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60th Anniversary Essay: Ruminations on How We Became a Mystery House and How We Might Get Out

Stephen R. Barley*

Abstract

This essay responds to, largely concurs with, and extends the concerns Jerry Davis expressed in his June 2015 editorial essay in *ASQ* about the state of research in organizational theory. In particular, it discusses the reasons novelty has become such a valued commodity in organizational theory and its unintended consequences. Fault lies with the way students are trained, the reward system that most universities implicitly or explicitly use to promote faculty, and the role that editors and reviewers play in wittingly or unwittingly rewarding the quest for novelty in the peer-review process. One way to revitalize organization theory while also addressing such problems would be for the researchers to begin to focus on the myriad ways that organizations shape our society and for organizational theorists to begin to collaborate with engineers and researchers in schools of public policy who are more aware of and interested in addressing problems that organizations, especially profit-making firms, create as they seek to shape their own environments.

Keywords: peer-review process, organizational theory advancement, academic reward systems

Wielding a clever but damning metaphor, Jerry Davis (2015) accuses organizational research of having become the academic equivalent of the Winchester Mystery House.[1] Jerry must have known that I am a fan of the Mystery House. (I have visited at least three times. If you live in the Silicon Valley, there

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[1] The metaphor is rather obscure for anyone who doesn't live in the Silicon Valley (or San Jose to be more precise) or who hasn't been sufficiently hard pressed to find things to do while visiting there. Jerry did a good job of describing the Mystery House, but in case you missed Jerry's essay or read it and were still confused, I strongly recommend that you take a moment to visit these websites now: http://www.winchestermysteryhouse.com or https://en.wikipedia.org/wiki/Winchester_Mystery_House.

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really aren't many places where you can take friends and family who come for long visits and overstay their welcome. Stanford has it charms, but they only last so long, and with long-term guests, sooner or later your options wear thin.) Why else would Jerry have asked me to write an essay in response to his to celebrate the 60th anniversary of the *Administrative Science Quarterly*? Let me start by recapping the major themes that Jerry sounds that lead him to compare our field to a house with stairways that go nowhere, doors that open into walls, and most troubling, toilets without plumbing that were never meant to flush away the waste of corporeal beings.

First, and most importantly, Jerry claims that we have come to value the novel, the interesting, and the counterintuitive over the accretion of cumulative insight. In the process, he employs the word ''truth,'' implying that this is something the field should value more than it currently does.[2] Second, he raises the question of ''who should benefit from organizational research,'' noting that *ASQ* was never conceived as an outlet for practical advice but rather as a place where scholars could publish papers about organizing and organizations that would yield a body of scientific knowledge that might ultimately prove useful. He implies that at least for some time, *ASQ* served this purpose when most organizations were large bureaucracies, but now that bureaucracies are in decline and organizations are morphing furiously into new forms, our old theories are no longer as relevant as they once were. Third, he argues that our reward system is largely responsible for an undue emphasis on novelty and impact, creating a quest for citations, rather than our plodding toward an accumulation of knowledge. Fourth, he argues that if we are to move beyond impact to progress, we ought to seek answers to important questions that can yield firm answers. Finally, he argues that the field is about to be hit by a tsunami of big data and that what all those data will mean for the field is currently unknown, but he worries that given the previous themes, we may be ''headed for disaster.''

I am largely sympathetic with many of the themes that Jerry sounds, although my take on some is a bit different than his. In fact, there are many other organizational scholars, young and old, who are worried about the same issues that concern him. I recently attended a conference of organizational theorists working in California universities held at the University of Southern California. Unlike most conferences where people give papers, this conference focused entirely on discussing larger issues in the field. Let me share some of the questions and statements that Paul Adler solicited to stimulate discussion as evidence that Jerry is not alone in his worries:

1. We are preoccupied with developing new theory, especially novel new theory: this has become an empty exercise. How can organization theory (OT) do a better job of addressing the big issues of our times, such as inequality, poverty, elites, environment, terrorism, and privacy? What would a ''problem-focused'' field (or subfield) look like? How should our journals change to reflect this?

[2] ''Truth'' is a complicated concept with a dense philosophical history. Hence I will overlook the whole idea of truth as it relates to research except to say that I do believe in demonstrable facticity.

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2. What should we make of the emerging alternatives to the big corporation—the sharing economy, networks of smaller firms, cooperatives, and the gig economy?
3. Where are the theory and the teaching of ''organization design'' and ''organization theory'' today? Where do we fit in the business-school world? The subject has largely disappeared from the core MBA curriculum: what kind of renewal would it need to make OT important enough to figure once again in the core curriculum?

I take both Jerry's essay and the kinds of questions discussed at the conference at USC to be indications that organizational theory is facing a kind of existential crisis. Over the remainder of this essay I am certainly not going to resolve the field's existential malaise—I have a hard enough time handling my own—but I would like to share a few thoughts about the issues raised by Jerry's essay and by the conference at USC.

I want to start with Jerry's third theme (as I have listed them above): the incentive system for researchers, especially young researchers. I begin here because if you want to intervene in a system, you eventually have to attack or at least undermine the pilings that support it. Incentives are important pilings. We are well into an era in which academic worth is judged not just by whether you publish or even by how good your research is, but also by where you publish, how many times you publish, and how many people cite your work. You hear faculty and students talking about A-journals, B-journals, C-journals, and so on. I may be suffering from early-onset dementia—or maybe it is because I was trained at MIT when being part of the ''mainstream'' was seen as something of a sell-out and what mattered most was the quality of one's empiricism—but I don't remember this kind of talk when I was a graduate student. Of course, everyone knew that publishing in *ASQ, AJS*, or *ASR* was a feather in one's cap, but journals weren't explicitly ranked. We thought that having some publication was certainly better than none, and the goal was to produce high-quality papers no matter what the outlet. Since that time nearly 30 years ago, school after school has adopted a system for rating journals (the A's, B's, and C's) with the explicit expectation that getting tenure will hinge, at least in part if not largely, on the relative number of A-publications a researcher obtains. Of course, most of the elite schools don't publish such lists, but their faculties operate with implicit lists that are not all that different from those used at schools that make their lists explicit. Note, then, that the problem with incentives starts before there's even a paper to cite!

The emergence of explicit and implicit grading systems for journals has caused me serious problems when mentoring Ph.D. students. Every student, regardless of the quality of his or her data or paper, believes that getting an A-publication is a matter of academic life or death. As someone whose first paper appeared in *Urban Life* (now the *Journal of Contemporary Ethnography)*, I try my best to convince my students that not all papers are A-level papers and that trying to turn them into one may be a waste of time. It might be better to learn about the writing and publication process by shooting for a journal whose audience is likely to be more interested in the substance of the work than to try to aim for a high-status, generalist journal with a specialist paper that's empirically sound but theoretically blasé. Besides, if you think about it, the odds are against you. The typical journal like *ASQ* publishes about 20 to 25 papers a

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year. Let's say there are 12 top journals in organization studies. If so, there is a feasible set of roughly 250 to 300 papers that these journals will publish each year. My understanding is that there are currently somewhere around 18,000 members of the Academy of Management. Suppose every member of the Academy of Management were to aim for an A-level journal each year (this, of course, is an unreasonable assumption, but my point is to make a general point, not to be accurate). All else being equal, a person's odds of having a paper accepted at an A-journal would be about .017 (300/18,000). In other words, the incentive system that schools use is way out of sync with what is achievable by most people. Despite my best efforts at persuasion, I have a difficult time convincing my students that their careers won't tank if they don't get an A-level publication before they get out the door. Frankly, if I had thought this way when I was a doctoral student, I would have quit school and found another line of work in which the money and the hours are better (say, carpentry). To change the situation that worries Jerry, we have to change our institutions, and we all know how hard that is. Perhaps it is time for a consortium of schools to meet, evaluate the way we make tenure decisions, and agree on a procedure that stresses quality of contribution over the reputation of a paper's outlet or the number of citations it receives. In theory, this was supposedly the purpose of evaluation letters, but too many evaluators have substituted counting for reading and thinking.

In the meantime, what are students to do? They focus on beefing up their theoretical contributions even if the theory doesn't fit the data. Students are being taught that a theoretical contribution is more important than data, evidence, and methods even though they are inundated with methods courses. In many ways, the students are right. I am reminded of an apocryphal story about James March that used to circulate among the students at Stanford. Reportedly, Jim once gave a qualifying exam composed of single question: ''Name one paper that has made a theoretical contribution to organizational theory that also included a regression equation.'' The question was brilliant and carries more than a grain of truth. If you look at the most-cited scholars in our field, you will find that their most highly cited papers are almost always primarily theoretical. Even if the papers happen to contain empirics, what most people remember and cite are their theoretical contributions. I once asked students in a seminar how they read journal papers. Their response took me aback. They told me they read the front of the paper and the back of the paper but skipped the middle. In other words, they skipped the empirics. Clearly, they thought the real contribution of a paper should be theoretical. No wonder everybody is seeking to be novel and interesting! Explicitly or implicitly, that's what we are teaching our students to do.

Journals and their reviewers are also partially responsible for the situation that Jerry laments. I was recently told by a scholar, who shall remain anonymous, that s/he had a paper desk rejected from *ASQ*. There are many good reasons to desk reject a paper: the paper doesn't fit the journal, the arguments are not well enough developed, the data are too thin for the claims, and so on. But the rationale reportedly given by the editor to this particular scholar was that the paper's contribution was not sufficiently ''novel.''[3] Back in the days

[3] I took my informant's word at face value. I did not ask to see the decision letter. But whether or not the editor actually told the author that he or she was not being sufficiently novel, that is the message the author heard. So the upshot is the same.

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when logical positivists ruled the field, you never got dinged for being insufficiently novel or interesting. Even though I was an ethnographer, reviewers almost always focused on my methods, my empirics, and the warrant for my claims—all of which seemed fair game to me. They might, for example, ask for additional proof: Just how many times did you see ''X''? or How many people said ''Y'' and under what conditions? Sometimes they might ask for comparisons: Were the people who said ''Y'' different in important ways from the people who either did not mention ''Y'' or who said ''not Y''? Sometimes reviewers asked for quotes or excerpts from my field notes to back up or illustrate a claim I made. If I argued that social dynamics evolved through phases, they wanted to know by what criteria I demarked phases and whether these criteria could be made explicit and potentially observable. Were interpretations mine or were they my informants'?

In the intervening years, reviewers' expectations and demands have changed. They have done an about-face. I rarely receive any comments these days on my findings, my data, or my analysis. In fact, I am usually complimented on these before being told why the paper can't be published as is. Instead, the vast majority of comments focus on the theoretical or substantive frame of the story I want to tell. The logic of such comments boils down to this: ''You say your paper is about X, but I think it is really about Y.'' Insisting that a paper adopt a framework different than the one the author prefers makes sense only if the framework better organizes the data. The problem is that unless you are familiar with the data, there is no way you can decide on the framework's relative utility, which is especially true in the case of field work. In the absence of such familiarity, urging an author to adopt a different framework comes dangerously close to admitting, however unwittingly, that organization studies is one of the humanities. In the humanities, interpretations of novels or philosophical works lie in the eye of the reader, and here novelty and surprise are expected. Worse yet, if insisting that a paper adopt a different frame amounts to telling the author to write the paper you would have written, and if authors were to take the advice, it is easy to see how we might begin to search for novelty rather than truth (whatever that might be) and how as a field we might wind up climbing staircases that go nowhere.

Let me turn to what I see as the real danger of seeking to be novel and interesting. Some years ago, the *Academy of Management Journal* asked its board to nominate the most interesting papers they had ever read. By a thin margin (one paper), I had more papers nominated than any other author, so I was asked to write an essay on how to write an interesting paper. In attempting to fulfill my charge, I concluded the paper with a warning:

> Finally, we should consider whether we would want all papers published in our journals to be interesting. To wish otherwise might, at first, seem foolish. Wouldn't it be nice to open up an issue of *AMJ, AMR* or *ASQ* to a random paper knowing that our reading would soon transport us to some peak of illumination or discovery? I certainly would like to have this happen a little more often!
>
> But what if after reading the essays in this section most of our colleagues committed to writing interesting papers and succeeded? If being interesting requires a paper to be different, before long the field would be a mess. Every paper would take on a new topic, devise a new method or offer a new way of seeing things. With all of us so busily striving for the next interesting paper, no subjects would be studied more

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> than once, no methods would be refined and no ideas would be worked though. The development of knowledge, at least in any scientific sense, would all but cease. Worse yet, because there would be no status quo to provide a measure of which new papers were interesting, the field would implode into humdrum. At that point only by taking the risk of sticking doggedly to a topic, a method, or a theory could scholars rescue us from the quicksand of being interesting. In the end maybe we are quite lucky that interesting papers only come along every so often and that no one can tell us how to write more interestingly. (Barley, 2006: 19–20)

I do think, however, that we should be very careful not to throw our babies out with the bathwater. There are, in fact, areas of research that provide the kind of focus and concern with cumulative insight that Jerry yearns to see. In macro-organizational theory, the best example is population ecology. Hannan and Freeman (1977, 1988) brought the insights of ecological dynamics in biology to organization theory to answer the question, ''Why are there so few organizational forms?'' Whatever else you may think of population ecology, the ecologists have stuck quite close to this original question and have accumulated fairly solid answers to the question over the last 30 years. In recent years they have moved toward working with categories to elaborate their earlier findings on density dependence, generalization, and specialization. The phenomenon of sustained focus is even more prevalent in micro- and meso-organizational behavior. Consider for instance the body of work on work–family conflict. After several decades of research we have a pretty good idea of how and why work demands spill over into family life, the kinds of problems the spillover causes people, and the accommodations they make. The primary unanswered question in this literature has been left unexplored because it is culturally taboo: namely, how often do people devote more time to work than to family because they find the workplace to be more congenial and rewarding than their home life? Arlie Hochschild (1997) is the only researcher of whom I am aware who has been brave enough to write about this issue, and the response she received in some quarters was less than welcoming.

I shall end my response to Jerry's essay by speaking to two of his remaining themes: on what questions should we be focusing, and for whom should organizational theorists be writing?[4] If Jerry and many others are right, as bureaucracy dies, a host of new organizational forms are arising to fill the void. I personally wonder if the rumors of bureaucracy's death haven't been greatly exaggerated. Nevertheless, if it is true, it behooves us to take a closer look at how we came to know so much about how bureaucracies work and fail to work. Importantly, our understanding of bureaucracy was not initially built on journal papers but rather on deep and lengthy studies usually published as books. I have in mind many of the books we now consider the classics of our field, for example, Blau's (1955) *The Dynamics of Bureaucracy,* Gouldner's (1954) *Industrial Bureaucracy*, Dalton's (1950) *Men Who Manage*, Simon's (1957) *Administrative Behavior*, March and Simon's (1958) *Organizations*, Burns and Stalker's (1961) *The Management of Innovation*, and Chandler's (1962) *Strategy and Structure*. Most were based on field studies or significant historical research. I propose that there was a reason our understanding of

[4] I am going to skip the issue of big data, because I am not yet sure what I think about the costs and benefits of exploring large data sets. If done correctly, I think there is value.

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bureaucracy was significantly shaped by research published as books. First, extensive and intensive research was necessary for gathering the kind of data necessary to understand the bureaucratic phenomenon. Second, books provided the space and freedom to work through the complicated implications of what the researchers discovered and learned. They were not forced to cut their data or their theory into bite-sized chunks. If it is the case that organizations are becoming more distributed, less hierarchical, less reliant on traditional forms of employment, and so on, perhaps what we most need right now are deep and lengthy studies of work and organizations of the kind characteristic of the 1950s and earlier. Such research is more likely to reveal the questions that are worth asking. As the history of the natural and biological sciences reveals, crucial questions worth investigating do not usually occur to brilliant minds sitting in an office. Rather they come from observations and the conundrums that surface from observations. But alas, we once again find ourselves up against the constraints of how our profession has evolved. Even though we need deep studies of how organizations and employment relations are changing, our field does not easily reward such research, and it has all but totally devalued the book as a form of communication.

On numerous occasions I have suggested that organizational theory should begin to look outward and ask how organizations are altering our society (Stern and Barley, 1996; Barley, 2007, 2010). I will not repeat those arguments here. Let me simply say once again that we live in a society in which organizations are the primary social actors and that profit-making organizations are the most powerful of all organizations. I believe their power surpasses that of most governments and that we will soon find ourselves in a world in which for-profit organizations and their alliances rule the world. It is worth remembering that nation states were not always so strong. Over a period of several centuries they gradually replaced the church as the dominant institution in society. Once one takes this perspective, the number of questions that one can ask becomes bountiful. How do organizations shape laws? How do laws shape organizations? How do organizations control governments? How do organizations shape family structures? How have organizations shaped our physical environment, including our climate? What is the future of democracy in a world of organizations? If we were to begin to ask such questions, organizational theory's audience would become much clearer. We would research and write neither for ourselves nor for those who manage organizations. Instead we would research and write for the betterment of all. It seems to me that that is what sociology should be about, and I find it a shame that organizational sociology, otherwise known as organizational theory, has asked so little of itself. Perhaps it is time for organizational theorists to consider leaving business schools and migrating to schools of engineering and public policy. The pay would be less, but one would find colleagues who are far more concerned with these bigger problems and ready to do something about them.

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