"On Genre: A Few More Tips to Article-Writers"
Ezra W. Zuckerman
MIT Sloan School of Management
July 30, 2015
Revised: August 14, 2017 (slight revision August 25)

Dear web-surfer:

Seven years ago, I posted a short document entitled "Tips to Article-Writers" that proved to be surprisingly popular. It certainly has been gratifying to hear from many young scholars that they have found it useful. Some of these tips are developed further in the 2010 "Guide to Evaluating/Refereeing Research Papers" edited by the faculty of the MIT Economic Sociology Program.

In the seven years between when I first posted that piece and the first draft of this one (7/30/15), I had many more conversations with colleagues and students on the question of how to write great papers, and many of these conversations have revolved around the question of *genre*. This question—i.e., how should I frame the contribution of my paper and organize it accordingly?—is perhaps a particularly challenging one in any field that encompasses a range of different methodologies and paradigms, and it is perhaps particularly salient in a research community where journals vary in the kinds of contributions they privilege. (Perhaps the issues are universal.) And so, in preparation for the 2015 "Organization & Management Theory Junior Faculty Consortium" at the Academy of Management meeting[1], I jotted down some more tips, focused on how I have come to think about such questions.

A few notes before I get into it (with a few additional notes from 2017):

- As with "Tips to Article Writers," my focus is almost entirely on what I think are effective ways of framing papers. I will also note which genres seem to be less popular with certain journals. I wish it were the case that scholars did not have to choose between the most appropriate or effective genre for a paper and the one that is most likely to get a paper published. But they often do. And while it is easy for me to say that one should always choose the former, I realize that I have tenure and so it is easy for me to say that. At the very least, I hope that when one finds oneself adjusting a paper's genre (or other feature of a paper) to please the journals, one is aware that one is doing this.
- For simplicity's sake, I will generally be discussing various genres as if every paper is written in one genre only. In reality, many papers combine genres in interesting ways. I do think though that as with any productive activity (insert your favorite references here), combining genres can be challenging insofar as they require different ways of presenting issues and organizing material, and therefore can be confusing to audiences. But pretty much every paper does some mix of genres, and often quite productively.
- In what follows, I use many examples from my own papers, though I discuss other papers as well. I think I have some good reasons for doing this (I know my own papers best, and especially can speak to the challenges of working with or avoiding various genres). I must admit that one motivation for discussing my work is that I prefer more readers[2] of my papers than fewer. Writing papers—including this one—inevitably involves some sales and marketing. One hopes

---

[1] I'd like to thank Brayden King for inviting me to participate in the consortium and also for some feedback on an earlier draft.

[2] When I say "readers" I mean "readers." Citations are nice when you're looking for a job or a promotion, but I'm not. What I most value is someone who actually reads. And if you ping me with a question or criticism, that's awesome. I certainly do that when I read something that intrigues me and when I want to understand the paper better.

however that the pitch is honest. You'll have to judge that for yourself. (You can't beat the price!)[3]

- 2017 note 1: I never posted this in 2015 as originally planned because, while folks at the 2015 event seemed to find it productive and so have other friends and colleagues, I was hoping to find some time to make the framework here more systematic. In particular, among the things one might do is array the 10 genres on a couple of dimensions. I haven't had the time to do this yet, and since I have gotten requests to share this, I'm going ahead and doing so now. But I also added one additional genre, the "horse race," and turned the erstwhile 10th genre into three problematic genres to avoid.
- 2017 note 2: Oliver Hahl deserves special thanks for being such a supporter of this document/project and for providing great feedback and input. It is simply a joy to work with him as closely as I do.
- 2017 note 3: With Catherine Turco, I recently published a statement that captures a lot of how I think social science should be done. Reading it might help clarify what I have in mind below on one issue or another. Insofar as it is more coherent on one issue or another, that is due to Cat, who has done more than anyone over the past decade or so to make me a better scholar. Here is a link to it: http://www.journals.uchicago.edu/doi/full/10.1086/690762.

With that preamble behind us, here is the problem of genre as I see it: *While the general objective of social science is to clarify puzzling empirical patterns in the social world, not all papers are best organized around a "puzzle framing" and not all "puzzle framings" are alike*. Let me begin then by discussing a basic puzzle framing and then demonstrate what I mean by introducing other paper setups.

1. **The "Known puzzle" framing**. This genre is quite straightforward. The paper begins by reminding us that we have yet to (fully) understand a well-known empirical pattern. And so the point of the paper is to develop (and perhaps to test) a new/revised theory that has two main properties: (a) it can explain what the received theory can explain; (b) it can also resolve the outstanding puzzle. One example of a known puzzle framing come from the literature on collective action. In the wake of the publication of Mancur Olson's *Logic of Collective Action* in 1967, social scientists had a puzzle to solve—*why do we find so much collective action in the face of incentives to free ride?* Many papers have been framed around this puzzle, each one beginning by referencing the general puzzle and then framing their contribution based on the progress that has been made since Olson.

   There are many other examples of known puzzles that are the basis for large literatures. Interestingly, many of them derive from apparent deviations from rationality (e.g., *Why do people vote in elections when their vote has no impact? Why do people give tips when at a roadside restaurant?*) or market efficiency (e.g., *Why do prices often seem to depart from their intrinsic value? If the free enterprise system is more efficient than planned economies, why are capitalist economies filled with firms?*). This seems to reflect the fact that strong (economics-based) paradigms provide clear predictions that are always at some variance with observable data. And this generates business for social scientists. And then there are two kinds of

---

[3] Another note of caution: even if I am trying to be honest, I might be wrong. In my experience many scholars— even (especially?) successful ones—are not good at reverse-engineering their own success. That is, they come up ideas about why they were successful that fail when they try to apply them to new questions or fields, and/or they fail when their students try them. For why this is, see Winter & Szulanski's excellent 2001 paper "Replication as Strategy."

strategies for approaching such "anomalies"—either (a) to show that there were problems with the paradigm and that the puzzle can be eliminated by reworking or replacing the paradigm; or (b) to "explain away" the anomaly by showing that the puzzle is of limited significance when the matter is regarded in the proper way.

Note finally that while this genre seems straightforward (*Here's a puzzle, and here's how I made progress on it!*), this does not mean that it is easy to write papers in this genre. One reason is that even when scholars in agree that there is a puzzle to explain, they may disagree about how much progress has been made in resolving it, or what is the right method to use for making progress. Accordingly, it is usually easier to identify when a puzzle first became known to a literature (e.g., Olson, Coase) than it is to say who has made the most progress in resolving it. Ask 10 economists whether they agree with the decision to award Nobels to Williamson and Ostrom.

2. **The "obscured puzzle" framing.** This genre should perhaps have been discussed first because it is temporally and causally prior to the "known puzzle" framing. In particular, before a puzzle is known, it needs to be pointed out. Indeed, the great contributions of Olson and Coase were less about making progress on a puzzle than in pointing it out in the first place. In each case, they turned to the reader and said, *Hey, look at this readily observed empirical pattern. It was sitting there in front of you all this time. You may have taken it for granted but I'm here to tell you that you should give it a more careful look. When you do, you see that it is inconsistent with our best theories on the subject. So we need to roll up our sleeves and figure it out!*

This kind of paper can make a large contribution, but it is not easy to pull off. The main reason is that if it is indeed a readily observable pattern, there must be a reason it has not been recognized as a puzzle before. One possible reason is that social scientists are always moving back and forth between two different approaches to interpret social patterns—(a) using existing social theory; and (b) using their intuition as participants in the social world. For the most part, we read and react to social situations using our intuition, honed by everyday experience. And insofar as it "works" for us, we may think we understand it. But even if we understand something at an intuitive level, this does not mean that we have good theory for it. Indeed, much of the art of theory-building is about working out the implicit logic in social situations and thereby formalizing our intuition. And much of the art in writing papers in this genre is to help get the reader comfortable with the fact that they were missing something very important that was lying in front of their eyes. Put differently, you must explain to the reader why the puzzle was always visible (such that the she can recognize as soon as you note it) but unnoticed.

This challenge is exacerbated insofar as (a) it is unclear what constitutes a significant or systematic deviation from a theory's predictions; (b) defenders of the theory are inclined to insist on a higher bar for calling something an anomaly; and (c) these defenders set that bar even higher when the anomaly is pointed out by outsiders. For instance, there were many good papers pointing out empirical and theoretical problems with the "efficient markets hypothesis" years before orthodox financial economists were willing to acknowledge the anomalies; and when they engaged with them, many (but not all) tended to reframe the issues in ways that neutered the challenges. Similarly, many sociologists and political scientists pointed out theoretical and empirical problems with the "Washington Consensus" package of neoliberal prescriptions for developing/transitional economies. But these points were largely ignored or dismissed by economists until they incorporated later in the new "institutions matter"

consensus. The foregoing examples reinforce the second of my tips to article writers—*know your audience!* In particular, you need to know what your audience is likely to regard as a true puzzle and whether they are willing to hear about it from you.

These observations also raise the question of how the heck we can ever get a defender to recognize a puzzle. I think it helps if your resolution "saves" the existing theory (see tip 8 from the tips to article-writers). An example: Oliver Hahl, Minjae Kim, and I have a working paper that is an attempt to explain the puzzling phenomenon by which members of high-status categories seem to have significant appreciation for low-status cultural products. This is odd both from the standpoint of theory that sees cultural consumption/expression as a bid for "distinction" as well as theory that sees individuals as avoiding associations with low-status groups. We are optimistic though that defenders of these theories can be comfortable with our paper because our resolution does not require the reader to give up these ideas, but just to regard them in a slightly different way. In the end, we don't think the defenders will need to feel defensive. Of course, I have been overly optimistic about such things before…[4]

3. **The "found puzzle" framing.** This framing is particularly common in qualitative research, and in case studies more generally. The key difference is that the puzzle in question was *not* always lying there in plain sight. Rather, it is one that the authors have discovered in their research. The author tells the reader, *Hey, you know how we all have been thinking that the world works in such and such way? Well, it turns out that it's not so. I've done some research that the literature needs to incorporate because it is a puzzle for the received theory. The good news though is that I have a way to reconcile the puzzle and move forward.*

In many ways, the "found puzzle" framing is more straightforward than the "obscured puzzle" framing. After all, if you really have strong evidence of an empirical pattern that flies in the face of existing theory, it should be easy to sell, no? Well, not necessarily. My experience with this genre is that it is no walk in the park, for at least two related reasons: (a) if the puzzle emerges from just one case, maybe there's something "weird" or unusual about the case such that it may be dismissed; and (b) it is hard to use the puzzle to motivate the reader's interest in the paper if the reader doesn't know about it yet. Each of these issues require the author to generalize the puzzle in some way. My coauthors and I have struggled with this challenge in at least three papers.[5] In each case, the solution was essentially to argue that the "found puzzle" is actually an "obscured puzzle."

This difference in approach can be seen in the introduction to these papers. In "obscured puzzle" framings (such as in that paper with Oliver and Minjae), the data are not mentioned until the very end of the introduction. But in "found puzzle" papers, the data make a much earlier appearance. At the same time, it does not show up in the first paragraph. Rather, each of these papers begins with a framing of the puzzle in general terms and then it turns to the case to make the puzzle concrete. Each introduction also ransacks the literature in order to find anything and everything that helps support the argument that the puzzle is quite general if often obscured from view.

---

[4] Good news from 2017 is that we just published this paper. See here: http://journals.sagepub.com/doi/abs/10.1177/0003122417710642.

[5] Here are the three papers I'm thinking of (see full references on my website or vita: "Peer Capitalism"; "Nasty, Brutish, and Short"; and "Betrayal as Market Boundary".

The end product from this process isn't necessarily the best one. For example, a paper of mine with Stoyan Sgourev ("Peer Capitalism") is framed around the puzzle of why there are so many relationships between non-competing peers in the U.S. economy. Just between you and me, this framing is not our favorite but it worked best with reviewers. We preferred an earlier framing that developed the question of why capitalists are motivated to work so hard to make a buck. As you are reading this, you are probably thinking, "How is *that* a puzzle?" Right, so that is why it didn't work. But it really is a puzzle, and you can begin to appreciate it if you read the discussion section of the published paper. But it was tough to make that work in the introduction. Conversely, while I really like the puzzle that opens my paper with Pierre Azoulay and Nelson Repenning ("Nasty, Brutish, and Short")—*Why would an industry adopt worse practices when managers are highly motivated and know about better practices?*—I think people are still skeptical that this is a general puzzle. (They're wrong, of course)

4. **The "no warrant" puzzle framing**. This is the final and perhaps least prevalent of the "true puzzle" genres (for "false puzzles," see below). At least, I haven't used it too much. It is similar to the "obscured puzzle" framing in that it gets its primary motivation from pointing out something that has been hiding in plain sight. What is different is that the empirical pattern is one that scholars did in fact notice and thought they had explained. In fact, however, they had not. Here the author tells the reader, *You know how you thought that our theory had explained this important empirical pattern? Well, actually, when we think about the theory carefully, it really does not. And so what you thought was understood really still needs elucidation. And I'm here to provide it!*

    A paper of mine with Cecilia Ridgeway, Shelley Correll, and three of their students is an example of this. Here we point out that the dominant theory for explaining why decision-makers favor high-status producers/actors cannot actually be applied to many cases where such favoritism is evident. The reason is that the theory actually depends on very restrictive assumptions (that members of a population use the same quality standards, and that publicly visible status hierarchy faithfully aggregates those members' views) that rarely apply. And so this leads to a puzzle of why we see such favoritism in those other cases.

    This genre is tricky for two related reasons. First, it requires you to convince the reader that they hadn't understood the theory as well as they thought they had (see the next few genres). Second, you must convince the reader that they have a problem that they didn't think they had. This latter problem is similar to the problem faced with "obscured puzzle" framings, though it is perhaps more difficult because scholars may be more willing to believe that they had resolved a puzzle with their intuition when perhaps they should not have. It may be harder to get them to believe that they had used a theory inappropriately.

5. **The "alternative hypothesis" framing.** The next several genres are related to the prior one in that they each are rooted in telling the reader that the literature had less warrant for believing something than it thought it did. As a result, authors face versions of the challenges noted in the prior paragraph.

    The first of these genres is like the previous one in that the author tells the reader that she had more confidence in the existing theory than it deserved. In this case though, the author tells the reader that she was *right* to believe that the theory can explain the known evidence. But the

reader was wrong to believe that the existing theory was *alone* in being able to explain that evidence. In fact, there is an alternative hypothesis that does just as well—and perhaps is even better in some respects. Ideally, the theory presented by the author is even better than the existing theory—either because it requires fewer assumptions, it is more realistic, or it can explain some things that the existing theory could not (obscured or found puzzles). The ultimate is when the new theory can make "out of sample predictions"—i.e., it predicts the presence of anomalous empirical patterns that can now be hunted for and found.

I personally have not really written papers in this genre but some of my favorite papers by others are in this vein. One example is David Strang and Michael Macy's excellent paper "In Search of Excellence." There they show (and see David and coauthors' follow-on work) that if we merely make the reasonable assumption that managers are "adaptive emulators"—i.e., that they adopt popular practices among their peers because they reasonably infer that popular practices are most effective—this mechanism can produce observed cycles in managerial fashion—a phenomenon which organization theorists had previously ascribed to the presence of pressures for "isomorphism" or conformity. Moreover, whereas the prior ("neo-institutional") literature had—and I would argue still has—no clear account of how an institutionalized practice might become de-institutionalized, Strang & Macy's approach can account for this (once one assumes that there are limitations on managers' ability to infer quality from popularity). It is a bad sign that a 1983 paper by DiMaggio & Powell continues to be more highly cited than the 2001 paper by Strang & Macy even though the latter clearly improved on the former.

Another example is in a 2007 paper by Jerker Denrell and Gael Le Mens called "Interdependent Sampling and Social Influence." Here they provide an alternative explanation for why two people in a social relationship might come to have similar attitudes and/or practices—i.e., interdependent sampling. The key idea is that given how costly it is to experiment, we will tend to stay with practices that we like after our initial experience with them while avoiding those with which we had a bad initial experience. But if there is a significant chance element in that initial experience, this means our initial experience is given more weight than it should. And so a new relationship gives that practice a second chance to make a good impression, thus causing people to update. This is a very interesting alternative explanation for social influence. I find it extremely appealing because I can intuit it quite readily and it seems extremely general. Indeed, Denrell and Le Mens have written a series of enormously insightful papers in this vein. And what makes them even more compelling is that they go beyond providing an alternative explanation for to existing theory, but they also show that their theory explains phenomena that the prior theory cannot. (In that sense, it generates puzzles for the existing theory, which it then resolves, clinching its claim to make theoretical progress).   In particular, Denrell and Le Mens discuss in this paper how "interdependent sampling" predicts that friend A is more influenced by friend B when the latter had a negative attitude before their relationship began than when friend B had a positive attitude. Go check it out and find out why!

6. **The "missing evidence" framing.** This is another "no warrant" framing where the issue with the past literature's claims is again that the existing theory explained less than was thought. In this case, the reason is not that the theory's scope is more limited than previously thought (genre 4) or that there are alternative explanations for what was attributed to the theory (genre 5) but that the evidence upon which our confidence in the theory rested is rather flimsy. Essentially, the author tells the reader, *You know how we are so committed to idea x? Well, we actually*

have little support for believing it. So I'm here to help by providing better evidence to justify our beliefs.

There are many good examples of this genre. They include at least three recent contributions to the literature on status advantages, all published in *Management Science*—Azoulay, Stuart, Wang, Simcoe & Waguespack, and Kim and King. In each case, the papers make convincing cases that we didn't know what we thought we knew and they put our theory on better footing by providing clearer, cleaner evidence. I have written a couple of papers in this vein myself.

In fact, this genre is the main reason I have long wanted to write up a document like this. That is because, while I suggested in "Tips to Article Writers" that social science papers are all about puzzle solving, my most influential paper—"Categorical Imperative"—is in fact framed as a "missing evidence" paper![6] I obviously did not think very self-consciously about genre in those days since I didn't realize this till years later.[7]

Note that one of the tricks of this genre is that while they are framed as mundane, empirical exercises, they can often make significant conceptual and even theoretical contributions. In a sense, they can be stealth versions of the "theoretical confusion" genre discussed below. Essentially, one reason that the evidence was missing is that the existing theory was a bit unclear and so it was unclear what would count as evidence. And so one task of the author is to restate the theory so as to clarify what kind of evidence is needed.

In my view, my colleague Roberto Fernandez is the master of this genre, including in its stealth mode. He repeatedly identifies weaknesses in the evidentiary basis for existing theory, clarifies what kind of data would really be needed to test it, and goes about finding such data. In so doing, he often cleans up some theoretical confusion. For example, while his 2000 AJS paper with Emilio Castilla and Paul Moore is motivated by the absence of evidence regarding conflicting theories about employee referrals, perhaps the paper's greatest contribution is to integrate and clarify these theories and how they relate to one another.

This genre is challenging to work in for at least four related reasons. First and foremost, while you may think that the evidentiary basis for existing theory is lacking, others are liable to disagree especially if they already believe these things. Second, someone must really be missing the data in that if they didn't have it, there would be good reason to doubt it; put differently, there must be some kind of alternative hypothesis that is lurking.[8] It seems that organizations journals such as *ASQ* and *Organization Science* do not like this genre. They insist that every paper make a "theoretical contribution." My own view is that most of what passes for a theoretical contribution tends to add more confusion than clarity (usually because the assumptions and logical moves that constitute the theory are unclear; see the next genre), and that the provision of missing evidence can be extremely valuable, especially because they often provide conceptual and theoretical clarity as well. Finally, this genre can produce tensions between different traditions for doing empirical research. For example, a paper by a recent MIT alum was framed in part based on the idea that the past literature had assumed that cultural

---

[6] FYI, some readers might be interested in the recently published "Categorical Imperative Revisited," which updates the theory.

[7] Two other papers that are basically framed in this way are two 2003 papers on the movie industry with Tai-Young Kim, one of which also included Ukanwa and von Rittmann: "Critical Trade-Off" and "Robust Identities or Non-Entities?"

[8] Thanks to Minjae Kim for reminding me of this point.

producers are not profit-maximizers ("art for art's sake," in the words of the economist Richard Caves). Some journal reviewers—operating in a different empirical tradition-- rejected this assertion, saying that this was already well-demonstrated. As a consequence of this experience and some others, one of the main dilemmas we face in the MIT Sloan Economic Sociology PhD Program is that we often find ourselves advising students to frame their papers as "missing evidence" papers but don't know what to say when they respond by saying that organizations and management journals will not like such papers. Maybe creating a bit more awareness of the problem will help?

7. **The "clarifying confusion" framing.** This is the last of the "no warrant" framings where the author tells the reader that they didn't have sufficient basis for believing what they thought they believed. And whereas genres 3, 4, and 5 involve a mix of theoretical and empirical issues with the past literature, and genre 6 focuses primarily on empirical issues, genre 7 is focused on theoretical issues. In this, the author alerts the reader, someone who is presumably well-versed in a literature and committed to it, that he has not understood the literature well. Sure, the existing theory is based on a very reasonable intuition and it seems to resolve an otherwise puzzling pattern. *But I'm here to tell you that when you look at it carefully, the theory is problematic (either because it has problematic implications or it doesn't entail what it purports to entail). The good news is that by the end of the paper, our theoretical edifices will be on surer footing.*

This genre is another once where my views are not representative of the fields I travel in, especially the "organization theory" field. In particular, my view is that it is quite possible—and indeed quite common—for large social science literatures to develop despite quite shaky theoretical foundations.[9] And this can lead to substantial confusion over what the assumptions and logical moves that constitute the theory. An example: there were very large literatures on "embeddedness" and "social capital" in the 20 years from 1985 to 2005. And these literatures often touched on the same empirical phenomena (e.g., the prevalence and effects of strong ties among industry practitioners), but I challenge you to ask 10 people who participated in these literatures to explain to how these two concepts and associated literatures are related to one another. You will get 10 answers (OK, maybe 7 or 8…). Relatedly, over the past decade or so, a new literature has grown up based on Zelizer's notion of "circuits" and which promises to turn the page beyond the embeddeness literature. But again, I doubt strongly that if you will get reliable answers were you to ask 10 people who participate in that literature what its relationship to the prior literatures is. Too often, the page is turned without our being clear what we learned.

So I think theoretical confusion reigns widely, and that is a very important job to try to clean it up. I have tried to do this on several occasions, and some of my favorite papers by others do this as well. The problem, however, is that these papers are tough to get published. Not only does one get defensiveness from the authors who contributed to the existing theory, but others in the literature are often unenthusiastic about copping to the charge that they haven't

---

[9] I am relatively ignorant in the natural sciences and so do not venture to speculate as to whether the situation is any better there. My sense, from my little exposure to natural scientists participating in social science areas that I know well, is that it is not. In particular, my view is that papers on social networks in the general science journals—*Science, Nature,* and *PNAS*—are generally inconsistent with the stereotype that the natural sciences have solved these problems.

understood a theory well. My sense is that these kinds of papers are easier to publish in sociology (and probably other social science) journals than in management and organizations journals where there is much stronger fealty to the myth that anything that is published has been validated as being good research. That said, I don't think they're so easy to publish anywhere.

They do exist however, even in the organizations and management literature. One example is Casciaro and Piskorski's (2005) *ASQ* paper on resource dependence theory. This is a very nice paper in my view, but it also illustrates some of the challenges of writing in this mode. Basically, what they suggest is that Resource Dependence Theory—as a theory, if not a "perspective"--is problematic at its core. In short, RDT conflated two different problems and their corresponding solutions. And once one teases them apart, one is left with two different theories that are each reducible to other theories, and so RDT is basically extraneous. As you read this, you might be saying to yourself, *Ouch!* Right. That is exactly why these kinds of papers are hard to write. But I think Jeff Pfeffer (and Gerald Salancik, may he rest in peace) can take it. RDT was very valuable as an orienting framework, and these guys did much valuable work that has stood the test of time. But as a theory, RDT was problematic from the get-go. In particular, it conflated the problem of *asymmetric dependence* (which is well understood by complementary traditions in exchange theory [see Emerson 1961 and later] and industrial organization economics [see especially Porter 1980]) and *mutual interdependence* (which, at least as discussed by RDT, is essentially identical with Williamson's Transaction Cost Economics). Despite its being a very fine paper, I fear that Casciaro & Piskorski is not widely read because it effectively buried RDT (which really was not widely used by that pointz) rather than reworking it in a way that it shed distinctive light on a puzzle. I do think though that the C&P should be taught widely in graduate seminars as an example of how literatures can get confused.

While I really like this genre, I have had mixed success with it. Ray Reagans and I spent the better part of the middle 2000s focused on a paper that was intended to clear up theoretical confusion regarding Burt's "structural holes theory." This paper was extremely challenging to write, not only because we are Burt's students. The original version of the paper had an empirical test and was devoted substantial focus to addressing problems with Burt's "constraint" measure. The paper that was eventually published was a pure theory paper using computational simulations (which Ray and I had to teach ourselves to do) to provide internal validation. The early version of the paper was rejected by two journals (AJS and ASQ) and the later version was rejected again by one of the earlier journals (AJS) in a highly contested decision and process. At that point, we were tired of the review process and we were grateful that Glenn Carroll was interested in our idea of publishing the paper in a symposium in *Industrial and Corporate Change*, where we got the opportunity to have a commentary and debate (which allowed us to add an extension that was essentially a second paper) with Burt and others.

Unfortunately, while we think we made a contribution in cleaning up substantial confusion in this literature,[10] and while we think the reworked theory provides a clearer, more robust

---

[10] In short, the confusion is that the original formulation of the theory conflated two kinds of "structural holes"—those within clusters of proximate/redundant actors and those between—and it thereby implied that all structural holes afford greater power and knowledge to those who bridge across them. In fact, this is only true for between-cluster holes, insofar as a bridge between clusters facilitates exchange or flow between otherwise disconnected parties. Such bridges are difficult and risky to build, however. Adding more within-cluster holes ("second sourcing") is a surer way to increase power, though it comes with less upside potential for enhancing knowledge and power. So there is a basic tradeoff that the existing theory obscures, and one can show that this tradeoff is

foundation for future research, I think its impact to this point has been limited. It may have been a mistake to publish it in a less prominent journal (though I think ICC has been consistently strong journal over the years, one of the few that facilitates genuine interdisciplinary conversation). But the main problem is simply a resistance to the idea that the literature was confused or that it is important to clean it up. Note for instance how, in Podolny's response to our paper, he essentially says, "*Who cares? We've been using this theory for awhile. We've moved on to other topics. How is it productive to go back and tell us we've been confused and try to rework the theory?*" Perhaps he's right. Ray and I didn't agree of course. We think it can only improve a social scientific literature when one shores up its foundations, especially if you can see further from the new vantage point. And our second paper in particular shows how we can. Did you know that structural holes theory can be recast in a way that it is a version of exploration/exploitation trade-off and a series of other well-known trade-offs? I certainly didn't till we worked through the confusion.

One needs to be realistic, however, and recognize that it is tough to sell such papers. So in several cases in recent years, I have taken the opportunity afforded me by being invited to write essays of various kinds to use such forums to attempt to clean up theoretical confusion.[11] These pieces may not be widely read, but at least I get to say my piece unfettered and I can always force my students to read them! ☺

I should conclude this long section (*did I say this was my favorite genre?*) on a more hopeful note, albeit with some sobering lessons as well. In particular, while "clarifying confusion" papers are indeed hard to publish, it is the case that one of my most impactful papers is in this genre. I am thinking of "Middle Status Conformity," written with Damon Phillips. This paper suggests that there is an old idea that fell out of favor because its (later) users did not conceptualize it clearly and operationalize it well. My sense is that this is a very common phenomenon. There are many papers that purport to present theoretical results with some bearing on our confidence in a theory, but where operationalization and identification is fuzzy such that it then becomes unclear where the literature stands. And so we end up rather confused about what we know or don't know—*this seemed like a good idea, but the data are noisy; ugh*. In this particular case, Damon and I thought that the problem did *not* reflect the original formulation of the idea (if you read the relevant papers from the 1950s and 1960s, you will generally find that the ideas are quite clear), but how they were applied and tested later. And so Damon and my task was to rewrite the theory in such a way that we would realize that the empirical tests that had sown confusion were largely off point, and the idea would become more readily translatable for future research.

So if this paper got published and has been influential, why do I say it too carries sobering lessons? Two reasons. First, publishing this paper was perhaps a fluke in that journal articles generally must pay lip service to the myth that recent ideas are best because the newer papers build productively on prior ones. (I do think that this is true in the long run, but we need to stop pretending that it's always true). So how did we avoid having to honor this myth? Well, maybe it was in part because the literature in question was so old that the people who mucked it up

---

kindred to a whole family of familiar tradeoffs that have similar risk/return profiles based on the redundancy/nonredundancy of the opportunities they represent).

[11] Examples include "Why Social Networks are Overrated;" "What if We Had Been in Charge?"; "A Sociological Approach to Market Efficiency;" "Optimal Distinctiveness Revisited"; "Construction, Concentration, and (Dis)Continuities in Social Valuations"; "In Either Market or Hierarchy but not Both Simultaneously" and "Categorical Imperative Revisited." The last one also includes some clarification of my own confusion.

were no longer around to defend themselves! (Brayden King once noted to me that this paper was remarkable in that it did not cite any paper published after 1970 in the introduction; naively, this had never occurred to me before) So this may be the exception that proves the rule. (And btw, I do think this paper would have had more trouble in organizations journals, where the myth of progress is stronger). Second, while this paper was perhaps successful in drawing attention to the idea that there is often a curvilinear relationship between status and conformity, Damon and I have long thought (indeed, we even predicted this would happen!) that we failed in getting the literature to build on this paper as productively as it should. In particular, if you've read this paper, do you recall that we actually emphasize that the parabolic relationship between status and conformity is relatively *rare*? If you didn't then we failed in clearly communicating what we were trying to accomplish. More often than not, when someone tells me that they have a paper that builds on "middle status conformity,' I tell them that if you read our discussion of scope conditions, the analysis in their paper is unrelated to our theory.[12]

8. The **"Extending Theoretical Scope" Framing**. This gene is in some sense the flipside of the "no warrant" genre. Rather than showing that the existing theory had overclaimed and that there is still a puzzle to be explained, this paper tells the reader, *You know that theory that we know and love? Well, it's even better than we thought! It can even explain an empirical pattern that had heretofore gone unobserved or unexplained*. Note that this genre is also akin to the "alternative explanation" framing insofar as papers in the latter mode are even more powerful when they make out-of-sample-predictions. Here the out-of-sample predictions are made in the service of already-dominant and popular explanations rather than unconventional or unknown ones.

An excellent example of this genre is Andras Tilcsik's recent ASQ paper "Imprint–environment Fit and Performance."[13] This paper came to my attention because it won two awards in the summer of 2015. When I perused it, it struck me right away that this is an exemplar of the "extending scope" genre. What it does is to extend imprinting theory to individual performance. In particular, it shows that employees perform better when the employer is experiencing environmental conditions that approximate the conditions when the employee was first hired (and socialized, and developed her skills). This is a very interesting extension of imprinting ideas. And while Tilcsik claims to be resolving a "puzzle" in the introduction (in particular, he suggests that other literature implies that employees will generally do best if they were socialized during times of scarcity), I'm not sure this is really a puzzle for anyone (he never repeats this point or really uses it to develop null hypotheses). But I think the paper works very well because it is a very compelling extension of imprinting ideas. Indeed, the theory section is an excellent treatment of the relevant issues and the empircs seem quite compelling.

I think the pitfalls of this genre is that reviewers and journals may like this genre *too much*. That is, the problem is that in their enthusiasm for the author's success in claiming new territory for a favored theory, that the reviewers allow the authors to overclaim—either not holding the paper

---

[12] Note that we ourselves are to blame for some of the confusion. See our recent paper with Catherine Turco ("Betrayal as Market Boundary"), which tries to clear up some of that confusion. A terrific recent article by Hu & van den Bulte clears up still more confusion to which we contributed.

[13] Another excellent example that comes to mind is by one of Tilcsik's advisors and coauthors, Chris Marquis—his 2003 ASQ paper that extending imprinting theory to explain the level of geographic concentration in corporate board interlock networks. This too is a fine piece of work.

to high enough standards when it comes to the evidence needed to support the theory, by not requiring sufficient theoretical clarity, or by not pushing them to rule out alternatives. Let me be clear that I don't believe this is the case with Tilcsik's paper.

But I do think it is all too common for literatures to lap up "extending" papers with too much enthusiasm. Since I've said so in print already, I will just point the reader to my recent AJS review of Lyn Spillman's book for an example. In that case, the issue was claiming too much on behalf of culture. And if you read my recent Contemporary Sociology review of Padgett & Powell's recent book, my worry there is too much is claimed on behalf of social networks. Since I'm pointing the finger at others, I'll close off discussion of this genre by noting that I myself have benefited from overly enthusiastic reception for scope-extensions. In particular, my most cited paper—"the Categorical Imperative" paper—was certainly preaching to the sociological choir insofar as it claimed financial markets as sociological territory. I did not have difficulty getting this paper published, and I perhaps had an even easier time publishing a 2004 paper in ASR that built upon it. I'd like to flatter myself in thinking that that is because these papers were particularly good. I think it's because they were saying things that sociologists wanted to hear.

9. **The "Horse Race" Framing**

Let me close out this grand tour of genres with two opposing genres: "the horse race" and the "false debate." In my 2015 version of this document I focused only on the latter but I actually now think they each have their place. So let's start with that one, which is well known. With the horse race, one motivates a paper by saying, *There are two competing ideas about why something we care about happens, and I have a unique way (via a distinctive method or source of data) of saying which one is right.*

This genre overlaps with genre 5, the "alternative hypothesis" framing. But whereas in that framing, the point is to introduce an alternate explanation that is a potential contender for explaining a particular empirical pattern, this "horse race" genre is about providing evidence to adjudicate between two rival explanations, ruling one in and/or ruling the other out. This genre is also related to genre 6, the "missing evidence" framing. But whereas that framing is about finding evidence to bolster an existing explanation, this one is about finding evidence to say which of two or more explanations deserves more confidence.

My sense is that it is well-known that this is a tough genre to pull off. In my mind, I hear the echoes of many cases where journal reviewers expressed skepticism about the whether the paper in question was "truly" a horse race in the sense that it effectively ruled in one theory and/or ruled out another. I think the main reason is that it is not clear how much any one empirical test can contribute to our confidence in one theory vs. another. Also, if each theory identifies important mechanisms of wide applicability, the real question is where they apply more or less strongly. And then the point of the empirical test is not really to make us more confident in one theory or another so much as to teach us something about the nature of an (important) empirical case.

When presented in these terms, I think horse races can be useful. Consider a series of recent papers by Roberto Fernandez and his students, as summarized here. The key question is whether the limited mobility of women up the corporate hierarchy (the "glass ceiling") is due to demand-side factors (i.e., employers' favoring men over women) or supply-side factors (i.e.,

women not presenting themselves in application pools). Roberto and his colleagues show that it is more often the latter. This of course does not mean that the supply-side is more important than demand-side *in general*. But it does it helps us diagnose what is going on in the contemporary workplace and thereby provide guidance as to where we might focus our intervention efforts.

Another example is from a working paper that Pierre Azoulay, Michael Wahlen, and I are about to complete. This paper actually mixes genre 8, "extending theoretical scope" and a "horse race." The extension in question involves taking ideas from the literature on "reputational entrepreneurship," which had previously been applied to domains of culture and politics and extending them to the domain of science. The horse race is between three predictions for what might happen to the citations to a scientist's papers after they die: (a) citations go down, because the scientist as reputational entrepreneur is no longer around to promote her work; (b) citations go up because associates of the scientist mobilize to memorialize her work and this creates more buzz around it;[14] and (c) there is no reaction, either because citations to the paper are unbiased estimators of a paper's quality or because the first two mechanisms cancel each other out. What's key about this horse race is that it has several compelling null hypotheses. Of course, no one really believes that the citations market is "efficient" but it's still an important benchmark because so much of academic/scientific practice proceeds on the assumption that citations are at least reasonably good useful measures of quality. In that sense, it is useful to know if it's either (a) or (b) rather than (c). And then adjudicating between (a) and (b) is also useful. Of course, both could be operative. But knowing that one is stronger than another tells us something useful about how promotion of scientific research works. If it's (a), it suggests that what's key is the motivation to promote the work (the author of a work is presumably most motivated for it to be recognized); but if it's (B), it suggests that what's key is that the promoters appear neutral. Have you ever noticed that people are reluctant to tweet their own work, but they'll tweet the work of others? The key point to notice here is that you can learn something from a horse race even if the point is not to completely rule out one or the other horse as a contender in other races.

10. **The "False Debate" Framing.** This framing is in some sense the reverse of the prior one but also can be seen as a subtype of genre 7, "clearing up confusion." In this case, the author tells the reader that what they thought was an unresolved debate between two arguments (one for which horse races had perhaps been run but they had failed to get either side to stand down) was really no debate at all. The argument is that *when you look at the matter carefully, it turns out that the literature had been confused to think that it had to choose between one or the other side*.

The best example from my own work is a paper that Ray Reagans and I wrote in 2001 called "Networks, Diversity, and Performance" and a follow-on paper in 2004 with Bill McEvily (which uses much better data). Here we address an issue that the literature on diversity and performance had been puzzling over (see 1998 review by Williams and O'Reilly), which is whether demographic diversity (e.g., having more than one gender or various cohorts in an organizational unit) promotes or reduces performance. Our contribution was to demonstrate that in fact there was relatively little debate about the underlying effects that diversity has, at least in terms of social network processes (as they relate to performance). And we tried to

---

[14] Here I have to give a shout-out to the awesome paper by [Lang and Lang](), which is the inspiration for this idea and our paper more generally.

explain why we thought there had been so much debate. The issue was that the literature had (quite reasonably) been framed around the assumption that managers could manipulate the demography of units but not their social networks. Ray also has a nice paper ("Close Encounters") that does something similar for the literature on how the demographic distributions (e.g, how many blacks are there relative to blacks) affect homophily (e.g., rates of intra vs. interracial friendships).

I think these can be very productive papers, and I have less to say here in terms of caveats. Perhaps reviewers and journals are always receptive to being told that everybody was right!

So instead of issuing a caveat about this genre, let me say how it can be more productively used. In particular, I think this genre should be used in combination with horse races to refine our understanding of how different mechanisms sometimes compete and sometimes complement one another. My general view is that if a theory really is well-constructed and has been shown to have some degree of external validity, it cannot and should not be put aside. (This is what I meant in "Tips to Article Writers" when I said that we should try to "save" the existing theory). But it may be more or less useful relative to other theories in a particular context. And with some work, it should be possible to integrate these theories so that we understand each as a special case of some larger theory.

As an example, I once wrote a fun paper with John Jost called "What Makes You Think You're So Popular?" Looking back, we could have framed this paper as a horse race between Scott Feld's theory that people generally have fewer friends than their own friends (*this is not only an empirical fact but a mathematical necessity!*) and the prediction from the social psychology literature that people think they are better than average on various positive outcomes, including popularity. Instead, I think what we do is retain Feld's insight but showing why the social psychological mechanisms are stronger, at least in the case we studied. Progress is made, I think, but not by throwing out the baby with the bathwater.

Coda: Three Problematic pseudo-genres

I will close this review by noting three related "pseudo-genres" I see often and wish I would see seldom if at all: (a) the "literature has overlooked" pseudo-genre; the "let's open the black box" pseudo-genre; and the "literature-based puzzle" pseudo-genre.

The first of these pseudo-genres gains its leverage not from resolving a problem but from extending the existing literature to show that it can raise its flag over more territory—that which has been "overlooked." The second motivates the paper by saying that we understand the relationship between x and y, but we need to understand the underlying process that mediates between x and y. Finally, the third pseudo-genre is focused on solving a puzzle that emerges from past research but without referencing whether that puzzle is one also in the real world.

Literature has overlooked. My sense is that many people (would like to) think that this is a puzzle framing. Indeed, since the posting of "Tips to Article-Writers," it has happened quite often times where someone has shared with me a paper and told me that it is framed in terms of solving a puzzle when in fact it is a "literature has overlooked" paper. To be clear, one can use the language of "overlooking" to discuss an obscured puzzle—as in, *Hey, we believe in a theory that entails empirical pattern x but actually, we sometimes/often see not-x; what's up with that?* So if this is what people mean when they talk about "overlooking" then fine. All too often

though, there is nothing surprising or puzzling about the empirical pattern in question. It's just that nobody has specifically studied it. In short, my view is that to constitute a contribution, it is insufficient to notice a phenomenon in a domain covered by a literature and push to incorporate it in that literature. To be worthy of our attention, the phenomenon must cause some kind of trouble for at least some relevant theory, thus requiring it to be reworked. Otherwise, I don't see why we—as social scientists-- should care.

A related observation comes from a paper of mine (with Kim, Ukanwa, and von Rittmann) called "Robust Identities." A key empirical pattern in that paper is that actors who concentrate their work in a particular labor market category are more likely to continue in that category (and therefore remain working in an industry). We argued there that one reason for this is because such actors have succeeded in becoming recognized (by labor market intermediaries) as being skilled in that category (and unlikely to be skilled in other categories). But an alternative explanation comes from the labor-market literature on matching processes—this pattern could reflect a matching process whereby workers and employers learn over time who is well-matched with which category. Note however that there are no papers in the labor economics literature (that we knew of) that specifically argued that matching processes pertain to labor-market categories. But nevertheless, a bit of thought leads one to recognize it as a reasonable extension of that literature. And so one that we needed to address (I think we did this adequately, but I would have loved to really have nailed it).

The general implication is that straightforward extensions that do not require much brainpower do not make for interesting papers. Just like we did not need an actual published paper to be forced to deal with the alternative from matching models, we do not need papers that tell us what we might imagine if a sophisticated user of a theory were asked to think about a case that has not yet been specifically considered.[15] And this is who we should be writing for: a reader who is at the forefront of their field and who is armed with the facts about our case. If we can teach something to someone like that then we really have something.

Let's Open the Black Box. This pseudo-genre is commonly articulated as an argument for attention to "process." Who could argue against the obvious point that it is better to examine the underlying process that is responsible for a causal relationship we care about? Me, that's who. I don't believe the problem lies in my lack of intellectually curiosity. Rather, the issue is that I place high value on *parsimony*. In general, theories reflect balances between explanatory power and parsimony. Even if we can increase explanatory power by having a less parsimonious theory, we should avoid it except when the increase in explanatory power outweighs the loss in parsimony. Obviously, this is something of a judgment call. For instance, there was a debate in the 1990s as to whether the parsimony of "density dependence theory" came at the expense of too little explanatory power. I think a reasonable argument could be made on either side, but I agree that parsimony was being undervalued by those who were attacking dependence theory for being too thin. But if it's tough to say how much to weigh parsimony versus explanatory power, the problem is that some others don't recognize the tradeoff between parsimony and explanatory power or don't even argue that opening up the black box will produce an increase

---

[15] When making this point lately, I often quote a character from Aaron Sorkin's HBO show "Newsroom" on this point. Through his show, Sorkin critiqued debates on TV news shows by arguing that the problem is that they failed to present "the best possible form of (each) argument." If that's the ideal for news, it should also be the ideal for social science.

in explanatory power. Rather, the case is often (if implicitly) made that process is *per se* desirable. I think this is wrong and that it is problematic because our conceptual and theoretical landscape is littered with open black boxes with a lot of confusion about how to make sense of the messiness we find inside and why it matters for outcomes we care about.

A point is worth making here as it relates equally to each of the prior problematic pseudo-genres. In particular, it is frequently the case that papers present themselves as exercises in "exploration." This seems perfectly reasonable, right? Often we find an unexplored context or process where it is unclear what we might expect to see, and it seems like it would be productive to report back from our travels to our fellow social scientists who don't know about that context or process. The key problem with this is that it then becomes unclear what constitutes progress (such that, e.g., the paper might merit publication). How much exploration is enough? How do we know that the context or process really is interesting? I would submit that the only way to measure progress is in the terms laid out in the ten genres laid out above. Basically, if the exploration exercise does not help to identify a puzzle (genres 2 and 3), resolve a puzzle (genres 1, 2, and maybe 3), provide important evidence for what we already believe (genre 6), develop a new idea to compete with one we believe in (genre 5), demonstrate the value of an existing theory in a surprising domain (genre 8), or help adjudicate between two rival ideas (genre 9), then it is unclear why we should care.   And this is why red flags always go up for me when I see "explore" in an author's statement about what the paper is meant to do. All too often, it is a sign that the author does not know what the contribution of the paper is, and so they are hiding behind the idea that exploration is a wonderful thing.

Literature-based puzzles. Finally, I think a warning is warranted: authors often motivate their papers by presenting us with "false puzzles" or what I would call "literature based puzzles." These are puzzles that are purely internal to a literature but do not have any referent in the real world. In these cases, the academic literature that originally developed to explain real-world phenomena has built up a theoretical or empirical paradigm that has, in the course of trying to make progress, become so decoupled from the real world that even when it solves puzzles, it is not really making progress. Usually, this is because there are very good reasons why the experimental or theoretical world that is conjured up by researchers cannot occur in the real world.

There are several examples of literatures that come to mind which I think were dominated for quite a while by false puzzles of this sort. I am not calling them out here but if someone wants those examples, just ask me some time. It is amazing to me how much intellectual firepower and public money is sometimes spent on enterprises that do not do anything to further our understanding of the real world.   To be sure, I guess one can argue that these are useful intellectual exercises much the way that pure math purportedly is (I certainly cannot judge that). But the key difference is that math is not billed as helping us understand, predict, or even intervene in the real world. And social science is always justified on the basis of one or more of those criteria. So then there is a mismatch between how the work is justified and what it can actually accomplish; and then many people can be led astray.   At the end of the day, it is crucial to have a sense of what real world puzzles you are trying to solve and to be able to justify your research on that basis.