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Science, Stories, and Impact: A Response to Zhang and Chen

Ray Friedman

Vanderbilt University, USA
Email: Ray.Friedman@Vanderbilt.edu

As I read Zhang and Chen's (2024) perspective paper, I was impressed with the authors' flexibility moving into the medical field and the impact of their research. Moreover, I agreed wholeheartedly with their call to learn by working across fields and with their assessment of how differently management and healthcare scholarship is created, disseminated, and used. Yet I was quite stumped by the editor's request that I suggest some 'urgent and pressing' issues that might help management scholars achieve the kind of practical relevance of medical researchers.

I immediately thought of big social issues, like poverty, inequality, and discrimination, as well as high-risk events like nuclear war and global warming. These are clearly important issues – they affect the lives of hundreds of millions of people – and they are certainly urgent, especially for those on the losing end of these social ills. Yet management scholars *have*, in fact, devoted efforts to address some of these issues. We have produced hundreds (if not thousands) of articles on discrimination and diversity (Triana, Gu, Chapa, Richard, & Colella, 2021) as have scholars in psychology (Kite, Whitley, Jr & Wagner, 2022), sociology (Petersen & Saporta, 2004), political science (Butler & Broockman, 2011), and economics (Becker, 2010). But we still do not offer the kinds of clear and immediate solutions that are often found in medical publications.

At the same time, it is worth reminding ourselves of the importance of many of our seemingly more mundane topics, such as motivation (Steel & König, 2006). Understanding how motivation works can help cancer researchers inspire their lab teams or managers to focus the efforts of social workers. Enhancing the motivation of workers in less altruistic-seeming efforts also contributes to social welfare. If a manager can motivate automotive engineers and workers to do better, they can produce cars that are safer (an obvious social good) and cheaper (freeing up resources for other needs, such as healthcare or education). The fact that we do already address important and urgent problems leads me to believe that our problem is not a lack of important and urgent problems. The issue is that we do not speak to these problems with the level of *clarity* and *certainty* that exists in medical fields. Zhang and Chen (2024) speak well about some of the reasons why we are less clear. I would like to reinforce those arguments – but from a slightly different angle – while also suggesting that the concern with clarity may be overstated.

One of my favorite ways to spark an intense conversation at an academic meeting is to tell another scholar that what we do is 'art, not science'. Well, not pure art, but 'constrained' art. One might call it 'story telling with *p*-values'. That means you can't fail to do accurate statistical analyses, but you certainly can approach a data set in many ways. If you give ten different management scholars the same data set, you'll get ten different stories. Not because they fake the data but because each analyzes the data with different theories, interests, and goals. Medical research, by contrast, is more likely to have one shared goal – to figure out what makes patients better.

Additionally, as Zhang and Chen (2024) point out, procedures for collecting and analyzing medical data are structured and routinized, while we collect and analyze data in whatever way is needed to assess a theory. In management, data exist in service of the story (or 'theory'), while in medicine the data exist in service of the patient. In that sense, I agree with Zhang and Chen (2024) that our

lack of structured approaches to collecting and analyzing data makes our findings less clear and action-able. But would being more like medicine really make us more influential? Would being more organized about what 'facts' we bring to bear, and being less theoretical, result in more useful information for practitioners? Perhaps, but I'm not so sure.

To begin with, we are dealing with a different audience. While physicians act on factually supported prescriptions (e.g., they prescribe drugs that are safe and effective in double-blind studies), it is not clear that our ultimate audience – managers – is as concerned with scientific rigor. They read *HBR*, not *AMJ* or *MOR*. I remember assigning Robbins' *Essentials of Organizational Behavior* textbook (2008) to executive students, which included Maslow's Hierarchy of Needs theory (Maslow, 1943) along with a statement that the empirical evidence did *not* support the theory. When students were asked to write papers about their work, they still latched onto Maslow's theory more than any other, presumably because it helped them think about the world in a way that was useful to them. In my own course on leading change, I teach that influence is not always achieved by providing correct 'facts' but also requires purpose, relationship building, and a compelling story.

One response might be to say, 'we need to help managers become more scientific'. But let's say we could get our audience to focus exclusively on the facts. It is not at all clear that our various facts cohere into single actionable suggestions. Looking at motivation, for example, there is good evidence for any number of theories. Would this evidence suggest any single plan of action? Should managers in a given situation focus on equity theory (Adams & Freedman, 1976) or expectancy theory (Van Eerde & Thierry, 1996) or path-goal theory (House, 1971) or job design (Hackman & Oldham, 1976)? Which brings us back to Zhang and Chen's (2024) view that we produce too many theories. But what should we do about having too many theories?

One approach would be to drop some of our many theories – to make our collective story more concise. Yes, we do have too much incentive to create new theories, but it may also be that many of those theories are correct because the phenomenon we study can't be reduced to one set of causes and practice recommendations. Returning to the example of motivation, motivation is affected by expectations that one can achieve a task (Van Eerde & Thierry, 1996), a sense of fairness in rewards across people (Adams & Freedman, 1976), having meaningful job design (Hackman & Oldham, 1976), feeling organizational identification (Riketta, 2005), and being presented with a credible and engaging vision for the desired actions (Rawolle, Schultheiss, Strasser, & Kehr, 2017), just to name a few. Managers are probably wise to focus on one of these at a time if they want to formulate a plan of action but that does not mean the other factors are invalid or unimportant. If we were to winnow down our range of theories, we would be losing some elements of truth.

What, then, allows the medical field to be so focused? The key may be that physicians have a more clearly bounded set of 'core' tasks than a manager does. Yes, a doctor has to manage a practice, understand people, and be compassionate, but their core task is to diagnose and treat patients. For a manager, there is not a core task. The manager has to build a team, encourage innovation, think about organizational design, figure out how best to hire people, solve logistical problems, deal with conflicts between employees, set priorities, and more. Physicians' jobs are also highly complex, but their *core task* is more clearly defined.

Another key may be the greater similarity of the research and practice domains in healthcare than management. In medical research, a drug trial is done on a population of people that are mostly (by design) similar to populations of patients. When we look at organizations, we typically either find organizations that will let us collect data (which will always be unique) or do carefully configured experiments with college students, mturk workers, or MBAs (producing even more highly unique situations). In such cases, the 'facts' may be so stylized as to not be helpful to managers. The ability to turn those stylized findings into useful knowledge may, in fact, require abstracting from those facts, which is exactly what theory does.

As you can see, while I agree with Zhang and Chen (2024) that we, as a field, are far less clear in our answers than are healthcare researchers, I am also somewhat sympathetic to our lack of clearly defined answers. I accept that some level of 'art' is needed in management and management research, and that

we often have the most impact on practice not when we have the best facts, but when we have the best stories.

But I do believe we shoot ourselves in the foot with some practices, often in the service of trying to be scientific. We report findings based on a shift of 0.3 on a five-point attitude scale – which may be statistically significant but does not make much difference. We create manipulations to isolate cause and effect but do so in ways that are so abstract from real situations that they stretch credulity. We publish three-way interactions that are statistically true but are beyond what any human can keep in their heads. We go from 'stories with *p*-values' to 'minor and incomprehensible stories with *p*-values'.

Which leaves me wondering if there is a middle way. Can we take Zhang and Chen's (2024) lessons to heart, about focusing on issues that matter, doing studies in ways that can be more easily aggregated, and keeping more of an eye on practice, while recognizing that we work in a very different domain, with practitioners who think very differently, and whose core tasks are less definable than occurs in healthcare? What is clear is that Zhang and Chen's (2024) foray into the world of healthcare research was essential for us to see – and now be discussing – these questions about how we do our work.

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