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Kant in pyjamas: Keeping a compass in a changing academic world

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Abstract. Reflecting on the twenty years that have elapsed since the launch of *M@n@gement,* I highlight some changes in the world of organizations, the world of organizational research, and the world of academic publications. I argue that, although notable changes have occurred in all three domains, how we do and assess research can still follow long-established canons. In particular, I suggest with Bourdieu that it all rests on reading statistics on pyjamas while thinking of Kant.

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INTRODUCTION

The first issue of *M@n@gement* was published in 1998, and I can't believe this was a full twenty years ago… Yet this fact offers us a good opportunity to reflect on changes in our profession, both those that have passed and those that may be required. Are our theories still relevant to current environments? Are our research practices compliant with scientific canons? And, since we are celebrating the anniversary of a journal, are our publication models sound?

Implicit in the willingness to engage in such reflection is probably some version of imposter syndrome as vividly described by Bothello and Roulet (Forthcoming: 8): "Is our research of any use beyond our narrow circles?" Organizational scholars have long worried about the relevance of their research for "the real world," oftentimes fearing a perceived trade-off between rigor and relevance (e.g., Carton & Mouricou, 2017; Palmer, Dick & Freiburger, 2009; Vo, Mounoud & Rose, 2012).

Pondering on professional practice and whether it is still relevant is always healthy. Yet at the same time, every generation believes they're facing unprecedented change. To avoid throwing the baby out with the bath water, we need to assess what has changed in our working environment, and what, if anything, we need to change in how we do our job. Accordingly, this essay is organized in two parts. In the first, I note changes that have occurred in the world of organizations, our object of analysis. I then turn to changes in how we do research, and how we communicate our results. Implicit throughout this first part is the question of whether observed changes warrant amendments to research practice. In the second part, I answer that question in the negative. I offer some suggestions linked to pyjamas and Kant.

OUR CHANGING WORLD

A sure way to alienate reviewers is to open your article with a sentence claiming that the world is changing at an unprecedented pace, and that it is increasingly complex. Unsubstantiated clichés don't look good in a research paper, even less so if they are only loosely related to the topic of the paper. In an attempt to avoid being frowned upon, let me try to document what has changed (and note that I make no claim as to pace or complexity). I look in turn at our object of analysis — organizations, how we approach it — research methods, and how we communicate our findings — academic publishing.

THE WORLD OF ORGANIZATIONS HAS CHANGED SINCE 1998, BUT THE WORLD IS ALWAYS CHANGING ANYWAY

In the same year as *M@n@gement* published its first issue, 1998, another venture started its operations: Google. Back then, Google indexed about 25 million pages (compared to 30 trillion now, i.e. 1.2 million *times* more).[1] A mere 41% of US adults were using the Internet, and their "top source of frustration" was "trying to find something on the Internet" (Pew Research Center, 1999: 6). In 1998, Amazon was only four years old, and it would take a few more years for social networks to appear (Facebook in 2004, Twitter in 2006…). The so-called sharing economy would take even longer, with Uber for example starting in 2009. This shouldn't come as a surprise since the web itself was so young, dating from the early 1990s.

Accordingly, the organizational landscape looked very different from what it is now. Comparing the top ten Fortune 500 companies in 1998 and now, as ranked by revenues, provides an illustration of this shift (see Table 1). Whereas automobile and petroleum companies dominated the 1998 landscape (GM 1st; Ford 2nd; Exxon 3rd; Chrysler 7th; Mobil 8th), the current landscape reflects the rise of healthcare (UnitedHealth 5th; McKesson 6th; CVS 7th) and tech companies (Apple 4th; Amazon 8th).

998 Ranking (revenues in USD bn)	2018 Ranking (revenues in USD bn)
1. General Motors (178.174)	1. Walmart (500.343)
2. Ford Motor (153.627)	2. Exxon Mobil (244.363)
3. Exxon (122.379)	3. Berkshire Hathaway (242.137)
4. Wal-Mart Stores (119.299)	4. Apple (229.234)
5. General Electric (90.840)	5. UnitedHealth Group (201.159)
6. International Business Machines (78.508)	6. McKesson (198.533)
7. Chrysler (61.147)	7. CVS Health (184.765)
8. Mobil (59.978)	8. Amazon (177.866)
9. Philip Morris (56.114)	9. AT&T (160.546)
10. AT&T (53.261)	10. General Motors (157.311)

Table1. Top ten companies by revenues, 1998 and 2018
(data source: Fortune)

1. Beyond their starting dates, M@n@gement and Google share other interesting commonalities. At the time, neither had their own domain name, but were hosted by universities (Dauphine and Stanford, respectively). Both were helped by external money: M@n@gement started with a grant from EDF's Institut du Management to buy its server, whereas Google started with a $100,000 check from Sun co-founder Andy Bechtolsheim (a more profitable investment so far).

Interestingly, besides shifts in industries, another striking observation is that those huge companies are getting even bigger (sometimes by merging, as with Exxon Mobil). In constant dollars, the 5th largest company in 1998 wouldn't appear in the 2018 top ten list. At equivalent rank, and adjusting for inflation, current companies are often twice as large as their 1998 counterparts when assessed by revenues (see Figure 1).

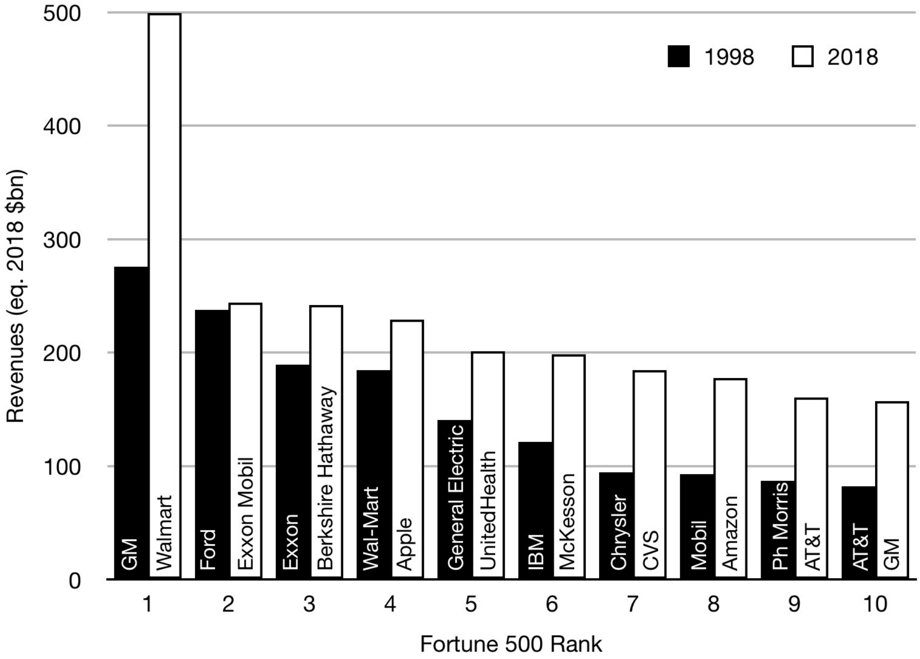

Figure 1. Top ten companies in 1998 and 2018, ranked by revenues in 2018-equivalent dollars (data source: Fortune)

At the same time, and perhaps more importantly for us organizational scholars, the nature of many of those organizations seems to be changing as well. Much of organization theory drew from empirical studies of twentieth-century organizations characterized by integrated operations ranging from supply logistics to production to sales. Some were even more vertically integrated. In contrast, many current giant companies are rather coordinating networks of external suppliers and contractors. In these network forms of organizations (Powell, 1990), not much is done in-house beyond managing the brand. As observed by Davis (2015), this raises an interesting ontological question as to the nature of organizations. We might have to rethink our core concept. A famous definition of organizations describes them as "goal-directed, boundary-maintaining, activity systems" (Aldrich, 1979: 4). Surely current-day organizations still have goals (albeit maybe different ones), but what about their boundaries? What are the boundaries of organizations in the sharing economy, like Uber (Acquier, Daudigeos & Pinkse, 2017)? And does this matter for our understanding of organizations? Similarly, activities have changed dramatically between what Standard Oil or DuPont were doing in the 1950s and what Amazon or Facebook are doing now. It seems there's always room for further chapters in the history of organizations (Chandler, 1962).

Changes in the organizational landscape matter for research because our theories are situated in time and space. The relationships captured by our empirical studies do not necessarily hold in different times and settings. Indeed, since organizations are non-linear dynamic systems, consequences of change have unpredictable long-term effects (Thietart & Forgues, 2011). More generally, cause and effect relationships evolve over time and across geographies, making attempts at cumulating results hazardous at best. Jerry Davis, as always illuminating, suggests in his essay "Do theories of organizations progress?" that the reason behind this problem is a case of David Hume's "problem of induction" (Davis, 2010: 704). Inferences drawn from past experience rest on the assumption that the laws found in the past will hold in the future. Of course such an assumption is somewhere between crude and naive for a social scientist to make, especially one studying potentially chaotic organizations (Thietart & Forgues, 1995). At any rate, given the changing landscape of organizations, our theories need constant updating, and lamenting a lack of cumulative results partly misses the point that our results are situated.

THE WORLD OF ORGANIZATIONAL RESEARCH HAS CHANGED SINCE 1998, BUT NOT ALWAYS FOR THE BEST

The one major change for organizational research in the past twenty years has been much improved data availability (Davis, 2010), and accompanying developments in software and computing power. Whereas data collection was difficult, time-consuming and tedious back in 1998, we now benefit from access to seemingly unlimited data troves, both quantitative and qualitative. Accordingly, sample sizes have increased significantly. As observed by Combs (2010), quantitative studies published in the *Academy of Management Journal* between 1987 and 1989 had an average sample size of 300 observations, whereas their 2007-2008 counterparts had 3,423 (after having discarded three outliers for which the average sample size was over 75,000 observations). This is more than a tenfold increase. Similarly, anecdotal evidence seems to point to increased datasets for qualitative research, thanks especially to easier access to archives from the internet. All this is definitely an improvement. After all, without data, we can only have opinions.

However, at the risk of raining on the parade, I'd like to point to two possible drawbacks. First is what we could call the big-data delusion. Given enough observations, everything is statistically significant, including relationships for which effect sizes are close to zero. Our articles are thus increasingly prone to report results which, although statistically significant, have smaller and smaller effect size (Combs, 2010). In other words, we are collectively at risk of being delusional when (mis)taking statistical significance for theoretical significance. Davis (2010: 696) laments our "statistical fetishism" and goes on to cite Meel (1978: 822, emphasis in original): "if you have enough cases and your measures are not totally unreliable, the null hypothesis will always be falsified, *regardless of the truth of the substantive theory*."[2] Interestingly, Meel made this point long before we were flooded with data, so the problem we face is not new. Further, it's important to note that this might happen not only when engaging in fishing expeditions and p-hacking (Davis, 2010), but also because wild *a priori* theorizing about underlying effects can be proven significant given enough data points. With an increased risk of Type I errors (rejecting the null hypothesis when it is actually true), we see substantive

2. I strongly encourage you to read this article by Meel. The points he raises are of utmost importance for research methods at a philosophy of science level, beyond a statistics level. As he writes (1978: 823): "I'm not making some nit-picking statistician's correction. I am saying that the whole business is so radically defective as to be scientifically almost pointless."

relations where none exist. This has been discussed in other fields as well (see, e.g., Ioannidis, 2005).

Second, as sadly observed by Davis (2010), more data does not necessarily lead to better theory. We are also facing a problem when we mistake data quantity for data quality. In other words, huge amounts of bad data are still bad data. If we were to ask current PhD students what acronym comes to mind when thinking of data, they would probably answer “GAFA”, given how easy it has become to find data on the web. A few years back, they would have answered “WRDS”, and twenty years ago, I have no doubt the answer would have been “GIGO”.[3] I know, I was there, and we all had been rightfully brainwashed into paying attention to data quality. Garbage In, Garbage Out. At the risk of sounding like a grumpy old man, I’d like to elaborate, hopefully arriving at the happy combination of GAFA-like wealth of data and GIGO-like careful attention by the end of this essay. To start with, the reason I’m alarmed arises from papers I’ve seen where authors draw inferences from questionable samples or data collection procedures. One famous example is the use of Amazon's Mechanical Turk (MTurk for short) to collect data. It's easy to see why so many of us love MTurk: one can collect data easily, quickly and for a fairly cheap price. Yet our dirty little secret here is that for all its convenience, MTurk comes with drawbacks badly affecting data and results. MTurkers form a very specific group and are “not representative of the populations they are drawn from” (Paolacci & Chandler, 2014: 185). Because they enroll for monetary reasons, they have incentives to please requesters (Shapiro, Chandler, & Mueller, 2013) and some participate in many studies. Paolacci and Chandler (2014) found that 10% of MTurkers are responsible for completing 41% of tasks, which raises questions as to whether their prior experiences with research protocols bias results. MTurk is just one example I'm using here to draw attention to data quality. Another example is data collected from social networks or review sites. Our easier access to massive quantities of data should not prevent us from addressing data quality issues. We need to be aware of possible problems and avoid turning a blind eye to the fact that a great many *non-randomly distributed* observations collected on social networks come from social bots (see, e.g., Varol et al., 2017) or are fake reviews (see, e.g., Dwoskin & Timberg, 2018).

THE WORLD OF ACADEMIC PUBLISHING HAS CHANGED SINCE 1998, BUT WITHOUT A CLEAR DIRECTION

I close this first part with a few observations on changes in academic publishing, since we are celebrating the twentieth anniversary of this journal.

One striking evolution over the last twenty years rests in the brute increase in published research. The number of papers published every year is in the millions. A low estimation was of 1.35 million for 2006 (Björk, Roos & Lauri, 2009), a higher one suggested 2.5 million for STM — science, technology, medicine— alone for 2014 (Ware & Mabe, 2015). Even with the lower estimate, this would be 3,700 articles every single day. Elsevier’s Scopus database references 22,800 serial titles. These numbers are mind-blowing, but there’s a silver lining: this should help to answer any criticism you might face for not having been exhaustive in your literature review.

3. The three acronyms respectively stand for “Google Apple Facebook Amazon” (the Big Four tech companies), “Wharton Research Data Services” (Wharton's financial data platform), and “Garbage In; Garbage Out” (the mantra of PhD research methods classes).

Closer to us, the *Academy of Management Journal* provides anecdotal evidence of this increase. Between 1997 and 2017, the number of published articles yearly has increased from 54 (in volume 40) to 90 (in volume 60), a 67% increase. Twenty years ago, most PhD students would read the top five journals cover-to-cover (that's *Administrative Science Quarterly, Academy of Management Journal, Academy of Management Review, Organization Science, and Strategic Management Journal*). When I tell this to current PhD students, their reactions range from plain disbelief to thinking students back then were inefficient or stupid. What current students are missing is that this was just reasonable back then. Not only was it doable since the number of papers was much lower, but it also reflected search conditions of the time. Email alerts and keyword search were underdeveloped at best, and Google Scholar did not yet exist (launching in 2004).

Besides changes in sheer numbers, apparently fueled more by an increase in the number of publishing scholars than by the productivity of individual authors (Fanelli & Larivière, 2016), other changes appear again from my quick analysis of the *Academy of Management Journal.* Again, this is no more than anecdotal evidence, but three points are worth noticing. To start with, the number of co-authors per paper has increased a little (2.52 on average in 1997 versus 2.89 in 2017). More importantly, the percentage of first authors based outside of the US has increased from 20% in 1997 to 51% in 2017, as has the percentage of women as first authors (from 33% in 1997 to 47% in 2017). Having progressed from such a heavy imbalance in terms of gender, reaching parity is important because it might change both the kind of topics studied and the perspectives taken. Here, again, I proceed with caution because I have only anecdotal evidence, coming from the virtual special issue on "Gender and Organization Science" put together by Fernandez-Mateo and Kaplan (Forthcoming) for *Organization Science.* The authors start by noting that from its debut in 1990 to 2017, the journal has published more than 1,640 articles, out of which a mere 38 – 2.3% – focus on gender. They go on to discuss the 14 papers in this special issue, and I find it interesting to note that women comprise 65% of the authors of those papers, and 79% of first authors. Accordingly, now that we are reaching gender parity at long last (at least in the AMJ!), we can hope that such topics will be better covered.

Finally, one source of disappointment is the extreme inertia of publishing models. When *M@n@gement* was launched in 1998, it was innovating in several respects, most of which related to embracing open access (for an exposure of academic publishing models and a history of *M@n@gement,* see Forgues & Liarte, 2013). Although open access publishing for academia presents many advantages, a combination of misunderstandings from all sides has mostly stalled the transition. Many academics continue to mistake open access for non-reviewed and are thus concerned with quality; learned societies fear losing a revenue stream; publishers engage in intense lobbying to preserve profits that reach shocking levels. Should open access on a large scale happen at all, it might well be thanks to laws being passed at national and regional levels requiring publicly funded research to be published in open access outlets.

I have noted above changes on all fronts: in the world of organizations, in how we conduct research, and in how we publish our results. However, I believe that none of these changes are alarming nor do they require profound changes in the way we practice our profession. On the contrary, I believe that strong, established mechanisms remain valid. What might require updating is how we operationalize our constructs, as our theories might require amendments to keep up with changes in the world of organization. Such required changes are mostly at the surface level. Davis (2010) observed that our operationalizations of firm size, as number of employees, sales, or assets, have been diverging over time. More precisely, he showed that the correlation between market capitalization and size has dropped differentially across the three measures of size for US public corporations between 1950 and 2000. Correlation with size dropped from 80% to 65% when measured with sales, from 80% to 50% when measured with number of employees, and from 85% to 50% when measured with assets. This interesting observation makes him question the construct validity of size, "a fairly fundamental concept" (Davis, 2010: 694). Maybe I'm too optimistic here, but if you agree with me that our theories are situated, as I said above, the problem is hopefully not as bad as it seems. It is common practice to measure size with assets in the financial sector, rather than sales as in other industries, for reasons beyond the scope of this essay. This reflects specificities of the industries under study and does not require abandoning the concept of size. Similarly, I believe that the differential drift observed by Davis reflects changes in the organizational landscape. Size and market capitalization are no longer as strongly correlated as they used to be. Sales remain a fairly strong indicator, number of employees less so. This is an important observation if we theorize about size, but is less of a concern otherwise. My point is, the differences between measures observed by Davis are equivalent, over time, to the differences between sales and assets over industries.

In what remains, I'd like to reflect on the practice of organizational research and offer a compass to help navigate all these changes.

A COMPASS TO NAVIGATE CHANGES

My entire professional life has been influenced deeply by one sentence buried in Pierre Bourdieu's *Sociology in Question*. This sentence is:

> "It's not easy to read statistics on pyjamas while thinking of Kant…" (Bourdieu, 1993: 22).

I'm not quite sure why this sentence made such a lasting impression on me. For sure, as a graduate student trying to understand what research is about, I was fishing for cues. Everything was so new and complicated, so many things seemed codified yet unwritten. Trying to understand what research really is was challenging. Philosophy of science seemed fairly clear, but moving from there to actually deciding about a research design was confusing. So much uncertainty, so many judgment calls to make and traps to avoid. And then, arriving at a theory? This seemed daunting. Even the all-star forum conveyed by *ASQ* found it easier to discuss what theory is not rather than defining what it is (Sutton & Staw, 1995; Weick, 1995; DiMaggio, 1995).

At any rate, like any other newcomer to academia, I had to make sense of all those readings from PhD seminars, to reflect on my first attempts at doing research. And I gradually came to an understanding that this sentence by Bourdieu somehow encapsulated. So I interpreted it as a definition of research, and made it more memorable by summarizing it as "Kant in pyjamas". I elaborate on this thought in what remains of this essay. Before I proceed, two points must be noted. First, I don't intend to provide an exegesis of Bourdieu (nor am I well-placed for that). Second, although I found this quotation of Bourdieu's inspiring, I don't mean to endorse his research methods unflinchingly. I'm aware they have been questioned and sometimes disparaged (see, e.g., Lieberson, 1992). Rather, I'd like to articulate a few thoughts on how I understand and practice research. I do so in three parts. I first focus on the “statistics on pyjamas” part, reflecting on data. I then move to the “thinking of Kant” part, addressing theoretical frameworks and contributions. Finally, turning to the “while” part, I discuss why the two previous components don't make much sense without a thorough imbrication.

READING STATISTICS ON PYJAMAS

I have an obsession with data. Not to the point I need to seek mental health care, but close enough. I have collected enough data to live off, even if I were to be reincarnated several times over as an organizational scholar. And I just cannot write a paper if I haven't plunged into the data myself. But my pathologies aside, what does it mean to look at statistics on pyjamas? How should we approach data collection? My stand on this rests on an understanding of organization studies as an empirical science, “not one that blindly mimics a model of scientific practice that is not fully appropriate for our situation, but nevertheless one that works with evidence” (Lieberson, 1992: 13).

As discussed above, data are now much more accessible, and have been accompanied by developments in computing power. Our empirics can be very strong, whether they are quantitative or qualitative (or both, as Ranganathan's [2018] exemplary research shows). This boon to empirical research should not divert us from paying attention to two important determinants of data quality and subsequent theorization.

One is closeness to data. Staying close to one's phenomenon of study is essential when one aspires to exemplary research (Hackman, 1992). Engagement with fieldwork is essential to qualitative research, but is as important for quantitative researchers. Making sense of a dataset requires thorough understanding of the phenomenon. Daft (1983: 543) cites an admonition that experimental psychologist Robert Grice used to give to his students: “No matter how much research money you may have, or how many assistants you may hire, always handle your own rat.” It is only through deep immersion into a phenomenon that one can get close to actors' experiences. This also alleviates the risks of losing relevance.

Another important point is to reflect on where our data are coming from. Observations, be they qualitative or quantitative, are always social constructions. The fact that we measure things lends them a varnish of objectivity that needs to be scratched. Without necessarily engaging in full-fledged ethnostatistics and studying the construction and interpretation of our data (Gephart, 1988), it is healthy practice to reflect on their origins. This increases awareness of possible biases and allows for better understanding of what the data convey precisely. For example, I earlier mentioned that many product reviews on websites are fake. Reflecting on this possibility could entail the following: 1/ acknowledging this possibility and thus avoid GIGO-like consequences; 2/ trying to sort genuine reviews from fake ones (algorithms exist to do so for social media data); 3/ consider whether the question asked can be addressed with the data at hand, or maybe think of other questions. For example, when Wang, Wezel and Forgues (2016) used TripAdvisor reviews of hotels, whether or not the reviews were fake was not important since they posed the same reputation risk to hoteliers (the focus of their research).

We have unprecedented access to data, and the ease of access on the internet also levels the research field. Provided we can remain close to our phenomena and reflect on the social construction of any data, we live in highly rewarding times for empirical research.

THINKING OF KANT

If I recall correctly, the one and only book in which Bourdieu uses statistics on pyjamas is *Distinction* (Bourdieu, 2010, first published in French in 1979). As you may remember, in this book, Bourdieu studies social stratification and suggests that social classes are not based solely on economic capital but also on cultural capital (and other forms of capital). Membership in a social class is displayed with markers, and this is where pyjamas enter the picture. Pyjamas, and underwear more generally, belong to the private sphere: they are not meant to be seen. Looking at statistics on clothing consumption, Bourdieu observed that working class people bought as much underwear as the upper class, if not more. But expenses for visible clothes increased with social class. In short, whereas the working class took a functionalist approach to clothing, middle and upper classes cared about external appearances.

Now, where does this leave Kant? Bourdieu departs from Marx, for whom social classes rest first and foremost on economic capital. He adds Weber, for whom social stratification also depends on status. And he draws from Kant to explain taste and aesthetic judgment. I believe this exemplifies what makes landmark research. Whereas in such a situation one might rush to the latest published paper on statistics on pyjamas (metaphorically speaking; replace with your topic of choice and related empirics), great research requires abstracting to the highest level. Indeed, what matters is not the idiosyncrasy of a research setting but how we can abstract away from concrete observations to obtain generalizable findings and relationships.

In a very influential piece, Stinchcombe (1982) suggested that the Classics of a discipline serve six different functions. As touchstones, they provide exemplars of high-quality work to emulate. As developmental tasks, they prompt elevated thinking. As intellectual small coinage, they are cognitive heuristics signaling the kind of work in which one engages. As sources, they provide a rich reservoir of fundamental concepts and ideas. As routine science, they provide puzzles with excess import. And as rituals, they bind together groups of like-minded researchers.

Thornton (2009) argues that the kind of problem-driven research that flourishes today risks derailing cumulative research. She suggests that to avoid this pitfall, we need to build from the classics, using the functions identified by Stinchcombe (1982). She illustrates her argument with three influential examples: how Tushman and Anderson’s (1986) work on technological innovation builds on Schumpeter; how Podolny’s (1993) work on status draws from Merton and Simmel; and how institutional logics (Friedland & Alford, 1991; Thornton & Ocasio, 1999) derive from Weber.[4]

Kant and the classics of our discipline provide powerful theoretical frameworks and endless inspiration. Naturally, in writing a paper we must connect to current conversations in the field: research is a conversation (Huff, 1999) that we cannot hold only with dead people, however famous they are. But infusion with the classics from early stages of the research process allows us to reach higher levels.

4. A fascinating source to tap for inspiration is this journal's “My Own Book Review” series, wherein authors reflect on their own classics (see, e.g., Brunsson, 2014; Burgelman, 2015; Mintzberg, 2015; Scott, 2014; Weick, 2015).

WHILE

As Bourdieu explains, his “analyses arise from applying very abstract schemes of thought to very concrete things” (Bourdieu, 1993: 22). This is the “while” part in his sentence.

Theory and empirics seem to be inseparable elements in exemplary research. As she is invited to reflect on what makes Gersick's (1988) punctuated equilibrium paper one of those exemplars, Beyer (1992: 65) observes that it has “outstanding virtues: (1) The study appears to spring from a genuine curiosity about groups and how they function. (2) It both acknowledges and questions existing theory. (3) It seeks new theoretical insights from immersion in the phenomena in question.”

Sixty years ago, Mills (1959) sorted research into grand theory and abstracted empiricism, the former being theory without evidence, the latter method without theory. Merton (1968) offered a solution to these extreme positions with his “middle-range” theory. This echoes the issue at hand here. But more generally, I take this small conjunction —while— as a reminder that the two parts are inseparable. Abstract theorization requires engagement with empirics, and reciprocally, empirical tests are validated against a conceptual background. Key to a successful integration of theory and empirics is how clearly the research question is defined (Alford, 1998). Lieberson (1985) goes further. To him, there is a false dichotomy between theory and data. He wishes for a theory of data to tell us not only why data are associated in a certain way but also what causes them to be there. Rather than being content with data to confirm or disconfirm some theory, we need “theory to account for why the data appear in the form that they do” (Lieberson, 1985: 231).

CONCLUSION

With the first part of this piece owing so much to Jerry Davis and the second part to Pierre Bourdieu, I’m reminded of Isaac Newton’s inspiring quotation about how science progresses as a cumulative process: "If I have seen further it is by standing on the shoulders of giants" (Newton, 1675). I’m grateful to our field for being populated with illuminating and benevolent giants, and I have always taken much delight in the long way up climbing their legs, backs, and shoulders. This is the actual climbing that I find exhilarating. Reaching the shoulder itself is a fantastic journey, often demanding and tough, but always rewarding. And once up there, being very aware that standing on the giant’s shoulder means still being way below their gaze, it’s time to jump and start another journey. After all, we’re well equipped with our Kant-in-pyjamas compass, and the ever-changing world of organizations is calling for further discoveries.

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