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WHEN I WRITE MY MASTERPIECE: THOUGHTS ON WHAT MAKES A PAPER INTERESTING

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Academic papers are a bit like rock and roll bands: whether an audience finds them interesting is a matter of perspective, if not taste. We all know there's no accounting for taste. There's no unanimity of taste, either. This is why readers seem to disagree vociferously with every list of the Top 50 albums *Rolling Stone* publishes. For proof that academia suffers similarly diverse tastes, one need go no further than the poll of *AMJ* review board members that motivated my writing this paper. Although the count of nominations for "most interesting paper" indicated that I had more papers nominated than anyone else, the honor was thinly won. The margin of decision was a small handful of votes. In fact, if just one ballot had fallen victim to a cluttered inbox, this moment in history might have turned out differently. Although I am honored by the results, the caveat is clear: You should be skeptical of any authority that might accrue to me as a result of the balloting. On this score, George W. Bush and I finally have something in common—although my election, however narrow, did actually reflect a plebiscite.

There is a second similarity between academic papers and rock and roll bands. One might think that finding a band's music interesting would be synonymous with liking it, but we all know better. For me, at least, King Crimson, Nirvana, and Primus are quite interesting, and I appreciate what they are (or were) trying to do. But I do not like them! I own one CD by each band; I've listened to each CD just once; and I have no intention of acquiring more music by any of them. On the other hand, I possess hundreds of recordings of Grateful Dead concerts as well as every album and CD the Dead ever cut (in some cases, multiple copies). Hardly a day goes by without my listening to at least one of these recordings. My oldest son even jokes, with good reason, that until he went to college the Dead provided the soundtrack for his life. Yet I never think of the Dead as being interesting. The Dead and I are way beyond that! So it is with academics. Whether our colleagues find our papers to be "interesting!" (I loved it!) or "interesting" (I read it, appreciated it, and shelved it!) is signaled by an intonation that anyone who has heard comments muttered in hallways after academic talks can easily mimic.

Rock also teaches us that being interesting does not imply being important. Consider the strange fascination the music of Tiny Tim, Herman's Hermits, Napoleon XIV, and the Count Five once held for some people. If you've never heard of these musicians, consider yourself lucky. Your ignorance is bliss and proves my case. Tiny Tim brings me to my second caveat: when it comes to being interesting, you should be careful what you wish for.

Finally, rockers and academics share another characteristic: a peculiar kind of cluelessness. Although many people can teach you how to play guitar, no one can teach you how to play guitar like, say, Jerry Garcia, including Garcia himself (even if he were still alive). Whatever it was that allowed Garcia to play like Garcia was tacit: A feel, or maybe a sensibility. I don't think Garcia or any other musician (including Robert Fripp!) ever woke up and said, "Today, I'm going to play something interesting." It just happened or it didn't. Yet, with a little effort and the right vocabulary, all of us could describe what it is about a virtuoso's style that strikes us as interesting.

The same is true of academic writing. I know of no scholars who can will themselves to write interesting papers. I also doubt that anyone can tell us how to write a paper that others will find interesting, although many scholars can teach us to write well. The most any scholar can do is describe the broad attributes of the papers that he or she has found interesting and then provide examples. This is all that I can hope to do too. Although a sufficient handful of people apparently think that I have written interesting papers, there is no way I can explain how I might have done what they believe I did, because I don't understand it myself. Thus, my final caveat: If you expect to learn how to write an interesting paper by reading this essay, you should stop reading now and go listen to rock and roll.

Difference Is the Root of All Interest

The otherwise diverse papers that I have found to be interesting over the years have one common denominator: They differed in some significant and striking way from most of the other papers in academic journals. For this reason they captured my attention, like scarlet begonias against a sea of gray. I might have loved these papers or I might have not. I might have agreed with their authors or I might have not. I might have thought the papers important or I might have not. But one thing is certain: because each of these works stood out like figure on ground, I haven't forgotten it.

Subject matter. Differences of several kinds seem to pique my interest in a paper. Some articles interest me simply because they address subjects that depart noticeably from the mainstream. Like dire wolves, researchers run in packs. Thus, the papers that appear in journals during an era often cluster around a relatively small set of topics and conversely, papers written on particular topics tend to cluster in time. Robert K. Merton (1973) and Derek De Sola Price (1986) argued that such clustering occurs in the physical sciences because members of invisible colleges agree on which questions and problems are currently important for their field's further progress. The same thing may also be true of the social sciences. Or it may simply be that topics fall in and out of fashion as researchers first crowd into areas and then eventually become bored. Either way, papers on rogue topics are spoonfuls of gold.

Mauro Guillén's (1997) "Scientific Management's Lost Aesthetic" is a paper that interests me for its subject matter, although it qualifies on other grounds as well. (For instance, Guillén uses the methods of a historian, which is rare in organization studies.) Contemporary management scholars who mention Frederick Taylor and "scientific management," for instance, usually go little beyond acknowledging that scientific management was the first "management theory." A few others argue, along with Braverman (1973), that Taylor provided the ideology that has justified deskilling labor. Guillén reminds us that scientific management was much more: it was a worldview with influence far beyond the shop floor.

Specifically, Guillén shows how scientific management influenced the aesthetic of modernist architecture. He substantiates his claim not only by drawing on the writings of well-known modernists, like Gropius and La Corbusier, but also by demonstrating that modernism only became influential in countries where architects trained beside (or as) engineers. Although scientific management and its imitators were enthusiastically embraced in the United States (Taylor's home) and in Great Britain, modernist architecture only appeared in these countries after it spread across the Channel and the Atlantic from Continental Europe. Guillén claims that modernism followed this diffusion pattern because American architects were trained in schools of architecture, where engineers could not contaminate them, and because in Britain, most engineers at the time had no formal training (also see Whalley, 1986). Guillén's paper makes me think that interesting academic papers in organization studies may need no clear relevance for management. Whether this is often the case is an empirical matter.

Methods. All too often in graduate classes we celebrate papers for their methods. Sometimes, I think this is a bit like admiring Mount Rushmore because of the dynamite and the shiny black steel jackhammers that chipped the faces out of the cliff. But the fact is, some papers are interesting precisely because their methods are so different from the ubiquitous secondary data sets, attitude surveys, and interviews of top managers that provide most of the grist for our field. Particularly interesting to me are methods that get close enough to behavior to show how people wittingly or unwittingly build and maintain their social worlds.

A recent example is David Gibson's (2005) "Taking Turns and Talking Ties." In what may be a first, Gibson combines the tools of conversation analysis with the methods of network analysis. In other words, he combines the techniques of the most "micro" of all sociologies with the tools of some of the most "macro" of sociologists. To put it yet another way, Gibson demonstrates that mixing what others see as apples and oranges is not always fruitless. Gibson devises a system for exhaustively classifying the turn-taking sequences in a conversation, which enables him to categorize the participation shifts ("p-shifts") that marked the in situ discussions of ten groups of managers who routinely worked together. Gibson shows that managers' p-shifts mapped to their positions in the networks formed by their friendship, coworker, and reporting relationships. In short, Gibson demonstrates that preexisting social relationships are correlated with—and may actually influence—the situated dynamics of group decision making. Gibson's paper interests me not so much because of his conclusion, but because he sings a rare and different tune. He combines techniques typically used by researchers with vastly different and in some ways opposing perspectives to speak empirically to the issue of how actions and structures are entwined.

Orlikowski and Yates's (1994) "Genre Repertoire: The Structuring of Communication Practices in Organizations" is another paper that I find methodologically interesting. Orlikowski and Yates use the concept of a genre, which they draw from literary criticism, to study the emergence, stabilization, and modification of the e-mail practices of the group of computer scientists who designed Common LISP, a computer language of considerable significance in the artificial intelligence community. The raw data for the study were the group's e-mail archive of over 2,000 messages sent between December 1981 and December 1982. By analyzing these messages, Orlikowski and Yates were able to identify distinct genres (or forms) of e-mail and the points in time when these genres emerged. By relating the genres' appearance to important events, challenges, and milestones in the LISP community's history, Orlikowski and Yates were able to show how the genres contributed to this community's organizing and development.

Gibson's and Orlikowski and Yates's papers share an important trait: both employ the methods of a field generally thought to be outside the boundaries of organization studies (respectively, conversation analysis and literary criticism) to shine a light on organizing. I do not know whether integrating tools from disparate disciplines is a hallmark of all methodologically interesting papers, but I do know that others have claimed that bringing together ideas or objects from previously unrelated domains is a hallmark of innovation (Basalla, 1988; Hargadon & Sutton, 1997).

Theory. Finally, some papers have interested me because they propose theories, or at least perspectives, that differ from what has gone before. These papers tend to be less common than papers with interesting methods or subjects, in part because theory building is an arena in which books excel. A paper is usually too short to provide adequate space for a full accounting of "why," especially if the primitives, logic, corollaries, and implications of a theory are complex. Thus, rather than forge full-fledged theories, interesting "theoretical" papers generally propose new models or metaphors that let us either see what we didn't see before or see in a new light what we thought we already understood.

Compared to other types of papers, an interesting theoretical paper may have a better chance of becoming famous, infamous, or both. I also suspect that writing theory papers is probably the hardest way to be interesting. They require more than a nose for a good problem or the proclivities of a jack-of-all-trades. Authors of this kind of paper must see things differently, systematize their visions, and then communicate them in language that is at once comprehensible and persuasive. Scholars who succeed on this front are likely to attract disciples as well as detractors, because paradigms, whether big or small, are usually at stake.

Hannan and Freeman's (1977) first major paper, "The Population Ecology of Organizations," is precisely such a treatise. It is worth observing that even though Hannan and Freeman use several equations, their paper contains no data or data analysis. Instead, the paper presents a logical and rhetorical case for applying the perspective, concepts, and tools of bioecology to organization studies. Hannan and Freeman argue that it is both plausible and fruitful to think in terms of populations of organizations and to approach such populations using such ecological concepts as competition, niche, environmental capacity, fitness, generalizing, and specializing. In other words, Hannan and Freeman invite us to see organizations in a strange and new way. Their lens is a new metaphor that draws attention away from individual firms toward sets of organizations in competition over resource spaces. Whether readers agree or disagree with what Hannan and Freeman call "the ecological perspective" (1977: 929), one cannot come away from this paper without at least contemplating organizations in a way one has never thought of before.

James Barker's (1993) "Tightening the Iron Cage" is another paper that I find interesting on theoretical grounds. Barker employs observations of self-managed teams on a factory floor to develop a grounded theory of "concertive control." At the time he collected his data, self-managed teams were a crucial and popular component of the total quality management (TQM) movement that was sweeping industry. TQM portrays self-managed teams as essential for decentralizing decision making by bringing it to where problems occur and for granting workers both autonomy and responsibility to solve them (as had sociotechnical systems theory and the quality of work life movement years earlier). Barker was one of the first researchers to study such teams in situ and over time. He discovered that teams replaced supervisory control with peer control and that peer control was subtler, more effective, and potentially more coercive than supervisory control, because workers now policed each other in the service of their organization's goals and objectives. As any high school student can tell you, peer pressure is always harder to resist than the pressure of an authority. Readers came away from Barker's paper viewing self-managed teams in an entirely different light. Barker's paper also required an extension, if not a modification, of traditional Marxist thought on control in organizations. Thus, in one fell grounding, Barker managed to reframe both mainstream and critical theories of control—not bad for a single paper.

Limits on Transgression

Although I've argued that interesting papers usually transgress the status quo, there are limits on how far transgression can go. Papers that break too many substantive, methodological, or theoretical rules are more likely to be called flaky or wrongheaded than interesting. At minimum, interesting papers need to conform to genre constraints. Empirical papers need to flow from introduction to problem statement to methods to data and then to a discussion and conclusions. Theoretical papers need to work though implications of propositions and consider counterarguments. In both cases, readers expect authors to warrant their claims in ways that scholars find legitimate: with logic, mathematical models, data, and counterfactuals, for example. Without such warrants, a paper too closely resembles opinion, and when it seems to be mere opinion, a paper is unlikely to survive academic skepticism long enough to have a chance to be considered interesting.

In my experience, failing to conform to accepted canons of warranted claims is the qualitative researcher's and the theoretician's Achilles' heel. Methods for designing quantitative studies and analyzing quantitative data come with built-in safeguards for warranting claims. Given that quantitative researchers have the edge on this score, I think it is intriguing that of the 17 papers that received more than one vote in the *AMJ* poll, 11 (65%) relied substantially on qualitative data. Given that so many academics admonish graduate students to avoid doing qualitative work, how can it be that so many of our interesting papers are qualitative?

There is certainly some wisdom to warning graduate students to eschew qualitative work. Qualitative studies seem to have a higher rate of failure (in the sense of being rejected from journals) than do quantitative studies, in part because they are harder to execute and lack clear genre constraints. Nevertheless, if a qualitative study is rigorously done, I suspect that it is more likely to yield important discoveries than a quantitative study, if for no other reason than this: qualitative researchers often discover something because they usually approach topics with little clue as to what they'll find.

Readers may also find qualitative research interesting because qualitative researchers, having already departed from mainstream methods, have less to lose by studying odd topics and taking theoretical risks. As Nancy Reagan's "just say no" campaign implied, once you step over the first line, it's easier to step over the second. Then again, all that may be going on is a trick of memory: qualitative research may simply yield vivid and involving tales that readers are more likely to remember when questioners ask them to nominate interesting papers. Although I cannot adjudicate among these explanations, I am reminded of an examination question that James March reputedly once asked students at Stanford: "Name one paper that has made a substantial theoretical contribution to our field that also contained a regression equation."

Some Final Conjectures

It is worth contemplating whether a paper's capacity to be interesting decays over time. Will students 20 years from now still find interesting those papers that interest us now? Suppose an interesting paper draws hordes of researchers to a topic, method, or theory. Over time, the topic, method, or theory will become, by definition, mainstream. Although the paper may remain famous, I suspect that young scholars schooled in the new status quo will find the paper far less interesting than did their mentors, unless it also has attributes that transcend its content and time. For example, the paper's author might have been such a clever writer that her words remain interesting long after her contribution has become blasé. For my money, the essays of Everett C. Hughes (1958) are like this. Their content is worn and holds few surprises for those who have read later scholarship on work and the professions. Nevertheless, Hughes's prose is so sprightly and well crafted that his essays twinkle with an enchantment that the work of few researchers attains.

Still, I suspect that Hughes is an exception that proves the rule: Interesting papers that start a successful line of inquiry are likely to lose luster for all but those who were drawn to the work when it was fresh. Originally interesting but less successful papers might actually remain more interesting to young scholars should they happen to come across them in some scholar's cut-out bin. Academic papers and rock and roll bands thus share one more similarity: most just fade away.

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 be transported a little more often!

But what if after reading the essays in this editorial, 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 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, method, or 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. If the world were made of candy, there could never be a Willy Wonka!

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