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What is the Eisenhardt Method, really?

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Abstract

This essay sharpens and refreshes the multi-case theory-building approach, sometimes termed The "Eisenhardt Method." The Method's singular aim is theory building, especially with multiple cases and theoretical logic. Its defining features (e.g. research questions without obvious answers, careful case selection, well-identified constructs and relationships, theoretical arguments, boundary conditions) reflect this aim. I begin with the influence of the 1980s, including grounded theorizing and case logic. Relying on exemplars, I illustrate the Method's defining features. I also address common misconceptions (e.g. types of data, number of cases, performance emphasis). These miss the Method's essence and imply a rigid template that does not exist. Instead, the Method's relatively few defining features enable a wide variety of research possibilities. I conclude with what I would write today like a richer palette of research choices, more emphasis on time, and flexible philosophy of science. Yet the core message of theory building would remain.

Keywords

case method, field research, grounded theory, qualitative methods, research methods

In 1988, I responded to a call from the Academy of Management Review (AMR) for theory development papers. I was an assistant professor focused on research, teaching, family, and of course, getting tenure. I had written a quantitative dissertation on agency theory, but had also launched a strategic decision making study with Jay Bourgeois. Yet what was to have been a 90-company quantitative study unexpectedly morphed into a much smaller and more intimate "deep dive" into 12 firms. The insights from these initial cases were too compelling to ignore. Coached by my colleague Bob Sutton, I published my first two theory-building case papers with Jay and then turned to the AMR opportunity.

Several aspects of the 1980's context shaped my writing. For one, there were few published qualitative studies and almost no guidance on how to execute theory-building research. So my aim was to write the paper that I wished that I could have read before I began theory building from cases. That is, I wrote for all of the first-time case study researchers, including myself. For another, qualitative studies were not recognized, well-accepted, or even remotely understood by the

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dominant quantitative (and deductive) community. They were seen as opinions, not "real science." Furthermore, the few qualitative researchers were largely self-referential and seemingly disinterested in connecting with the broader research community (Pettigrew and Mintzberg are major exceptions). Yet, I thought that the influence of the method and research using the method depended on this connection. So I also wrote with the aim of bridging the inductive-deductive divide. After about 6 weeks of writing, I submitted the paper, and received an acceptance with virtually no revisions. This paper was Building Theories from Case Study Research (Eisenhardt, 1989a).

Building Theories became more influential than I could have ever imagined. No one is more surprised than I at its roughly 60,000 (and counting) Google Scholar citations. After all, I did not self-identify as a methodologist. Instead, I saw myself as an explorer of theoretical and phenomenological puzzles in technology-based firms using multiple methods. My few subsequent theory-building case methods papers were primarily responses. In 1991, I responded to a commentary by unpacking the trade-off between rich description and theoretical development (i.e. better stories and better constructs) that I saw as driven by journals' page limits (Eisenhardt, 1991). In 2007, Melissa Graebner and I pushed back against self-proclaimed arbiters of grounded theory (Eisenhardt and Graebner, 2007). We also addressed publication challenges that we faced from both quantitative and qualitative reviewers. In 2016, Melissa, Scott Sonenshein, and I were inspired to write by the aggressive promotion of data structures and other rigidities (Eisenhardt et al., 2016). In writing about rigor (vs rigor mortis), we emphasized common features of major qualitative approaches (i.e. ethnographic, interpretivist, and multi-case theory building) like deep dives into field data, theoretical sampling, and grounded theorizing rather than their distinctions, and the few essential criteria for rigor.

In 2011, Langley and Abdallah (2011) coined the term The "Eisenhardt Method." Although I was unaware of this for several years, it was a humbling designation. Yet, it was challenging for these authors to infer from several studies what the method is versus what reviewers wanted versus what were my choices but not the Method. Confusion was particularly likely because of the Method's "almost too simple" organizing principle of theory building. Some others were less careful or even inaccurate. And so I am grateful to Ann and Paula for graciously offering SO! as a platform to clarify what The "Eisenhardt Method" is, really.

The Eisenhardt Method

The "Eisenhardt Method" (the Method) is first and foremost about theory building. So while it relies on Yin's work (1984) on cases (and replication logic) and Glaser and Strauss' (1967) iterative process of constant comparison of data and theory (and theoretical sampling and saturation), the Method's unique contribution is theory building from multiple cases (with particular emphasis on theoretical arguments). By theory, I simply mean a set of constructs linked together in relationships that are supported by theoretical arguments (i.e. mechanisms) that seek to explain a focal phenomenon. This emphasis on theory building has several implications that shape the core features of the Method.

First, the Method addresses *research questions* for which there is little or conflicting prior theory and/or empirical evidence. The reason? These questions are particularly likely to provide fertile opportunities for theory building. Yet within this broad theme, many types of questions are possible. In my work, some research questions look inside the "black box" of a process. For example, Bingham and Eisenhardt (2011) extend the vast learning literature that links experience to outcomes but infers learning by asking: How and what is actually learned from process experience? Similarly, Davis and Eisenhardt (2011) unpack the process between the antecedents and outcomes of R&D collaborations, and so ask: How do successful (or not) R&D collaborations

occur? Another common source of questions is an under-represented perspective in a well-researched literature. For example, Graebner (2009) studies how acquisitions occur from the seller point of view, not just the dominant buyer perspective. Garg and Eisenhardt (2017) examine how the corporate governance process unfolds from the CEO lens, not just the prevailing board lens. Another source is simply asking how a "cool" yet under-studied phenomenon like fast decision making (Eisenhardt, 1989b) occurs. Or how a significant puzzle like cross-business collaboration (Martin and Eisenhardt, 2010) or technology adoption in an ecosystem (Ozcan and Hannah, 2020) is solved.

Alternatively, the research question may explore a unique setting or what Siggelkow (2007) terms a "talking pig." Omni, a pseudonym for a global firm that led the tumultuous technology sector for 3 decades, is such a setting. Galunic and Eisenhardt (1996, 2001) ask how Omni achieved such resilience. Similarly, Bremner and Eisenhardt (2020) ask how organizing form influences innovation by studying the unfolding rivalry between DJI and 3DR (i.e. 2 talking pigs) in the burgeoning drone industry. DJI was the first Chinese firm to lead innovation in a nascent global market while 3DR was its US rival with a unique user-community form. Many of these research questions address process (i.e. how phenomena evolve over time). For the Method, however, their unifying theme is that they are questions for which there is little or conflicting prior theory and/or empirical evidence, and so no obvious answers.

Second, the Method emphasizes *careful case selection* (i.e. theoretical sampling). This means choosing cases where the focal phenomenon is likely to occur, and case designs where the similarities and differences across cases are likely to improve theory building. Why? These considerations typically sharpen the empirical focus on the focal phenomenon, mitigate alternative explanations, and enhance generalizability. Regarding choosing cases where the focal phenomenon occurs, theoretical sampling for this reason is often difficult for deductive researchers to accept because it is not random sampling. Yet it fits well with effective multi-case theory building. For example, Hallen and Eisenhardt (2012) examine how entrepreneurs raise funding and more abstractly, form ties efficiently. They selected ventures that had raised investment at least once in order to focus on the tie formation process and eliminate firms that were not of theoretical interest (e.g. insufficient quality to raise money or did not try). Similarly, Kirtley and O'Mahony (2021) chose early-stage ventures with high uncertainty (e.g. technology and market) to increase the likelihood of strategic change (i.e. "pivot"), their focal interest. Cohen et al. (2019) selected eight major accelerators to ensure sufficient quality such that at least some entrepreneurs would learn, their focal interest.

Regarding case designs, a frequent one is to choose cases with *common antecedents* that are likely to influence the focal outcome and then study the intervening process. For example, Davis and Eisenhardt (2011) study cross-firm R&D collaborations by choosing collaborations that had the antecedent characteristics that predict successful outcomes from prior research like similar size and national culture. They then unpack the "black box" of their collaboration process. This design "controls" for well-known alternative explanations, and so enables focus on the intervening process and its implications for outcomes of primary interest. The twist of this and related designs is that, despite seemingly similar cases, they often reveal different processes and/or outcomes.

A subset of this design is the *matched pair* in which the researcher chooses two cases with similar (or even the same) antecedent features, and then compares subsequent processes and/or outcomes (Bechky and O'Mahony, 2015). Since there are only two cases, matched pairs are often particularly comparable and can be richly described in space-limited journals. For example, Navis and Glynn (2010) explore two firms that pioneered the market category of satellite radio. Despite a comparable start, these firms evolved differently and experienced different outcomes. Likewise, DiBenigno and Kellogg (2014) examine collaboration by comparing to two medical units in the

same hospital with staffing and other similarities. Despite these similarities, nurses and technicians in one unit successfully collaborated while the others did not.

Another design is *racing*—that is, cases (often ventures) begin at the same time with similar initial conditions like founders, location, and funding, and then "race" to some natural endpoint like IPO, unicorn valuation, or other temporal marker (Hannah and Eisenhardt, 2018; McDonald and Eisenhardt, 2020; Ozcan and Eisenhardt, 2009). This design is, in effect, a natural experiment which dovetails well with the recent interest in causal identification in the broader research community. It is also effective for studying changes of pace during the "race" (Tidhar and Eisenhardt, 2021) and successive temporal eras (Hannah and Eisenhardt, 2018). For example, Tidhar and Eisenhardt (2021) find that ventures that achieve profitable growth are initially slow to design their business models, but then accelerate as they scale. In contrast, their less successful counterparts rapidly design their business models and initially grow quickly, but then fade. The racing design is particularly useful for new organizations where their frequent failure often provides an unequivocal outcome.

A related design is *polar types*—that is, cases are chosen for their extremes (e.g. high and low performance, fast and slow decisions), yet are similar along many dimensions. This is the design that Langley and Abdallah (2011) identify. For example, Martin and Eisenhardt (2010) chose high- and low-performing collaborations within each of six software firms, thus "controlling" for the firm and its executives. They then uncovered distinct collaboration processes that were likely to lead to one or the other outcome. Murray et al. (2020) study how ventures mobilize crowdfunding resources by choosing similar ventures with highly and much less successful fundraising outcomes. Zuzul and Tripsas (2020) pick four firms in the nascent air taxi industry—two firms that made adaptive adjustments and two that did not. They then unpack the distinct founder identity-related processes that triggered reinforcing cycles of flexibility or inertia. Polar types are particularly useful for sharply illuminating stark differences across cases.

The above designs involve similar cases that may nonetheless reveal different processes and/or outcomes, thus mitigating alternative explanations. In contrast, the *common process* design involves choosing cases about the same focal phenomenon in purposefully different settings, thus improving generalizability (i.e. transferability) of the emergent theory across settings. For example, Graebner and Eisenhardt (2004) study how acquisitions occur from the seller's perspective by using cases from three different industries with four sellers each. Similarly, Bingham and Eisenhardt (2011) examine what entrepreneurs learn from their repeated process experience by studying internationalization. To increase generalizability, the authors study ventures from three culturally distinct countries: Singapore, United States, and Finland. Overall, the twist of the common-process design is that, despite seemingly different cases, they often have similar processes and/or outcomes.

Finally, sometimes case designs take unexpected turns that offer serendipitous theoretical opportunities. One example is Ott and Eisenhardt (2020) who explore how entrepreneurs form complex, novel strategies by studying ventures in three distinct marketplaces. One venture was initially on a successful path, strayed, but then returned. This turnaround sharpened the causal theoretical logic. Ozcan and Eisenhardt (2009) take advantage of different founding dates of focal firms to explore alliance formation at different times during market emergence in mobile gaming. Some strategies available at the outset of the industry like establishing an industry architecture were less available to later entrants. An especially intriguing example is Bechky and Okhuysen (2011) who independently collected data on film crews and a SWAT team. The authors then realized that they were studying the same processes and combined their studies. They reveal how (although working in very different settings) SWAT teams and film crews address surprise with common processes. Overall, there is no specific formula for case selection, but rather an

appreciation of the need to consider whether the focal phenomenon is likely to be present and where similarities and differences across cases should best occur.[1]

Third, the Method is particularly explicit about developing (and defining) *constructs and measures* during the analysis. Why? Constructs are essential components of any theory, and measures help to ensure that the emergent theory is well-grounded and testable. I use these terms to link better to the broader research community and avoid unnecessary new terms. That said, other researchers using grounded theorizing prefer different terms like first- and second-order themes (Gioia and colleagues) or stay closer to the original wording of grounded theory (Glaser and Strauss, 1967). Regardless, the common notion is iteratively organizing and grouping raw data and then forming more abstract conceptualizations as part of constant comparison between theory and data (Walsh et al., 2015).

Some have observed that the cases often seem to fit the theory surprisingly well. There are several reasons for this. First, the analytic process of constant comparison and replication logic attempts to find a common pattern across the focal cases via persistent and creative iteration. So, the aim of the analysis is fit! Second, the fit is typically not perfect. Rather, careful readers will recognize that some cases often fit better with the emergent theory than others. After all, R^2 never equals one. Authors, however, rarely emphasize this variation because perfect grounding is neither essential nor realistic for theory building. It can also be misunderstood by reviewers. The data, however, are there to be seen by readers.

More subtly, the fit is often high because of the skilled use of conceptualization (i.e. categorization) and abstraction in defining and grounding constructs. For example, simply identifying precisely the right construct can change how commonly it occurs across cases. Similarly, raising the abstraction level of a construct can change its occurrence across cases. Indeed, categorization and abstraction are at the heart of effective theory building. Parsimony is also relevant. Single case studies often yield more complicated and over-determined theories than multiple cases. These theories may not generalize well. In contrast, multiple cases are advantageous because they make it easier for researchers to identify and sharpen theoretically relevant construct definitions at an appropriate abstraction level and often mitigate alternative explanations and over-determined theory. Yet categorization and abstraction are also part of the "art" that is hidden, creative, and hard to describe.[2] Indeed, theory building from cases is often challenging for novice researchers because they are typically less adept than experienced researchers at shifting among levels of abstraction and construct definitions.

Fourth, the Method emphasizes explicit *theoretical arguments* (i.e. mechanisms) that support *why* particular emergent relationships between constructs are likely to hold. Such arguments are at the heart of theory building because they address the internal validity and logical coherence of the emergent theory. These arguments can be based on the data, logic, and/or prior research especially from distant literatures like cognitive science and biology. For example, Ott and Eisenhardt (2020) identify the decision weaving process for solving novel, complex problems in their study of strategy making in marketplaces. The authors support the theoretical relationships (i.e. tying a recursive process of sequential focus, plateaus, and stepping stones to outcomes) with cognitive science arguments. Arguments and evidence from this other discipline lend logic, credibility, and clarity to the emergent theory.

One misconception is that using theoretical arguments from prior research diminishes the novelty of the emergent theory. While possible, this view misses that the theoretical arguments are developed after the theory is emerging. Instead, studies can break new conceptual ground in established theories (Valentine, 2018). More subtly, this view misses the importance of drawing on prior literature that is outside the focal research of the study such as using arguments from cognitive science to support a theory in the strategy field. Distant literature can reveal unanticipated

theoretical mechanisms that can strengthen the logic of the emergent theory, lift its abstraction level, and improve its generalizability. As noted in the Ott and Eisenhardt (2020) example above, it can offer insights that make the theory surprising yet logical and convincing. Of course, if the relevant literature is nearby and the relationship is well known in the focal field, then it is better to acknowledge this and move on.

Another misconception is that these "why" explanations are simply rhetoric. They are not. More than a rhetorical move, theoretical arguments are *central* to the Method and to theory building generally. They take relationships between constructs from patterns of association to theory. Yet, unfortunately many researchers omit these arguments, and so end up with descriptions or "findings" but not much explanation as to why. That is, they have little or no theory (e.g. Sutton and Staw, 1995). Furthermore, lack of theoretical arguments limits credibility and obscures generalizability. That said, many researchers (multi-case theory builders and others) purposefully use some rhetorical language. For example, I often open a paper with a vignette about a well-known exemplar to motivate the paper. I also frequently underscore when emergent theoretical insights are novel or surprising. While this may seem like "over-claiming," it is what many reviewers for top journals demand, especially for qualitative research.

Fifth, the Method includes identifying *boundary conditions* and as appropriate, addressing *alternative explanations*—just as any strong theory would. Boundary conditions clarify the scope of the theory, and so the domains to which it is likely to apply. A key point is that boundary conditions relate to the theoretical logic, not surface features like industry or location. Reading literature and puzzling over why the results emerge can help to identify boundary conditions. Addressing alternative explanations sharpens the theoretical arguments and strengthens confidence in the internal validity of the theory. That said, these were not explicit in the original AMR article. Rather, they have become part of the Method due to the encouragement and even insistence of reviewers and editors.

Finally (and as may go without saying), the Method emphasizes analysis using *constant comparison* between the theory and data (Glaser and Strauss, 1967), *replication logic* (Yin, 1984), and *cross-case analysis* (Eisenhardt, 1989a). Constant comparison refers to the extensive iteration between the emergent theory (particularly constructs and relationships) and data to create an increasingly close fit between the two—both within cases and across cases. Replication logic refers to repeating this iteration by examining each case as a standalone observation, not as a data point in a sample for which pooled logic (and statistical analysis) is relevant. Cross-case analysis refers to various approaches to improve the creativity and reliability of the analysis (within-case and especially cross-case). Importantly, it also involves developing the underlying theoretical arguments—that is, "why" explanations of patterns in the data.

Overall, multi-case theory building centers on creating strong theory about under-explored, yet significant phenomena. Its core features reflect this aim. The successful outcome of a multi-case theory-building study is a theory that balances parsimony (not complicated spaghetti diagrams), accuracy (captures the core features of the phenomenon), and generalizability (relevant beyond the immediate setting), is logically coherent . . . and is (hopefully) surprising![3]

What the Eisenhardt Method is *Not*

Conversely, other features that are often associated with the Method are *not* fundamental to it. These features are often superficial (and so easily seen) characteristics or forced "boxes" in ill-fitting typologies. They are not, however, central to the Method and its theory-building aim. Here are some prevalent ones.

The Eisenhardt Method is *not* about a particular type of data. Many multi-case theory-building studies (including mine) use interviews and archival data, but this choice is not inherent in the Method. Rather, it reflects decisions about the best data to collect given factors like the research question and data availability. For example, Gurses and Ozcan (2015) use only archival data to examine the emergence of pay TV while Kellogg (2012) uses interviews and ethnography in her study of change processes within hospitals. Garg and Eisenhardt (2017) rely on interviews, observations, and quantitative questionnaire data in their study of how CEOs engage in strategy making with their boards. Furthermore, this study highlights that although the terms "case study" and "qualitative" are often conflated, case study research can include qualitative data, quantitative, or both. Indeed, "qualitative research" as a method is something of a misnomer given that qualitative is more a type of data. Similarly, others including Glaser have re-asserted the relevance of quantitative data in grounded theory building (Walsh et al., 2015).

The Method is *not* about a specific number of cases. While 4 to 10 cases are common and often work well (Eisenhardt, 1989a), a specific number of cases is not inherent in the Method. Rather, the number is influenced by theoretical issues like case design and theoretical saturation as well as by pragmatic factors like data availability, cognitive limits, and time. So statements like "usually about eight" are incorrect (Pratt et al., 2019). Instead, some researchers use two cases like Battilana and Dorado (2010) in their comparison of Bolivian microfinance organizations and McDonald and Gao (2019) in their study of "pivot" processes. Given data demands, multi-case ethnographers like DiBenigno and Kellogg (2014) often stop at two as well. In contrast, Zuzul and Tripsas (2020) use four cases to explore of inertia and identity. Hannah and Eisenhardt (2018) examine 5 ventures in studying ecosystem strategies, Li and Piezunka (2020) examine the CEO succession process of 7 Chinese family firms, Martin and Eisenhardt (2010) explore 12 cross-business unit collaborations, and Baker and Nelson (2005) study bricolage processes in 29 small businesses.

Embedded designs (i.e. multiple units of analysis) are possible as well. They offer more opportunities for replication and variation. For example, Hallen and Eisenhardt (2012) study the fundraising process of 9 ventures and their embedded 30 or so funding rounds. Cohen et al. (2019) study learning in 8 accelerators with 37 embedded ventures. Single cases can also have embedded designs, making them amenable to multi-case theory building. Galunic and Eisenhardt (2001), for example, unpack 10 charter changes in one firm while Dalpiaz et al. (2016) study how recombinant strategies unfolded over three successive temporal eras at Italian manufacturer, Alessi.

The Method is *not* about performance. I often study performance as an outcome because I am intrigued by what works and not. But I do not always study performance (e.g. Bingham and Eisenhardt, 2011; Graebner and Eisenhardt, 2004). Similarly, other researchers, especially those in the strategy field, often study performance or a variant like innovation, but again not always. Powell and Baker (2014), for example, look at how founder identity influences the direction of strategic adjustment under adversity, but not performance. Shah (2006) examines evolving motivations in open-source communities. Anthony et al. (2016) reveal the unfolding product positioning choices for new music synthesizer products by four incumbent firms. The key point is that studying performance is a research choice and is not inherent in the Method.

The Method is *not* about variance (vs process) theory. Instead, the Method is relevant for theory broadly.[4] Many researchers using multi-case theory building collect longitudinal data and address process questions (e.g. how phenomena evolve over time). Sometimes, the emergent theory captures *process similarities* across cases. For example, Bingham and Eisenhardt (2011) examine how entrepreneurs learn from their repeated process experience. The emergent theory describes process similarities like the temporal order of learning, and cycles of expansion and contraction of "simple rules" heuristics. Likewise, Bechky and Okhuysen (2011) develop an emergent theory that

describes the successive processes by which actors—that is, film crews and SWAT teams—similarly cope with surprise.

Sometimes, the emergent theory captures *process differences* across cases. These can lead to different outcomes. For example, McDonald and Eisenhardt (2020) develop their emergent theory of "parallel play" to describe how entrepreneurs design successful business models. They identify a sequence of behaviors (e.g. borrowing from rivals, experimenting, committing to a template, pausing before elaborating) that form the parallel play process used by the successful entrepreneurs. In contrast, entrepreneurs who followed different processes designed poor business models and failed. Yet, process differences can also lead to similar (even equifinal) outcomes. For example, Hannah and Eisenhardt (2018) identify three different ecosystem strategies and how they successfully played out while two other strategies failed. This study also illustrates the critical point that different processes are often "qualitatively" different—for example, presence (or absence) of some process features, not just more (or less) of the same features. Different processes can also work well for particular actors, but not others (Hallen and Eisenhardt, 2012) or be faster (or different in some other way) than other processes (Volmar and Eisenhardt, 2020).

Emergent theories also vary in their representation of the progression of *time.* Sometimes the emergent theory is *sequential.* For example, Hallen and Eisenhardt (2012) develop a process theory of efficient tie formation in which actions like "casual dating" precede other actions that unfold in sequence. Sometimes the emergent theory is *recursive*, Ott and Eisenhardt (2020) illustrate. They identify a sequence of actions that recursively repeats in successive strategic domains in order to form complex, novel strategies. Emergent theories can have *reinforcing cycles* (Zuzul and Tripsas, 2020). Sometimes the emergent theory is *synthetic*—that is, describes process characteristics (e.g. specific actions), but not time (Langley, 1999). This type of theory can emerge when there is no clear temporal pattern in the data. For example, Santos and Eisenhardt (2009) study the boundary formation process of entrepreneurs in five Internet stars. The emergent theory reveals three processes (i.e. claiming, demarcating, and controlling) that interleave and recur, but not in a discernible temporal pattern. Finally, the emergent theory is occasionally cross-sectional, not processual (Tidhar and Eisenhardt, 2020).

More deeply, emergent theories (and their related research designs) can vary in the conceptualization of *time itself*. Sometimes the emergent theory describes processes with well-defined beginnings and ends like an acquisition negotiation (Graebner, 2009) or a fundraising round (Hallen and Eisenhardt, 2012). Sometimes the theory captures processes during a slice of ongoing time. For example, Ozcan and Eisenhardt (2009) track firms from founding until they build a complete ecosystem. Ozcan (2018) then follows what these firms did next. Sometimes the emergent theory conceptualizes time as a flow where processes like becoming are germane. For example, Brown and Eisenhardt (1997, 1998) describe an emergent theory of continuously changing organization at the "edge of chaos" (a paradoxical state of dissipative equilibrium). Overall, the Method is not about a type of theory. Rather, the type of theory depends on research choices like question and case design as well as the patterns that occur in the data.

Finally, the Method is *not* about vertical (vs horizontal) knowledge. Roughly, this dichotomy categorizes research as vertical (i.e. building cumulative insights) versus horizontal (i.e. building insights that are situational or stand apart from other knowledge) (Pratt et al., 2019). While I often look for connections with other research and the original article favors this, vertical versus horizontal knowledge is not part of the Method. In fact, since horizontal and vertical are orthogonal dimensions, it is unclear if they are mutually exclusive. More important, many studies are both. The balance reflects both research choices like the question, case design and decision to connect (or not) with other knowledge, and the knowledge itself that emerges from the data.

In sum, there are common (and understandable) misunderstandings about the Eisenhardt Method. They often reflect superficial features (e.g. number of cases, types of data), confusing categorization (e.g. process vs variance) and misfit dichotomies (e.g. vertical vs horizontal knowledge). They suggest a rigid template that does not exist. Instead of these, the fundamental features of the Method are few, leaving open a wide variety of research possibilities. Yet, they are all the implications of the Method's core aim of theory building.

If I wrote "Building Theories" today

If I were to write the AMR article today, I would make a few changes. First, I would describe the Method as ontologically (i.e. nature of reality) and epistemologically (i.e. how that reality is known) flexible. In the context of the late 1980s, I was interested in making links to the much larger deductive research community, and so used the term positivist. I saw the Method then (and now) as an alternative to verbal theorizing and formal modeling methods. In retrospect, I was a novice researcher who did not anticipate the philosophy of science "minefield" that this word would trigger or its debates that sometimes seem "religious." I also did not expect that some colleagues would use this word to create foolish caricatures and drive unproductive wedges in the field. In my view, there is an objective reality. As a colleague recently noted, "If a brick hits you on the head, it actually happened." But there is also a social construction of reality.

I would also write more carefully about the role of the researcher. In the late 1980s, I was attempting to legitimate multi-case theory building and convince the larger research community that the work is not a biased jumble of opinions and preconceptions. So I emphasized then (and still believe) that the data help to keep the researcher "honest" (or at least relatively so) just as math does for formal modelers. Moreover, in my experience, the most novel insights emerge from the data (not preconceptions) which, of course, is an aim of grounded theory. That said, it is incorrect (and foolish) to caricature the Method as about being "fully detached." Rather, there is always researcher influence. This is particularly apparent in, for example, the choices of research question and data. An illustration is a terrific scholar like Denny Gioia who is drawn to different questions and data than I and others (Gehman et al., 2018). I am also increasingly aware of the "art" of theory building around identifying concepts and relationships. This requires deft use of abstraction, categorization, and a range of literatures that not all researchers can muster. I would also be careful to avoid drift into "boiler plate" methods language like "court room questioning" in my own papers. Methods sections often reflect reviewers and current "fashion," making them prone to over-interpretation by later readers. In sum, ontology and epistemology are research choices that are not core to the Method—a view consistent with Glaser and others who remind us of the ontological and epistemological flexibility of grounded theory (Walsh et al., 2015). And, these choices would not arise in Building Theories 2.0.

Second, if I were to write Building Theories today, I would offer a rich palette of research questions, case designs, and theory-building possibilities. In the late 1980s, there were few studies from which to draw inspiration and my own work was nascent. I could not anticipate the many types of research questions and where they might best be discovered. Building on my and others' work, I now have a much clearer view (some of which I summarized above). I also better appreciate the relevance of a thorough literature review to reveal promising research questions and set up contributions. Similarly, I would include a richer range of research designs with more clarity about why they work. Here, reviewers and editors have been helpful in encouraging me and others to articulate the meaning of theoretical (vs random) sampling, why it is useful and legitimate, and how this sampling might mitigate alternative explanations or

enhance generalizability. This feedback also helped to build a vocabulary of research designs (e.g. racing). Finally, I would be more expansive about the range of theory-building possibilities. When I wrote the original article, I focused on theory building as close as possible to a "blank slate." Since then, I have come to recognize the broad range of theory-building possibilities (including theory elaboration – i.e, abduction) from scholars like Edmondson and McManus (2007), from personal involvement in the field (Anteby, 2013), and from the review process that encouraged my coauthors and me to be precise in describing our proximity to prior work (e.g. Bingham and Eisenhardt, 2011).

Third, if I were writing Building Theories today, I would place time more directly in the spotlight. When I wrote the original paper, I was intent on providing a roadmap for multi-case theory building, legitimating it, and bridging to the larger research community. I have, however, become increasingly cognizant of the significance of temporal phenomena and the central role time in understanding strategies, organizations, and groups. I am also more aware of the many intriguing ways time unfolds (Langley et al., 2013), and the relevance of multi-case theory building (among other approaches) for exploring them. So Building Theories 2.0 would have many more citations to Langley and other process pathbreakers.

Finally, if I were to write Building Theories today, I would have a wider view of the research enterprise. In the late 1980s, I thought of theory building and theory testing as two sides of a coin. This fits with how research seemed to be partitioned: theory-to-data or data-to-theory. And this fit with my own work. For example, Kaye Schoonhoven and I used insights from my strategic decision-making case studies (e.g. Eisenhardt, 1989b) for the theoretical arguments in our deductive econometrics-based paper on venture growth (Eisenhardt and Schoonhoven, 1990). Yet I now realize that many deductive researchers are neither able to gather a sufficient sample size nor have the statistical tools to test process theories. This, however, opens up other possibilities. For example, insights from multi-case theory-building research about the "edge of chaos" (Brown and Eisenhardt, 1997) led to a computational simulation about simple rules (Davis et al., 2009). Insights about charter change processes (Galunic and Eisenhardt, 1996, 2001) were the genesis of formal models (Sakhartov and Folta, 2014). Theory-building case studies increasingly build on prior ones. Multi-case theory building and machine learning can work together as complementary pattern recognition methods (Tidhar and Eisenhardt, 2020).

In summary, Building Theories 2.0 would offer a richer and better articulated palette of research choices, an emphasis on temporal processes, a wider view of the relationship among methods, and no philosophy of science pitfalls. Yet at the end of the day, I would still mostly write the same core message.

Looking backward and forward

When I wrote Building Theories from Case Study Research, I was an untenured assistant professor with few publications and no idea about what the article would become. So it is gratifying to realize now how pivotal Building Theories became for many researchers as they launched their own careers or expanded their methods repertoire. Yet over time, aspects of the Method have become misunderstood or forced into misfit typologies. Admittedly, it can be challenging to discern from the outside what the Method is versus what reviewers want versus what are research choices. At the same time, understanding and use of the Method has deepened and broadened with the help of editors, reviewers, and the many scholars who use and extend it. The Method has never been about types of data, number of cases, performance, variance (vs process), and the like. Rather, its aim has always been theory building. Its core features are the implications of that aim like open-ended research questions, theoretical sampling of cases, well-identified constructs and relationships, and

underlying theoretical arguments. The Eisenhardt Method has always been an approach to building theory that offers advantages over alternatives like arm-chair theorizing and analytic modeling, and can bring the theory builder into close and even intimate contact with phenomena.

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Notes

1. Not every combination of cases is useful. For example, Pratt et al. (2019) incorrectly state that the Eisenhardt Method involves choosing cases with different independent variables and then explaining different outcomes. Instead, this design is typical of deductive research and is rarely, if ever, seen in multi-case theory-building research because it assumes that independent variables are known in advance.
2. In contrast, fit is not achieved by eliminating cases. For example, see Grodal et al. (2020) re implications of categorization in theory building.
3. Some like Weick (1989) define accuracy by a theory's fit with the data. The view here is a modeling one that defines accuracy by a theory's fit with the essence (i.e. core features) of the phenomenon, not every detail (Hannah et al., 2020), a view relevant for multiple cases.
4. The process versus variance distinction (Mohr, 1982) is confusing and likely incorrect. It appears to conflate similarity with process, and difference with cross-section. This leaves the puzzle of process theories about differences. Its treatment of difference (variance) is a weak regression-like representation that models processes and outcomes by variables that cannot represent, for example, qualitatively different processes, cycles, and equifinality.

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