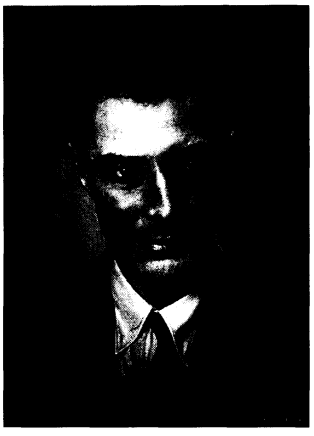
It was a face that made Chevalier think about glorified faces he had seen in paintings: 'I associated it with the faces of apostles.... A kind of light shone from it, which illuminated the scene around him'.[46] There was 'something about his eyes [that] gave him a certain aura' (see Figure 1).[47]

It was a face and a body that summoned up similar images in Robert Wilson's wife Jane. When, towards the end of the project, Oppenheimer was ill with the chicken pox and reduced to about 115 pounds, she thought that 'our thin, ascetic Director looked like a 15th-century portrait of a saint'.[48] He was so thin that 'he has been referred to by geometricians as a refugee from a plane geometry textbook', lacking a solid or fleshly dimension.[49] In describing Oppenheimer, Rabi was reminded of a friend he had known as a student at Cornell:

> Physically and perhaps intellectually and emotionally, he was very like Oppenheimer. One day he announced: 'I give the lie to the materialist. I am a disembodied spirit'.

'In Oppenheimer', Rabi concluded, 'the element of earthiness was feeble'.[50] The physicist Leona Libby contrasted Enrico Fermi's earthiness

FIGURE 1
Oppenheimer as Wartime Director of the Los Alamos Laboratory



Source: LANL 88973.

with 'the poetic, disembodied, spiritual emanations that were the basis for Oppenheimer's charisma'.[51] The biographer of the military director of the Manhattan Project describes Oppenheimer as 'frail, almost ethereal'.[52] In Richard Rhodes' high-octane prose, 'Oppenheimer's emaciation suggests he had an aversion to incorporating the world. His body embarrassed him and he seldom allowed himself to appear, as at the beach, undressed'.[53] Oppenheimer was, another writer said, 'frail to the point of transparency', adding that this sense that his body was all but absent advertised and appeared to augment Oppenheimer's intellectual presence: 'The power of his personality is stronger because of the fragility of his person. When he speaks he seems to grow, since the largeness of his mind so affirms itself that the smallness of his body is forgotten'.[54] So Oppenheimer was identified, in part, as an ascetic, with the moral authority that has been associated with the ascetic way of life over a great sweep of Western history.[55]

Charisma and Organizational Order

As participants talked about Oppenheimer's special personal endowments of mind and body – his charisma – so they talked about, and constituted, organizational order at Los Alamos, and so they offered partial solutions to the normative uncertainties of that place. Oppenheimer's charisma may therefore be treated as an *interactional accomplishment*: the constitution of Oppenheimer as charismatic – and as a charismatic of a certain kind in a certain context – helped some people (but not others) to say what kind of place Los Alamos was and ought to be. To say this is apparently to move charisma from the supernatural to the wholly natural level, to give a hard-

headed *functional*, *instrumental* and *naturalistic* account of what was organizationally accomplished in and through constituting Oppenheimer as charismatic. It is a move we do want to make, even as we offer a concluding qualification to the scope and legitimacy of this hard-headed account.

We will here talk about Oppenheimer's charisma in connection with a series of related practical organizational problems at Los Alamos: its identity as a civilian-scientific place *versus* a military place, and its internal organizational structures as these bore upon fragmentation, the flow of information and the maintenance of morale. A point of methodological principle in science and technology studies is here reinforced by common sense: these problems were at once organizational and technical; they bore on what people knew, what they were able to do with the knowledge they had, how they felt about the organization as a whole, and the nature of the commitment they had to the organization's technical goals. We will sketch the nature of these problems and how they presented themselves to various participants, and we will then note how Oppenheimer's special gifts of body and mind were mobilized as partial solutions to those problems.[56]

The social worlds of Los Alamos were defined by two fences. One fence marked off a highly secret Army post from the state of New Mexico, geographically, legally, socially and intellectually. Within this fence there was another which marked off the 'Technical Area' from the Army post proper, thus making a significant distinction between domains of civilian and military authority, even while the whole of Los Alamos was formally an Army installation. The logic of military security dictated as complete as possible a separation of Los Alamos from the rest of the world. The civilian scientists lived in a self-sufficient planned community in which their living conditions and their basic needs were provided for by the Army. Its inhabitants ate in mess halls and shopped at the Army Commissary and Post Exchange. Law enforcement was provided by the Military Police. Some services, such as fire protection, developed into a curious mixture of civilian and military authority and responsibility. The Army also, in the interests of morale, provided cheap beer at the Post Exchange and movie nights in a theatre that on Sundays doubled as a church. What was not provided, the inhabitants improvised for themselves, throwing parties in their homes or dormitories, and clearing trees for their own ski piste.[57] This is what sociologists might mean if they saw in Los Alamos something resembling Goffman's 'total institutions', or what its inhabitants gestured at when they described Los Alamos as a 'campus'.[58]

Its inhabitants even set up their equivalent of 'student government', the 'Town Council'. This administered civil regulations dealing, for example, with traffic offences, relying on voluntary cooperation because there was no civil court on the post. The legally and socially anomalous character of Los Alamos thus threw up surprising forms of institutional improvisation. The Technical Area, inside the inner fence, was nominally under civilian administration, under a contract with the University of California. Oppenheimer was the civilian Scientific Director – one of *two* heads at Los Alamos itself, the other being the military post commander (first Colonel

J.M. Harman, later Colonel W. Ashbridge, and then Colonel G.R. Tyler). However, Oppenheimer himself, like the post commander, reported to General Leslie R. Groves of the Army Corps of Engineers, who was in overall command of the Manhattan Project and who had been responsible for selecting Oppenheimer as Scientific Director.[59]

Groves was no merely nominal authority: he maintained close supervision of the technical programme, both by telephone and teletype from Washington and through frequent site visits. Groves exercised his control personally through Oppenheimer, and also via the Albuquerque District Engineer, the post organization at Los Alamos, and Army and Navy liaison officers assigned to the technical programme. Thus, in practice, 'the role of the prime contractor, the University of California, was narrowly confined to the details of business management and procurement for the laboratory', and the nature of civilian versus military control was never organizationally unambiguous at day-to-day and issue-by-issue levels.[60] The inner fence was a highly porous barrier between military and civilian authority, and it was virtually immaterial to Groves. Oppenheimer's vaguely defined position between the scientists and the military matched the anomalous and uncertain character of the laboratory, placed in a field of tension between military and civilian authority.[61]

Nor did the inner fence define an absolute distinction between types of personnel. The Corps of Engineers furnished a large proportion of the laboratory workforce through its Special Engineer Detachment (SED), which was established to channel technically and scientifically skilled enlisted men into the Manhattan Project.[62] These military personnel formed a sizeable part of the laboratory's workforce. By May 1945, of a total laboratory workforce of just over 2200, roughly half were SEDs. About a hundred members of the Women's Army Corps (WACs) also worked in the laboratory, several as scientific researchers and others as librarians, clerks and telephone operators.[63] The H.E. (high explosives) programme relied particularly on SEDs, carrying out explosives casting, testing and research at outlying field sites. The SEDs, working in the laboratory or at testing sites under civilian group and division leaders, spent the majority of their working day beyond the reach of their military commanders. The position of the SEDs, between the military authority of the post and the civilian authority of the laboratory, was contested and unclear. At one point, General Groves moved to have junior officers of the Corps of Engineers 'put in charge of the enlisted men while the latter work at the various Sites used by the H.E. Project'. The head of the H.E. programme, the chemist George Kistiakowsky, wrote to Oppenheimer strongly objecting to this. Scientific expertise and responsibility, on the one hand, and military rank, on the other, are unlikely to be commensurate, he argued: 'It is very doubtful whether young officers can be obtained with research experience at least equivalent to a PhD degree. Consequently, these officers will not be superior in their technical training and experience to some S.E.D. enlisted men but, at best, will be on the same level'. And

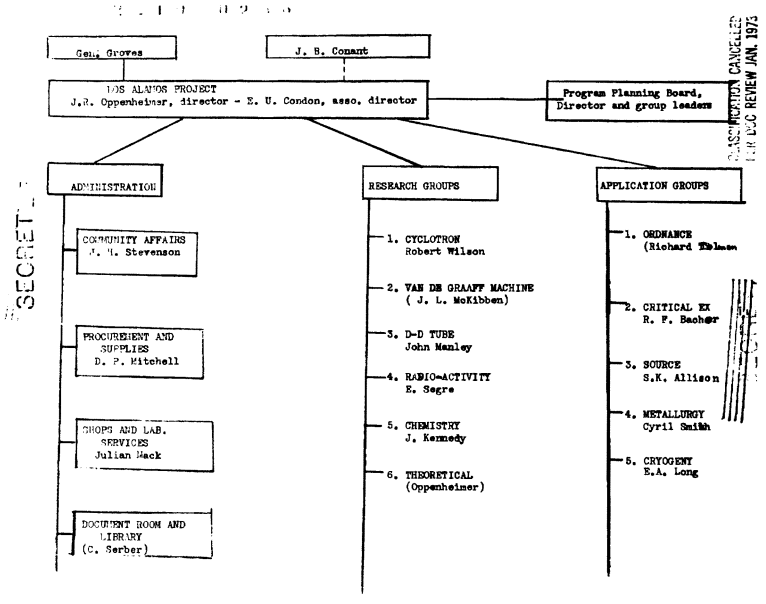
Kistiakowsky pointed out the danger of 'a state of divided authority [on the testing sites] which cannot but result in damage to progress'.[64]

It appears that Kistiakowsky's opposition was decisive in this case, and, in general, the ability of the SEDs to perform their rôle in the laboratory effectively was given priority over the enforcement of normal Army procedures. In an early 1945 conference of senior Corps of Engineers staff at Los Alamos, the question was raised of whether the SEDs were being 'subjected to too much military work'. This idea was dismissed by Major T.O. Palmer who, after becoming commanding officer of the SED in August 1944, had eliminated required morning reveille and callisthenics. Given these reforms, he did 'not see how the SEDs could have any fewer military duties'.[65] An Army psychiatrist, reporting on the 'mental hygiene' of the Los Alamos community, noted in passing that 'The S.E.D. cannot be regarded as regular soldiers, having been segregated soon after enlistment for specific jobs because of specialized education. The work under civilian administrators places military regulations as secondary to the scientific program'. This 'anomalous situation' of the SED was, in his view, a 'knotty one'.[66] As former SEDs have put it, 'This was the least military of any military outfit I have ever been in. Now I understand that other units, the construction group and the MPs [Military Police], were really G.I., but not us. We were quite un-G.I. . . . Tech Area pressure kept the military from interfering and trying to make us G.I'.[67] The position of the SED, instantiating the often confused division between civilian and military authority, was just one example among many of the normative uncertainty pervading life at Los Alamos.

This uncertainty was increased by the rapid growth of the Los Alamos population which, throughout the War, continued to outstrip both plans and provisions. In February 1945, a post administrator complained that the increase in population 'taxes practically all our facilities beyond capacity'.[68] Construction on the project began in January 1943 with approximately 1500 workers. Scientific personnel began to arrive on a permanent basis in March 1943 and, by the end of the year, the population had reached an estimated 3500. This rose to 5675 by December 1944, and by June 1945 the total population had reached a wartime peak of approximately 8750.[69] These figures stand in marked contrast with early projections, by Oppenheimer and his fellow physicists, about the likely scale of the project. Oppenheimer's original guess was that perhaps as few as six scientists (or, with support personnel, several hundred) might do the job.[70] The physicist Hugh Bradner has told us that, when he was recruited for the project in winter 1943, the anticipated number of scientists was between 30 and 70.[71] Both the laboratory and the post expanded beyond all expectations as new configurations of scientific knowledge, technological activity and organizational form were thrown up as emergent properties of the work. The forms that had been loosely envisaged as appropriate to relatively small numbers of scientists had been replaced by those necessitated by a large, highly differentiated workforce. The transformation was ad hoc and 'seat-of-the-pants'.

Throughout the War, the organization of Los Alamos was in a continual state of flux and turbulence. The production and display of organizational charts were interesting to participants as symbolic representations of coherence and stable order amid uncertainty and change (see Figure 2). So the mathematician Stanislaw Ulam writes of the Los Alamos 'fascination with organizational charts. At meetings ... whenever an organizational chart was displayed, I could feel the whole audience come to life with pleasure at seeing something concrete and definite'.[72] This symbolic display of coherence was one way of making the organization appear legitimate and functional. The informal everyday life of the laboratory, of course, massively belied the neat divisions and lines of authority displayed on such charts and the status of these representations *vis-à-vis* quotidian organizational realities was uncertain.[73] The problem of keeping pace organizationally with rapid change and expansion was faced not only by the laboratory, but also by the post, in its work of ministering to the needs of the laboratory and the community. For example, Colonel Tyler, who became post commander early in 1945, was moved to say at one post

FIGURE 2
An Organization Chart for the Los Alamos Laboratory, dating from early March 1943



The chart shows the situation of Oppenheimer in relation to the civilian authority of J.B. Conant, on the one hand, and the military authority of General Groves, on the other. This chart also shows Oppenheimer as head of the Theoretical Division. This dual responsibility for Oppenheimer was abandoned by the time the laboratory began operation. Subsequent charts (e.g. LANL A-84-019 34-17) also dispensed with the distinction, maintained here, between research and application, and demonstrate the further differentiation of divisions into groups.
Source: LANL A-83-013 1-1.

administration meeting 'that he had been unable to find any organizational chart which had any meaning'.[74]

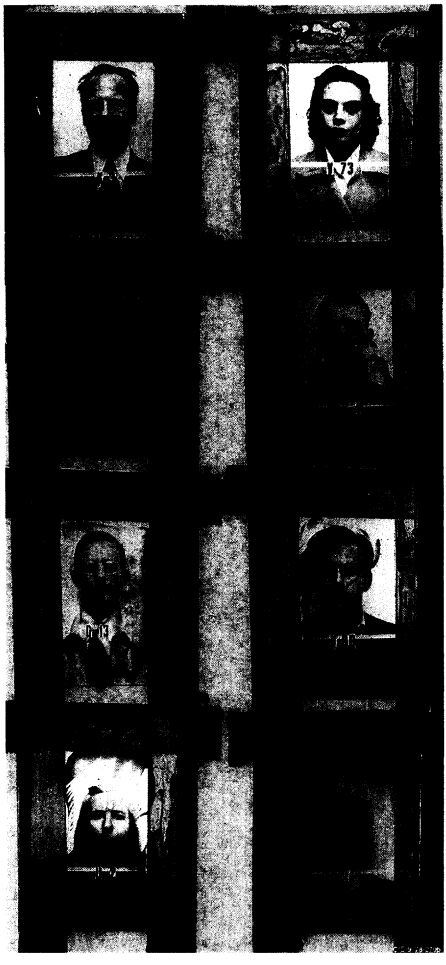
Military control of Los Alamos posed several kinds of practical organizational and technical problems for its scientific inhabitants. Given overall communal dedication to beating the Nazis to the Bomb – at least for those inhabitants who knew what the Project was all about – military-imposed censorship and secrecy was not one of them (the spy Klaus Fuchs excepted). Richard Feynman liked to play games showing up the silliness and inefficiency of security measures, and other scientists bridled at the mandated name-changes (Farmer for Fermi, Baker for Bohr) (see Figure 3).[75] But, in the main, even those inclined to the view that nuclear secrets might properly be conveyed to *all* the Allies accepted the necessity of secrecy. Robert Serber enthusiastically took on the job of intentionally spreading disinformation about activities 'on the hill' in local saloons – thought by the military to be crawling with spies – though he came away impressed with the difficulty of getting anyone even to listen.[76]

The problems that some scientists saw with military control lay, rather, with the organizational forms thought to be preferred by the military, and with the effects these forms might have on the technical programme's internal flow of information, the effective distribution of human and material resources, the morale of the laboratory and the effective commitment of its scientific members. Military forms of organization were not only unpleasant and antithetical to scientific custom, they also – in the opinion of these scientists – posed potential obstacles to the speed with which the Bomb could be built, if not to its eventual construction. It was in these connections that significant features of Oppenheimer's charisma offered substantial solutions to organizational problems.

By midsummer 1943, the basic organizational structure of the technical programme was in place. There were four technical divisions: Theoretical, Experimental Physics, Chemistry and Metallurgy, and Ordnance and Engineering. Between autumn 1943 and summer 1944, all the divisions expanded. By 1944, the largest division was Ordnance and Engineering, headed by the Naval Ordnance specialist, Captain William S. ('Deak') Parsons.[77] Within each division, groups and sections, usually of between five and ten people, worked on specialized problems.[78] Group leaders reported to Division leaders who reported in turn to Oppenheimer. The organization was thus functionally differentiated and had a formal hierarchy and division of labour. After summer 1944, the laboratory grew faster and its division of labour became more complex.

In August 1944, there was an important shift in work dedicated to methods of bomb assembly and detonation. The so-called implosion method had originally been a marginal effort but, because of changing understandings of the rate of neutron emission in plutonium, and because of the nature of the material being produced at Hanford, implosion came to be given much higher priority than the originally envisaged, but relatively slow, gun method. Parsons' Ordnance Division continued to work on the gun method for the uranium bomb, but personnel were taken from this

FIGURE 3
A Page from the Military Police Identification Book



Hans Bethe and his wife, Rose, are shown at the top. The bottom left photograph is of Priscilla Greene Duffield, Oppenheimer's personal secretary. Residents and personnel were also fingerprinted.
Source: LANL 78-9706.

and other divisions to create a new Explosives (X) Division, under the chemist George Kistiakowsky, to work intensively on the high explosive implosion method for the plutonium weapon.[79] From this point, the engineering work of Los Alamos was bifurcated in a major way.

In July 1945, the Explosives Division made up about 20% of the laboratory's workforce and was itself divided into 16 groups, many of these themselves subdivided into specialized sections. Its personnel were also geographically dispersed, some work being done in the main technical

area, while a large number carried out high explosives work at the outlying S-Site, and at Anchor Ranch testing ranges.[80] Similarly, the Experimental Physics Division was split into gun and implosion programmes, the new R Division for gun research and the G (or 'Gadget') Division for implosion work. By July 1945, the implosion work under G and X Divisions engaged the efforts of almost 35% of the laboratory's personnel. The other large divisions were Chemistry and Metallurgy, the Machine Shops, and Administration. Personnel demands created by the as-yet-uncertain implosion method, as well as the shift towards engineering in the final phase of the project, led to the large increase in the number and proportion of enlisted SEDs in the laboratory. By the end of the War, the laboratory was a formally hierarchical, highly differentiated and instrumentally directed scientific-engineering organization.

Such an organization, developing so quickly and under such ambiguous control, posed all sorts of mundane problems of direction and management. Oppenheimer was, of course, its Scientific Director, and therefore the last court of appeal for the scientists, though the precise nature of his authority *vis-à-vis* General Groves remained uncertain. But even in an organization as problematic, contested and rapidly changing as Los Alamos there was much institutional routine, as well as personal authority. Overall coordination was effected by a framework of committees, while formally structured meetings aimed to produce integration at a number of points in the organizational hierarchy. At the top of the structure was the Governing Board. This was established to advise Oppenheimer, but it came to make policy itself.[81] The Board consisted of Division leaders, key administrative personnel and those with important liaison functions. Through its members it received reports from the Divisions of Los Alamos itself, as well as liaison information from the other sites of the Manhattan Project. The Governing Board was the main policy-making and coordinating body of Los Alamos and it set technical policy, for example, in giving priority to implosion. The Board also dealt with a wide range of administrative problems, from salary scales to relations with the Army post. Indeed, in June 1944, Oppenheimer decided that the Board was too burdened with 'non-technical' matters and disbanded it, dividing its work between a new Technical Board and an Administrative Board.

Technical discussions in the Governing Board, due to its wide range of responsibilities, were necessarily at a fairly high level of abstraction from day-to-day technical work. The potentially problematic gap between the particular and the general was meant to be mitigated by the Coordinating Council, including group as well as division leaders. The Council's task was geared towards integrating specialized technical work at a greater level of participation and concrete detail. The next level of intended integration was the Colloquium, a weekly forum open to all scientifically qualified staff members. At Colloquia the staff heard reports from the different parts of the laboratory and were expected to contribute suggestions to work outside their own special fields of competence. It was in the Colloquia that scientific members were supposed to be put in the big picture of what was

going on in different parts of the laboratory: the establishment of the Colloquia responded at once to concerns over morale, over hierarchical authority and over the efficient flow of technical information and the efficient mobilization of expertise.[82]

Formally, the laboratory was hierarchically organized, its work integrated at the top and directed from the top. However, the Colloquium, in particular, expressed and institutionalized an egalitarian or collegial order whereby integration and cross-communication took place at lower levels of the organization, and through face-to-face interaction rather than through formal, written reports. While the laboratory's organizational structure into Divisions and Groups was hierarchical and functionally differentiated, there were competing impulses in the day-to-day operation of the laboratory. The social order of Los Alamos was in tension between competing definitions of the situation. To what extent was the laboratory to be hierarchical and instrumental, and to what extent egalitarian and academic? This tension was never resolved, and these opposing desires for the organizational and moral order of Los Alamos competed for the duration of the project.[83]

As the hub and nerve-centre of the Manhattan Project, Los Alamos was created as a means of both *enhancing* and *controlling* the flow of technical information. Los Alamos was to be both a microcosm of Scientific Community and a place where the scientists could be controlled and their work formally monitored. Before the creation of the Los Alamos laboratory, and with the hope of specifying requirements for the new weapon, research on fast neutron reactions was carried out under the umbrella of the Office of Scientific Research and Development (OSRD). By 1942, research sites were dispersed throughout the country. Initially under the directorship of the theoretical physicist Gregory Breit, problems with his temperament and administrative abilities led to his resignation and replacement by Oppenheimer. Working under Oppenheimer, it then fell to the experimentalist John Manley to coordinate the research taking place at the Carnegie Institution, the University of Minnesota, the University of Wisconsin, the Metallurgical Laboratory at the University of Chicago, the University of California at Berkeley, Cornell University, Stanford University and the Rice Institute, and to relay information back to Oppenheimer's theoretical group at Berkeley. Manley spent summer 1942 shuttling between these sites. But it did not take long for him, Oppenheimer, A.H. Compton and J.B. Conant to decide that this was no way to run a project. They recommended the centralization of fast neutron research in a single site.[84] Its isolation and geography made northern New Mexico a strong candidate for the site. Jemez Springs was suggested by the man from the Army Corps of Engineers sent to survey possible sites, but was turned down by Groves and Oppenheimer. It was Oppenheimer who suggested nearby Los Alamos.[85] Oppenheimer knew the area well from boyhood holidays which he regularly spent at a ranch in the neighbouring mountains. This relationship of Oppenheimer to northern New Mexico – his knowledge of and love for the land – was significant, and was considered to

be so by many participants who knew of his personal connection to the place. It was felt to give Oppenheimer a kind of symbolic ownership. It was *his* place in that sense, as in others.[86]

One of Breit's legacies in the OSRD-run uranium project was a system of organizational 'compartmentalization'. This involved strict regulation of the flow of information between sites and between participants in the project. As the historian Stanley Goldberg put it, 'each task was to be performed in a cubicle isolated from all the other cubicles'. Breadth of knowledge increased with position in the administrative hierarchy, but 'virtually no one save for those in the office of the overall director would have a perspective on the entire project'.[87] Compartmentalization was widely judged by scientists to be a routine feature of military organizational patterns, to be informed by understandable security considerations, but to be potentially destructive of the project's goals. Accordingly, compartmentalization was offensive to many of the scientists, who insisted upon both the civility and the technical necessity of free, informal communication among specialized researchers. Professional networks and friendship networks often mapped on to one another. Compartmentalization formalized the structure of communication and imposed an alien hierarchy.

So said the physicist Leo Szilard late in 1942, then working at Chicago's Metallurgical Laboratory: compartmentalization caused 'strain' and 'embarrassment' between 'old friends' such as himself and Teller when they found themselves ordered to withhold information from one another. It 'poisons the discussion' even over areas not explicitly restricted. According to Szilard, this interference with scientists' friendship relationships led to misunderstandings in technical communication.[88] Compartmentalization simultaneously distorted proper moral order *and* effective technical order. Its incivilities caused technical inefficiencies. The tensions over secrecy in the Manhattan Project were, therefore, conflicts over the nature and shape of social bonds and over proper organizational forms. These were conflicts about whether the social order was to be informal and collegial or formal and hierarchical or, more precisely, what combination of the two it should be. Technical compartmentalization both presupposed and produced the formalization and differentiation of social relationships.

Compartmentalization was, to be sure, driven by security concerns. General Groves said that 'Compartmentalization of knowledge, to me, was the very heart of security'. But it also concerned control over work, and Groves clearly envisaged compartmentalization as providing a means of imposing elements of military-industrial work-discipline. The system, he said,

> ... not only provided an adequate measure of security, but it greatly improved over-all efficiency by making our people stick to their knitting. And it made quite clear to all concerned that the project existed to produce a specific end product – not to enable individuals to satisfy their curiosity and to increase their scientific knowledge.[89]

Compartmentalization defined scientists' work in instrumental or utilitarian terms and this, in turn, defined a structure of control and authority. Instrumental goals were formulated at the highest rungs of the Manhattan District, and the scientists were to labour to realize these externally determined ends.[90] Rabi, who consulted for Los Alamos while primarily working on radar at MIT, recognized the close connection between hierarchy, instrumentality and control over information. Angered that the Navy requested that he develop a device without deeming it necessary to tell him its tactical uses, he lectured his military superiors: 'Now look, you bring your man who understands radar, you bring your man who understands the navy, who understands aircraft, you bring your man who understands tactics, and then we'll talk about your needs'. According to Rabi, 'That was a pretty hard thing for them to swallow', but it 'started a relationship with the navy that was important to us ... [W]e came to be friends with great mutual respect'.[91] Rabi sought to establish the researchers as equal partners of the military, rather than employees or technicians, and access to information was essential in this.

'Cometh the Hour, Cometh the Man': Making a Natural Leader

We have now briefly described several closely related organizational predicaments confronting Los Alamos scientists: was Los Alamos characteristically a civilian-scientific or a military place? What organizational forms were most appropriate to executing its intended technical tasks? What forms were most conducive to the efficient flow of information and the maintenance of high morale? Answers to these questions were unstable: in important areas of Los Alamos life you could not appeal to the routines appropriate to a specific kind of place just because the identity of Los Alamos as a specific kind of place was always uncertain and potentially contested. It was in this context that Oppenheimer's charisma proved such a powerful organizational resource for his scientific associates.

The first thing to appreciate about Oppenheimer and his charismatic authority is that few, if any, colleagues predicted his success as a leader of a large organization, and several had forebodings of failure. If Oppenheimer was, as Rabi and others said, a 'natural leader', then his natural leadership had to be both individually and collectively achieved. (Indeed, Rabi, as we will note, was one of the people who helped Oppenheimer achieve natural leadership.) Although many commentators noted Oppenheimer's prewar cosmopolitanism, suavity and personal generosity (supported by his family's enormous wealth), his charm and intensity, and the tendency of his students to ape his every mannerism, nothing about that personality led anyone to find his appointment to the scientific directorship of Los Alamos anything but surprising.[92] John Manley was 'astonish[ed]' by the 'rapid transformation of this theorist ... into a most effective leader and administrator' as the laboratory's work got underway. Manley wondered whether Groves might have had some intuitive sense of Oppenheimer's capabilities

'in areas for which his previous activity had given so few objective hints for the future'.[93] Luis Alvarez relates that E.O. Lawrence supported Oppenheimer's appointment as Director 'when some of Robert's closest friends were skeptical. "He couldn't run a hamburger stand", I heard one of them say'.[94] Rabi himself said that Oppenheimer 'was absolutely the most unlikely choice for a laboratory director imaginable': 'He was a very impractical sort of fellow. He walked about with scuffed shoes and a funny hat, and, more important, he didn't know anything about equipment'.[95]

Samuel Allison, a physicist from the University of Chicago, was called in by Oppenheimer to advise on organization. He was appalled by the state of Oppenheimer's preparation:

> Just before Christmas of 1942, Oppenheimer asked me to come and help plan the preliminary layout. . . . On the Mesa he and I sat down and planned the laboratory. He showed me what he called an organization chart for a hundred personnel. I looked at it and felt sure that something was wrong, but I didn't know what. The best I could do was to poke at random, 'Where are the shipping clerks?' I asked. He gave me a thoughtful sympathetic look, 'We're not going to ship anything', he answered. I completely underestimated the size of the installation but not so much as he did.[96]

Manley was also concerned that a definite organizational structure had not been settled. He found Oppenheimer 'about as unresponsive to such mundane matters as an experimentalist would expect a theoretical physicist to be, perhaps more so'. At one point, Manley's urging seemed to have had an effect. In January 1943, exhausted from work and travelling, Manley visited Oppenheimer in Berkeley: 'I had scarcely opened the door when he shoved a paper at me, saying "Here's your damned organization chart!" '.[97] However, this did not put an end to administrative problems. When Wilson made a visit to Los Alamos in March, to inspect the construction, he found the site in a very poor state and building work behind schedule. Following the trip, Wilson and Manley met with Oppenheimer in Berkeley, to inform him of the project's 'state of chaos':

> Manley and I nagged at Oppy all day about his indecisiveness. We insisted that he had decisions to be made. . . . We wanted to know who was to be in charge of what, not just vague talk about the scientific problems nor even vaguer ideas about democracy. There were immediate problems to be faced, and from our point of view Oppy was not facing up to them.[98]

When the two experimentalists pressed him to get on with organizational planning,

> Oppenheimer became extremely angry. He began to use vile language, asking us why we were telling him of these insignificant problems, that it was none of our business, and so on. Both of us were scared to death. We were frightened because, if this was the leader and, if the leader was going to have a tantrum to resolve a problem, then how was anything going to get sorted? So we withdrew, John and I, and discussed some more, and decided that we would take more initiative and not look for so much leadership from Oppy.[99]

Rabi agreed with Wilson that Oppenheimer

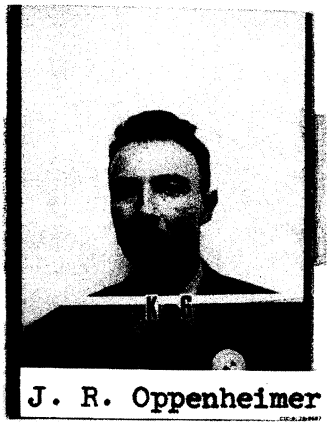
> ... was not a strong character. He was indecisive, and definitely not a fighter. If he couldn't persuade you, he'd cave in, especially to group opposition. Groves, on the other hand, could provide him with strong backbone in the form of consistent policy.[100]

General Groves himself had grave misgivings about Oppenheimer as Director, even while acknowledging that Oppenheimer 'knew everything that was then known' about the relevant physics: 'My own feeling was that he was well qualified to handle the theoretical aspects of the work, but how he would do on the practical experimentation, or how he would handle the administrative responsibilities, I had no idea'. Oppenheimer just didn't have any administrative experience. Moreover, lacking a Nobel Prize – or, indeed, any single towering scientific achievement to his name – there were doubts about his authority. Lawrence at Berkeley, Urey at Columbia and Compton at Chicago were all Nobelists: 'There was a strong feeling among most of the scientific people with whom I discussed the matter that the head of Project Y should also be one'. Because of this attitude, and 'because of the prevailing sentiment that he would not succeed, there was considerable opposition to my naming him'.[101] Groves just couldn't find anyone better.[102]

Worried about Oppenheimer's Ivory Tower tendencies, Groves insisted that he take on an industrial scientist as 'No. 1 assistant'. It was Edward U. Condon (from Westinghouse Research Laboratories) who was meant to be 'the one to establish the working rules and the administrative scientific rules in the establishment, while Dr. Oppenheimer was thinking about how was the actual scientific work to be done'.[103] As it turned out, however, Condon abruptly resigned in April 1943 – interestingly, citing obsessive concerns with compartmentalization and security – and Oppenheimer took on primary responsibility for both 'administration' and 'scientific work'.[104] Oppenheimer himself was soon daunted by the job he had taken on, and by summer 1943 the nuclear physicist Robert Bacher found him 'depressed with the magnitude and the complexity of the director's task'. He told Bacher he could not go through with it, but Bacher's advice was simple: 'Oppenheimer had no alternative, for no one else could do the job'.[105] Naïve, inexperienced, otherworldly, obscure, oracular, emotional, quirky, and sometimes acerbic, Oppenheimer was, so to speak, far from a natural 'natural leader' of a large community of scientists and engineers.

Among the many handicaps he brought to his Directorship, the statutory authority he derived from being General Groves' appointee carried with it its own problems. Early on, Oppenheimer was himself enthusiastic about the idea of Los Alamos as a military installation and its scientific inhabitants as military personnel. In that respect, he was, and was seen to be, the General's man, and Oppenheimer's evident early expressions of keenness for militarization may have influenced Groves' decision to appoint him. Groves' biographer, indeed, suggests that

FIGURE 4
Oppenheimer's Photograph for his Los Alamos Identification



Source: LANL 88973.

> Oppenheimer's unique compliance with what for him was one of the linchpins of bomb lab planning may have been the deciding, if not the overwhelming, consideration in determining that the physicist was 'the best man, the only man' for the director's job.[106]

Groves intended, and Oppenheimer agreed, that the higher-ranking scientists at Los Alamos 'should be made members of the Army with officers' rank'.[107] 'I would have been glad to be an officer', Oppenheimer said, and he fancied himself in khaki, visiting the Presidio in San Francisco to begin enrolling as a Lieutenant Colonel, even taking his enlistment physical examination. 'He'd become very patriotic', Wilson observed.[108] Alvarez noted how quickly 'the unworldly and long-haired prewar Robert' metamorphosed in appearance as well as in manner. Robert and Jane Wilson thought that the length of Oppenheimer's hair reflected the various rôles which he assumed throughout his life. So, as a 'young radical professor at Berkeley . . . his hair was all little black curls. And then he was much more subdued at Los Alamos, the curls were not so curly'. According to Alvarez, at Los Alamos Oppenheimer's 'hair-cut was almost as short as a military officer's . . .' (see Figure 4).[109]

But Oppenheimer quickly found himself almost alone among Los Alamos scientists – or those he hoped to recruit – in his attitudes to militarization. And a situation developed in which the scientists needed Oppenheimer to act as a champion – against Groves and against militarization. So to speak, they needed Oppenheimer to be Oppenheimer (see Figure 5). The first time Robert Serber met Groves was in Oppenheimer's Berkeley office in October 1942. The General's behaviour there stuck in his mind:

> Groves came in with a colonel in tow. . . . He walked in, unbuttoned his tunic, took it off, handed it to [the colonel], and said, 'Take this and find

FIGURE 5
Contrasting Personalities

Oppenheimer and Groves are shown revisiting the site of the Trinity atomic bomb test at Alamogordo, NM. In front of them is what remained of the tower on which the bomb was mounted.
Source: LANL PUB 80-353-1.

> a dry cleaner and get it cleaned'. Treating a colonel like an errand boy. That was Groves's way.[110]

And a colonel under Groves is what Oppenheimer initially wanted to be. Rabi was one of the physicists' leaders in putting steel in Oppenheimer's backbone. Rabi had already seen how these things played out at MIT's Radiation Laboratory and didn't like what he saw. He and Bacher both threatened to dissociate themselves from the project if military forms of organization were adopted. When, in January 1943, they winkled out of Oppenheimer the information that he had in effect already agreed that the laboratory be under military control, 'with scientists taking commissions in the Army, we were horrified'.[111]

Their main objection was that the formal hierarchy of the military would damage scientific communication, including scientists' freedom to criticize superiors in rank. They told Oppenheimer 'that lieutenant colonels didn't have anything to say, and that if he tried to establish a scientific laboratory [with] a hierarchy that was composed of military people, that it just plain wouldn't work'. Rabi said: 'We *knew* the military. We'd been engaged in making military things, had the military around us. We knew it wouldn't work. In the first place, none of us would come'.[112] Oppenheimer wrote to Conant what he had been told by McMillan, Bacher and Alvarez, as well as by Rabi:

> That the Laboratory must demilitarize: the arguments here were first that a divided personnel would inevitably lead to friction, and to a collapse of Laboratory morale, complicated in our case by social cleavage, and, more important, that in any issue in which we were instructed by our military

> superiors, the whole Laboratory would be forced to follow their instructions and thus in effect lose its scientific autonomy.[113]

From the perspective of the military establishment, Rabi's 'determined position' in demanding civilian laboratory control was a serious nuisance. After the War, General Groves claimed that 'Dr. Rabi's influence was such that many of the troubles in the operations of the Los Alamos laboratory stemmed from his original stance'.[114]

Rabi played Aristotle to Oppenheimer's Alexander, tutoring him in his new rôle as moral and instrumental leader of the Los Alamos scientific community. 'Rabi made Oppenheimer more practical', Bethe recalls: 'He talked Oppie out of putting on uniform'. 'Without Rabi', in Bethe's view, the project 'would have been a mess'.[115] That is one indication of the extent to which military and scientists' assessments of organizational efficiency diverged. It was the opposition of men like Rabi and Bacher that constituted Oppenheimer as the kind of man who could and would effectively press his military superiors for a civilian form of organization. Oppenheimer wrote to Conant that his own 'efforts to persuade the men' that militarization would not entail the loss of scientific autonomy had been 'unsuccessful'. He warned that 'the solidarity of physicists is such' that, if the laboratory remained military, not only would those from MIT not come, but that many others would back out of the project.[116] Conant and Groves acquiesced, and the Los Alamos Scientific Laboratory became (as it remains) a formally civilian organization, under the management of the University of California.

However, throughout the War its status was unsure. Conant's and Groves' letter to Oppenheimer, formalizing the duties of the Scientific Director, stipulated that the laboratory would become a military installation once the work passed from scientific to large-scale ordnance work and the 'handling of highly dangerous material'.[117] Bacher's letter accepting his position included his resignation effective the date the laboratory was militarized. This change in the status of the laboratory never materialized: the laboratory shifted from scientific to ordnance work without formally changing its status or control. The ambiguity was also inherent in the position of Oppenheimer, who was statutorily responsible to General Groves. The definition of the situation, throughout the War, was contested and, as that contest played out, so Oppenheimer became the personal embodiment of the virtues of scientific forms of organization, the paladin of the physicists against the generals and the G-men. During a Coordinating Council meeting, the Army's security officer, Colonel Peer de Silva, complained of the lack of respect shown him a by young SED who sat on the edge of his desk. Oppenheimer replied that 'In this laboratory anybody can sit on anybody else's desk', and de Silva, according to the account, 'was slammed'.[118] Oppenheimer was good to his word – a young scientist found that 'His office was always open and each of us could walk in, sit on his desk, and tell him how we thought that something could be

improved'.[119] In a situation of competing and conflicting norms, Oppenheimer gave voice to, and signalled in more subtle ways, a definition of the situation which made it legitimate to act in ways contrary to militaristic or bureaucratic codes of behaviour. The charismatic vocabulary of extraordinary personal openness, collegiality and sympathy attached to Oppenheimer plausibly traces back to just this organizational setting. His scientific colleagues helped Oppenheimer be the kind of person who embodied their values, just because there was no one else who could effectively ensure limits on the militarization of the laboratory.

Interestingly, when the laboratory became increasingly 'weaponized', and organizationally routinized, in late 1944 and throughout 1945, Los Alamos scientists did *not* generally link these developments to Oppenheimer's personal leadership. Having associated Oppenheimer with collegiality and consensus-building rather than coercion, they continued to do so, attributing countervailing tendencies to others in the organization. This dissociation was itself, on reflection, seen partly as a product of Oppenheimer's special personal gifts, the apparent effortlessness of his power to persuade. John Manley, Oppenheimer's right-hand man in setting up Los Alamos, claims that Oppenheimer 'had no great reluctance about using people', but that he could make this an 'enjoyable experience' in which one found oneself freely collaborating: 'It was like a ballet . . . each one knowing the part and the role he's playing, and there wasn't any subterfuge in it'.[120] But Oppenheimer's attributed ability to direct on the basis of consensus rather than coercion was also due to his institutional separation from instrumental functions. Enforcement of schedules and policy decisions was deputized down the line to division leaders such as Bethe, Kistiakowsky, Parsons and Bacher. Teller, a year and a half after the end of the War, described Bacher as 'a great administrator' who 'loves organization charts and loves reports in proper shape and [who] is completely devoted to priorities'. 'To Oppy', Teller said, 'he was the ideal yes-man'.[121] Parsons, in particular, pressed Oppenheimer to create a committee with powers to enforce schedules and design decisions. The 'ruthless, brutal people' so empowered 'must feel that they have a mandate to circumvent or crush opposition above and below . . .'.[122] The so-called 'Cowpuncher Committee', chaired by Samuel Allison, was set up in March 1945 to 'ride herd' on these final stages of the implosion programme. Allison, also head of the Technical and Scheduling Conference, became Oppenheimer's 'whip' to enforce work-schedules in the final engineering stages of the programme.[123] Deputizing such responsibilities meant that Oppenheimer himself was largely associated not with coercive, instrumental and routinizing tendencies in the organization, but rather with attempts to generate solidarity through face-to-face interaction. The historian Lillian Hoddeson has commented on the co-existence at Los Alamos of mission-directedness with a strong 'sense of free inquiry'.[124] It was an aspect of Oppenheimer's charismatic authority among the scientists to be largely dissociated from the former and given credit for the latter.

Omnipresence and Omniscience: Oppenheimer Knows It All

So one practical organizational context in which Oppenheimer's charisma cashed out in functional terms was concern over the implementation of vertical forms of authority, a command structure subjecting scientists to military discipline and control. Yet military control was also feared to work its effects on horizontal forms of organization, blocking the free flow of information between specialized colleagues. And here, too, the interactional constitution of Oppenheimer as charismatic featured importantly. Just as Oppenheimer was personally dissociated from weaponization, and its attendant organizational forms, so his special gifts of body and mind were specially invoked in the context of those features of Los Alamos scientific life that made for solidarity and integration.

The Colloquium, more than any other local organizational form, was understood both to express and to enable that solidarity and integration. Los Alamos scientists were, almost without exception, highly concerned that each should have an overall sense of how their specialized work fitted into the specialized work being done by others, and into the instrumental goals of the laboratory as a whole. Information, they reckoned, should circulate throughout the laboratory as efficiently as practicable. Concern with information flow was heightened due to its contrast and conflict with the system of compartmentalization, and scientists fought a continual battle against security-driven compartmentalization, so far as it was possible to do so. However, their struggle was also with the inevitable *de facto* compartmentalization that inevitably arises in all complex organizations.[125] The solution was simply to provide for more face-to-face and free interaction, to encourage meetings and discussions at as many levels as possible and among as many specialized work groups as possible. This is how and why the weekly Colloquium for all staff members assumed such importance. The Colloquium was considered important as a means of disseminating information, and also as a way of creating solidarity and face-to-face accountability. In negotiating the organizational forms in and through which technical information would flow, Los Alamos scientists were also trying to specify and fix the moral order of the laboratory, what kind of place it was.

From the start, the idea of the Colloquium ran up against the facts of compartmentalization and the considerations recommending substantial barriers to information flow. The idea of regular Colloquia for the whole staff was proposed by Bethe at a Governing Board meeting in early May 1943. While the Board endorsed Bethe's recommendation, appointing Teller to organize the meetings, it was added that the Colloquia should be 'carefully supervised'.[126] In allowing for Colloquia, it was understood that, due to its geographical isolation, compartmentalization could be somewhat relaxed at Los Alamos. The laboratory was to be one cell within the system, and its internal freedom would be made up for by the rigid policing of its external boundaries: Manhattan Project information was to come into Los Alamos (as needed and requested) but none was to go out (unless General

Groves deemed it fit to do so). However, the application of compartmentalization to particular cases was always problematic and contestable. So Groves told Oppenheimer that he and the post commander Colonel Ashbridge were 'disturbed' about Oppenheimer's review of the whole programme given at the first Colloquium meeting. The resulting compromise was the classification of personnel into staff-members (possessing scientific first degrees) and non-staff (lacking such qualifications), the Colloquium being open only to the former. This distinction effectively excluded most of the SEDs, and also most civilian technicians. The compromise also involved staff members being vetted in a formal 'vouching' procedure. This ostensibly formal procedure in fact relied only on Oppenheimer vouching for the senior scientists while others were passed by statements from three laboratory employees.[127] There were to be restrictions on the discussion in Colloquia of the work of other sites, such as Oak Ridge, Hanford, Chicago and Columbia. But how these restrictions were to operate was always open to dispute. Teller, for example, argued that restrictions were 'contrary to the spirit' of the Colloquium. He noted the 'vague[ness]' of the criterion that information had to be 'justified by its connection with the work here', especially when one purpose of the Colloquium was to encourage new ideas at Los Alamos.[128]

Compartmentalization was not only produced by Groves' requirements for secrecy. Scientists were at the same time grappling with the *de facto* compartmentalization which, we have noted, arose from the sheer complexity of laboratory work. For example, it is ironic that the Liaison Committee, established in November 1943 to coordinate communication with other Manhattan Project sites, was by January 1944 forced to turn its attention to the problem of so-called 'internal liaison'.[129] The problems of communication, faced in 1942 due to the geographical dispersion of the fast neutron laboratories, and to which Los Alamos itself was to be the solution, were now being reproduced *within* Los Alamos due to specialization and its accompanying organizational differentiation. This had, in fact, been an abiding concern. Already by November 1943, Oppenheimer said that 'he felt the laboratory was now so complicated that he should call to the attention of the board the problem of relations between divisions'.[130] That concern was never eliminated. Rather, Los Alamos existed in tension between, on the one hand, the expanding, specializing and compartmentalizing tendencies endemic to complex organizations and, on the other, attempts to assert the importance of the personal, the informal and the collegial.

Despite restrictions, the Colloquium was a symbolic gesture towards an informal, a face-to-face and an egalitarian conception of legitimate order at scientific Los Alamos. Gathering together, in one hall, personnel from all the different Divisions and their Groups served to render visible the organization's intellectual and social coherence, to *display* scientific Los Alamos to its inhabitants. The members of the Governing Board were sensible that the Colloquium was something more than a means of disseminating information. On a mundane level it was suggested that the

first Colloquium be used to re-instil 'habits of work' after the chaotic first month spent setting up buildings and equipment.[131] Oppenheimer had defended the concept to Groves on the grounds of 'effectiveness, morale and security'.[132] Bacher, Head of the Experimental Physics Division, observed that the 'most important value of the colloquia' was 'integration'.[133] And, quite generally, epistemic integration and information flow were seen as crucial elements in 'morale'.

The Berkeley philosopher, David Hawkins, recruited to coordinate liaison between the laboratory and the post, wrote that 'The colloquium was less a means of providing information than an institution which contributed to the viability of the Laboratory and to maintaining the sense of common effort and responsibility'.[134] And General Groves agreed that the Colloquium 'existed not so much to provide information as to maintain morale and a feeling of common purpose and responsibility'.[135] Bethe said that in the Colloquia 'everybody in the laboratory felt a part of the whole and felt that he should contribute to the success of the program'.[136] Years after the Manhattan Project ended, Victor Weisskopf praised the Co-ordinating Council in similar terms: it contributed to the 'morale of the place' because it gave you 'the feeling that you knew what was really going on'. Even if this feeling was an illusion, 'the point is you had the feeling and that was of such importance'. All of this he attributed to Oppenheimer who, as chairman of the committee, 'did that extremely well'.[137]

Weisskopf similarly ascribed the Colloquium and its integrating effects personally to Oppenheimer: 'Oppenheimer insisted on having these regular colloquia against the opposition of the security-minded people, who wanted each man only to know his part of the work. He knew that each one must know the whole thing if he was to be creative'.[138] Bethe wrote in the same vein: 'Oppenheimer had to fight hard for free discussion among all qualified members of the laboratory. But the free flow of information and discussion, together with Oppenheimer's personality, kept morale at its highest throughout the war'.[139] Alvarez insisted that 'The laboratory's fantastic morale could be traced directly to the personal quality of Oppenheimer's guidance'.[140] The physicist Rudolf Peierls also credited the comparative openness of discussion at Los Alamos, and the laboratory's consequent high morale, to Oppenheimer personally: 'Inside the laboratory he was able to maintain the completely free exchange of information between its scientific members'.[141]

But Oppenheimer was not only credited with creating integrative devices such as the Colloquium. He was importantly credited with being a site and source of integration *in himself*. One after another, Los Alamos scientists draw attention to Oppenheimer, in effect, 'knowing it all'. He was, it is recurrently said, the one person at Los Alamos who had a total vision of all that was going on there; the one person who was in a position to have an overview; and the one person with the intellectual range and ability to possess a global view in fact. So Bethe recalls that Oppenheimer

> ... knew and understood everything that went on in the laboratory, whether it was chemistry or theoretical physics or machine shop. He could keep it all in his head and co-ordinate it. . . . There was just nobody else in that laboratory who came even close to him. In his knowledge.[142]

That embodied integration was both mental and physical, conceptual and moral. Oppenheimer symbolically integrated the laboratory by his physical circulation through it, visiting meetings in theoretical physics, experimental physics and metallurgy. Many commentators insisted on the importance of his physical presence – as a symbol of coherence, as a display of concern and even as a hard-to-articulate causal element in the solution of technical problems. Some commentary, indeed, ascribes Oppenheimer's skill at integration to the circumstance that he just knew an enormous amount of the relevant physics and, more generally, that he had a grasp of a greater range of sciences than anyone else at Los Alamos. He could walk into a technical discussion in an area about which he might be presumed ignorant and make a decisive intervention, if not because of his factual or theoretical knowledge, then because of his ability to cut incisively to the heart of any kind of problem. So, it is said, Oppenheimer

> ... once joined a metallurgy session during an inconclusive argument over the type of refractory container to be used for melting plutonium. Although this was hardly familiar ground to a theoretical physicist, after Oppenheimer had listened for a time, he summed up the discussion so clearly that the right answer, though he did not provide it, was immediately apparent.[143]

But other commentary on Oppenheimer's intellectual scope, and its integrating power, is not so easy to understand in these terms, gesturing at the mental rôle of his physical presence. Weisskopf noted 'some almost super ESP kind of connection' by virtue of which Oppenheimer managed to be on the spot when and where exciting developments were taking place.[144]

> [He] was intellectually and even physically present at each significant step; he was present in the laboratory or in the seminar room when a new effect was measured, when a new idea was conceived. It was not that he contributed so many ideas or suggestions; he did so sometimes, but his main influence came from his continuous and intense presence, which produced a sense of direct participation in all of us.[145]

Weisskopf emphasized 'how tremendously important it was for the morale at Los Alamos ... if you come to the final experiment and the director is there'. Oppenheimer 'always went to the important discussions at seminars, in spite of his administrative load'.[146] This was a display of human concern and of personal involvement. Oppenheimer's presence was significant and it made a difference, both intellectually and morally. Wilson said: 'In his presence, I became more intelligent, more vocal, more intense, more prescient, more poetic myself'.[147] The head of metallurgical work at Los Alamos, Cyril Smith, recalled that an informal five-minute discussion with Oppenheimer was all it took to give 'the necessary perspective'.[148]

FIGURE 6
'Like a Good Host with his Guests': Oppenheimer at a Social Gathering in Fuller Lodge at Los Alamos

Source: LANL LAT-422.

Something potently intellectual was said to happen in face-to-face interaction with Oppenheimer that was hard to express, and that itself was not even wholly verbal. This guidance was not experienced as domination; rather, Oppenheimer was regarded as a facilitator, in Bethe's analogy, 'like a good host with his guests' (see Figure 6).[149] According to physicist Eugene Wigner, who worked at the University of Chicago's Metallurgical Laboratory during the War, the scientists at Los Alamos 'disliked being visibly directed. Oppenheimer understood that. He knew their strengths and weaknesses without asking and treated them with some sensitivity'.[150] Like an ideal early modern gentleman, he was considered to have mastered the art of seeming artless – effortless superiority, *sprezzatura*. He signalled his authority and communicated his expectations, as Wigner described it, 'very easily and naturally, with just his eyes, his two hands, and a half-lighted pipe'.[151] Oppenheimer was seen personally, and even physically, to catalyze the emergence of a unity and coherence that already existed in potential. Hawkins recalled that, if there was 'an incipient disagreement' during a Governing Board meeting, 'one would listen patiently to an argument beginning and finally Oppenheimer would summarize, and he would do it in such a way that there was no disagreement'. 'It was', he says, 'a kind of magical trick that brought respect from all those people, some of them his superiors in terms of their scientific record, brought them to acknowledge him as the boss.... So that's why ... there was never any disagreement that he was the leader of that enterprise'.[152] If, as Bethe said, 'wherever he was there was life and excitement', there was also *science* and the solution to dispersed and complex scientific problems.

Commentators sometimes talk in the same frame about Oppenheimer's omniscience, his omnipresence and his virtue. He was supposed not only to know all the science of Los Alamos, but also all the scientists – and not just the scientists. *Noblesse oblige* joined to the common touch: 'The indefatigable Oppie', a historian writes, 'knew not only all the scientists, but also most of the laborers by their first names'.[153] One young SED

wrote home to his parents after the bombing of Hiroshima, and described the 'informality' of Los Alamos which, he said, was 'unparalleled in any other organization that I have seen'. 'For example', he told them, 'several times Dr Oppenheimer has called me for something or other ... and every time, when I would answer the phone with "Doty", the voice at the other end would say, "This is Oppy" '.[154] Oppenheimer set the tone, and the laboratory followed his example. This moral example was communicated, crucially, through his physical presence. He showed himself, and by doing so, showed his concern and his integrative knowledge. It was a kind of squire's passage, as well as a display of mastery and of belonging to the place. A Group leader wrote:

> Each Sunday he would ride his beautiful chestnut horse from the cavalry stable at the east side of the town to the mountain trails on the west side of town greeting each of the people he passed with a wave of his pork-pie hat and a friendly remark. He knew everyone who lived in Los Alamos, from the top scientists to the children of the Spanish-American janitors – they were all Oppenheimer's family.[155]

This is possibly the kind of thing the English physicist James Tuck had in mind when he called Oppenheimer 'a great gentleman'.[156] When the Oppenheimers' daughter was born, the 'whole town' came to give its blessing: 'The sign "Oppenheimer" was placed over baby Toni's crib and people filed by in the corridor for days to view the boss's baby girl'.[157] Oppenheimer was also celebrated for his concern over the lives of scientists' spouses and families. Like a secular saint, he was praised for tending the sick and consoling the bereaved: 'A little aloof, but still a warm and comforting presence'.[158]

Knowing everything about Los Alamos meant knowing human and moral things as well as natural and technical things. The man who was supposed to be personally responsible for the integrative and morale-enhancing Colloquia was the same man who was supposed to know essentially everything about the lives of Los Alamos people, to know them as emotional and social beings as well as the bearers of scientific thought. In trying to say just this, Edward Teller concluded that the man whose power he was eventually to destroy was indeed a charismatic leader:

> Throughout the war years, Oppie knew in detail what was going on in every part of the Laboratory. He was incredibly quick and perceptive in analyzing human as well as technical problems. Of the more than ten thousand people who eventually came to work at Los Alamos, Oppie knew several hundred intimately, by which I mean that he knew what their relationships with one another were and what made them tick. He knew how to organize, cajole, humor, soothe feelings – how to lead powerfully without seeming to do. He was an exemplar of dedication, a hero who never lost his humanness. Disappointing him somehow carried with it a sense of wrongdoing. Los Alamos' amazing success grew out of the brilliance, enthusiasm and charisma with which Oppenheimer led it.[159]

The Conditions of Modern Charisma

Not everyone who was at wartime Los Alamos attributed the success of the project, the solidarity of the place or the morale of participants to Oppenheimer's charisma. Not all even agreed that he *was* a charismatic character, or even a very admirable one. That should be in no way surprising: all organizations contain diverse points of view on many organizational matters, and an organization as complex as wartime Los Alamos is no exception. We have already indicated that reference to the effective rôle of Oppenheimer's personality was most dense among the scientific inhabitants of Los Alamos and, from the evidence we have presented, especially among the physicists for whom Oppenheimer was, of course, a natural spokesman. That too should be no surprise, and we have described in detail the problems of organizational life that were of particular concern to the élite scientists, and whose effective resolution mobilized Oppenheimer's charisma.

Given the variability of individual temperaments and points of view, however, there would be no reason to expect that even all physicists at Los Alamos fell in with what seems clearly to have been the dominant view of the matter in their group. Seth Neddermeyer – the so-called 'father of implosion' – had notably difficult relations with Oppenheimer, and said so: 'I didn't look up to him. From my point of view, he was an intellectual snob'.[160] Teller, whose comments about Oppenheimer's charisma and organizational brilliance have already been quoted, has continued intermittently to project quite negative views of Oppenheimer's capacities and conduct, and his notorious denunciation of Oppenheimer at the 1954 security hearings speaks for itself. Rabi, whom we have also quoted on Oppenheimer's charisma and its consequential rôle in Los Alamos life, was, in other moods, capable of articulating less rosy views. Writing to Weisskopf about the latter's article on Los Alamos, Rabi said:

> I liked your piece about Oppenheimer in Los Alamos very much. However, I feel that the mood of exaltation which you and I had was not shared by everybody.... One forgets at this long distance of almost a quarter of a century the extreme tension which existed on the hill.[161]

Emilio Segrè was always unimpressed by the Oppenheimer phenomenon, regarding the prewar Berkeley cult of personality as so much embarrassing colonial naïveté:

> [A]t the time, he was considered a demigod by himself and others at Berkeley, and as such he spake in learned and obscure fashions.... Oppenheimer's loyal disciples hung on his words and put on correspondent airs.... I had the impression that their celebrated general culture was not superior to that expected in a boy who had attended a good European high school. I was already acquainted with most of their cultural discoveries, and I found Oppenheimer's ostentation slightly ridiculous.

Segrè thought that Enrico Fermi was the real thing; Oppenheimer a pale imitation of genuine cultural sophistication and intellectual depth:

> Perhaps I did not sufficiently conceal my lack of supine admiration for Oppenheimer, and I found him unfriendly, even if covertly, for a good part of my career, except when he wanted me to join his team at Los Alamos.

Nor was Segrè taken with either the authenticity or the content of Oppenheimer's politics, and he professed himself fed up with Oppenheimer's repeating 'with the faith of the true believer, the nonsense originating from Stalin's Cominform'. Interestingly, Segrè was one Los Alamos physicist who gave General Groves full credit for managerial nous, for his adaptability in dealing with a temperamentally difficult group of scientists, and, indeed, for coping sensitively with the crisis over compartmentalization: 'General Groves wiggled out of this impasse with good sense'.[162]

Groves learned to get along with Oppenheimer, getting the best out of him and using him deftly as a channel between military concerns and scientific sensitivities. He respected Oppenheimer but, understandably, he never recorded any sense that Oppenheimer was the 'boss', or that the success of the Manhattan Project was due to Oppenheimer's force of personality.[163] The final judgement of history on the matter has yet to be rendered but, as things presently stand, that judgement has undoubtedly been affected by the volubility of Los Alamos's scientific inhabitants; the public eminence to which they – and especially the physicists – rose after Hiroshima; the general individualism of our culture (if not its academic sociological sector) and its receptiveness to heroically-individualistic stories; and the sensibilities held in common between academic scientists and the academic historians who have tended to write about Los Alamos.

Yet not even all historians are willing to ascribe the effective working of Los Alamos to Oppenheimer's personal leadership, or to see the academic forms for which Oppenheimer spoke as crucial to the project's success. The historian of technology, Thomas P. Hughes, notably disputes the view that Los Alamos was primarily a scientific success story and, at least by implication, down-plays the pertinence of the academic point of view. For Hughes, it is vital to remember that the Manhattan Project was run by the Army Corps of Engineers, an organization that had much prior experience in managing large-scale technological-engineering enterprises. Other models were provided by such massive government projects as the Tennessee Valley Authority. Moreover, Hughes argues, the conjunction of scientists, engineers and managers had many precedents in such innovative American production companies as General Electric, Du Pont, Tennessee Eastman, Ford, Allis-Chalmers, American Telephone & Telegraph, and several others. And if Los Alamos was, so to speak, mission-control for the Manhattan Project, its success also depended upon the resolution of production problems at Oak Ridge and Hanford. It is true, Hughes acknowledges, that many Los Alamos scientists were highly uncomfortable with military forms of organization, and that their sensibilities had to be effectively addressed for Los Alamos to succeed, but that is not the same

thing as saying that Los Alamos *was* a scientific show, or that its Scientific Director was Los Alamos 'personified'.[164]

It is vitally important to recognize these other voices, and other interpretations, testifying about organizational life at Los Alamos. They do not diminish the story we have told, because we neither want to nor can represent any such story as exhaustive or definitive. This is how matters appeared to many influential Los Alamos inhabitants, and these are some of the resources they used to make sense of their situation, and to shape that situation into the forms and norms that they regarded as necessary to the project's success. This was their point of view; this was how they experienced and made sense of Los Alamos; and, accordingly, if we want to speak of the realities of Los Alamos life, this is an ineliminable aspect of those realities.

For all the specificity of our account, there is still something we want to retrieve from the Los Alamos story that has possibly general implications for understanding late modern technoscientific organizations. We have described wartime Los Alamos as a place marked by a high degree of normative uncertainty. That is to say, many inhabitants were unsure what sort of place it was, and therefore what norms pertained to behaviour in it. Other inhabitants were evidently sure, but their certainty about the form and norms of the place was confronted by the dissenting certainties of associates and neighbours on whose effective work they were dependent. And we have argued that it was in connection with this high degree of normative uncertainty that many scientific inhabitants recognized Oppenheimer's charisma and, indeed, helped Oppenheimer to become a charismatic leader of a specific kind.

We also pointed out that for Max Weber, and many modernity theorists following him, modernity is signed not just by 'disenchantment' but by the disappearance – or, at least, the attenuation – of charismatic forms of authority. What need of embodied personal authority – indeed, what room for such authority – is there in the world of rational bureaucracy and of rational science? But here, characteristically, such social theorists tend to mistake the part for the whole, absolute for relative change, ideal-typifications of bureaucratic and complex organizations for rich and detailed descriptions of what quotidian organizational life is actually like. Normative uncertainty is endemic, and it is important to remember that. It is arguable that the normative life of any organization – however routinized, however complex, however bureaucratic and however instrumental in its goals – cannot be adequately described by its organizational charts or by its inscribed rules and regulations.[165] Normative uncertainty does not exist only in the *absence* of rules. Formal structures, plans and rules are themselves generative of uncertainty.[166] In such a situation, the right thing to do has recurrently to be articulated and vouched for *by someone*.[167] Consequently, modern formal and instrumental modes of organization do not eliminate the conditions for charisma. On the contrary, formal organization may create a space and provide resources for the flourishing of charismatic authority.

Much circumstantial and anecdotal evidence suggests that the experience of intense normative uncertainty is becoming more and more characteristic of late modern technoscientific organizations and, accordingly, that charismatic forms of authority are becoming more and more important in the functioning of these organizations. Far from disappearing, charismatic authority thrives right at the heart of the technoscientific institutions that are busily making late modernity what it is. Journalists, business historians and participants themselves recurrently describe the dependence of such organizations on the embodied personal authority of key individuals. Quite often, as it happens, 'charisma' is the term of art they choose to use. So Wernher von Braun of the Marshall Space Flight Center during the Apollo period was said to be charismatic; so too was Akio Morita of Sony Electronics; so was Seymour Cray (the supercomputer engineer); so is Steve Jobs of Apple Computers, Larry Ellison of the Oracle Corporation, Daniel Cohen of the Centre d'Etude du Polymorphisme Humain (CEPH), and Kári Stefánsson of the controversial Icelandic genomics company deCode. Increasingly, in the late modern world, innovating organizations are marked by innovative institutional forms. Is this work or play? Profit-orientated or disinterested? Business or academic? How is working to be recognized and assessed? Where may it take place? Who may tell whom what to do, and with what consequences? Some of these questions, as we have seen, formed part of the texture of normative life and instrumental activity at wartime Los Alamos and, variously stressed and combined, some have heightened pertinence in the world of contemporary high-tech and biotech. Indeed, a few social scientists are now beginning to tell persuasive stories about charismatic authority in late modern technoscientific organizations, and about the uncertain norms and forms that call charisma into being and that make it consequential. John Law has interestingly described how his ethnographic work at the Daresbury SERC laboratory in England occasioned a change of his professional, sociological mind about charisma. Paul Rabinow's studies of the Cetus Corporation (in which the polymerase chain reaction was discovered), of the CEPH, and now of deCode, all refer in a matter-of-fact way to the embodied personal authority – the 'charisma' – of key actors, and all show how that authority counted as a partial solution to problems of normative uncertainty.[168]

The charisma to which present-day social scientists, and we, refer is not understood as a supernatural gift, nor is it likely to be for the late modern technoscientists who use such vocabulary. Nor, again, did Los Alamos participants who happened to use other vocabulary to describe Oppenheimer's embodied personal authority ascribe such attributes to divine grace. That is just to say that they, we and, of course, Max Weber, are moderns of a secular frame of mind and, in the culture we tend to share, such religious sentiments are not very common. We have described a secularized charisma, a charisma construed as an interactional accomplishment, not as a gift of grace. But, while the ultimate sources of charisma may differ between the followers of Jesus and those of Oppenheimer, the modern form is no less consequential, nor qualitatively different in its

organizational functions, nor even very easy to exhaust by talking about it in approved naturalistic modern modes. For all our ability to produce locally persuasive accounts of charisma as interactional accomplishment, charisma remains, far richer than any form of academic talk about it. Charismatic forms of authority, we suggest, are not marginal survivals of pre-modernity; they are right at the heart of those forces shaping the late modern world.

Appendix: Note on Sources

For reasons that should be obvious, it has been impossible to find any 'real-time' documentary commentary by wartime Los Alamos participants on Oppenheimer's personal characteristics and their organizational consequences. No one can be absolutely sure about what does *not* exist, but we are not alone in suspecting that there are no such documents. Alice Kimball Smith (a historian of wartime Los Alamos who was there herself) notes that 'personal journals were forbidden, and so far none has come to light' [Smith, op. cit. note 17, 37]. Written and oral communication containing any substantial reflections or details about Los Alamos life was very strictly prohibited for security reasons.

A letter home from Los Alamos by a young member of the Army Special Engineer Detachment immediately after Hiroshima testifies both to the swiftness with which that prohibition was lifted and to the ambiguity of its terms. Writing to his mother and father on 7 August 1945, Ed Doty said that 'We have been told, in a letter from Dr Oppenheimer, that we can mention in letters anything that has come out in the papers or over the radio. [But] the security policy on a lot of things is still unsettled, so I can't tell you much more than that right now'. Elsewhere, we quote that letter's sentiments about Oppenheimer's admirably informal personal manner, and this letter is, arguably, among the earliest such documentary testimony that survives in any form [Ed Doty to his parents, 7 August 1945; Los Alamos Historical Museum].

There is no reason for us to side-step the fact that the bulk of the anecdotal evidence pertinent to talking about Oppenheimer's special virtues and capacities – and, for all we can now tell, every single use of the word 'charisma' in reference to him – derives from the period *after* the successful conclusion of the Manhattan Project. Success, it might plausibly be said, retrospectively precipitates charismatic leaders with 20/20 hindsight. We can, however, think of charismatic failures, and technical success in constructing a working bomb does not equate to a universally sanguine view of the *range* of effects brought about by the Manhattan Project. A portion of the commentary we use interestingly emerges from the time surrounding the Oppenheimer security hearings and the withdrawal of his clearance by the Atomic Energy Commission in 1954, when he took on the rôle of the tragic hero of the American scientific establishment. The official judgement that Oppenheimer had defects of character generated an immediate and vigorous defence of that character by his friends and sympathizers.

The particular setting of *post hoc* accounting, therefore, may well have something to do with participants' stories about wartime Los Alamos but, on due consideration and with qualifications to be made, we are content to accept these stories as substantial reflections of genuine wartime sentiments. What we are faced with here is just a special (albeit a very interesting) version of an endemic historians' predicament. As a matter of practice – and perhaps even of principle – historians must always infer what happened and what was believed from testimony removed – in some degree and to some extent – from the scene of the happening or the believing. Samuel Pepys' diary, for example, is superbly direct testimony of the Great Fire of London, just on the condition you understand that he wrote it up some days after the Fire was over, that he relied massively on others' evidence of what happened, and that he believed the Fire was an act of arson by the Catholics.

Like other historians, we keep the possibility of *post hoc* rationalization or romanticization firmly in mind, and gauge the security of our inferences by using a set of robust and routine maxims for the evaluation of testimony: the directness of anecdotal

sources; their multiplicity, diversity and coherence; sources' possible reasons for misrepresenting matters; the plausibility of anecdotes, given the wide range of other information we have about what went on at Los Alamos. We can't do better than that, but then, historians never can.

Notes

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1. Bernice Brode, *Tales of Los Alamos 1943–1945* (Berkeley: University of California Press; bound from *LASL Community News* issues of 2, 16 & 30 June; 14 & 28 July; 11 & 25 August; 8 & 22 September 1960) quoting 2 June 1960, 8.
2. Kenneth Bainbridge, the director of the Trinity test, to Oppenheimer, as the first bomb went off, quoted in Lansing Lamont, *Day of Trinity* (New York: Atheneum, 1965), 242. Oppenheimer himself blandly wrote to his former high school teacher that 'You will believe that this undertaking has not been without its misgivings': Alice Kimball Smith and Charles Weiner (eds), *Robert Oppenheimer: Letters and Recollections* (Cambridge, MA: Harvard University Press, 1980) 297; but later told President Truman that he felt as if he had 'blood on his hands': quoted in Nuel Pharr Davis, *Lawrence and Oppenheimer* (New York: Fawcett Premier, 1969), 258, and James W. Kunetka, *Oppenheimer: The Years of Risk* (Englewood Cliffs, NJ: Prentice-Hall, 1982), 5.
3. Luis W. Alvarez, *Alvarez: Adventures of a Physicist* (New York: Basic Books, 1987), 152.
4. Quoted in Ruth Marshak, 'Secret City', in Jane S. Wilson and Charlotte Serber (eds), *Standing By and Making Do: Women at Wartime Los Alamos* (Los Alamos, NM: Los Alamos Historical Society, 1988), 1–19, at 5.
5. Françoise Ulam, interviewed in *The Day After Trinity: J. Robert Oppenheimer and the Atomic Bomb*, a documentary film by Jon Else (1980).
6. For example, Smith & Weiner (eds), op. cit. note 2, 247 (attributing the phrase 'a magic place' to Smith).
7. Robert R. Wilson, 'A Recruit for Los Alamos', *Bulletin of the Atomic Scientists*, Vol. 31 (March 1975), 41–47, at 41.
8. Smith & Weiner (eds), op. cit. note 2, 253.
9. Noted by Ulam, loc. cit. note 5.
10. Elsie McMillan, quoted in Richard Rhodes, *The Making of the Atomic Bomb* (New York: Simon & Schuster, 1986), 567.
11. Edward Teller, quoted in Davis, op. cit. note 2, 176.
12. Ulam, loc. cit. note 5.
13. Brode, op. cit. note 1, quoting 16 June 1960, 8, and 11 August 1960, 6; see also Davis, op. cit. note 2, 221–22.
14. Both Tuck and Rabi are quoted in Davis, op. cit. note 2, 185.
15. Hans Bethe, 'Oppenheimer: "Where He Was There Was Always Life and Excitement" ', *Science*, Vol. 155 (3 March 1967), 1080–84, at 1082; see also Smith & Weiner (eds), op. cit. 2, 264.
16. James Tuck, quoted in Davis, op. cit. note 2, 184–85.
17. Alice Kimball Smith, 'Los Alamos: Focus of an Age', in Richard S. Lewis and Jane Wilson (eds) (with Eugene Rabinowitch), *Alamogordo Plus Twenty-Five Years* (New York: Viking Press, 1971), 33–46, at 34.
18. See 'Appendix: Note on Sources' for discussion of the *post hoc* nature of most of our sources.

19. I.I. Rabi, 'Introduction', in Rabi et al., *Oppenheimer* (New York: Charles Scribner's Sons, 1969), 3–8, at 8; and another unnamed source, quoted in Howard Gardner, 'J. Robert Oppenheimer: The Teaching of Physics, the Lessons of Politics', in Gardner, *Leading Minds: An Anatomy of Leadership* (New York: Basic Books, 1995), 89–109, at 99.
20. Davis, op. cit. note 2, 181.
21. Edward Teller, quoted in Rhodes, op. cit. note 10, 570. The 'disaster' judgement is that of Robert Serber, 'Preface', in Richard Rhodes (ed.), *The Los Alamos Primer: The First Lectures on How to Build an Atomic Bomb* (Berkeley: University of California Press, 1992), xxiii–xxxiii, at xxx. See also Edward Teller and Allen Brown, *The Legacy of Hiroshima* (Westport, CT: Greenwood Press, 1962), 13: '[Oppenheimer] was the constituted authority at Los Alamos. But he was more: His brilliant mind, his quick intellect, and his penetrating interest in everyone at the laboratory made him our natural leader as well'.
22. Bethe, op. cit. note 15, 1082.
23. Robert R. Wilson, 'The Conscience of a Physicist', in Lewis & Wilson (eds), op. cit. note 17, 67–76, at 71.
24. Quoted (and paraphrased) in Peter Goodchild, *J. Robert Oppenheimer: Shatterer of Worlds* (Boston, MA: Houghton Mifflin, 1981), 278.
25. Rabi, 'Introduction', op. cit. note 19, 4, 8; see also John S. Rigden, *Rabi: Scientist and Citizen* (New York: Basic Books, 1987), 243.
26. Wilson, op. cit. note 7, 45.
27. Glenn T. Seaborg, 'Public Service and Human Contributions', in Rabi et al., op. cit. note 19, 45–59, at 56.
28. Edward Teller, quoted in Rhodes, op. cit. note 10, 539; see also Warren Bennis and Patricia Ward Biederman, *Organizing Genius: The Secrets of Creative Collaboration* (Reading, MA: Addison-Wesley, 1997), 187.
29. This general point was briefly made in Donald MacKenzie's study of the supercomputer designer Seymour Cray, providing one of the inspirations for our study: D. MacKenzie and Boelie Elzen, 'The Charismatic Engineer', in MacKenzie, *Knowing Machines: Essays on Technical Change* (Cambridge, MA: The MIT Press, 1996 [chapter orig. publ. 1991]), 131–57, at 133–34.
30. Magali Sarfatti Larson, 'The Production of Expertise and the Constitution of Expert Power', in Thomas Haskell (ed.), *The Authority of Experts: Studies in History and Theory* (Bloomington: Indiana University Press, 1984), 28–80, at 58. Larson goes on to acknowledge the existence of 'scientific charisma', while making it out as the product of 'structured and depersonalized consensus'.
31. Max Weber, 'The Sociology of Charismatic Authority', in H.H. Gerth and C. Wright Mills (eds), *From Max Weber* (New York: Oxford University Press, 1958), 245–52, quoting from 245–46.
32. Gerth and Mills write that Weber's 'defensive pessimism ... is reinforced by the fate he sees for charisma in the modern world ... it is clear that the concept serves him as a metaphysical vehicle of man's freedom in history. [This] freedom, as carried by charisma, is doomed ...'. Weber regarded both charisma and freedom as 'now on the defensive against both capitalism and bureaucracy': H.H. Gerth and C.W. Mills, 'Introduction: The Man and his Work', in Gerth & Mills (eds), op. cit. note 31, 3–74, at 72–73.
33. Clifford Geertz, 'Centers, Kings, and Charisma: Reflections on the Symbolics of Power', in Geertz, *Local Knowledge: Further Essays in Interpretive Anthropology* (New York: Basic Books, 1983), 121–46, at 121. There is no way, we believe, plausibly to assimilate Oppenheimer's charismatic authority at wartime Los Alamos to Weber's conception of routinized charisma.
34. Amitai Etzioni, 'Compliance and the Distribution of Charisma', in Etzioni, *A Comparative Analysis of Complex Organizations: On Power, Involvement, and Their Correlates* (New York: Free Press, 1961), 201–32, at 207.

35. For example, Helen Constas, 'Max Weber's Two Conceptions of Bureaucracy', *American Journal of Sociology*, Vol. 63 (1958), 400–09; Etzioni, op. cit. note 34, 209–11.
36. For further commentary and development of Weberian usage, see, for example, Edward Shils, 'Charisma, Order, and Status', *American Sociological Review*, Vol. 30 (1965), 199–213; Karl Loewenstein, *Max Weber's Political Ideas in the Perspective of Our Time* (Amherst: University of Massachusetts Press, 1966), esp. 74–87; Peter Haley, 'Rudolph Sohm on Charisma', *Journal of Religion*, Vol. 60 (1980), 185–97; Luciano Cavalli, 'Charismatic Domination, Totalitarian Dictatorship, and Plebiscitary Democracy in the Twentieth Century', in Carl F. Garumann and Serge Moscovici (eds), *Changing Conceptions of Leadership* (New York: Springer-Verlag, 1986), 67–81; S.N. Eisenstadt, *Max Weber on Charisma and Institution Building* (Chicago, IL: The University of Chicago Press, 1968), esp. xlix–liv; Peter L. Berger, 'Charisma and Religious Innovation: The Social Location of Israelite Prophecy', *American Sociological Review*, Vol. 28 (1963), 940–50; W.H. Friedland, 'For a Sociological Concept of Charisma', *Social Forces*, Vol. 43 (1968), 18–26; Thomas E. Dow, Jr, 'The Theory of Charisma', *The Sociological Quarterly*, Vol. 10 (1969), 306–18; and Charles Lindholm, *Charisma* (Oxford: Blackwell, 1990). For some anthropological sensibilities, see, for instance, Bryan R. Wilson, *The Noble Savages: The Primitive Origins of Charisma and Its Contemporary Survival* (Berkeley: University of California Press, 1975).
37. Reinhard Bendix, 'Charismatic Leadership', in Bendix and Guenther Roth, *Scholarship and Partisanship: Essays on Max Weber* (Berkeley: University of California Press, 1971), 170–87, at 170; see also Joseph Bensman and Michael Givant, 'Charisma and Modernity: The Use and Abuse of a Concept', in Ronald M. Glassman and William H. Swatos, Jr (eds), *Charisma, History, and Social Structure* (New York: Greenwood Press, 1986), 27–56; Dennis H. Wrong, 'Max Weber: The Scholar as Hero', in Wrong, *Skeptical Sociology* (New York: Columbia University Press, 1976), 242–64, esp. 254–56; and Shils, op. cit. note 36.
38. By the 1940s and 1950s, the word seems to have become substantially a part of the educated person's vernacular, and it is as a quasi-vernacular usage that we have engaged with charisma. An important appreciation of the ways in which the concepts and vocabularies of modern human-scientific expertise have become vernacularized (and, hence, realized) is Graham Richards, *On Psychological Language and the Physiomorphic Basis of Human Nature* (London: Routledge, 1989), 85–90.
39. For stress on the embodied, the familiar, and the face-to-face in late modern technoscience, see Steven Shapin, 'Cordelia's Love: Credibility and the Social Studies of Science', *Perspectives on Science*, Vol. 3 (1995), 255–75, and many of the contributions to Christopher Lawrence and S. Shapin (eds), *Science Incarnate: Historical Embodiments of Natural Knowledge* (Chicago, IL: The University of Chicago Press, 1998); for suggestions with respect to sociological biography, see S. Shapin, 'Personal Development and Intellectual Biography: The Case of Robert Boyle', *The British Journal for the History of Science*, Vol. 26 (1993), 335–45, and Shapin, 'Who Was Robert Boyle?' in his *A Social History of Truth: Civility and Science in Seventeenth-Century England* (Chicago, IL: The University of Chicago Press, 1994), Chapter 4.
40. Goodchild, op. cit. note 24, 29.
41. Brode, op. cit. note 1, 5.
42. Dimas Chavez, quoted in Katrina R. Mason, *Children of Los Alamos: An Oral History of the Town Where the Atomic Age Began* (New York: Twayne, 1995), 146.
43. Eleanor Jette, *Inside Box 1663* (Los Alamos, NM: Los Alamos Historical Society, 1977), 123.
44. Rabi, 'Introduction', op. cit. note 19, 5.
45. Quoted in Goodchild, op. cit. note 24, 248.
46. Haakon Chevalier, *Oppenheimer: The Story of a Friendship* (New York: George Braziller, 1965), 11.
47. Eleanor (Jerry) Stone Roensch, *Life Within Limits* (Los Alamos, NM: Los Alamos Historical Society, 1993), 32.

48. Jane S. Wilson, 'Not Quite Eden', in Wilson & Serber (eds), op. cit. note 4, 43–55, at 50; see also Goodchild, op. cit. note 24, 145, and Mason, op. cit. note 42, 148, for a young woman telling Oppenheimer to his face that she thought he was 'a saint'.
49. J. Alvin Kugelmass, *J. Robert Oppenheimer and the Atomic Story* (New York: Julian Messner, 1953), 4–5.
50. Rabi, 'Introduction', op. cit. note 19, 8.
51. Leona Marshall Libby, *The Uranium People* (New York: Crane Russak, 1979), 109.
52. William Lawren, *The General and the Bomb: A Biography of General Leslie Groves, Director of the Manhattan Project* (New York: Dodd, Mead, 1988), 96.
53. Rhodes, op. cit. note 10, 444.
54. John Mason Brown, *Through These Men: Some Aspects of Our Passing History* (London: Hamish Hamilton, 1956), 286.
55. For asceticism in relation to the authority of scientific and philosophical knowledge, see, for example, Steven Shapin, 'The Philosopher and the Chicken: On the Dietetics of Disembodied Knowledge', in Lawrence & Shapin (eds), op. cit. note 39, 21–50; Shapin, ' "The Mind is Its Own Place": Science and Solitude in Seventeenth-Century England', *Science in Context*, Vol. 4 (1991), 191–218; Shapin (1994), op. cit. note 39, Chapter 4; Hélène Mialet, 'Do Angels Have Bodies? Two Stories about Subjectivity in Science: The Cases of William X and Mister H', *Social Studies of Science*, Vol. 29, No. 4 (August 1999), 551–81. It was not, however, thought that Oppenheimer had an insouciant disregard of his dietetics, or that matters of taste in general were of no concern to him. On the contrary, he devoted great care and attention to his food, drink and equipage, and that care was also widely remarked upon by those endeavouring to describe his special personal force.
56. These problems are treated in greater detail in Charles Thorpe, *J. Robert Oppenheimer and the Transformation of the Scientific Vocation* (PhD thesis, University of California, San Diego, forthcoming).
57. Vincent C. Jones, *Manhattan: The Army and the Atomic Bomb* (Washington, DC: US Army Center of Military History, 1985), 468–74; Wilson & Serber (eds), op. cit. note 4; Jette, op. cit. note 43; Brode, op. cit. note 1.
58. Erving Goffman, *Asylums: Essays on the Social Situation of Mental Patients and Other Inmates* (Garden City, NY: Doubleday Anchor, 1961), esp. 1–124.
59. Leslie R. Groves, *Now It Can Be Told: The Story of the Manhattan Project* (New York: Harper & Brothers, 1962), 153–54.
60. Jones, op. cit. note 57, 485–86.
61. Groves' deputy, Col. Kenneth D. Nichols, has said that 'I have always considered it unfortunate that we began with the concept of a military laboratory, because it resulted in Los Alamos having such a hybrid organization': Major General K.D. Nichols, USA (Retd), *The Road to Trinity* (New York: William Morrow & Co., 1987), 153.
62. Lawren, op. cit. note 52, 164–65.
63. David Hawkins, *Project Y: The Los Alamos Story*, Part I: *Toward Trinity* (Los Angeles, CA: Tomash Publishers, 1983), 486; and graphs, 'Number of Persons Employed and Distribution among Civilian, WAC, and SED', 'Percentage Distribution of Personnel among Civilians, WAC, and SED', in ibid., 484 and 485 respectively. See also Manhattan District History Book VIII – Los Alamos Project (Y) Volume 1 – General, 7.3–7.6; 'Manhattan District History', 1942–46; Records of the Office of the Commanding General, Manhattan Project; Records of the Office of the Chief of Engineers, Record Group 77; National Archives II, College Park, Maryland.
64. Kistiakowsky to Oppenheimer, 'Organization of Field Sites', 28 July 1944, Los Alamos National Laboratory Archives [LANL] A-84-019 4-1.
65. Conference with Senior Staff Officers, n.d. [ca. 1945], 3–4; File 337, Staff Conferences; Decimal Correspondence Relating to Los Alamos Laboratory, 1942–48; Records of the Santa Fe (NM) Engineer Office; Records of the Office of the Commanding General, Manhattan Project; RG 77. See also Lillian Hoddeson, Paul W. Henriksen, Roger A. Meade and Catherine Westfall, *Critical Assembly: A Technical*

History of Los Alamos During the Oppenheimer Years, 1943–1945 (Cambridge: Cambridge University Press, 1993), 98.

66. Eric Kent Clarke, MD, to Colonel S.L. Warren, 'Mental Hygiene Survey at "Y", August 23–27 1944', 29 August 1944, 2; File 330.11, Morale; Decimal Correspondence Relating to Los Alamos Laboratory, 1942–48; RG 77. Groves also referred to the situation of the SED and, in general, of soldiers working in the laboratory alongside civilians, as 'an anomalous one'. He said that 'it was artificial, unsatisfactory to both groups, and one upon which the scientists and the military never did quite achieve agreement in viewpoint': 'Complications of the Los Alamos Project', 12 November 1946, 12–13; File 322 [Los Alamos]; Decimal Files 1942–48; General Correspondence, 1942–48; General Administrative Files; RG 77.
67. Respectively: McAllister H. Hull, Jr, interviewed by Los Alamos Historical Society (24 March 1992), LAHM-M1992-112-1-64, Los Alamos Historical Museum, Los Alamos, NM; Charles H. Bagley, interviewed by Charles Thorpe (22 June 1998).
68. Captain Musser to Colonel Tyler, 'Post Population', 5 February 1945; File 091.4 Peoples (Census); Decimal Correspondence Relating to Los Alamos Laboratory; RG 77.
69. Manhattan District History Book VIII – Los Alamos Project (Y) Volume 1 – General, 7.15 and Appendix B-1: 'Hospital Statistics – Los Alamos, New Mexico'. These hospital statistics provide the most reliable available data on the post population since, for security reasons, there was no official census at Los Alamos until April 1946: 'the total population was considered highly classified information' (ibid., 7.15). Probably due to the lack of census data, published estimates of Los Alamos's wartime population vary wildly. For example, Oppenheimer's testimony before the Gray Board in 1954 put the 'size of the laboratory' in the spring of 1945 at 'almost 4000': United States Atomic Energy Commission, *In the Matter of J. Robert Oppenheimer: Transcript of Hearing before the Personnel Security Board and Texts of Principal Documents* (Cambridge, MA: The MIT Press, 1971), 12. Laura Fermi writes that at the end of the War the population of Los Alamos was 6000: Fermi, *Atoms in the Family: My Life with Enrico Fermi* (Chicago, IL: The University of Chicago Press, 1954), 206.
70. John H. Dudley, 'Ranch School to Secret City', in Lawrence Badash, Joseph O. Hirschfelder and Herbert P. Broida (eds), *Reminiscences of Los Alamos, 1943–1945* (Dordrecht: D. Reidel, 1980), 1–11, at 3; Goodchild, op. cit. note 24, 71. Other sources place Oppenheimer's initial guess at anywhere from between 30 to 100 scientists. Oppenheimer told the Gray Board that he and his colleagues had drastically underestimated the growth of the laboratory, but he added that 'Even the initial plan of the laboratory called for . . . more than 100 highly qualified and trained scientists, to say nothing of the technicians, staff and mechanics who would be required for their support': US AEC, op. cit. note 69, 12.
71. Personal communication (April 2000).
72. Stanislaw Ulam, *Adventures of a Mathematician* (New York: Charles Scribner's Sons, 1976), 156.
73. On this point, see Egon Bittner, 'The Concept of Organization', *Social Research*, Vol. 32 (1965), 239–55.
74. Conference with Senior Staff Officers, op. cit. note 65, 4.
75. Richard B. Feynman, 'Los Alamos from Below', in Badash et al. (eds), op. cit. note 70, 105–32, at 111–19, 129.
76. Robert Serber, interviewed in loc. cit. note 5.
77. Hoddeson et al., op. cit. note 65, 93; see also Richard G. Hewlett and Oscar O. Anderson, Jr, *The New World 1939/1946:* Volume 1: *A History of the United States Atomic Energy Commission* (University Park: Pennsylvania State University Press, 1962), 237–39; and Hawkins, op. cit. note 63, 6–7, 17–25.
78. Victor Weisskopf, interviewed by Lillian Hoddeson and Gordon Baym (10 March 1978), LANL TR-78-006, 16.
79. Lillian Hoddeson, 'Mission Change in the Large Laboratory: The Los Alamos Implosion Program, 1943–1945', in Peter Galison and Bruce Hevly (eds), *Big Science:*

The Growth of Large-Scale Research (Stanford, CA: Stanford University Press, 1992), 265–89.

80. Hawkins, op. cit. note 63, 208–16; Hoddeson et al., op. cit. note 65, 245.
81. Jones, op. cit. note 57, 493.
82. Weisskopf, loc. cit. note 78, 16–17; Groves, op. cit. note 59, 166–67; see also Alvarez, op. cit. note 3, 128–29.
83. For a valuable discussion of similar problems of organizational order in a weapons laboratory, see John R. Sutton, 'Organizational Autonomy and Professional Norms in Science: A Case Study of the Lawrence Livermore Laboratory', *Social Studies of Science*, Vol. 14, No. 2 (May 1984), 197–224.
84. Hewlett & Anderson, op. cit. note 77, 103–04, 227–29.
85. John H. Dudley, 'Ranch School to Secret City', in Badash et al. (eds), op. cit. note 70, 1–11; Groves, op. cit. note 59, 66.
86. Bernice Brode (op. cit. note 1, 5) remembered how Oppenheimer 'once had said that his two loves – physics and desert country – never existed together. Had he joined them at last?'.
87. Stanley Goldberg, 'Groves and the Scientists: Compartmentalization and the Building of the Bomb', *Physics Today*, Vol. 48, No. 8, Pt 1 (August 1995), 38–43, at 41; Hewlett & Anderson, op. cit. note 77, 227–28.
88. Szilard to A.H. Compton, 25 November 1942, '[re] Compartmentalization of Information and the Effect of Impurities', in Spencer R. Weart and Gertrud Weiss Szilard, *Leo Szilard: His Version of the Facts* (Cambridge, MA: The MIT Press, 1978), 160.
89. Groves, op. cit. note 59, 140. And the historian Peter Bacon Hales nicely observes that compartmentalization was 'a means to redesignate scientists and engineers as workers, equivalently obligated to management': P.B. Hales, *Atomic Spaces: Living on the Manhattan Project* (Urbana: University of Illinois Press, 1997), 118; see also Charles Thorpe, 'Atomic Spaces and American Culture (Review of Hales, *Atomic Spaces* and Gusterson, *Nuclear Rites*)', *Social Studies of Science*, Vol. 29, No. 4 (August 1999), 617–27.
90. The Top Policy Group was headed by President Roosevelt, though he never attended meetings, and included Vice President Henry A. Wallace, Secretary of War Henry L. Stimson, Army Chief of Staff General George C. Marshall, as well as Vannevar Bush and J.B. Conant. Under this Committee, and, with the takeover of the atomic bomb project by the Army, a Military Policy Committee (MPC) was responsible for directing the programme. This was chaired by Bush (with Conant as his alternate) and consisted of General Wilhelm D. Styer, Rear Admiral William R. Purnell and General Groves. Groves' position was defined as 'the executive head of the development of the enterprise', responsible for carrying out MPC policy: Jones, op. cit. note 57, 31, 77, 89, quoting from 77.
91. Quoted in Rigden, op. cit. note 25, 141.
92. Daniel J. Kevles, *The Physicists: The History of a Scientific Community in Modern America* (New York: Vintage, 1979), 216–18; Emilio Segrè, *A Mind Always in Motion: The Autobiography of Emilio Segrè* (Berkeley: University of California Press, 1993), 138.
93. John Manley, 'Assembling the Wartime Labs', *Bulletin of the Atomic Scientists*, Vol. 30 (May 1974), 42–48, at 45.
94. Alvarez, op. cit. note 3, 78; see also 77, 128.
95. Quoted in Jeremy Bernstein, *Experiencing Science* (New York: Basic Books, 1978), 97.
96. Allison, quoted in Goodchild, op. cit. note 24, 71.
97. Manley, op. cit. note 93, 45.
98. Wilson, op. cit. note 7, 45; Manley, op. cit. note 93, 45.
99. Wilson, quoted in Goodchild, op. cit. note 24, 72.
100. Quoted in Lawren, op. cit. note 52, 101.
101. Groves, op. cit. note 59, 61–63; Lawren, op. cit. note 52, 100. Groves was not excessively anxious about Oppenheimer's well-known political associations.

102. Groves' first choice for the job was Lawrence, but he could not be spared from the Radiation Laboratory and its work on electromagnetic separation: Davis, op. cit. note 2, 143–45; Herbert Childs, *An American Genius: The Life of Ernest Orlando Lawrence* (New York: E.P. Dutton & Co., 1968), 337.
103. And also, presumably, to lubricate anticipated friction between Oppenheimer and the military Commanding Office at the post, and to help Groves control an awkward Scientific Director: Groves, op. cit. note 59, 154; Groves' testimony to Gray Board, in US AEC, op. cit. note 69, 166.
104. Groves, op. cit. note 59, 156, 429–32.
105. Summary of interview with Bacher (1978), in Smith & Weiner (eds), op. cit. note 2, 261.
106. Lawren, op. cit. note 52, 102.
107. Compton, quoted in ibid., 101.
108. Ibid., 98, 102; Goodchild, op. cit. note 24, 73.
109. Alvarez, op. cit. note 3, 128; see also 77. And, respectively, Robert and Jane Wilson, quoted in Mary Palevsky, *Atomic Fragments: A Daughter's Questions* (Berkeley: University of California Press, 2000), 139. Jane Wilson adds that when Oppenheimer was a 'big wheel in Washington, he was quite shaved and quite sharp', with the hairstyle of a Marine. By contrast, in the wake of the 1954 Security Hearings, 'after he lost his position and was "defrocked" as it were, then his hair got gray and a little curly'.
110. Serber, 'Preface', op. cit. note 21, xxxii; see also Kevles, op. cit. 92, 330: 'To the physicists and their families, the army personified was General Groves . . .'. Interviewed in 1992, Bethe thought that 'Groves was a very disagreeable man. And Groves was apt to consider all the scientists his slaves. Even if we were not in uniform, we were just lieutenants. And somehow Oppie managed to hold his own with Groves . . .': Hans Bethe, interviewed by Silvan S. Schweber (July 1992), in Schweber, *In the Shadow of the Bomb: Oppenheimer, Bethe, and the Moral Responsibility of the Scientist* (Princeton, NJ: Princeton University Press, 2000), 216. (This book was available to us only after final revisions to the present paper were completed. It contains much useful material on Oppenheimer's authority, both at Berkeley before the War and, later, at Los Alamos.)
111. Bacher, quoted in Lawren, op. cit. note 52, 103–04.
112. Respectively, Bacher and Rabi, quoted in Rigden, op. cit. note 25, 150.
113. Oppenheimer to Conant, 1 February 1943, in Smith & Weiner (eds), op. cit. note 2, 247–48, at 247. Even Luis Alvarez disagreed with Oppenheimer's approval of Groves' suggestion that scientists should be inducted into the Army: 'I don't think science can be done under authoritarian arrangements' (Alvarez, op. cit. note 3, 128).
114. 'Complications of the Los Alamos Project', op. cit. note 66, 5.
115. Quoted in Rigden, op. cit. note 25, 149.
116. Oppenheimer to Conant, loc. cit. note 113, 247; on Rabi's position, see Rigden, op. cit. note 25, 150.
117. Conant and Groves to Oppenheimer, 25 February 1943, LANL A-84-019 57-27.
118. David Hawkins, interviewed by Charles Thorpe (16 November 1998).
119. Joseph Hirschfelder, 'The Scientific and Technological Miracle at Los Alamos', in Badash et al. (eds), op. cit. note 70, 67–88, at 78.
120. Quoted in Smith & Weiner (eds), op. cit. note 2, 263. Again, not all scientists agreed: those, like Teller, who had fundamental conflicts with Oppenheimer about organizational goals or forms, saw Oppenheimer's art as a sinister form of 'psychological finesse': Teller, quoted in Stanley A. Blumberg and Gwinn Owens, *Energy and Conflict: The Life and Times of Edward Teller* (New York: G.P. Putnam's Sons, 1976), 126; see also Segrè, op. cit. note 92, 177–83, and the concluding section of this paper, 576–80.
121. Teller to Maria Mayer [ca. early 1947], Maria Goeppert Mayer Papers, Mandeville Special Collections Library, University of California, San Diego.

122. Parsons to Oppenheimer, 'Home Stretch Measures', 19 February 1945, quoted in Albert B. Christman, *Target Hiroshima: Deak Parsons and the Creation of the Atomic Bomb* (Annapolis, MD: Naval Institute Press, 1998), 161.
123. Christman, op. cit. note 122, 161; Hoddeson et al., op. cit. note 65, 248.
124. Hoddeson, op. cit. note 79, 286.
125. This routine compartmentalization is what organizational sociologist Diane Vaughan calls 'structural secrecy': *The Challenger Launch Decision: Risky Technology, Culture and Deviance at NASA* (Chicago, IL: The University of Chicago Press, 1996), 238–77; see also Vaughan, 'The Rôle of the Organization in the Production of Techno-Scientific Knowledge', *Social Studies of Science*, Vol. 29, No. 6 (December 1999), 913–43.
126. Minutes of the Meeting of the Governing Board, 6 May 1943, LANL A-83-0013 1–3, 1.
127. Hoddeson et al., op. cit. note 65, 94; Segrè, op. cit. note 92, 183; Minutes of the Meeting of the Governing Board, 15 July 1943, LANL A-83-0013 1–17, 2.
128. Minutes of the Meeting of the Governing Board, 5 August 1943, LANL A-83-013 1–20, 2–3.
129. Minutes of the Meetings of the Liaison Committee, 11 January and 11 February 1944, LANL A-83-013 3–25.
130. Minutes of the Meeting of the Governing Board, 11 November 1943, LANL A-83–013 1–33, 3.
131. Minutes of the Meeting of the Governing Board, 31 May 1943, LANL A-83-013 1–10, 3.
132. Minutes of the Meeting of the Governing Board, 5 August 1943, LANL A-83-013 1–20, 2–3.
133. Minutes of the Meeting of the Governing Board, 31 May 1943, LANL A-83-013 1–10, 3.
134. Hawkins, op. cit. note 63, 30. This official history was written in 1947 and declassified in 1961.
135. Groves, op. cit. note 59, 167. Groves tellingly went on to say that 'From the standpoint of security, it presented a major hazard, and it was one of the reasons why the treachery of Fuchs was so disastrous to the free world'.
136. Bethe, op. cit. note 15, 1082.
137. Victor Weisskopf, interview, loc. cit. note 78, 16–17.
138. Victor Weisskopf, 'The Los Alamos Years', in Rabi et al., op. cit. note 19, 23–28, at 24.
139. Bethe, op. cit. note 15, 1082.
140. Alvarez, op. cit. note 3, 128.
141. Rudolf E. Peierls, *Atomic Histories* (Woodbury, NY: AIP Press, 1997), 51.
142. Quoted in Rhodes, op. cit. note 10, 570.
143. Smith & Weiner (eds), op. cit. note 2, 264.
144. Weisskopf, interview, loc. cit. note 78, 2.
145. Weisskopf, op. cit. note 138, 24. On Oppenheimer's circulation through the laboratory, see also Kunetka, op. cit. note 2, 63.
146. Weisskopf, interview, loc. cit. note 78, 2–3. Weisskopf says here that in his later career as director of the European nuclear physics laboratory, CERN, he tried to model his leadership style on the example Oppenheimer set at Los Alamos: 'I very often quite consciously asked myself, "what might Oppenheimer have done?" '. As a result, Weisskopf adopted what his CERN colleagues came to call 'directorship via lunch table. . . . Namely the two hours of luncheon I spent in the cafeteria, not eating, but sitting with different people'. See also Victor Weisskopf, *The Joy of Insight: Passions of a Physicist* (New York: Basic Books, 1991), 133, 230.
147. Wilson, op. cit. note 7, 41–47, at 45.
148. Cyril Stanley Smith, interviewed by Los Alamos Historical Society (28 May 1992), LAHM M1992-112-1-71.
149. Bethe, op. cit. note 15, 1082.

150. Eugene Wigner, *The Recollections of Eugene P. Wigner as told to Andrew Szanton* (New York: Plenum Press, 1992), 245.
151. Ibid., 245–46, at 246.
152. Hawkins, interviewed by Charles Thorpe (16 November 1998).
153. Robert Jungk, trans. James Cleugh, *Brighter Than a Thousand Suns: A Personal History of the Atomic Scientists* (New York: Harcourt Brace, 1958), 135.
154. Ed Doty to his parents, 7 August 1945; Los Alamos Historical Museum.
155. Joseph O. Hirschfelder, 'The Scientific and Technological Miracle at Los Alamos', in Badash et al. (eds), op. cit. note 70, 67–88, at 78.
156. Tuck, quoted in Davis, op. cit. note 2, 185. For gentility in relation to scientific authority, see Shapin (1994), op. cit. note 39, esp. Chapters 1, 4, 6, and Epilogue.
157. Bernice Brode, 'Tales of Los Alamos', in Badash et al. (eds), op. cit. note 70, 133–60, at 142. The baby was Katherine 'Toni' Oppenheimer, born in the first year of the project: Goodchild, op. cit. note 24, 127.
158. Smith, op. cit. note 17, 38.
159. Quoted in Rhodes, op. cit. note 10, 539.
160. Quoted in Goodchild, op. cit. note 24, 116.
161. Rabi to Weisskopf, 11 April 1967, Papers of I.I. Rabi, Manuscript Division, Library of Congress.
162. Quotations from Segrè, op. cit. note 92, 138–39, 183. Goodchild (op. cit. note 24, 9) rightly notes 'contradictory responses' to Oppenheimer's character: 'a substantial majority' viewing him as charming, selfless, and honest; a 'minority' remembering him as 'arrogant, calculating and selfish'.
163. Though, when informing Oppenheimer, over the telephone, of the successful bombing of Hiroshima, Groves did say, 'I think one of the wisest things I ever did was when I selected the director of Los Alamos': General Groves call to Dr Oppenheimer, Santa Fe at 2:00 pm, 6 August 1945; File 201 Groves, L.R. Lt. Gen. Telephone Conversations; Decimal Files 1942–48; General Correspondence, 1942–48; General Administrative Files; Records of the Office of the Commanding General, Manhattan Project; RG 77; extracted also in Rhodes, op. cit. note 10, 734–35. Groves also told an interviewer off the record, immediately after the end of the War, that Oppenheimer was 'A real genius. . . . Why, Oppenheimer knows about everything. He can talk to you about anything you bring up. Well, not exactly. I guess there are a few things he doesn't know about. He doesn't know anything about sports': Rhodes, op. cit. note 10, 448–49. Leona Libby thought that 'Groves was caught up in Oppenheimer's charisma also. . . . In Oppenheimer's presence Groves became omniscient; his insistence on having it his way, backed by his military dictatorship, became imbued with an intelligent logic he had never enjoyed before': Libby, op. cit. note 51, 102.
164. Thomas P. Hughes, *American Genesis: A Century of Invention and Technological Enthusiasm, 1870–1970* (New York: Viking, 1989), 381–421, esp. 383, 390–91.
165. Erving Goffman's description of the 'underlife' of mental hospitals and other total institutions is a classic illustration of this point: Goffman, op. cit. note 58, 171–320.
166. So quotidian life in formal organizations may in fact be much less taken-for-granted than in most arenas of everyday life. Karl Weick writes that a great deal of organizational life 'is fair game for continual negotiation. . . . The only thing people do not do is take things for granted, which is what they spend most of their time doing everywhere else': Karl E. Weick, *Sensemaking in Organizations* (Thousand Oaks, CA: Sage, 1995), 63.
167. This we take to be one of the implicit messages of Alvin Gouldner's fine (but now undeservedly neglected) study of the function of rules in industrial organizations: A.W. Gouldner, *Patterns of Industrial Bureaucracy* (New York: The Free Press, 1954), Chapter 9, 'About the Function of Bureaucratic Rules'.
168. John Law, *Organizing Modernity* (Oxford: Blackwell, 1994); Paul Rabinow, *Making PCR* (Chicago, IL: The University of Chicago Press, 1996); Rabinow, *French DNA: Trouble in Purgatory* (Chicago, IL: The University of Chicago Press, 1999); and MacKenzie & Elzen, op. cit. note 29 (for Seymour Cray). For an entry to

management studies of leadership, see Alan Bryman, *Charisma and Leadership in Organizations* (London: Sage Publications, 1992). The communications researcher Phillip K. Tompkins describes Wernher von Braun's leadership of the George C. Marshall Space Flight Center during 1967 and 1968, the Apollo period, in terms of charisma. He writes that von Braun's 'organizational "audience" conferred on him a status, perhaps *construct* is a better word, which seemed to fall somewhere in that unexplored territory between Aristotle's concept of *ethos* and Weber's concept of charisma'. Von Braun's charisma seems to have been operationalized in terms remarkably similar to that of Oppenheimer. Tompkins writes that the MSFC employees 'spoke of the morale-building effects of von Braun's visits to the laboratory or of their respect for his ability to assimilate even the minutest detail of the disciplines at work on the project': P.K. Tompkins, 'Management Qua Communication in Rocket Research and Development', *Communication Monographs*, Vol. 44 (1977), 1–26, at 6; see also Tompkins and George Cheney, 'Communication and Unobtrusive Control in Contemporary Organizations', in Robert D. McPhee and Tompkins (eds), *Organizational Communication: Traditional Themes and New Directions* (Beverly Hills, CA: Sage Publications, 1985), 179–210; and Tompkins, *Organizational Communication Imperatives: Lessons of the Space Program* (Los Angeles, CA: Roxbury Publishing Company, 1993), esp. 82–84. Some earlier historical studies of creative research groups and the styles of their leaders lend themselves to our analysis: for example, for studies of two Nobelists (Enrico Fermi and Martin Ryle), see Gerald Holton, 'Striking Gold in Science: Fermi's Group and the Recapture of Italy's Place in Physics', *Minerva*, Vol. 12 (April 1974), 159–98, and David Edge and Michael Mulkay, *Astronomy Transformed: The Emergence of Radio Astronomy in Britain* (New York: Wiley Interscience, 1976), esp. Chapter 9, 'The Social Structures of the Groups', 289–349.

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