



Celebrating the Work of Keith Murnighan

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Celebrating the Work of Keith Murnighan

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Abstract

In this tribute, four scholars highlight research published during the career of 2015 International Association for Conflict Management Lifetime Achievement Award Winner Keith Murnighan.

In the four sections of the paper, each scholar highlights one of Keith's published papers and discusses the impact it had on them, and the field of organizational behavior more generally. The authors close by going beyond each article to identify common themes that emerge across the works.

In June 2015, Keith Murnighan received the Lifetime Achievement Award from the International Association for Conflict Management. At this ceremony, four speakers provided some commentary about what it is like to both collaborate with and be friends with Keith. Some references were made to Keith's research, but much of the commentary was more personal in nature. Afterwards, the Editor of NCMR approached this group to gauge their interest in writing a more formal set of remarks about the impact that Keith Murnighan and his research have had in our field. What follows are reflections from four scholars who have had the pleasure of working with Keith. In fact, three of the four were doctoral students who worked with Keith during his academic journey from the University of Illinois to the University of British Columbia to Northwestern University. Collectively, the authors hope that their four different takes on how Keith Murnighan's scholarly work has made a difference will serve as an inspiration for future scholars looking to make important contributions in research.

A behavioral economist before the field got its name (Max)

Behavioral economics is one of the fastest growing and most influential subfields of economics. Wikipedia defines behavioral economics as being:

“concerned with the bounds of rationality of economic agents. Behavioral models typically integrate insights from psychology, neuroscience and microeconomic theory... Behavioral economics is sometimes discussed as an alternative to neoclassical economics”

Much of this field is dated to the psychologists Amos Tversky and Daniel Kahneman. And, back in the 1970s and 1980s, the field was more commonly called behavioral decision theory or behavioral decision research. If people are looking for an economist rather than a psychologist

to date the start of the field of behavioral economics, many would mention Dick Thaler, who first published a paper in this area in 1980. And, Thaler is certainly connected to the shift in names from behavioral decision theory to behavioral economics. While I (Max) am extremely positive about the work of Tversky, Kahneman, and Thaler, there is ample evidence that Keith Murnighan and Al Roth were doing behavioral economics before it was known by this term.

In 1982, I was in my third year as an assistant professor, and traveled to Aschaffenburg, Germany to attend a conference on bargaining and economic decision making. I was 26 years old, and it was my first trip out of North America. I was presenting work that I was doing on the winner's curse with Bill Samuelson, my excellent colleague at Boston University (Bazerman, and Samuelson, 1983). There are many things I remember about the conference, including a bowling alley in the basement, an excellent wine tasting event, and watching Keith eat an amazing amount of blood sausage (stuff that I wouldn't touch, even though this was before I became a vegetarian), but certainly the most important gift that I obtained from the conference was meeting Keith Murnighan – who became my life long friend. Keith was a tenured associate professor at the University of Illinois, and was in the middle of a long series of papers with Al Roth, now a recent Nobel Laureate. This was my first exposure to the Murnighan/Roth line of work.

I was fascinated by Keith's presentation (Roth and Murnighan, 1983). I really liked the work, and the entire series of work Murnighan and Roth were up to, but did not have a clear way to describe it at the time. But, in retrospect, there was much about the style of the work that overlapped with work that I would later do with Bill Samuelson, Hank Farber, Bob Gibbons, and Iris Bohnet – all excellent economists, and I was very influenced by the Murnighan/Roth research. An overly simplistic summary of the Murnighan/Roth research was that they were

doing behavioral economics before behavioral economics existed – in the context of bargaining games.

More specifically, they published a dozen papers in leading economic, psychology and management journals, including:

- Murnighan, J. K. & Roth, A. E. (1977)
- Roth, A. E. & Murnighan, J. K. (1978)
- Murnighan, J. K. & Roth, A. E. (1978)
- Murnighan, J. K. & Roth, A. E. (1980).
- Roth, A. E., Malouf, M. W. K., & Murnighan, J. K. (1981).
- Roth, A. E. & Murnighan, J. K. (1982).
- Murnighan, J. K. & Roth, A. E. (1983).
- Murnighan, J. K., Roth, A. E., & Schoumaker, F. (1987).
- Murnighan, J. K., Roth, A. E., & Schoumaker, F. (1988).
- Roth, A. E., Murnighan, J. K., & Schoumaker, F. (1988).

These papers examined actual behavior in stylized games, including prisoner dilemma games, coalitional bargaining games (Keith did a bunch of work on coalitions in those days), and binary/ternary lottery games. While Keith had mathematical training, the formal modelling (economic theory) was Al's strength, and experimentation was Keith's. As Al Roth wrote in his Nobel autobiography:

“My arrival at Illinois is memorable for two psychologists I met there in my first year. The first, in the first weeks after my arrival, was my colleague Keith Murnighan. We were both new assistant professors in 1974. He had just received his Ph.D. in social psychology from Purdue. One of our senior colleagues suggested we would enjoy talking to each other, and we did, so much so that we decided to do some experiments together, on the kinds of games I had studied in my dissertation. Experiments were newer to me

than game theory was to him, but over the course of the next decade we taught each other how to do experiments that would say something useful about game theory.”

Based on their collaboration, these papers had a fairly consistent style, very unique from other work in economics or psychology.

Like much work in economics, these papers provided the formal analysis of what rational actors would do in the games that Murnighan/Roth studied. They were particularly fond of games that had more than one equilibrium – or possible results that could emerge by fully rational actors. But, then, in ways that departed from most of experimental economics at the time, they allowed their “data to speak”. That is, they reported what people actually did in their experiments, rather than foisting their assumptions onto research participants. And, they used sociological and psychological concepts to make sense of how their study participants departed from rational action. This was an unusual package at the time, and what resulted was a series of papers that were rigorous in their specification, rooted in formal theories, yet accurate and insightful in describing actual behavior – hallmarks of the field we now call behavioral economics.

Interestingly, their work overlapped in time with the early work of Kahneman and Tversky (e.g., 1974, 1979), but their tools for describing actual behavior were broader, and they invoked sociology as often as psychology. In the words of Roth, Malouf, and Murnighan (1981):

The results strongly support the conclusion that sociological factors, unrelated to what we normally consider to be ‘economic’ parameters of a game, can decisively influence the outcome of bargaining, in a systematic manner... The underlying idea is that in conflict

situations involving a wide range of rational potential agreements, social convention may serve to make some arguments and demands more credible than others.

Or, from Murnighan and Roth (1980):

All of the (economic) models, however, might benefit from modifications that incorporate the dynamic nature of coalition bargaining.

This combination of a rigorous economic structure with insights from psychology and sociology had a profound influence on me. In fact, the style of the research may be more important than any of their specific and important results. But, this research also highlighted many original results, results that we now often take for granted. These results include:

Game theoretic models do a pretty good job of explaining behavior, but actual behavior departs from purely rational behavior in systematic ways.

Behavioral models are extremely useful for predicting between multiple game theoretic models.

Asymmetry of information and communication structure are critical determinants of bargaining outcomes.

As competition in games increases the predictability of economic models also increases.

The likelihood of a relationship continuing has important effects on the likelihood of cooperation.

While risk aversion is disadvantageous to a bargainer, the magnitude of the effect is smaller than the predictions of economic in predictable contexts.

Deadlines really do matter – the deadline effect. This is the first empirical evidence on this topic.

Across these results, Murnighan and Roth highlighted the power of integrating other social sciences with economics, or what we now call behavioral economics. For me, this early research was transformational.

The master at transposing social and organizational problems into a bargaining experiment (Madan)

Bargaining games provide wonderful empirical contexts to examine important social and organizational problems. Keith has done more than most people to advocate and demonstrate the usefulness of Coalition, Prisoner's Dilemma, Dictator, Ultimatum and Trust games as vehicles to examine important organizational issues. I am confident that he holds some kind of a record for the number of experimental participants he has put through the experience of playing these games—I know the effort it takes to reach these numbers as I had personally run many of these experiments as part of our joint work (as a PhD student, you were never ever just an RA for Keith. You were always a co-author!).

My introduction to these games came in the spring term of 1992 when I made the incredibly fortunate mistake of registering in Keith's bargaining class for MBAs at the University of Illinois (I thought that I was registering for the PhD class on research methods which he was also teaching at that time). The class was educational and entertaining in equal measures. Of even greater value was the opportunity to talk to Keith about the research that used such games and formed the academic foundation for the course. The greatest gift was of course in finding the most generous mentor that one could every wish for. I saw Keith almost every day

for the next three years and view that period as the most intellectually stimulating and fun period in my academic life (a totally delightful perk was to get to sample his wonderful cooking and beer and wine collection many times).

I enjoyed reading many of Keith's papers but did not realize then, that the strong theory, elegant designs, and clear writing that characterized his work were rare. Even rarer, I realized much later, was how he used bargaining games to study problems that were of general interest to organizational scholars. Keith's work with Ken Bettenhausen illustrates how studies using bargaining games could make contributions beyond the bargaining context. The papers are justly famous for articulating and testing a theory on norm formation in small groups that has stood the test of time. Keith and Ken use coalition (Bettenhausen & Murnighan, 1985) and repeated Prisoner's Dilemma games (Bettenhausen & Murnighan, 1991) to examine how similar structures can beget different norms depending on prior experiences and expectations of group members.

Keith was aware that in addition to making a significant contribution to our understanding of groups, this set of papers was important to make the point that bargaining games were a versatile empirical context in which one could examine important issues. Ken and Keith made this explicit in the conclusion when they noted, 'A final substantive conclusion results from the approach this study has taken to the study of coalition formation and n-person bargaining. Much of the research in this area has focused on theoretical testing. Although the plethora of theories provides a strong basis for such research, the external validity and even the enduring theoretical significance of straightforward empirical comparisons are questionable. By approaching the bargaining context from a broader, less pointed perspective, research on bargaining has the opportunity to proceed theoretically and expand its currently restricted

domain (1985, p370-371).’ An example of how this broader perspective improves our understanding of interpersonal phenomenon is the important finding that cooperative outcomes are less stable than competitive outcomes in interacting groups (Bettenhausen & Murnighan, 1991)—cooperative norms were compromised with groups turning towards competition when structures incentivize competition; competitive norms remained intact even when structures incentivized cooperation in their experiment. A focus on testing game theoretic predictions would not have allowed for the emergence of this finding.

I do not intend to say that Keith does not value game theory (the theory associated with bargaining games) or tests of game theoretic models. Anyone who knows Keith knows that this is not true and he is explicit about his impatience with organizational scholars who are dismissive of game theory (Murnighan, 1994). Keith is also a fan of the classic social psychological theories on small groups and the many citations to Solomon Asch, Morton Deutsch, and Muzafer Sherif in his writings bear testimony to that. But Keith has always been clear, both in his papers and in his teaching to PhD students, that the value of theory was in its usefulness to explain social and organizational life.

I think Keith’s choice of research topics (e.g., cooperation, fairness, trust, etc.) is on account of his deep sensitivity to issues that make groups and organizations good contexts for individuals to thrive in and a commitment to use his resources as a social scientist to do something about them. The commitment keeps him enthusiastic about his research and the sensitivity throws up fresh phenomenon for investigation. This is also the basis, I think, for his strong negative reactions to the rejection of papers! To see a scholar with such an impressive list of publications react to a rejection so passionately is instructive for young scholars who might let themselves get a tad cynical on account of the vagaries of the journal review process.

Keith is the master of transposing social and organizational phenomenon into experiments that use bargaining games. The games and associated theory model structural elements that are important in organizations and allow for precise tests. One can then get a handle on how to design structures that ensure individual and organizational wellbeing. As noted above, a consideration of interpersonal issues further enhances our understanding of the phenomenon. You can see how useful this approach can be in his two papers with Bettenhausen and other papers such as the one on volunteering (Murnighan, Kim & Metzger, 1994), trust restoration following transgressions (Bottom, Gibson, Daniels & Murnighan, 2002; Lount, Zhong, Sivanathan & Murnighan, 2008), and contracts (Malhotra & Murnighan, 2002). The influence and impact of this approach is being constantly reinforced by the fine work conducted by many students and junior colleagues he has mentored (c.f., the work of Bill Bottom, Long Wang, Robert Lount, and Deepak Malhotra).

For people, like me, attracted to the simple elegance of bargaining games and game theory, Keith's work shows us how the games can be used to understand broad social and organizational issues. For people interested in specific organizational and social issues, Keith's work points to the value of using games (and the associated rigorous theory) as the empirical context.

Understanding that the fault lies not in our diversity, but in our faultlines (Deepak)

Diversity had been an active area of research in organizational behavior for decades before Keith Murnighan and Dora Lau, who was then a doctoral student, pointed out that there was a glaring omission in the way scholars and practitioners were conceptualizing the problems and promise of diversity. Consider two teams, Team A and Team B, each of which is comprised

of six people. Both have three men and three women; both have three Asians and three Hispanics; both have three senior managers and three young analysts. By all prior accounts—and by all prior measures (e.g., Blau, 1977; O’Reilly, Caldwell, and Barnett, 1989)—these two teams are equally diverse. We might also expect, it follows, that the effects of diversity (e.g., creativity or task conflict) would be equal in likelihood and magnitude across both teams.

Lau and Murnighan (1998) pointed out a serious flaw with this conclusion—and in doing so, introduced what is surely among the most compelling and important new ideas, not only in diversity research, but also in research on group dynamics, norm formation, creativity and conflict. Their article, “Demographic Diversity and Faultlines: The Compositional Dynamics of Organizational Groups”, is Keith’s only *Academy of Management Review* article. It also happens to be his most cited article, with over 1,200 citations as of March, 2016. (It was Dora’s first publication.)

Lau and Murnighan explained that to accurately assess the impact of diversity, we can’t simply look at the summary statistics of a group’s demographic factors, we also need to look at how the various demographic factors “stack up”. They introduced a new construct, which they termed *faultlines*:

“Faultlines divide a group’s members on the basis of one or more attributes...Just as diversity can vary within a group, so can the strength of its faultlines. In particular, faultlines become stronger as more attributes align themselves in the same way. For instance, when all of the women in a group are over 60 years old and all of the men are under 30, the sex and age faultlines align and form a single, stronger faultline.” (Lau and Murnighan, 1998; p. 325)

In our original example of two teams, imagine that the demographic breakdown in Teams A and B is as follows:

TEAM A	Senior Managers	Analysts
	<ol style="list-style-type: none"> 1. Asian Female 2. Hispanic Male 3. Hispanic Female 	<ol style="list-style-type: none"> 1. Hispanic Male 2. Asian Female 3. Asian Male
TEAM B	Senior Managers	Analysts
	<ol style="list-style-type: none"> 1. Asian Male 2. Asian Male 3. Asian Male 	<ol style="list-style-type: none"> 1. Hispanic Female 2. Hispanic Female 3. Hispanic Female

As is evident, while the ratio of managers/analysts, males/females, and Asians/Hispanics is identical across the two groups, their distribution is quite different. In Team A, there is a mix of genders and races in each job. In Team B, on the other hand, the three senior managers all happen to be Asian and male, whereas the analysts are all Hispanic and female. Lau and Murnighan argue that we ought to expect group dynamics to differ across these two teams. For example, they hypothesized, that while traditional measures of diversity might indeed predict the prospects and positive consequences of creativity, faultlines would better predict performance losses due to conflict among subgroups.

The implications of this work are everywhere. Consider, for example, an organization that decides that it has a “diversity problem” and wants to hire more women and minorities. A distinct possibility is that these new hires will be younger than the existing group of employees, and they will share the characteristic of less tenure in the organization. This may solve the “diversity” problem, but accentuate the faultlines problem. This is all the more likely if the organization tries to “solve two problems at once” by hiring minority women. A wiser strategy

might be one that ensures that new hires are not all in the same cohort, or all junior, or all too similar across multiple salient faultlines.

As this example demonstrates, solutions to diversity problems and solutions to faultline problems may differ, and even conflict. And although diversity and faultline problems are related, they are related in non-linear ways. As Lau and Murnighan point out: “At minimum and maximum diversity, faultlines are either absent or unlikely. By their nature, *faultlines become most likely in groups of moderate diversity.*”¹ For example, if everyone on a four-person team is the same on all dimensions, faultlines do not exist; at the other extreme, if there are 10 demographic factors across which four people differ, it is unlikely that all of the faultlines will align. One way to solve faultline problems, then, is to keep divisive demographic (or other) factors from becoming too salient. Instead, if team members identify with a wide range of characteristics, or if they identify predominantly on the basis of factors that they share (e.g., team identity), faultline problems may be avoided.

In 1998, when Keith and Dora were about to publish this seminal article, I was in my first year as Keith’s doctoral student at the Kellogg School of Management. I have no memory of him working on this paper at the time, but I remember, quite well, reading Keith’s earlier research on coalitions (e.g., Murnighan, 1978) and on norm formation (e.g., Bettenhausen & Murnighan, 1985), both of which were clearly foundational for his work on faultlines. I also have very vivid memories of the many people who, about two years later, were telling me how much the faultlines article had already changed the way they thought about diversity, identity, creativity and conflict. Over the years, I think I have heard more praise about Keith’s work on

¹ Italics in original

faultlines than any other. Having worked with him on a number of projects and topics (but not on faultlines) I would say that it is easily one of his greatest contributions—if not the greatest single contribution. And for a die-hard empiricist like Keith, the fact that it came in a *Review* article is all the more interesting.

Perhaps I find it especially important because the implications of the work extend far beyond management and organizational behavior. Almost a hundred years ago, peacemakers in Paris were negotiating the aftermath of the Great War (WWI) and Wilsonian notions of self-determination were sowing both hope and confusion. Looking back, most people agree that many of the decisions made at the time helped fuel future conflict—in Europe, the Middle East and elsewhere. But as historian Margaret MacMillan (2001) points out, decision makers at the time were simply ill equipped to make sense of the problems they confronted. In the case of the Balkans, for example:

“The practice of defining oneself by nationality was so new that many inhabitants of the Balkans still thought of themselves primarily in terms of their religion or clan or, as they had done under the Turks, of their religion. Were Serbs and Croats alike because they spoke virtually the same language, or different because the former were mainly Orthodox and used the Cyrillic script and the latter were Catholic and used the Latin? Where did the Macedonians belong—with the Greeks because of their history, or with the Slavs because of their language? How could you draw neat boundaries where there was such a mixture of peoples?”

Indeed, the “peacemakers”—the leaders of the U.S., Great Britain, France, and Italy—and their advisors were quickly overwhelmed by the difficulty of balancing idealistic notions of self-determination with practical problems of diversity, identity, security and governance. It is interesting to imagine how some of the conversations might have been reshaped if a more

nuanced perspective on diversity and conflict had been accessible during the deliberations. It's hard to say that history would be radically different, but it is easy to imagine that the dealmakers and diplomats in 1919 would have had to give greater pause, and to examine more carefully the implications of the decisions they were making.

Even today, as similar discussions take place regarding the possible futures of Iraq or Syria, and as European countries deal with an unprecedented influx of refugees, and as the world gets flatter and organizations more diverse, Keith and Dora's work provides a perfect example of how social science research can help leaders, managers and policy makers think more carefully, and deliberate with greater nuance and precision, as they confront problems of identity, diversity, and conflict.

Sowing the seeds of the ethics movement in organizational behavior (Don)

When it comes to publishing, many of us are socialized to eschew book chapters in favor of referred journal publications, as the latter are usually more influential when it comes to promotion and tenure decisions than are the former. However, book chapters can be an enjoyable read because they often allow the writer to say a lot more about a topic. Stated differently, book chapters allow writers to discuss a topic in the way they want to talk about the topic, not in the way reviewers or an editor would like the writer to talk about it. Because of this, book chapters can allow the reader insights into the passions, opinions and beliefs of the "real" person in a way that a refereed journal publication often does not.

A book chapter by Keith that I have always enjoyed, and one that foreshadowed Keith's and the organizational behavior field's current emphasis on ethical decision making, is titled "bounded personal ethics and the tap dance of real estate agency" (Murnighan, Cantelon &

Elyashiv, 2001). The now well-known phrase “bounded ethicality” can likely trace its origins back to this chapter, along with two other publications from around the same time that used similar phrasing (Banaji & Bhaskar, 2000; Brief, Dietz, Cohen, Pugh & Vaslow, 2000), so until proven otherwise, I stand by this assertion.

What was it that gave Keith the impetus for doing his original work in this area? Some of the credit likely goes to his original training in social psychology and coalition behavior.

However, I also think that a lot of the motivation stems from the summer of 1992, which Keith spent at the Center for the Advanced Study in the Behavioral Sciences at Stanford, co-leading a Summer Institute on Negotiation and Dispute Resolution. There were four “adults” (Keith, Max Bazerman, Bob Gibbons, and Hank Farber—I use the term “adult” loosely here) and 20 junior faculty. While some of these junior faculty were from business, economics, psychology, or communications departments—the programs that produce many of the regular attendees at conferences like IACM—there were a few faculty from less traditional disciplines including education, forestry and philosophy. Keith eventually published work with 15 of the 20 junior faculty who attended that summer—including the faculty members from education and forestry.

In addition to the volleyball games (organized by Keith, and always played right after lunch, which I always found strange—why not before lunch?) and random social events (Thai dinner and bowling; beer and darts at the Rose & Crown Pub) there was research, and here Keith found time to get to know Kevin Gibson, an assistant professor of philosophy whose interests in ethics and dispute resolution resonated with Keith, and the two of them (with others from the Summer Institute) crafted a handful of publications with ethical decision making overtones (McLean Parks et al., 1996; Gibson, Bottom & Murnighan, 1999; Bottom, Gibson, Daniels & Murnighan, 2002; Gibson & Murnighan, 2009). Whether it is factually correct or not, I want to

believe that the summer of 1992, which I and the other attendees will never forget, was the fertile soil that helped Keith's interests in ethics come to fruition.

Back to the book chapter. The article is a qualitative study: Keith and his colleagues interviewed highly successful real estate agents working in Vancouver. They were initially interested in agents' thoughts about two particular ethical dilemmas they could face. One, labeled the *Revelation Dilemma*, concerned agents' reticence to disclose to buyers—as required by law—that their responsibilities and obligations were to the seller, and not to the buyer. The second, labeled the *Agency Dilemma*, considered whether agents would protect buyers' interests by not disclosing potentially relevant information about them to the sellers, or more often, to the seller's agent. Murnighan and colleagues analysis of the interviews revealed that the majority of agents behaved inappropriately with regard to both of these dilemmas. Moreover, connecting these unethical behaviors to agents' financial outcomes revealed that agents who did not reveal and who did not protect were the most financially successful as they generated more listings and higher sales volume.

One might think that the chapter would end there, but it doesn't. Instead of having the typical “Intro-Methods-Results-Discussion” structure, this work then breaks with conformity to take a second dive into the qualitative data. And it is a deep dive: the paper continues for another 15 pages of text *after* the first Discussion section, including another results and discussion section, before reaching its conclusion. It is in these additional pages where Keith and his coauthors introduce and develop the concept of bounded personal ethics, giving proper credit to Herb Simon's (1976) earlier idea of bounded rationality. They highlight that moral awareness is something that is often not surfaced when agents make decisions, and that some sort of stimulus might be required to get agents to think more ethically more of the time.

One sentence seems to effectively foreshadow what would happen in the future, when the author team states (page 35) “Whether bounded personal ethics applies only to agents or, more generally, to most people, is another open empirical question, but one worth addressing”. Certainly the answer to this question, borne out in the future research done by Keith and many others, is that bounded ethicality applies to everyone. Staying close to the realm of ethical decision making, we see that authors have incorporated ideas from this work into models of how people respond to (or ignore) ethical issues at work (Sonenshein, 2009; Gino, Moore & Bazerman, 2009). Others have begun to apply ideas of bounded ethicality to other areas of organizational behavior such as detrimental citizenship behavior (Pierce & Aguinis, 2015). Keith of course has continued to work with his coauthors in this area as well (Gunia, Wang, Huang, Wang, & Murnighan, 2012; Wang & Murnighan, 2011).

Work on ethical decision making reflects some of the most interesting and most important work of our time. And, like the other topics in this essay, it is yet another area where Keith Murnighan has made a hefty contribution. So if you are interested in ethics, agents, or maybe you are simply looking to buy a house, I strongly encourage you to find this chapter and give it a read.

Concluding Remarks

Looking beyond the specifics of the four reviews, two conclusions become clear. The first conclusion is that Keith cannot be accurately described as “just an experimentalist”. Not that he would mind! But while Max and Madan note some of his key contributions that derive from his experimental work, Deepak and Don note that contributions that he has made to our field via theory development and via qualitative methods, respectively. Keith is the rare scholar

who seems at home across multiple methods, and in the realm of theory as well as experimentation.

The second conclusion is how effective Keith is at collaboration. It would not surprise any of us, given that we have each coauthored with Keith. But in viewing the work reviewed in this article, we note that each involved Keith working with entirely different coauthor groups. Keith is a great example of how science advances through the efforts of teams of researchers, rather than through the solitary work of individuals (e.g., Wuchty, Jones & Uzzi, 2007). While Keith has certainly published sole-authored work (especially early in his career), he would likely agree that two heads (or even three or four heads) are better than one.

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