

REASONING WITH QUALITATIVE DATA

Balancing a Theoretical Contribution

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When we first get acquainted with the world of scholarly publishing, receiving our first decision letter from a journal or attending a publishing workshop taught by a more senior colleague, most of us are struck by the importance of making a *theoretical contribution*. It's quite surprising how important such contributing is made to be, really. The intuition I at least had when I entered the business was that the quality of a publication largely hung on the shape and implementation of empirical research design, that is, was the data large, of good quality and from a sexy company. I was convinced that great papers were built on great methodological designs. Yet, funny enough, while it would be unfair to say that such methodological concerns are insignificant, the make or break issue in whether most manuscripts ultimately get accepted and read seems to be whether the authors manage to influence a particular theoretical program by having something novel and interesting to say.

This chapter is about how theoretical contributions are made. I will draw mainly on my experiences in writing, reviewing and editing scholarly papers, as well as in teaching qualitative research to doctoral and master's students in Europe and North America. This is reflected in the writing style, which I will attempt to keep pretty close to the ground. Readers interested in the background assumptions for my arguments will find a more rigorous take in the two papers that I have co-authored with Mikko Ketokivi on reasoning in organizational research (Ketokivi and Mantere, 2010; Mantere and Ketokivi, 2013).

I have never had an easy time publishing my qualitative papers. This is not to say that hypothesis testing is easier to publish than qualitative work: getting published in good journals is hard. But qualitative work has its idiosyncratic challenges. Most processes involve seemingly endless rounds of complete rewrites, and the best outcome one often dares to hope for is another major revision request with a high risk of failure. I have a sense that most of my colleagues would report a similar sentiment and those who don't are blessed, inexperienced or dishonest. The good news is that for the most part, I seldom feel that the published argument, despite being radically different from the one I initially submitted, is worse in its first formulation.

"Reversal of strategic change," a paper I published in the *Academy of Management Journal* with Henri Schildt and John Sillince in 2012 was perhaps the clearest example from all my work where the argument changed framing radically through the process of revisions. The first

manuscript that was submitted was about the role of narratives in organizational identity change. The final manuscript has neither narratives nor identity in it; it's a strategic change paper. The final manuscript has a pretty clear contribution on how thinking about the change experienced by one relatively small branch of Finnish government could influence thinking about change endeavors that get canceled before realization. Most importantly, the first version of the manuscript did not have a theoretical contribution, but the final product did.

Indeed, it would seem that few qualitative papers are like Baby Jesus: perfect in every way from the moment of conception.¹ Papers seem to reach maturity in the review process. Sadly, as editor and reviewer, I have also perceived a growing tendency of writing papers for the review process rather than for publication; a practice which the economist Bengt Holmström has compared with showing up at the Metropolitan Opera and expecting to be caught how to sing.

Contribution as a "Sweet Spot" between Data, Theory and Argument

Theoretical arguments are derived from empirical data through reasoning. Reasoning lies somewhere between the domains of methodology and theory; it has cognitive, computational and rhetorical aspects (see Ketokivi and Mantere, 2010; Mantere and Ketokivi, 2013). Reasoning is foundational to argumentation, and thus at the heart of scientific enterprise, because argumentation fuels critical discourse. Argumentation consists of presenting "reasons" for scientific claims, for which scholars seek acceptance from their peers in adding to the body of knowledge (Toulmin, 2003). Reasoning is by nature hard because human beings are more comfortable intuiting ("thinking fast") than reasoning ("thinking slow") (Kahneman, 2011).

Reasoning about qualitative data involves working with three core components: your data, the argument you present, and the theoretical discourse in which you seek to make that argument. The resulting triangle (Figure 24.1) is the playing field that you face when you are called upon to make a point. Typically, you face this challenge when you defend a thesis or revise a paper: you will not get far if all three elements are not present in your work at least at some level.

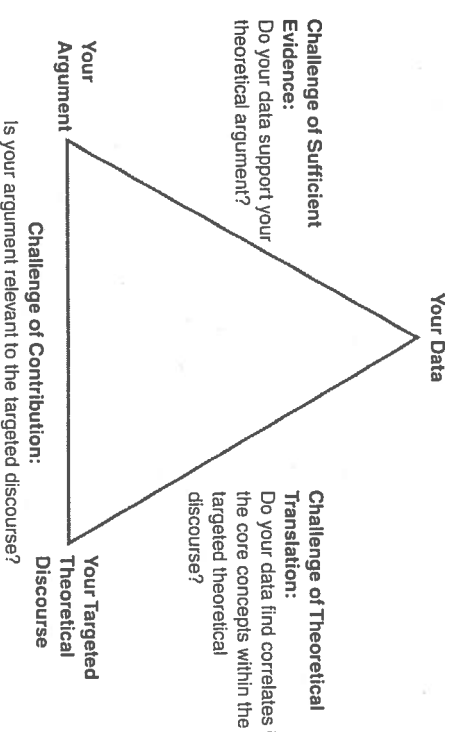


Figure 24.1 Challenges in Reasoning with Qualitative Data

If your research design is not strong enough to warrant examination, you don't get to play. If your argument is so unclear that it is not visible, same thing.

And if there is no sense of a theoretical discourse, there is little hope that your arguments can reach an interested audience. But I say this with some reservations. There are those that claim the field is too preoccupied with theory and that hurts us. Those papers that make relevant arguments with sufficient empirical support but without theoretical translation are thus interesting. I have seen very few of those but I know people think they do exist and that such papers, because they are free from dogmatic beliefs inherent to theoretical programs, can be particularly innovative. Some people feel that cross-disciplinary work is also hampered by our over reliance on theory. New journals have been founded with the intent of capturing such papers, most notably the *Academy of Management Discoveries*. Established institutions such as the *Strategic Management Journal* are now allowing for what they call empirical contributions alongside theoretical ones. It may be that a new genre of writing will emerge out of these efforts and for the better, for now, this is my story and I stick to it. I am saying no theoretical framing, no contribution in our field.

The end points of the triangle themselves are a bit hard to pin down. What is a good argument? What constitutes a good theoretical framing? The figure starts to make a whole lot more sense when you turn your attention to the edges between the end points. A part of what makes a good argument is sufficient evidence: the edge that lies between your argument and your data. Do you have data to warrant your argument (Ketokivi and Mantere, 2010)? Qualitative data is complex and often ambiguous; how do you prove your point?

The other edge, leading out of your argument, to your targeted theoretical discourse presents the challenge of relevance; even if you manage to spell out your findings in the terminology of that theory, is that going to be novel or interesting to the participants in that research program? Will you be able to convince scholars in that program that they do not already know what you claim, or even worse, that they have disputed your claims?

Between your data and the theoretical discourse lies the challenge of theoretical translation. Often, again due to the complexity of qualitative data, many stories can be told, depending on which discourse one targets. During the revisions on our 2012 *AMJ* article with Henri Schildt and John Sillince, we debated about whether our story at hand should be told in the context of sensemaking, framing, identity, organizational change or organizational knowledge. This is because our cases surprise us: we could not have imagined what we would end up writing about when we were producing the data. Organizations do not behave the way we expect them to and often the really interesting story emerges to address questions pertaining to a different theory than anticipated.

How to Avoid Getting Paralyzed by One Challenge

Meeting all challenges is not a simple matter of checking off boxes one by one. The requirements tend to conflict. Indeed, I have found that revising papers involves taking steps back while taking steps forward. You fix one thing and end up being criticized for a problem that was not there before. This is because you get sucked in by one of the challenges presented in Figure 24.1 and fall with the others. I have tried to illustrate this tendency in Figure 24.2. It suggests that making a contribution involves not only satisfying a number of challenges, but often trying to satisfy one challenge may cause one to fall short of satisfying another.

Metaphorically, the three sides of the triangle can be thought of as magnetic bars, and the scholar as a metal ball. The scholar's job is to avoid getting sucked in by one of the challenges, being paralyzed by the pull of one of the bars. There is a "sweet spot," a point of equilibrium

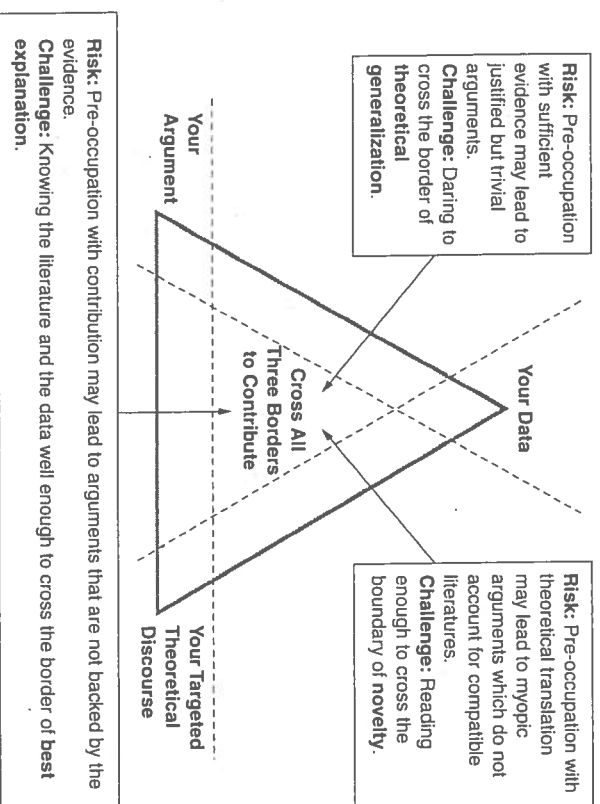


Figure 24.2 Problems Caused by Being Preoccupied with One Challenge

between the three forces that cancel each other out at the center. That's where you'll find your theoretical contribution.

Figure 24.2 introduces three demands for a theoretical argument, and three "borders" which illustrate that pre-occupation with any of the three demands tips the argument off balance. If you cross all borders, you are at least close to equilibrium and getting your story accepted. If you veer past one border, you risk getting stuck and unable to satisfy the remaining challenges. Game over. Start again. It is not uncommon with qualitative work to cross one border (say that of relevance) to find oneself back behind another border (typically sufficient evidence). With support from reviewers and a promising enough project, you may even get another chance to make things right. And if not, there is always the next journal.

Border of Best Explanation

Between your argument and your targeted theoretical discourse lies the border of best explanation. We often find a cool story of great theoretical resonance to meet our findings. What remains is the discomfort of saying whether we can in all honesty claim that the story is the best explanation for what we see in our data. Best explanations are arguments that do most justice to the data among several competing explanations (Lipton, 2004). The only way to meet this challenge is to interrogate your data in a way that opens up alternative explanations and gives them a fighting chance alongside your shiny story. This is the difference between strong abduction and weak abduction; case studies tend to be explained by multiple arguments but looking at just one rarely provides a very strong argument (Mantere and Ketokivi, 2013).

Manuscripts fall into this trap during, rather than at the outset of the process of revisions. Authors try to come up with general and novel explanations and forget to check they make

sense against their data. They get in trouble as they find that their newly minted theoretical arguments fail to be supported by their data; they fall prey to the lure of contribution and fail to pass the border of best explanation. This has happened to me more than once. I find that it is easy to get carried away and find myself struggling to find support for the argument I would like to make.

We have a natural tendency for theorizing, and once we find the courage and do the reading, we like making bold statements. This is wonderful, but also a risk; the challenge of best explanation is real. Your best friend against this risk is doubt. Our tendencies towards hasty generalization and over-interpretation are tempered by doubt (Locke et al., 2008). Doubt is so helpful for reasoning because it pushes us to re-examine our interpretations, address counter arguments, and revise our arguments up until the point that we feel confident that we at least believe them ourselves; I don't find much use for the word "truth," but if using it makes you happy, knock yourself out and say "up until the point that our arguments are true."

Doubt can lead to anxiety, in particular among students and junior colleagues who are learning the ropes. Yet, as texts ranging across the pragmatism of Charles Sanders Peirce (1878) to the hermeneutics of Hans-Georg Gadamer (1975) to influential methodological accounts in our field by authors such as Locke et al. (2008) or Alvesson and Kärreman (2007) demonstrate, doubt can be the single most important asset in scholarly reasoning.

Border of Theoretical Generalization

Manuscripts that get revision requests often cross the border of best explanation at least more or less, but fall short of crossing two others. Such papers provide a credible explanation of data but fail to rise above the specifics of the case. First round submissions tend to be pretty focused on data and claims are thus not well rooted in theoretical language. The challenge of theoretical generalization boils down to the simple question: "What is it that management and organization scholars know about management and organizations (and *not* just about your often anonymous case organization) after reading your paper that they did not know before?" There is a whole lingo for masking this lack of argument in sentences like "we have provided a *rich* and *nuanced* account of X," or "we have *shown how* (i.e., rather than *argued that*) X is more complex than previously thought of." If you glue in a learned review of some theoretical discourse to begin your story, some journals let you get away with that. The ones I tend to like to read don't.

I have found that first submissions of papers – including those written by myself – are rarely strong on theoretical contribution. Rather, they pass the bar of entering the review process by virtue of showing a data set which appears rigorously produced, and arguments that are supported by that data set. The arguments that *are* made, while not particularly strong or novel, do pass as best explanations of that data due to the competent presentation of their authors (Figure 24.2). But the arguments themselves are often vague or lack ambition. This is often an indication of a pre-occupation with the problem of sufficient evidence.

Crossing this border involves daring. Mintzberg (1979) has called it "taking a creative leap." Not all authors are able to cross this boundary and some papers fail because their arguments are limited to explaining what the author sees in the data. Theoretical argumentation is founded on interpretation of the data, and all interpretations are to an extent incomplete, somewhat biased and potentially unfair. As such, the only risk-free choice is to abstain from theorizing and never cross the boundary of theoretical generalization.

Border of Novelty

The failure in novelty relates to a preoccupation with finding a theoretical framing for one's findings, but doing this too soon, in too much haste and/or with too narrow a focus. The idiom "give me a hammer and I will treat all my problems as if they were nails" hits close to the mark in many cases. Scholars get lost in trying to explain their findings with their favorite theory and while the story may be novel to a small group of researchers, researchers from neighboring research programs find the lack of novelty disturbing and the argument myopic. The challenge of novelty is a particularly salient issue in the study of organizations due to the multi-paradigmatic nature of the field. If you look at the publication records of many senior scholars known for their qualitative research (say Kathy Eisenhardt, Ann Langley or Steve Barley), you tend to see contributions to various theoretical programs. Contributions are made into *a* theory, but over longer periods of time, scholars may contribute to various theories. This also suggests that when faced with empirical data, such scholars can draw from a rich source of explanations, and make sure that similar contributions are not made in a neighboring research program.

The trouble is that scholars tend to specialize. Junior scholars typically have the challenge of reading enough to get a sense of the opportunities in various literatures. To cross the border, one needs to stop just trying to explain what you see in the data in theoretical terms. This is often our first instinct when somebody says theoretical contribution – "hey, I will write my story in the language of some theory." The bar for providing a theoretical generalization that way is way too low; your story lacks ambition! The real challenge is to ask what your data can deliver for the needs of a particular theoretical discourse, and beyond its narrow confines. That is, the conundrum should ultimately not be empirical ("how can theory help to explain what I see in my case?") but theoretical in nature ("how can my data help a theoretical program to advance?"). Reading helps in making sure that the research program where you choose to locate your findings is not a limitation, but indeed the best explanation for your findings.

Discussion

Figures 24.1 and 24.2 are founded on the assumption that authors have done their basic homework. The game described in Figure 24.2 in particular makes sense in the context of papers that have a fair chance of being published in the first place. It goes without saying that papers with weak research designs or data sets, or papers that are so poorly written that their arguments are not visible at all, have poor chances of survival; in such cases there is very little to balance: one flies out of the playing field altogether. Such papers are, and should be, typically desk rejected by editors and not face the review process at all.

What I have left out is the process of negotiating with reviewers. This is intentional. While reviewers and editors often offer valuable advice, an approach to conduct a revision as satisfying the reviewers' demands is often a kiss of death for a manuscript. Reviewers tend to point to important problems and areas of potential underpinning the argument, but the responsibility for finding these fundamental challenges ultimately lies with the author, not with the reviewers or editor.

I have focused on the activities or reasoning from qualitative data. I do not wish to suggest, however, that the challenges are somehow limited to qualitative data. One student of mine at a Ph.D. seminar on qualitative research complained: "but I'm better at math!" What I assume she meant was better at math than at making interpretations. With some forms of analysis, math gives us powerful tools to see regularities and patterns in our data. But what explains those regularities and patterns is as much a matter of interpretation as it is with qualitative data. Regardless

of whether you are analyzing numbers, or turning text into numbers followed by analysis, or work with text alone, you face the challenge of interpretation that lies at the heart of any theoretical enterprise. There are multiple competing explanations for an empirical finding (Lipton, 2004). I have written this chapter to help you navigate this challenge.

Note

1. I borrowed the analogy from Henry Mintzberg who has used it to challenge the planning conception of organizational strategy.

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