All Information Systems Theory is Grounded Theory

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Everybody does grounded theorizing, but not everybody knows that they are doing it. Ask yourself where theory comes from. You might have taken a doctoral seminar on research methods which had a session devoted to this topic, where you learned that usually theory comes from prior theory in your field (e.g., IS), other fields (e.g., economics, sociology, or psychology), or both. Obtaining theory from existing theory might be implicit if your get your theory from your advisor. If your methodological training included qualitative methods, you also learned that theory may come from data, especially when it concerns poorly understood phenomena. Finally, some of us might have learned about a possibility of "in between," where parts of the theory are drawn from prior theories and parts from data. What I will argue in this piece is that IS theory *should be* of this "in between" variety and should be developed through a particular process, namely, through grounded theorizing. Moreover, I will illustrate how two of our field's prominent theories, TAM and IT Productivity, were developed by following the logic of grounded theorizing, even if not implementing the formal procedures of this research approach. I will argue that we will all benefit if we embrace this logic in our work and publications.

In graduate school, I became familiar with Grounded Theory Method (GTM) as part of my preparation for the qualifying exam. The original book on this topic (Glaser and Strauss 1967) gave me a remarkably clear guide on how to go about conducting qualitative research using both data and extant theory. Yet, as I was trying to publish my papers, I was told that the "in between" approach to theory development was not well-accepted in journals and that the GTM label created a lot of confusion and misconception (Urquhart and Fernandez 2006; Walsh et al. 2015). It was best to claim that either my key theoretical insights came from extant theory or that they were developed in a grounded way from the data. I was confused. I knew that both played a role in my research process but ended up writing my papers to fit reviewers' expectation around boxes that they needed to put qualitative research in (Sarker et al. 2018). In one paper, I stuck to the claim that GTM was used, introducing external theory only after the data was presented (Levina and Ross 2003). In the other, I explained that my work was guided by an *a priori* sociological theories, even though I knew that I brought in relevant part of these theories specifically to explain most interesting emergent themes in the data (Levina 2005).

Twenty years since I have submitted my first MISQ paper (Levina and Ross 2003), I still find myself debunking myths about GTM, justifying why I stuck to it, and explaining why impactful IS theories are likely to be developed in this way, even if only some of us are admitting it openly in writing.

GTM: What is it?

Briefly, GTM was formalized by its founders, Glaser and Strauss, as a response to frustrations each of them had personally experienced in legitimizing their approach to theory development. Glaser has conducted a quantitative dissertation on careers of scientists in organizations by following research methodology advocated by his advisor, Prof. Paul Lazarsfelt, who is acclaimed as the co-founder of mathematical sociology. Glaser, however, had found that the most interesting part of his data was in analysing survey responses not directly corresponding to his hypotheses -- something many of us can relate to. He was struggling, however, in legitimately writing about theoretical insights that were not derived on the basis of *a priori* theory. Later, Glaser came across Strauss, who was a qualitative interpretive researcher, who was also experiencing similar challenges in publishing his work. As Glaser writes, *"Part of the trend (in 1960's) toward emphasizing verification was the assumption by many sociologists that our "great men" and theorist forefathers (Weber, Durkheim, Simmel, Marx, Veblen, Cooley, Mead, Park etc) had generated a sufficient number of outstanding theories on enough areas of social life to last for a long while"(Glaser 2012, p. 1).* Why was following the traditional route not

acceptable to Glaser and Strauss? They felt that bringing an abstract theory to write about contemporary phenomenon in a deductive way without knowing much about this phenomenon resulted in poor insights!

Learning about the phenomenon in a haphazard way, however, would not solve this problem either. To provide theoretical insights on a contemporary phenomenon, one needed to get close to this phenomenon by collecting data – be it qualitative or quantitative -- so as to investigate it *systematically* and *critically*. This involves focusing on problems or puzzles in data and looking for consistent patterns, themes, and explanations addressing these problems. Formalizing GTM meant outlining how one should do this analysis systematically, including how to find interesting questions in data, define a sampling strategy, and conduct systematic comparisons in data to find insightful patterns and verifying their consistency. The next step was to abstract out conceptually on the basis of these patterns. Contrary to the popular myths about GTM (Urquhart and Fernandez 2006), *extant theory plays a critical role in this process* as it helps challenge emergent findings, suggest ideas for new data collection, and enable better conceptualization of emergent patterns. At the same time, researcher must work hard to resist the temptation to "force" data to fit their own or reviewers' favourite theories (Seidel and Urquhart 2016).

One of the biggest challenges of GTM is how to avoid conceptualizing patterns in data in a way that results in theories that are already well known in the field. First, addressing a really challenging practical problem with a good conceptualization is often a worthwhile pursuit in its own right even if it does not result in a novel theory. Yet, we are in academia, so Glaser and Strauss (1967) offered us guidance on developing a formal theory that is likely to result in a publishable contribution. They argued that in abstracting out from a given context, *one should apply many relevant existing theories* to the emergent patterns in data (Charmaz and Bryant 2011). This exercise can provide useful insights on the data or lead to further data collection. If existing theories do not match the core emergent patterns or need to be extended to match them, it opens up room for a novel theoretical contribution. This is likely to happen if the phenomenon at hand is relatively new or poorly understood with prior theories – something typical of many IS phenomena. While in the original writings on GTM, the analytical focus of the researcher was supposed to stay on the struggles experienced by participants in the study, subsequent writing on the GTM broaden the method and suggested that the focus of investigation could be based on researcher's own interests and passions (Charmaz 2006). For those of us who are interested in making a novel theoretical contribution, we should focus our investigation on unresolved struggles in our academic discipline (Davis 1971). Whether the research focus is driven by practical or academic problems, if GTM scholars want to develop insightful theories, they must be extremely well-read. Exposure to a wide set of prior theories reduces the chances that scholars will narrow in on a problem that is easily addressable by an extant theory. At the very least, addressing this problem would require integrating multiple theories potentially producing an interesting theoretical contribution in its own right.

I posit that IS scholars who develop impactful theoretical insights on contemporary phenomena have a very similar approach to theory development even if we don't call it GTM. We may learn about a new technology-related problem from exposure to specific organizational settings, public media, academic publications, or our daily lives, or we make come across a data sets that presents a puzzle. We then start wondering what is going on here. What do we know about similar phenomena? What is different? We narrow in on particular aspects of this phenomenon that are unfamiliar. We start thinking about what dimensions should guide our investigation based on our prior knowledge – prior theory. If we do hypothesis-testing research, these dimensions need to be fixed fairly soon, so *a priori* theory is brought to bear to design survey questions, extract certain variables from archival data, or set up experimental conditions. If we do exploratory quantitative and, especially, qualitative research, we may go into the field and seek data on the new phenomenon. We start seeing some patterns in data that we deem interesting or puzzling. We often have our favourite "grand theory," which may lead us to see the new phenomenon immediately through the lens of this theory, or we may be able to hold off putting this lens on and explore the phenomenon a bit further. All combinations of the above are possible, but, *if we agree*

that IS research is a research on contemporary phenomena, then we all – whether we are doing quantitative or qualitative scholarship -- do some degree of grounded theorizing.

I will discuss the development of two highly impactful theories in our field that were published as hypotheticodeductive theories and demonstrate that the development of both of these theories followed the principles of grounded theorizing, even if not reported as such in publications.

Technology Acceptance Model

The first theory is the famous Technology Acceptance Model (TAM) theory, which in its published form appears to have been built deductively based on *a priori* theory in psychology. It may be a surprise for many to learn that TAM has its roots in inductive qualitative research project conducted by the theory's founder, Fred Davis. The story is documented by Davis in a book chapter (2006). Before joining his PhD program. Davis worked as IS development and implementation consultant in late 1970s. He observed an empirical puzzle: in spite of the growing power of computer-based decision support models, the software implementing such models was rarely used by practitioners (ibid, p. 395). This observation motivated Davis to pursue his PhD starting in 1980.

As part of his studies, Davis "interviewed numerous end users regarding their acceptance or rejection of various technologies." (ibid, p. 396). In his recollection of how the key theoretical insight crystalized, he writes:

> One day I was returning by helicopter from New Hampshire to Massachusetts after completing a day of interviews. Watching a brilliant sunset, a simple but important insight occurred to me. Although interviewees expressed it in many different ways, the dominant reasons they cited for accepting or rejecting a new system at work strongly hinged on two issues: how useful and easy to use they found the system to be. We landed, the attendant opened the helicopter door, and my folder of interview notes spilled out. While the spinning rotor blade scattered yellow sheets all around, I was amused instead of concerned because the simple insight about usefulness and ease of use was now in my head. I recovered most of the interview notes, but never typed them up. Instead I turned my focus to the literature on MIS attitudes and implementation success to craft the idea into a dissertation topic. (ibid, p. 396)

Comparing this data-driven insight to published research in IS, Davis had noticed that dominant IS theories in the early 1980s emphasized top management support and user involvement as the two most important factors driving implementation. These two theories did not jell with his data, but he discovered a significant body of research on "MIS implementation attitudes." This work could be related to the emergent concepts of "usefulness" and "ease of use." Yet, published studies in this area produced mixed results in terms of the relationship between implementation attitudes and system adoption. Why did published result not fully align with strong patterns in the qualitative data?

To address this puzzle, Davis drew on existing theory in psychology that focused on attitudes to further theorize emergent concepts. Psychological theories predicting behaviour on the basis of attitudes were gaining popularity in organizational scholarship, and the theory of reasoned actions was one of them. It was a good fit for explaining the role of attitudes (perceived usefulness and ease of use) in producing behaviour (system adoption). Davis also had developed deep expertise on this theory, having worked on measurement issues pertaining to this theory in his pre-thesis work (Warshaw and Davis 1985). This is how TAM emerged -- by bringing an existing theory to formalize grounded emergent theoretical themes. The combination helped Davis to develop new empirical instruments enabling quantitative data collection.

A fascinating part of the story is that the software firm that Davis was working with had just suffered a costly software product adoption failure. This presented the young researcher with a "*core concern"* of how to improve product development so as to prevent such failures in the future. This led Davis to develop the last piece of the puzzle: the administration of TAM instrument with video prototypes to avoid costly investments into software that users would never use. The approach had proven very relevant to practice.

The rest is history: over 150,000 Google Scholar citations to Fred Davis' collective works and unprecedented intellectual influence on our field. Davis writes, "*TAM is often mentioned as an example of a true IS-specific theory, which is ironic given the extent to which TAM was derived from reference field theory such as the theory of reasoned action*" (Davis 2006, p. 399). What is also ironic is that TAM, perhaps the most popular theory for hypotheticodeductive scholars in our field, is actually a grounded theory. Yet, because it was written as a hypotheticodeductive theory that is based on extant theory in psychology, it is hard to recognize that it is original insights came from our discipline's practice – IS implementation failure. Perhaps had Davis reported in his original publications the inductive parts of his theory development, our field would've moved faster towards unpacking the role of organizational factors in influencing key TAM variables – an extension of TAM that took a decade to develop (Venkatesh 2003).

IT Productivity Theory

The second theory I would like to discuss does not have a familiar abbreviation, in the original publications it was referred to as "The Theory of IT and Organizational Architecture." While the name is not familiar, the theory is well-known to IS scholars as a key theoretical advancement in resolving the IT Productivity Paradox. The theory is outlined in a series of papers related to Lorin Hitt's thesis (Hitt 1996) that were published in late 1990s and early 2000s. The theory posits that firm-level investments in IT are likely to be more valuable when coupled with other organizational transformations such as decentralization of decision authority, emphasis on subjective incentives, and increased reliance on skills and human capital (Hitt and Brynjolfsson 1997). Hitt's collective works have resulted in over 30,000 Google Scholar citations to date.

At the time when Hitt started his graduate program, his advisor, Erik Brynjolfsson, was focusing on untangling the IT Productivity Paradox, which refers to the statement that "despite enormous improvements in the underlying technology, the benefits of IS spending have not been found in aggregate output statistics" (Brynjolfsson and Hitt 1996). [1] Paradoxes found in primary or even secondary data are excellent opportunities for grounded theorizing. The Brynjolfsson and Hitt team started exploring this empirical paradox by considering three ideas: 1) Returns on IT investments may be reflected in performance measures other than the usual productivity measures; 2) IT investments may need several years to result in a productivity gains; 3) returns on IT investments may be uneven across firms and industries (Brynjolfsson 1993). GTM approach would suggest collecting data to explore these possibilities. This is exactly what the team has done by analyzing a quantitative data set they already had on hand with different measure and by subsequently collecting longitudinal firm-level data (Brynjolfsson and Hitt 1996; Hitt and Brynjolfsson 1997). Analyzing existing data set with new measures resulted in two new puzzles. The analysis revealed, first, that contrary to the IT Productivity Paradox, the rates of return on IT were actually quite high, and, second, that there was a great degree of heterogeneity where firms making similar investments got very different outcomes.

Resolving these puzzles took qualitative insights, new quantitative data, and several extant theories. The qualitative insights came from Hitt's prior experience in management consulting, teaching cases focused on the idea of "business process re-engineering" (Hammer and Champy 2003) and qualitative research

[1] The story related below is based on my personal correspondence with Lorin Hitt (May 2020).

studies of IT in organizations (Orlikowski 1992; Zuboff 1988). They all pointed to the idea that when IT was introduced into organizations, this was likely to lead to organizational change that might, in turn, lead to higher performance. To theorize what kind of organizational changes may positively influence performance, Hitt turned to the human resources literature, which at the time focused on the new idea of innovative work systems (Ichniowski et al. 1996). The final piece of the puzzle came from the newly developed economics theory of complementarities by Milgrom and Roberts, which formally modeled how certain inputs into a firm's production function were complementary, while others created conflicts. Putting all these insights together, Hitt & Bryjolfsson pursued new data through a survey instrument focusing on IT investments and HR practices at the firm level. The survey data combined with firm performance data indicated that higher returns on IT investments were gained primarily by firms that also invested in transforming their human capital management practices.

While GTM was not explicitly used by this research team, the theoretical insights were developed in accordance with core GTM principles: 1) getting in-depth understanding of the phenomenon through data and finding patterns, 2) identifying key concerns or puzzles, 3) proposing how these concerns can be addressed using patterns in data and extant theories, 4) pursuing more data specifically to verify or challenge these emergent theories, and 5) continuing this process until a satisfactory answer is obtained. Yet, published papers based on this project present a "cleaned up" and "modularized" story of theory development. Hitt & Brynjolfsson 1997 paper comes closest to explaining how various conceptual pieces end up fitting together, but even this story does not reveal the full picture. Instead of starting with the IT productivity puzzle as the motivator, it starts by asserting a correlation between flattening of organizational hierarchies and the rise of modern computing technology (1997, p. 82). The paper then concludes by saying that organizational IT investments are associated with organizational architectures that include decentralized authority. The reader unfamiliar with other papers from this project would make an erroneous conclusion that firms that make higher investments in IT also change their organizational architectures; whereas, the takeaway of the overall project was that *only* firms that change their organizational architecture when they invest in IT reap performance benefits from their IT investments.

Way forward

Given the profound influence these two theories had on our field without referring to GTM, or perhaps even being aware of Glaser and Strauss' writings, what is the added value of engaging with this formalized theory development method? Indeed, research processes may not require formal codification as long as the knowledge of the process can be learned through tacit aspects of academic life such as advising, workshops, and published papers. The knowledge, however, is not readily accessible in our field. Had I not come across the back stories about the development of these two prominent theories through personal communication, I would've inferred that they were the result of following the process of identifying a theoretical gap in the published literature, filling this gap by applying extant theories from reference disciplines (psychology and economics respectfully), and testing these "ivory tower" theories with quantitative data. This would've been quite far from the reality.

Given that science is filled with judgment calls, a hallmark of scientific writing is that we clearly explain and justify the choices we make so as to enable the readers to judge the appropriateness and implications of our choices. Being transparent in this way should apply to every part of the paper from how we formulate problems to the conclusions we draw. If we present post-hoc rationalized stories, the readers could make incorrect inferences about the meaning of our research. Yet, our publications force us to comply with a set of fairly rigid expectations about the form our theory development section ought to take, especially within the hypotheticodeductive tradition. Even for scholars who do not follow this tradition, such as myself, only a handful of theories can be published in a way that they were really developed. This is because there is a great degree of confusion among interpretive scholars about the use

of existing theory as a valid and necessary part of grounded theorizing (Charmaz and Bryant 2011; Urquhart and Fernandez 2006).

Our field would advance the state of theory building if we asked the authors to demonstrate rigor in their theory development process beyond weaving together a logically convincing sequence of abstract arguments. Because our field is focused on contemporary phenomena, such rigor can be achieved by following GTM principles. I have provided a short summary of such principles, but it would take our field some time to properly adapt these "conceptualization process rigor criteria" to suit different types of IS scholarship while not making such criteria overly rigid. The first step towards achieving this goal is to ask the authors to report their theory development process. Such reporting does not need to be voluminous as it can be integrated with the actual presentation of the theory, perhaps even making such presentation livelier. As we are working towards these changes, theoretical advancement in our field would benefit from exposing all students, without regard to their disciplinary training and favorite methodological tools, to grounded theory building ideas. This should in no way replace extensive exposure to existing theories in our own and other fields, but, instead, should be seen as a promising way of building theory of contemporary phenomena for long-lasting impact.

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