Problem formulation for theorizing at the frontier: An Oliver Williamson inspired approach

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Abstract

Ostensibly, the evolving science of strategic management is geared towards addressing vexing managerial problems. In practice, however, scholars in the field have a marked tendency to formulate problems to fit *existing* theoretical and methodological frameworks, even at the expense of committing type III errors. While the tendency to do so is often attributed to institutional pressures and the like, we submit that an equally or more compelling reason is the absence of guidance on how to engage in problem-driven inquiry and formulate problems to explore theoretical frontiers. In the strategic management field's problem-solving spirit, we provide an approach for problem formulation and theorizing inspired by Oliver Williamson and two of his accomplished advisees. We abduce five principles and six dialectic conversations. We synthesize these principles and dialectics into five protocols to enable canonical problem formulation directed at exploring theoretical frontiers, that is, a "white space." Using a recently rejected manuscript, we show how our Williamson inspired approach can be useful in formulating problems that are both managerially relevant and theoretically fruitful.

Keywords: abductive reasoning, dialectics, pragmatism**,** paradigms, problem formulation, theorizing

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"I want to stay as close on the edge as I can without going over. Out on the edge you see all kinds of things you can't see from the center."

Kurt Vonnegut (Player Piano)

1. INTRODUCTION

According to Popper, "social sciences always start from *problems*, from the fact that something inspires *amazement* in us" (1999: 3, emphasis in original). Solving these problems entails a trial-and-error process that directs attention to anomalies that are the inspirations of amazement (Peirce, 1923; Popper, 1963; Van de Ven, 2007). Surprised by the failure of extant theories to explain and predict, scholars develop new theories with the intent of subjecting them to rigorous empirical evaluation and discarding those that are "falsified" (Lakatos, 1970; Popper, 1963). Theories that are not falsified are *provisionally* retained, contributing to the overall growth of science.

Popper admits that his proposed approach is normative and is arduous to apply (1994: 29). Indeed, most scholars operate within the theoretical and methodological frameworks in which they are trained (Kuhn, 1977; Makadok, Burton, & Barney, 2018) and attempts at genuine falsification are rare (Mahoney, 1993; McCloskey, 1983). Instead, much theorizing in strategic management is driven by considerations of data availability and methodological training (Bettis & Blettner, 2020; Chaudhari, Leiblein, & Reuer, 2021). The resulting theory production contributes to a growth of knowledge but results in an overall body of work that is fragmented and difficult to integrate (McGahan, 2022; Pfeffer, 1993). Increased specialization and incentives encourage doctoral students and (junior) scholars to avoid risk taking and discourage attempts at breaking out of their partly self-imposed paradigm prisons (Bresser & Balkin, 2022; Miller, 2007). In fact, scholarly attempts at prison breaks are actively penalized in terms of publications and promotions (Akerlof, 2020; Weintraub, 1989).

Scholars in the field are typically encouraged to develop more theories that are "interesting" and counterintuitive without considering that neither of these attributes provide any confidence that the knowledge claims that follow are useful (Tsang, 2021; Vermeulen, 2005). Yet, an overemphasis on theory (Hambrick, 2007; Miller, 2007) can be counterproductive and encourage opportunistic practices (e.g., Anonymous, 2015; Goldfarb & King, 2016) – after all, if you torture data long enough, it will confess (Coase: 1994: 27). Worse still, research resources are wastefully directed almost exclusively at elaborating on existing theory by gap filling (Makadok et al., 2018; Sandberg & Alvesson, 2011), when scholarly imagination could instead be employed to tackle grand challenges and neglected strategic issues (Langley, 2021; McGahan, 2022; Teece, 2020) that are urgent *problems* of practical relevance.

Ultimately, the field bears the opportunity cost of misdirected resources and misaligned incentives in the form of decline of managerial relevance (Drnevich, Mahoney & Schendel, 2020; Fisher, 2020; Starkey & Madan, 2001). We submit that this decline equates to a cavalcade of Type III errors – solving the wrong problem (Mitroff & Featheringham, 1974; Nickerson & Argyres, 2018) – that advance theory for reasons other than managerial relevance.

Addressing the problem of declining relevance requires that some scholars engage in the admittedly riskier approach of managerially relevant problem-driven theorizing (Fisher, Mayer, & Morris, 2021; Laudan, 1978; Nickerson, Yen, & Mahoney, 2012). Formulating such problems is critical – just as theory does not emerge from data (Hanson, 1958; Popper, 1963), problems do not simply emerge from phenomenon and need to be constructed (Rittel & Webber, 1973; Simon, 1973). In fact, problems need to be formulated to guide observations (Lakatos, 1980; Popper, 1994). To shed light on problem formulation, a critical and often neglected aspect of theorizing, we turn to Oliver Williamson as an archetype to understand his approach to problem formulation. One of the most cited scholars in strategic management (Nickerson, 2010), Williamson is the only scholar that strategic management claims as its own to have won the Nobel Memorial Prize in Economic Sciences. Williamson passed away in the spring of 2020, which is before we launched our inquiry and made impossible any direct interviews with him on this topic. We therefore relied on documented evidence to understand his approach to canonical problem formulation. As a means to triangulate his process and avoid relying on a single data point from which to draw inferences, we also interviewed two of his students, Nicholas Argyres, and Janet Bercovitz, both successful scholars. With an imprinting from Williamson, interviews with two of his students can help reveal those aspects of his formulation process that others found productive and valuable, which helps to triangulate vital insights. We reconstruct through publications and interviews key and systematic moves that sparked the discovery and formulation of hitherto unseen canonical problems – a white space.[1]

Our inquiry revealed five principles guiding these scholars' inquiries that lead to "dialectical" engagement *via* multiple internal and external conversations (Evered & Louis, 1981; Schweiger, Sandberg, & Ragan, 1986). By dialectical engagement, we mean a conversation between two views in tension such that one view can potentially *alter* the other (Kuhn, 1977; Putnam, 2002). Such conversations can be useful in revealing both commonalities and differences between views, thus generating the prospect of providing a novel canonical formulation. Our inquiry revealed six kinds of dialectic conversations that these scholars engaged in during the problem formulation process. These conversations led to the formulation and reformulation of questions until a problem statement indicating a white space emerged. We embed these five principles and six dialectic conversations into five protocols that provide specific steps and questions that can aid scholars in discovering their own white space.

Overall, we contribute to the literature on (pragmatic) theorizing (King, Goldfarb, & Simcoe, 2021; Mantere & Ketokivi, 2013; Sætre & Van de Ven, 2021) as well as the problem-finding and problem-solving perspective (Alvesson & Sandberg, 2011; Baer, Dirks, & Nickerson, 2013; Nickerson,

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[1] White space can also be understood as *theorizing at the frontier*. Such theorizing launches a new research program in that its core theoretical assumptions and percepts typically differ (substantially) in at least one way from existing research programs, and often reveal "facts" (Lakatos, 1970). For example, TCE's discriminating alignment hypothesis that acknowledges the limits of bounded rationality differs from the neoclassical economic approach that adopts an optimization and "unbounded" rationality approach. TCE also reveals new "facts," such as asset specificity. Nicholas Argyres' s pursuit of capabilities can be seen in the same vein.

Silverman, & Zenger, 2007). By focusing on how scholars' dialectical engagement can enable them to formulate a canonical problem and to theorize as well as persuade their peers about the value of their theorizing, the current paper sheds new light on the (often-overlooked) dialectical aspect of theorizing (Leone, Mantere, & Faraj, 2021; Van de Ven, 2007). Finally, we offer an approach that can aid scholars in breaking out of their (self-imposed) methodological and theoretical prisons (frameworks) while maintaining the role of theory in various forms of inquiry (Bettis, 1991; Hanson, 1958; Popper, 1970).

2. BACKGROUND

Oliver Williamson was a co-recipient of the 2009 Sveriges Riksbank Prize in Economics in Memory of Alfred Nobel for his analysis of economic governance, especially the boundaries of the firm. This prize signifies that he formulated a problem largely ignored by law, economics, and organization – a substantial white space – and, as a result, created a new theoretical paradigm of great significance for real-world practitioners (e.g., managers, courts of law, policy makers) and the field of strategic management (Mahoney & Nickerson, 2022; Shapiro, 2010).

We chose to study Oliver Williamson because of his success in problem formulation, which led to the exploration of white space and creation of a new paradigm. Further, Williamson was known for imprinting students with his approach. We therefore reviewed Williamson's entire personal memoirs and archive not yet available to the public. In addition, two of the authors recently published a biography of his life (Mahoney & Nickerson, 2022). Our examination and review of Williamson's memoirs and work provided us considerable insight into how he approached problem formulation.

To enable a deeper understanding of Williamson's approach, we also interviewed two of his accomplished dissertation advisees, Nicolas Argyres and Janet Bercovitz. These successful scholars offer a convenience sample that enabled a more in-depth exploratory triangulation of Williamsonian approaches to problem formulating approaches than otherwise would have been feasible. By choosing to interview two of Williamson's students, we anticipated observing specific approaches to the discovery of problems and paradigms that Williamson imparted to his students that they found valuable. From an evolutionary epistemological perspective (Campbell, 1974; Popper, 1963), Williamson's and his advisees' academic success indicate that their problem formulating approach likely has currency more generally.[2] We developed a semi-structured interview guide to uncover their approach for formulating problems and generating paradigms for research. The primary directions of inquiry for the interview guide included (1) how Williamson's advisees engaged in problem finding, framing, and formulating (2) how they reasoned/integrated insights from differing theories and paradigms, and (3) how they extended paradigms to new domains. Appendices A and B summarize our semi-structured interviews with Argyres and Bercovitz.

After each interview, we reflected to understand better the arc of the conversation, clarify fundamental themes that emerged, and compare insights across interviewees. These reflections were the first step in an ongoing engagement in abductive reasoning, which is the first stage of a scientific inquiry (Peirce, 7.218) and the *only* form of reasoning that yields explanations (Peirce, 5.145, 5.171).[3] Because abductive reasoning involves the generation of understanding developed in light of a specific perspective – like a background theory (Behfar & Okhuysen, 2018; Mantere & Ketokivi, 2013) – each author of the current paper brought a differing view to interpreting the interviews (Ketokivi & Mantere, 2021; Lewis, 2017). Following Rescher (1977) and Toulmin (2003), we engaged in multiple constructive dialogues until we converged to the principles, dialectics, and protocols that constitute our framework. In what follows, we present our developed framework.

[2] Indeed, as noted by Mahoney and Nickerson (2021), Williamson's Ph.D. advisees as a whole have a remarkable record of publications and impact as measured by citations.

[3] "Peirce" refers to the Collected Papers of Charles Sanders Peirce: https://colorysemiotica.files.wordpress.com/2014/08/peirce-collectedpapers.pdf

3. FRAMEWORK TO GUIDE INQUIRIES

Numerous conversations among the authors of this study led to a convergence of understanding how these scholars engaged in inquiry. Three categories of inquiry emerged, which we label principles, dialectics, and protocols of inquiry. The following section describes as well as illuminates several elements of each category of inquiry (Figure 1).

Insert Figure 1 here

3.1 Principles of inquiry

Our interviews revealed five principles that undergird successful efforts at discovering problems and paradigms: curiosity, epistemic humility, self-criticality, courage, and reflection. We now define each principle by an activating question and illuminate each question from our interviews.

Curiosity: A recurring principle of inquiry displayed by all three scholars is a curiosity illuminated by the question repeatedly mentioned: *What is going on here*? This question arises observing phenomena and being surprised because some previously held expectation did not hold or seems incongruent in that it lacks explanatory power. For Williamson, the question was pivotal in examining the Schwinn bicycle case and his evaluation that the dominant paradigm from pricing theory was inadequate for explaining the use of vertical restraints (e.g., Williamson, 1985: 183-189). Similarly, in response to her very first interview question, Bercovitz responded that the question "What is going on here?" guides her inquires.

Epistemic humility: All three scholars showed epistemic humility in their approach to inquiry by explicitly asking the question, *how can the phenomenon be understood from multiple perspectives*? [4] Epistemic humility accepts limits of bounded rationality (Simon, 1957), recognizes that complex phenomenon

[4] The very nature of this question encourages scholars to escape their own "paradigm" prison (Miller, 2007).

can be explained via multiple theories, and that scholars can speak the language of more than one theory (Fuller, 2006; Huff, 1981; Laudan, 1990). For example, Williamson's engineering degree and working for the CIA as well as graduating from what is now called Carnegie Mellon University provided him with an engineering, organizational, and behavioral imprinting, along with an advanced understanding of economics. Observing phenomena from multiple lenses and later inviting additional perspectives like organization theory (Williamson & Ouchi, 1981; Williamson, 1995) revealed new problem formulations and, ultimately, a white space centered on understanding the canonical problem of vertical integration. Perhaps as a result of Williamson's imprinting, both Argyres and Bercovitz seek out multiple perspectives, some of which stem from their multiple imprinting, and others from practitioners, co-authors, and other theories. For example, Argyres speaks of the usefulness of Marx, Williamson, and Teece's imprinting as key sources for formulating research questions in the governance and capabilities space (see, e.g., Argyres & Zenger, 2012). Argyres, along with Bercovitz, emphasize the importance of co-authors for generating different perspectives and *respectfully* engaging in conversations to understand each other's views (Mahoney, 1993; Van de Ven, 2007). Seeking understanding from multiple perspectives is a mark of epistemic humility that all three scholars enact.

Self-criticality: All three scholars are self-critical in that they ask, "*what am I missing*?" For example, Williamson is known for advocating taking logic to the limit of its natural progression to illuminate whether the theory cannot explain canonical problems or not. Bercovitz directly describes her anxiety about not framing a problem or phenomenon correctly, which stimulates her to find additional perspectives. Argyres seeks to identify what is missing by considering different perspectives to address the same question. In other words, these scholars adopt the Popperian approach of mentally trying to "falsify" their own theories, which then motivates them to seek alternative perspectives to evaluate whether their understanding is deficient in some way or not (e.g., Argyres, Gil, & Zanarone, 2022). Doing so, naturally, improves the rigor of their theorizing.

Courage: With strong professional incentives related to tenure and robust ideologies in the disciplines, rejecting existing (dominant) theoretical and methodological frameworks is fraught with professional and publication challenges (Akerlof, 2020; Weintraub, 1989). But at the same time, uncritically (or opportunistically) embracing (dominant) frameworks is unlikely to yield problem formulations that are both managerially relevant and theoretically fruitful.[5] Williamson, Argyres, and Bercovitz' research inquiries implicitly ask *why might the dominant (theoretical/methodological) framework be wrong*? For example, Williamson reacted to the luminaries at the Department of Justice (e.g., Richard A. Posner and Donald Turner) who viewed Schwinn's vertical restraints as *per se* antitrust violations because of a strict price theory orientation. Williamson considered what might be wrong with the dominant barriers-to-entry framework of vertical contracting, and provided an efficiency-based explanation, which accounts for organizational and behavioral challenges. Somewhat similarly, Argyres, as a Ph.D. student, was willing to reject his advisor's (Williamson's) strict adherence to a governance perspective. Somewhat differently, Bercovitz seeks out different perspectives under the presumption that no single paradigm can readily explain the phenomena of interest to her – an approach that is decidedly pragmatic in embracing the idea of the limitations of all theories (Drnevich et al., 2020; Sergeeva, Bhardwaj, & Dimo, 2022). Collectively, these authors embrace the principle of positing that existing paradigms do not fully explain the organizational phenomena that each examines. While embracing this courageous approach is decidedly risky, the rewards of intellectual satisfaction (and academic success) are commensurate.[6]

[5] Popper (1970), somewhat unfairly, in a rebuttal to Kuhn (1970), refers to this uncritical attitude disparagingly, and maintains it does not advance science. Yet, he fails to recognize that this piecemeal advance contains within it the seeds of destruction for the framework as it reveals more anomalies that cannot be addressed (Fuller, 2006). Courage involves *pursuing* these anomalies rather than glossing over them.

[6] We caution doctoral students and junior scholars concerned about tenure to seek guidance from their intellectual mentors (veteran scholars) before embarking on this path for two reasons. First, epistemic humility: one might simply be wrong and dialectically engaging with one's intellectual mentor and veteran scholars may reveal these flaws. Second, veteran scholars are more likely to understand the extent of the "challenge," and will likely be better placed to inform junior scholars about the full extent of the "risk." Indeed, one of the

Reflection: Although not immediately discernable in the interviews, each scholar showed an unmistakable drive to reflect, learn, and improve their abilities to contribute to social science (Popper, 1963, 1994). In essence, we maintain that a core principle displayed by these scholars reflects the question: *How can thinking be improved*? Williamson took steps like accepting a position with the Department of Justice while an Associate Professor to encounter phenomena to expand his thinking. As one of the co-authors was his research assistant, we can attest that Williamson would weekly have a list of books and articles from diverse fields of study to retrieve from the library.[7] Williamson also sponsored the well-known seminar IDS 270 to learn about institutions, which brought scholars from a wide array of disciplines to the Berkeley campus. These activities constitute the ongoing effort throughout Williamson's career to reflect upon and improve his thinking. Argyres acknowledges that he expands his knowledge through reading, engaging with co-authors, and case studies. Similarly, Bercovitz is continually on the lookout for new perspectives from a wide variety of stakeholders to advance her understanding and knowledge. In essence, reflection and learning are ongoing activities to improve thinking among these scholars.

3.2 Dialectics of Inquiry

A dialectic is a conversation from multiple points of view, and as our interviewees expressed, to develop a deeper and more nuanced understanding of phenomena. A successful dialectic develops theory and evidence capable of convincing peers of the soundness of one's claims (Duede & Evans, 2021; Green, 2004; Toulmin, 2003) and even altering their view (Kuhn, 1977; McCloskey, 1983). Good

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authors can testify to the frustrating but ultimately satisfying journey adopting this path entails – his first dissertation chapter and job market paper was rejected from two UTD journals before finally being accepted in a third (Bhardwaj & Ketokivi, 2021). That journey took almost four years *after* the first submission.

[7] In commenting on the state of the universities, Popper noted that, "more and more Ph.D. students are trained only as technicians [without being told about] more fundamental problems" (1994: 124). Requiring doctorial students to read classics such as the works of Smith, Marx, Simon, Selznick, etc., as well as a foundational course on philosophy of science during coursework may contribute to improve thinking.

science, we discover from our interviews, is good conversation, which includes respectful disagreements and essential tensions (Leone et al., 2021; Mahoney, 1993).

From our examination of Williamson's work and interviews with his advisees, we abduce that formulating new problems to explore "white space" and theorize are associated with engaging in six overlapping dialectical conversations. We label these six dialectical conversations as (1) ideological, (2) collegial, (3) cross-framework, (4) theoretical-practical, (5) micro-macro bridging, and (6) getting-it-right and getting-it-out. Broadly speaking, these dialectical conversations surface tensions among frameworks that reveal anomalies and expose "white space" for theorizing.

Ideological Dialectic: Some form of ideological dialectic acted as an engine for theorizing in all three cases examined in the current paper. By ideological we mean a (non-scientific) framework for observing and thinking about the world. This dialectic involved an interplay between pre-PhD ideology and the ideology imparted by formal Ph.D. training (Ghoshal, 2005; Schumpeter, 1949). For example, Williamson's efficiency approach, in which transaction costs are characterized as *frictions* impeding economic exchange, can be seen as "clashing" with the zero-transaction cost *frictionless* assumption adopted in neoclassical economics. The conceptualization of these impediments to exchange as signifying frictions has its roots in the engineering discipline of tribology and reflects his pre-PhD ideology. A similar engine of theorizing is revealed in the dialectic between Argyres' imprinting of Marxism that regards capital and labor as being distinct and neoclassical economics that views them as substitutable. This dialectic was a source of dissatisfaction for Argyres with Williamson's governance approach because it privileged considerations of efficient economic exchange to the neglect of critical considerations of production. This tension became more salient because of Argyres presenting an article by David Teece in Williamson's Econ 224 class. Teece's (1982) early writing on organizational capabilities appeared to be a theoretically fruitful direction to explore to resolve the tension between production and transactions. Argyres' exploration led him to recognize and highlight this difference between production and exchange, and engage in novel problem formulation – why do some firms grow and become rich and other firms do not do so? – to generate theory.

Collegial Dialectic: Williamson, Argyres, and Bercovitz were able to further their inquiries by engaging in dialectics with their colleagues, albeit in different ways. Williamson engaged in conversations with his graduate students to discuss the industrial organization literature, which helped him better grasp the state of theorizing and what was missing. Argyres maintains that dialectical contact with colleagues who often have different intellectual backgrounds aided him in bringing intellectual conflicts and tensions into focus. Further, Argyres attributes to Jackson Nickerson his increased attention to more nuanced and precise problem formulation.[8] Bercovitz, who often works with colleagues from different intellectual backgrounds and training, notes that such dialectics enable the development of common grounds that serve as a foundation for theorizing and surface hitherto hidden assumptions embedded in a theoretical framework.

Cross-framework Dialectic: Williamson recognized agreement between economists and organization scholars concerning the central problem of economic organization – adaptation. However, a critical difference existed between these scholars regarding how to address the problem of adaption. While economists referenced spontaneous adaptions made by local economic agents using price as a signal, organization theorists appealed to authority inherent in hierarchies. Williamson realized that scholars in both paradigms were glossing over the insights offered by the other.[9] Indeed, *Markets and Hierarchies* reflects that realization, and Williamson's theorizing can be viewed as an outcome, of cross-framework dialectics. Such was his belief regarding the fruitfulness of this approach.

[8] Argyres notes that his advisor, Oliver Williamson, was open-minded and willing to engage in dialectics with him despite their differences in ideology. Collegial dialectics include being epistemically humble and allows for respectful disagreement – after all, *disagreement does not imply disrespect and does not entail being disagreeable*.

[9] Williamson references Hayek (1945) and Barnard (1938). Hayek and Barnard seemed to be embedded in fundamentally different ideologies – Hayek famously abhorred "central planning" while Barnard directed the New Jersey state relief system during the Great Depression.

Williamson repeatedly called for such cross-framework dialects (e.g., in Swedberg, 1990). Similarly, Argyres fused insights from two different (though not opposing) frameworks – transaction costs and capabilities – to examine vertical integration. Bercovitz maintains that in her experience, engaging in cross-framework dialectics results in better description and understanding regarding the phenomenon investigated and promotes clarity in thinking, which naturally enhances the quality of theorizing.

Theoretical-practical Dialectic: As Simon (1967) and Rousseau (2012) have commented concerning the design of a business school**,** developing theory that is relevant for practice faces many challenges. Such challenges include a knowledge transfer problem (Carlile, 2004; Green, 2004), that theory and practice are distinct forms of knowing (Nonaka 1994, Polanyi, 1962), and a knowledge production problem (Huff, 2000; Van de Ven, 2007). In their theorizing, Williamson, Argyres, and Bercovitz, show a keen awareness of the practical relevance of theory. To enhance the practical relevance of their theorizing, they engage with the world of practice by seeking experience (as Williamson did at the Antitrust Division), understanding, "manufacturing" experience from practitioners (as Argyres does by conducting case studies), and engaging in conversations with practitioners (as Bercovitz does). Bercovitz notes that questions of managerial importance emerge in conversation with practitioners. Argyres applies a litmus test that takes the form of a question: will the outcome of this study yield insight that I would like to tell a manager? [10] This focus on the empirical (pragmatic) implications of theorizing led to the development of theories that offer empirically testable predictions.

Micro-macro Bridging Dialectic: Adam Smith's (1776) metaphor of the invisible hand famously captures the idea that what occurs at a micro level can have macro implications. Williamson's investigation of the Schwinn case can be seen as a need to bridge the micro (Schwinn's actions) to the

[10] In addition to Argyres' test, we recommend our own rooted in a pragmatic approach: will some managers *change their minds* after we tell them our insights?

macro (Antitrust Policy). Williamson found that the "science of choice" approach may be an ill-fitting bridge and developed the "science of contract," i.e., transaction cost economics. Similarly, mindful of the often-hidden connection between micro-level phenomenon (e.g., contracts) and macro-level outcomes (e.g., policy), in her theorizing, Bercovitz seeks discoveries to bridge and link these perspectives. Her goal is to understand the phenomenon and discover the bridge that suffices to explain outcomes. Argyres' (1999) examination of how an information system, directly and indirectly, enabled coordination across organizations by creating a "virtual organization" is another example of bridging micro with macro. In this sense, Williamson, Argyres, and Bercovitz can be seen as discovering anomalies by focusing on the lacuna between micro and macro, and then proceeding to theorize (Kuhn, 1970, 1977). These anomalies can be surfaced by paying attention to mismatches between the phenomenon and theory-guided expectations about the phenomenon (Popper, 1999; Sætre & Van de Ven, 2021). Any mismatch between them offers an opportunity to formulate problems that might lead to a "white space."

Getting-it-right *and* Getting-it-out Dialectic: Williamson recounts that the theoretical construct of opportunism was not evident to him from the outset (in Swedberg, 1990). Instead, its importance became evident over time as he continued his path of inquiry. Yet, that did not deter him from seeking to get his work out, i.e., publish. In other words, Williamson was willing to expose his work to peer-review once he was convinced that he had the story about right in that he understood what was going on. Similarly, Argyres and Bercovitz are also guided by their desire to understand and ensuring they get it right. Unlike Williamson, as their body of work is mostly empirical, these scholars "test" their understanding via empirical contact. If empirical contact indicates that their understanding is adequate, they proceed to "getting it out," i.e., attempting to publish. After all, as both Williamson advisees indicate, publishing is an *outcome* but not a driver – despite publishing pressures, both are exemplars for the strategy field who would not consider getting it out *before* they think they got it right.

3.3 Protocols of Inquiry

We synthesize these five principles and six dialectical conversations to provide five Protocols to help scholars formulate canonical problems aimed at theorizing at the frontier (white space).

Empathetically seek out multiple perspectives: In the course of our inquiry, we found multiple instances of Williamson empathetically seeking out other perspectives to sharpen his theorizing, even to the point of engaging in debates concerning TCE. Not only did Williamson seek out other perspectives to sharpen his theorizing, but he also sought out and integrated insights from entirely unrelated domains. For example, in providing insights on achieving governance safeguards by both giving and receiving credible commitments, Williamson drew on *Ulysses* and *The Prince*. Similarly, Argyres (2011) can be seen as open-minded and seeking multiple perspectives in arguing that insights from organizational economics and organizational learning can be melded to generate theory. Bercovitz explored the evolution of contract structure in university-industry agreements by combining a contracting perspective with behavioral theory logic (specifically learning) (see, e.g., Bercovitz & Tyler, 2014). We maintain that the richness of Williamson-Argyres-Bercovitz' theorizing is partly a consequence of seeking out different perspectives and melding them fruitfully.

Engage in multiple dialectics: Our inquiry suggests that addressing highly complex problems requires scholars (and practitioners) to engage concurrently in multiple dialectics to generate requisite variety (Ashby, 1968; Fox, Simsek, & Heavey, 2022). Indeed, we see evidence of multiple dialectics with Williamson, Argyres, and Bercovitz, actively seeking interlocutors from the domains of academia, law, and business while concurrently channeling internal conversations across their closely held theories and ideologies. Consider Williamson's co-authored work with Ouchi in 1981, which integrated insights from law, economics, and organization theory, and focused on practical implications, and emphasized the feasibility of proposed governance solutions (Williamson & Ouchi, 1981). By adopting this approach of engaging in multiple dialectics and drawing on insights from multiple theoretical and practical perspectives, Williamson enriched his theorizing and ensured that it had widespread managerial and legal implications. Similarly, Argyres' detailed published empirical work in domains ranging from auto firms to personal computers and defense reveals how he weaves together multiple dialectics that managers find useful.

Create a new dialect as needed: Williamson created a new dialect to overcome barriers posed by divergence in language, terms, assumptions, and mechanisms across theoretical frameworks. TCE contains a plethora of terms that were new (e.g., asset specificity, and remediableness), contains assumptions that were not used or combined (e.g., bounded rationality, and opportunism), and suggests mechanisms that explain outcomes in new ways (e.g., designing low-powered incentives in bureaucracies; see Williamson, 1999). Williamson's new dialect was novel and also a continuation of older dialects, which made it easier for his peers to grasp and legitimize the meaning of his theory.[11] Similarly, Argyres has combined insights from capabilities and TCE to explain firm boundaries, and in doing so, has introduced the dialect of dynamism in theorizing. Challenging/introducing a new dialect simultaneously creates concordance across differing perspectives, and can convince peers that a bridge is needed to generate better understanding. Stated differently, the new dialect must illuminate a blind spot (e.g., linking asset specificity and vertical integration). A straightforward test of whether the new dialect is fruitful can be conducted by working out the logic to completion and deriving empirical implications/offering explanations, which differ significantly from current theories.[12]

[11] A challenging requirement for the emergence of a new paradigm is the need for some continuity with respect to the old paradigm it seeks to displace (Kaplan, 1964: 304). Williamson was able to meet this challenge by ensuring some continuity of the old dialectic while creating his own by "borrowing" from the former.

[12] To be clear, we are not in favor of introducing a new dialectic simply for the sake of doing so any more than we are in favor of unnecessarily introducing new "theories" to solve "old" problems. Indeed, we sympathize with scholars expressing concerns about a proliferation of theories (e.g., Ghemawat, 2002), and maintain that it indicates a *poverty of canonical questions*, that is, "white space." Rather than attempting to explore theoretical frontiers and develop progressive research programs that continually generate questions and uncover new unexplored territory (Lakatos, 1970), much of the action is within a limited space well-defined competitive space. Combined with the hankering for "interesting" counterintuitive theories (Davis, 1971) proliferation of theories then, is an expected outcome (Tsang, 2021) and a *symptom*, not a problem.

Explore and exploit tensions: Williamson identified a conflict between the dominant neoclassical view on vertical restraints and the actual motives of practitioners for imposing them (e.g., Schwinn). This conflict, and his subsequent challenge of it via novel problem formulation, led to discovery of "white space," a hitherto unrealized frontier fertile for theorizing. Williamson employed this approach repeatedly, engaging in comparative assessment to explore and exploit tensions, and use them to find weaknesses and discover new frontiers (e.g., power versus efficiency). Similarly, Argyres, dissatisfied with the explanations offered by Williamson's transaction cost economics, continues to explore and expand the frontier of theorizing about capabilities. Recently, Argyres (with Jackson Nickerson) have zeroed in on a critical gap in theorizing in the field of strategic management – the lack of attention on problem formulation.[13] Bercovitz was early to find a gap in supply chain management – relative lack of attention to contracting and governance issues – and realize that insights from TCE could be extended to environmental supply chain management. Indeed, Williamson himself did not explicitly try to apply TCE to supply chain management until 2008, when he published a research article in the *Journal of Supply Chain Management*. The fruitfulness of these approaches is evident – TCE is an empirical success story.

Repeatedly reformulate in search of a canonical question: Formulating the "right" question is an arduous task and, failing to attend to it, can have pernicious outcomes for theorizing and subsequent solution development (Cummings & Nickerson, 2021). In contrast, directing ample attention to the task and formulating the correct question can be very rewarding. For example, after fruitfully leveraging his canonical question (make or buy), Williamson was able to reformulate and apply it more widely (e.g., equity and debt financing; Williamson, 1988). Similarly at the outset of his

[13] SMR is noteworthy for an advisory board of senior executives and consultants who are requesting research on problems related to the usefulness of current strategy tools and phenomena (e.g., the implications of digital strategy, the changing nature of global competition, and shifting investor priorities). Here we suggest the relevance of examining and learning from *disasters* (Dekker, 2016; Hopkins, 2012, 2020; Reason, 1997) for the strategic management field.

Ph.D., Argyres' question was, what makes some *countries* grow and get rich? Over time, Argyres reformulated and arrived at his canonical problem: What makes some *firms* grow and get rich? This question has remained key for Argyres' theorizing (on capabilities). Bercovitz notes that engaging with practitioners can sometimes lead to reformulation as unrecognized factors or questions may emerge or old questions may appear in a new light. Each iteration, i.e., reformulation, holds the potential to lead to a "white space," and formulating the right question is a matter of trial and error that requires perseverance, a learning orientation, an open mind, and creative thinking (Campbell, 1974; Weick, 1989). This process of formulating and reformulating the question is an ongoing process whereby even after arriving at the canonical problem, many variations on a theme can be produced and pursued.

Figure 1 provides a summary of the proposed values, dialectics, and protocols.

3. APPLICATION

According to James, "Any idea upon which we can ride, so to speak; any idea that will carry us prosperously from any one part of our experience to any other part [can be regarded as being] true *instrumentally*" (2017: 22, emphasis in original), that is, valuable. To explore if our proposed framework is valuable, we applied it to a study (written during one of the current authors' doctoral studies) that received mixed reviews and was rejected from a top peer-reviewed journal. The rejected study sought to explain an accident involving an unattended oil-laden runaway freight train that derailed in the town of Lac-Mégantic and resulted in the death of 47 people. Surprisingly, despite considerable efforts, five years after the tragic accident, no root cause was found; that is, the ultimate source of the accident remained partly unexplained. The rejected study's explanation adopted a somewhat technical perspective and drew on theories of accidents (e.g., defense-in-depth) to offer a mid-range theory of slow-moving faultless accidents.[14] However, this approach turned out not to be theoretically fruitful.

[14] The author of the original study worked in the railroad industry, where part of his job was conducting tear-down investigations of failed locomotive engines. This pre-Ph.D. ideology directed attention towards an empirical puzzle but likely introduced a bias towards a narrow formulation of the research question centering

In light of the shortcomings of the previous approach, we decided to apply the framework developed in the current study to examine the fatal train derailment. We read the original paper and then scheduled a 2.5-hour video call to work through and apply the protocol. The call was recorded. A lightly edited transcription of the call is available in Appendix C.[15] We then reflected on the transcript and assessed whether the conversation fulfilled and reflected the principles, dialectics, and protocols. Appendix D reports our assessment of the conversation and provides illustrations of each principle, dialectic, and protocol step reflected in the conversation.

Following the protocol and being mindful of the principles of inquiry, our conversation and its six dialectics led to a reformulation of the canonical question. Whereas the original question was "How can the derailment of an unattended freight train be explained," the reformulated question is "Are catastrophes the inevitable outcome of day-to-day operations of complex socio-technical systems, and, if so, why are regulators unable to mitigate their likelihood of occurrence?" The former question naturally led to a focus on technical reasons or what is sometimes referred to as "point" sources for the catastrophe. In contrast, the reformulated question shifts focus away from examining technical factors that may have contributed to the accident to exploring *systems* (Dekker, 2016; Weinberg, 1975) and *governance* (Williamson, 1985, 1988) perspectives that consider the entirety of the socio-complex system. As the protocol peeled back layers of the onion, sort of speak, we kept asking how our (systems and governance) thinking could be improved.

What we discovered challenges the idea that accidents are "normal" (Perrow, 1984) or occur due to regulatory capture (Dal Bó, 2006) by asking what is going on here and why might these existing theoretical frameworks be wrong. In challenging these theories, we developed a responsive theory that

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on the inability of root-cause identification. In other words, the author may have been trapped in a mental prison of his own making, which he escaped by dialectically engaging with his co-authors (among others).

[15] Upon request, the 2.5-hour video recording is available from the authors.

draws on insights from organizational economics, safety science, and complexity theory, alters the unit of analysis, and creates a new dialectic as needed while maintaining the old ones. While it may not be possible to foretell whether the reformulation of the research question and initial theorizing will be fruitful, early indications appear promising.

Our developed approach shows promise for several reasons. First, the author of the original study describes the reformulation as a conceptualization he had not thought of previously. Thus, at a minimum, the principles, dialectics, and protocols produced something new and potentially valuable. Second, this new theory is superior to extant ones because it explains more high-profile catastrophes of a complex socio-technical system than the two primary prevailing theories. Third, when shared with a group of seasoned emergency managers, the theory received confirmation of face validity, especially when compared to alternative theories. Further, the theory seems to apply to other accidents as well as failures in different domains (e.g., aircraft, finance, information technology, oil platforms). While the theory has not yet faced peer review at the time of the submission of this paper, these early indicators signal that the reformulation and resulting theory development was fruitful. This exercise is only a single application of the principles, dialectics, and protocols. Nonetheless, if our experience is an indicator, then the principles, dialectics, and protocols, developed in this study offer a promising approach for discovering canonical problems and theorizing at the frontier, i.e., "white space."

4. CONCLUSION

Robinson submitted that "[p]rogress in science is won by the application of an informed imagination to a problem of genuine consequence; not by the habitual application of some formulaic mode of inquiry to a set of quasi-problems chosen chiefly because of their compatibility with the adopted method" (2000: 41). The first step to this wise counsel is to formulate a problem of genuine consequence. Inspired by Oliver Williamson, we launched an inquiry into how to increase the likelihood of formulating problems that can lead discovering "white space" and theorizing at the frontier because of our and others' perceptions that doctoral students and faculty alike lack training and exemplars to do so. Consistent with Robinson's counsel, our journey focused on developing the canonical problem and its associated white space.[16]

Our journey revealed how Oliver Williamson and two of his advisees, Nick Argyres and Janet Bercovitz, formulated problems that constitute the foundation of their scientific inquiries. We discovered five principles, six dialectics, and five-part protocols, that guide their problematizing and theorizing. Combining these elements hold the promise of sparking the discovery of canonical problems – Johnson's problems of genuine consequence. Empirically evaluating the usefulness of these discoveries is difficult. As a start, we applied the protocols while being mindful of the five principles to a rejected paper developed by a then Ph.D. student. Our experience in this reformulation activity led to a surprising and promising reformulation, suggesting that principles, dialectics, and protocols add value even for experienced scholars, let alone relatively new ones.

Strategic management scholars have noted a tendency, even disposition, amongst (experienced) scholars, towards engaging in inquiries within existing theoretical and methodological frameworks (Bettis & Blettner, 2020, Chaudhari et al., 2021; Makadok et al., 2018). To be sure, this tendency is beneficial in that it produces rapid (but incremental) progress – existing dominant theoretical frameworks supply a steady stream of questions or "puzzles" to solve (Kuhn, 1970). Current dominant (methodological) approaches (e.g., hypothetico-deductivism, inductive theorizing) and training perpetuate this tendency to engage in incremental theorizing as they often fail to provide the impetus to *challenge* existing theoretical frameworks and discover a "white space" – the problems

[16] The key roles of remediable problem formulation and problem solving, as well as coherent implementation have received considerable attention in the popular (practitioner) press (Berger, 2014; Carroll & Sorensen, 2021; Chevallier, 2016; Rumelt, 2012, 2021). However, theorists theorizing about how to theorize have neglected the vexing problem of problem formulation. To some extent, this neglect stems from the dominant research designs employed in strategic management – hypothetico-deductivism and inductivism. Hypothetico-deductivism takes the hypothesis as given (Hanson, 1960) while inductivism starts with data – both neglect problem formulation.

these approaches seek to address exist within accepted frameworks.[17] Consequently, a danger exists that the growth of real-world problems will outpace the development of new theories as the questions supplied by current frameworks become increasingly stale (Akerlof, 2020; Alvesson & Sandberg, 2013). As this outpacing accelerates, strategic management theories will lose relevance (McGahan, 2007; Teece, 2020). Without a methodology for increasing the likelihood of formulating new canonical problems, strategic management scholars will be ill-equipped to address the grand challenges they are sure to encounter (George, Howard-Grenville, Joshi, & Tihanyi, 2016; Langley, 2021; McGahan, 2022). Attenuating this danger requires bold thinking and breaking free of the "prison" imposed by the "framework of our theories" (Popper, 1970: 56); it involves identifying anomalies that can lead to the formulation of canonical questions and construction of new research programs.

Our framework is not the only approach to formulating canonical problems. Well-known alternative research approaches to theorizing include: (a) collaborative research; (b) action/ intervention research, and (c) design science (Bartunek & Louis, 1996; Lewin, 1951; Simon, 1996). Collaborative research requires a team composed of inside practitioners and outside academic researchers who share activities in a knowledge co-production process (Bartunek & Louis, 1996). Lewin's (1951) pioneering *action/intervention research* program recommends engaging with and intervening in practitioner settings, which can lead to needed experimentation for vexing problems.

These two approaches, while surely beneficial, each provide only one of the six dialectics discovered. Thus, even when research approach runs smoothly, these approaches are comparatively limited in the likelihood of developing a canonical problem because it neither explicitly calls upon other dialects nor specifically offers a set of principles and motivating questions to guide inquiry.

[17] "[N]either deduction nor induction contributes the smallest positive item to the final conclusion of the inquiry. They render the indefinite definite; deduction explicates; induction evaluates; that is all […] every plank of its advance is first laid by [abduction] alone." (Peirce, 6.475).

Simon's *design science* (1996) differs from the prior two because it is concerned with "how things ought to be" more than the explanatory sciences of "how things are." Successful design depends on formulation of the challenge (Rindova & Martins, 2021; Sergeeva, Bhardwaj, & Dimo, 2021). Our proposed framework offers a complementary approach as it provides a protocol to discover and formulate canonical problems.

In addition to these well-known alternate approaches, some scholars have also begun to advocate atheoretical data mining of large datasets for pattern detection, which can then be used to construct a theory that yields robust predictions (Glaser, 2008; Shrestha, He, Puranam, & von Krogh, 2021). With large N datasets increasingly becoming public (e.g., data related to Covid-19), the approach is likely to seem attractive to doctoral students and early career researchers who face time constraints for professional. We maintain that such approaches should be treated with caution as identified patterns may not be managerially relevant. Also, a danger exists of falling into the Feynman trap – such atheoretical data mining increases the likelihood of finding (improbable) meaningless patterns (Smith, 2020), which, given the under-determination of theories by evidence (Laudan, 1990; Quine, 1961), can lead to meaningless theorizing. Such data mining may predict but the resulting theory fails to be bolstered by explaining (Blaug, 1980; Rozeboom, 1997).

The strength of strategic management as a multidisciplinary field resides in its theoretical and methodological pluralism (Mahoney, 1993; Van de Ven, 2007). Therefore, we do not claim that the phenomenological approach of Williamson, Argyres, and Bercovitz, is *the* right way – after all, no rules guarantee scientific riches (Feyerabend, 1993; McCloskey, 1983). Yet, we maintain the "proof of concept" is sufficiently ample to warrant exploring these principles, dialectics, and protocols for the purpose of formulating their canonical problem. Doing so is a potentially fruitful research approach for the engaged strategy scholar seeking to achieve scientific rigor and practical relevance.

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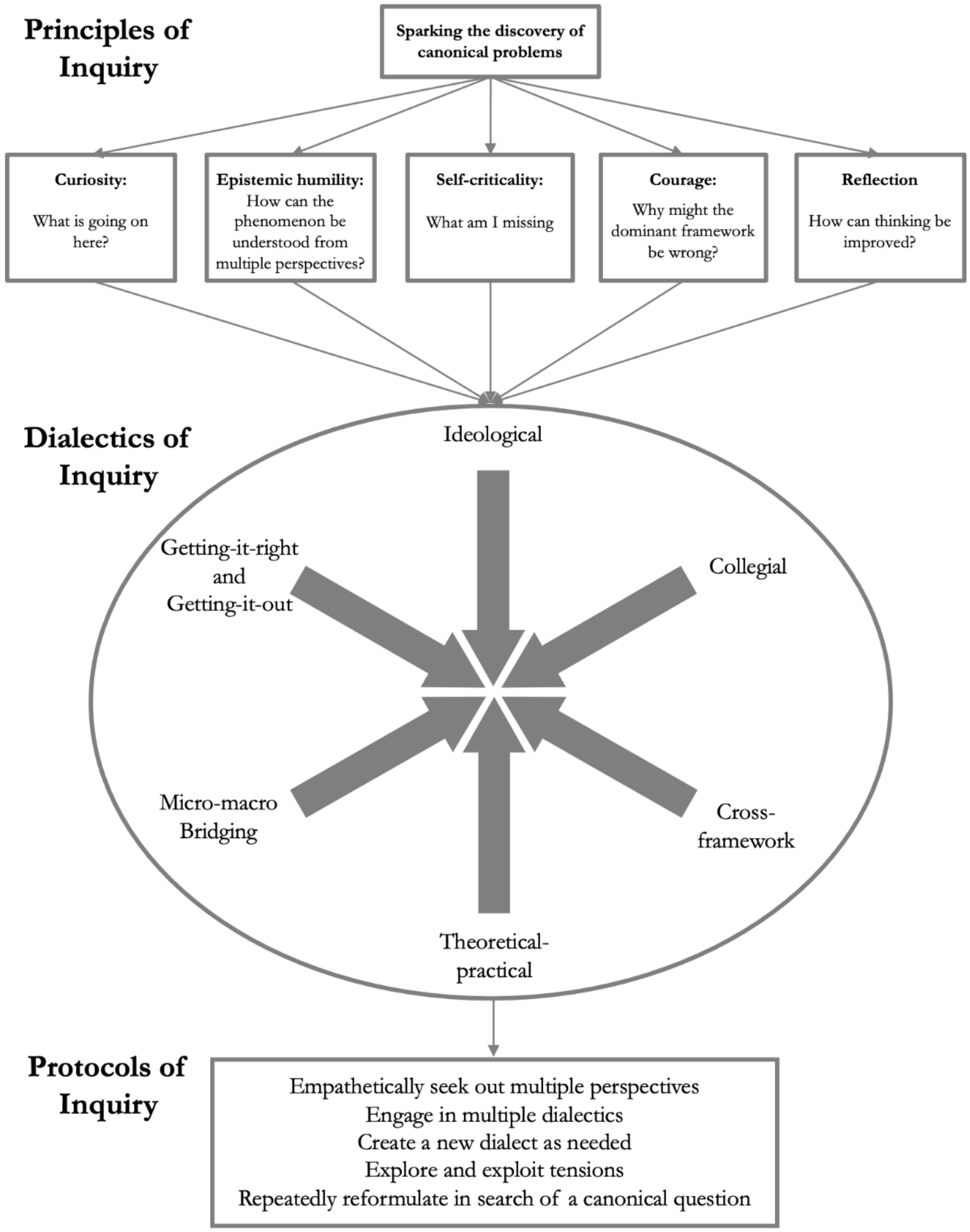
Figure 1: Framework to Guide Inquiries



APPENDIX A

Interview with Nicolas Argyres

One of Nicholas Argyres primary research interests is to understand the origins, antecedents, and consequences of organizational capability differences. One of Nick's intellectual foci is exploiting *the difference between production and exchange* to generate theory. Moreover, Nick identifies this big idea, or what he refers to as "his orientation," as enabling him to find gaps in the strategic management literature. During the interview, Nick described sources of the genesis of his approach to discovering "white space."

He first described that following his Greek father's footsteps; Nick was a Marxist in college and studied with a Marxist historian. Yet, he also studied with neoclassical economists like Harold Demsetz. The philosophical traditions of Marx versus markets created inherent conflicts and tensions that Nick sought to resolve. That said, his early imprinting gave him more affinity toward Marx compared to markets at this early stage in his life.

Nick noted another insight derived from his father, an academic physicist, an insight that was a recurring theme throughout the interview. On the one hand, he recognized that his father believed physics delivered a single right answer, free from ideology. Yet, on the other hand and social science side, ideology mattered in terms of how he saw the world. These differing perspectives were themselves an intellectual tension within his father.

The connection with his dad redounded to an interesting parallel with one of Nick's professors at Berkeley, George Akerlof. Winner of the 2001 Sveriges Riksbank Prize in Economics in Memory of Alfred Nobel, Akerlof was explicit with students about his motivation for becoming a researcher. He was passionate about understanding why his dad was unemployed for so long during the great depression, especially when classical economics predicted that markets adapt quickly to a market clearing price.

In commenting on Akerlof's passion, Nick observed that some bias is needed to develop a hypothesis. Emotion and caring are essential elements of theorizing because one must care about something to develop a hypothesis about it. Marx wanted to deny ideology as a prime mover, and economics is supposed to be devoid of ideology. However, ideology is lurking beneath the surface. Social science is inherently embedded in ideology, which is one source of this bias.

The idea of the importance of experiencing intellectual tension was further illuminated when Nick described the impact of his graduate school education at U.C. Berkeley's department of economics. His initial imprinting in Marxism was a source of tension and inquiry in graduate school. The labor theory of value and its view of capital maintains that not all exchanges are the same, whereas neoclassical theory views labor and capital as substitutable. Nick's Marxist imprinting drew his attention to the idea that something is different between production and exchange. This tension was a source of his dissatisfaction with Williamson's governance approach with its primary focus on efficient exchange. Nick perceived Williamson missed how groups of people in firms create capabilities, especially concerning solving problems.

A pivotal moment occurred when Nick was required to present a paper by David Teece in Williamson's Econ 224 seminar. His presentation crystallized discomfort with the tension between capabilities and governance. He had initially entered the Ph.D. program to focus on development economics in which markets were not functioning, and property rights were not evolving toward efficiency. However, the lack of functioning markets seemed only partially responsible for the lack of economic growth. While Adam Smith and the spread of markets may explain how feudalism transitioned to capitalism, better-functioning markets will only partially explain the rise of industrialization. He claims that such growth depends on new ideas; growth is more about cultural change as people accept new ideas than it is about formal institutions.

TCE did not explain from where technological innovation came. Similarly, Chandler (1977) sidesteps from where technological change came.[18] Teece was an early writer of capabilities, which appealed to Nick's desire to resolve the tension not only between production and transactions but also Marxism and markets.

What makes countries grow and get rich was Nick's interest when entering graduate school. Yet, an easy and natural transition occurred to what makes firms grow and get rich. At the time, this question applied to firms seemed to be much less investigated and more *tabula rasa* than the question for countries, so Nick switched his interest. He formulated a research question as: Why do some firms grow and become rich and other firms do not do so?

Ideological tension, or at least Nick's initial imprinting in Marxism and his exposure to TCE, including Teece's view of capabilities, led to him identifying a *tabula rasa* to develop and extend a theory to resolve the tension. Nick observed that improved understanding could be gained by viewing Alchian and Demsetz' (1972) team theory, and Marx's teachings in seeking to "find a greater truth."

The various tensions and attempts to reconcile these tensions reflect a process of inquiry that Nick had been thinking about for a long time and to which he attributes to his own psychological need to find "reconciliation" and avoiding conflict. An important attribute of Williamson as a dissertation advisor was his open-mindedness and willingness to entertain these ideas that emerged from the conflict and tension.

Beyond his dissertation, Nick remarked that his co-authors often have intellectual imprinting that differs from their graduate degrees and himself. Interacting with co-authors brings new intellectual conflicts and tensions into focus. Janet Bercovitz, a co-author and the subject of the next interview, has an undergraduate imprinting in chemistry. Jackson Nickerson, a co-author and one of the others of this paper, was mentioned as having an engineering degree in systems theory and with whom interactions sparked Nick's increasing attention to more nuanced and precise problem formulation. Nick mentioned that personality and cognitive style might matter. Some people are brilliant at working with data, and others are great at developing theory and big ideas.

Another source of intellectual conflict and tensions can come from reading. Though reading broadly is essential to inform different questions and discover tensions, Nick maintains that he drew limits of which theories to combine. He noted that he "painfully" reached his cognitive limits as he moved to readings on social psychology, which are far afield from his home discipline. One must trade off in being broad versus narrow in knowledge in the sense that the former may lead to more significant insights but at the expense of career advancement as more time and risk are involved.

Occasionally, the same question from different perspectives is answered. More often, Nick likes to force two perspectives to address the same question, try to extend them, and see if they worked together. Therefore, he wrote a paper about capabilities and vertical integration and discovered new

[18] See Crossan and Apaydin (2010) for a systematic review of the innovation literature.

hypotheses. Combining provided perspective and gave new insight. In essence, he relies on an *evolutionary epistemology* of variation, selection, and retention (see Campbell 1974; Weick, 1989).

As a member of the field of strategic management, Nick wanted to provide valuable ideas to managers. In all of Nick's projects, he asks if he has something interesting to tell a manager; otherwise, he might abandon it. His goal is to produce a paper that leads to a practical result. Prediction and the development of managerial implications are important foci. Following in his dissertation advisor's footsteps, Nick likes working with theories in which prediction is the touchstone of science. Historians focus on explanation, not prediction. Some of Nick's research uses history yet also makes predictions.

Nick did not have much experience outside of academia before entering graduate school. He had to "manufacture" experience, so he engaged in many case studies and conversations with lots of businesspeople. These conversations gave him confidence in identifying what business problems are of importance to managers. He was aware of challenges moving from an economics department to a business school, and he had a deep desire to fit in and belong. He also wanted to be a good teacher.

Like Williamson's idea of sequential adaptive decision-making, Nick believes in following his intuition as one thing leads to another. He does not think of himself as following a strategic plan concerning research.

APPENDIX B

Interview with Janet Bercovitz

Janet Bercovitz seeks to understand how value is created through contracting. Janet is known for her research on franchise contracting and university licensing and technology transfer. During the interview, Janet discussed how she came to discover and understand new ideas.

Before attending graduate school, Janet was a chemist. The marketing-driven company where she worked often did not seem to fully comprehend what was going on in its scientific laboratory. She reports that conflicts between marketers and scientists in the lab were common. This conflict sparked her desire to go to graduate school to study and seek ways to resolve the conflict.

In addition to this earlier scientific imprinting from chemistry, graduate school gave Janet another and different imprinting, this time on transaction cost economics. A central aspect of this latter imprinting led her to ask the question constantly, "What is going on here?" Various phenomena caught her attention and interest, which she believes is not always in the mainstream. Janet likes to look at the micro-detail of the phenomena, like contracts, and likes to look at the big picture, such as policy implications. Yet, looking at the micro and the macro is not enough to discover new ideas. This dialectic is only the beginning, as Janet strives to make connections between the two. Doing so illuminates a phenomenon up close as well as from afar, which helps to identify the limits of extant theory and where other or new theory is needed to form these connections.

A lot of research frustrates her because it does not make the connection between micro and macro. Therefore, when exposed to a phenomenon, Janet has two streams of thinking running parallel in her head: one is about understanding a phenomenon, and the other is about what theory is telling her about the phenomenon. Her goals are to understand the phenomenon from multiple perspectives and find a way to bring them all together to get to an outcome that will serve the purpose of creating value for academics and practitioners alike.

Working with co-authors offers another way to develop this understanding and achieve a valuable outcome. The fun of working with co-authors, Janet explains, is that typically they come with disciplines and perspectives that differ from hers. As a result, she and her co-authors spend much time describing and explaining the phenomena from different perspectives, which facilitates dialogue about why each perspective is valuable. Throwing a paper back and forth over the proverbial wall is just not her style. She claims that these conversations open her eyes to many things. Conversation with co-authors makes her question her assumptions, clarifies her thinking and shifts her beliefs.

Typically, a collaboration ends up on a middle road that melds aspects of each co-authors' differing perspective to create a new one. This new perspective also can lead to new questions. Yet the emergence of new questions is not like a discrete function; what often happens is that the questions evolve.

Understanding or making progress on a research topic to discover a new question or empirically address one does not always happen quickly. Janet's second-year Ph.D. paper is a case in point. Written on the idea of governance inseparability, only recently—more than 25 years later—does she now have access to data from franchising that may enable her to evaluate her question empirically.

In essence, ideas are not dropped. Janet keeps questions on the proverbial shelf in the back of her mind and ruminates on them because not figuring them out bothers her. She ruminates on many such opportunities because she feels that research cannot tell people what they already know. Instead, the research needs to provide something new, what Janet calls "bringing it up to the next notch."

To find that notch, she offers the metaphor that research is like Brownian motion. You go in a direction until you hit something that changes your direction. Her father once told her that everybody out there knows something that you do not know. Thus, she keeps moving forward, exposing her thinking to other perspectives to discover insight and reassess ideas on the shelf in light of learning others' perspectives.

Exploring university technology transfer is an illustration of this Brownian motion. Shortly after graduating with a Ph.D., David Mowery, who was on her dissertation committee, invited her to a conference he was hosting with Nate Rosenberg. Maryann Feldman also attended. During the conference, a conversation arose about research on lead universities involved in technology transfer. No research was available on universities that were later starters. David suggested that Janet and Maryann should talk because their two universities had great medical schools yet were both late to technology transfer. Several conversations led to a Mellon Foundation application and the receipt of a substantial grant. The conference invitation was happenstance, as was the conversation on late-start universities and the recommendation for Janet and Maryann to get together. Janet concludes that if you do not go out, engage in the academy, and put yourself in places where interactions occur, you will reduce potential idea flow.

Upon reflection, Janet would advise her younger self to engage more, listen more, and take better notes to help the conversation when sparks arise, which sometimes occur a year later. By "engage more," Janet refers not just to academics but also stakeholders more broadly. For example, understanding what the data mean requires an understanding of the situation. Talking to practitioners and other stakeholders in the domain being investigated is vital to understanding the situation and the meaning of the data. Consequently, her approach leads to relevance being woven into her inquiry from the beginning.

Another aspect of looking from multiple perspectives is that Janet is always anxious that she has not understand a phenomenon correctly. She is especially nervous when going into a new domain because a huge body of work always exists, and she wants to make sure she is offering something new. Collaborating with co-authors already in that area is vital to building the requisite understanding. That said, she states that she never gets to the point where she feels like she fully understands.

Notice that Janet's focus is on her understanding and on bringing something new to the conversation. She does not expect to be the one to discover an overlooked puzzle. Instead, she seeks to understand a phenomenon through a learning orientation. She must have a reason other than to get the publication to engage in inquiry. This perspective comes from her background in science. In science, she believes that one can seek truth; but in social science, which deals with human nature, she does not expect to discover laws of human nature. Therefore, through her inquiry, she is seeking understanding, which is a quest for wisdom.

In general, she does not think of herself as a theorist; although, she believes that she is a good logician. She is constantly reflecting and seeking ways to strengthen her logic, especially by asking what else could explain the phenomenon?

Teaching has been an integral component for her discovery of new questions. While her research career started with franchising, her teaching began in entrepreneurship. Going back and forth between teaching and research in entrepreneurship, she discovered an interest in how start-ups scale up over time. Indeed, this topic is what she is currently exploring and is related to one of her long-term interests, which is the role of dynamics in creating value.

APPENDIX C

Protocol Application Conversation

AB: In 2013, an unattended freight train that was improperly secured and carrying millions of liters of crude oil "ran away," and derailed in the town of Lac-Mégantic, Quebec Canada, resulting in the death of 47 people, fires that decimated the town center, and a mass evacuation involving 2,000 residents. Despite exhaustive investigations, even 5 years after the accident occurred, no one seemed to be able to pinpoint the root cause. And so, for me, the interesting question was: what can possibly explain this accident?

JN: Okay, so give me the nub of your theory.

AB: First, hazards arise from a misalignment between decisions rights and expertise. Second, there was an issue with smallness of size that meant that resources were often unavailable. Third, the system seemed to drift into failure. An added issue was nothing that the regulatory bodies had done would seem to do anything to prevent accidents of this sort in the future. Possibly considerations of bounded rationality are also important. I think these are my key takeaways.

JN: I see the situation entirely differently. Your description of the accident recalls an investigation I conducted when I worked at NASA years ago, and the insights from it can be applied here. Consider, the innovation of reducing from two to one engineer on the train and leaving the train parked on the main line are decentralized decisions that were Okayed by regulators. At NASA, a similar dynamic occurred – maintenance practices were incrementally dropped until, eventually, a failure occurred. Without the original system designer or their equivalent evaluating each of these adaptations the system can lose its safety redundancies.

AB: I think that's certainly a possibility. I would like to share a couple of ideas. First, rules can be captured in routines, but there is an organizational memory loss in that no one understands why it was justified. When those routines are adapted over a long period of time and nothing happens, folks mistakenly assume it is safe to do so – this leads to drift. Second, time is compressed in an emergency situation and people make incorrect inferences – the engine fire at Mégantic was mistaken to be something else altogether.

JN: But those inferences are associated with this decomposed structure. People dealing with each element are specialists who don't have the overall system knowledge. I think this time dimension is really important. I think the mechanisms are less about having to make quick decisions and it's more about … you use the word "drift" and I use the word "innovation"; we also can use the word "adaptation." The reason why I use innovation instead of adaptation (and we can use either one) is because of an incentive component to it. With an innovation I'm saving time or saving money. Some sort of incentive reason is needed to change what you're calling a routine.

AB: What you're suggesting does make sense. The other thing I've always found interesting is James Reason's Defense-in-depth model that says that artifacts have layers of defense. These redundancies, layers of defense in his language, are being removed which ultimately creates path for an accident to occur. Over time, changes in the environment occur. In response, you add or remove layers of defense locally without realizing its broader implications.

JN: I see this situation as one where there isn't a mechanism for the governance to evolve at a system level; instead, what was put in place was a governance system that was decomposed at the subsystem level. So, I see this accident more as a governance issue about long term governance. I don't see the accident so much as an incentive issue; although, incentives played a role. I don't see it, so much the technical issue, although of course technical stuff plays a role. I don't think so much as a routines issue; although, they played a role as routines did change. What's really special about this accident, as you mentioned, is the long-term nature of it. Now we haven't heard from Joe, and I want him to participate. So, let's shift to him.

JM: Alright! I have a whole bunch of questions and comments about each part of the story. Let's go back to the beginning. When you park the train and you leave, how do you know that you have a sufficient number of hand breaks to make sure that it secures the train? Just the fact that it doesn't get done is a real puzzle. And how does the engineer apply hand breaks?

AB: To use handbrakes, you have to first secure the train using air brakes, walk to the back of every car, and apply the handbrake. The handbrake is a big wheel.

JM: And to set the handbrake you turn the wheel.

JN: To secure the train the rule of thumb was to count the number of cars and apply the hand brake to 10% of the cars plus 2. Yet, I suspect that this heuristic is incorrect for heavy-set cars parked on a hill. But it may have been fine if the system was working the way it should have been, which was on a side rail because side rails are unlikely to be on hills.

AB: You're right. I've never seen a sidetrack on a hill.

JN: That's exactly my point. The system likely was designed so that side rails are in safe locations. No one ever contemplated in the original design for trains to be parked on the main track, but it just so happened that this insight may have never been documented because the governance structure was rule based and the rule didn't anticipate decentralized decision making for adaptations or innovations.

JM: My next comment is thinking about the misalignment of decision rights and expertise and just thinking about that in terms of the question earlier about the long-run governance. An element of decentralization is connected to the misallocation of decision rights. You can decompose or don't decompose in a way that puts the people who are in the know with the people making the decisions.

JN: You bring up a great point, Joe. This decentralization of decision rights occurred in a regulatory environment, which in some ways is kind of special here. The regulatory environment allowed the misalignment of decision rights to happen. Why did the regulatory environment support the decentralization of decision rights that allowed these adaptations or innovations? That's curious.

JM: Yes! As you said earlier, this system designer is gone. So, there's no ownership of the system. Let me get back to a few other comments. One: if people don't understand how an incremental change in combination with other people's incremental change is going to affect the system, they will find out in a painful way. The second one is this idea of memory loss. My comment is that over time--I suppose the know-how remains with the routine--the know-why degrades and there's a scarcity of resources in the area of systems knowledge. So, that's part of the story.

JN: Hold on a second, I'd like to probe a little deeper to understand our collective thinking on why does the system knowledge dissipate? If you don't have major failures, do people lose sight of the value for maintaining that knowledge?

JM: Oh, here's my initial comment. It may not be the sophisticated correct answer, but when you ask the question, it makes me think of a scene from Forrest Gump. In the scene the sergeant asks, "What is your sole purpose in this Army?" and Forrest says, "To do whatever you tell me to sergeant." And the sergeant says to him "that has to be the most brilliant answer I've ever heard. You must have an IQ of 160." Then Gump says, "Being in the Army is not really hard. Every time the sergeant speaks you say, 'yes drill sergeant.'" So, if you're the engineering systems guy and I'm a young person just starting out and you have all the experience and you tell me that this is the way it's done, I go "Yes, drill sergeant" and I do it and I get rewarded for it. You take it forward ten years and maybe you've moved on and promoted somewhere else and I'm now in charge. I'm going to follow all the rules because that's my job and I've been successful for ten years following the rules.

JN: So, the designer conveys rules instead of thinking. Is that true in academia with faculty and PhD students?

JM: I don't think so. Maybe it is true for some. I try to transfer everything I can.

AB: I think there is also this notion of not being *able* to transfer knowledge.

JM: Systems designers know more than they can codify easily and tell others.

JN: I don't know if I buy that so much.

AB: Okay, for the second one, I think know-why is linked with justification. In the beginning, when the engineer came up with a rule on contaminants in the engine oil, he or she probably understood why you didn't want that level of contaminants because it will damage the engine. He tells the maintenance guy to make sure it's never exceeded. And then the maintenance guy economizes on bounded rationality and never actually tries to understand the justification and the old person leaves.

JN: Okay, maybe you've convinced me. It could be the loss of tacit knowledge.

JM: We are stunningly unaware of the knowledge that's lost over time, especially when paradigms change, as Kuhn points out.

JN: It may not be a loss of tacit knowledge; it may be a loss of knowledge that comes from one paradigm replacing another and that resonates more with me. Since the origin of the railway in Canada, which could be a paradigm, could a new regulatory regime or some other paradigmatic shocks have triggered a loss of knowledge?

AB: Canada used to have nationalized railroads. The railroads used to be bigger and now you have smaller local players and so that's something which has happened at the industry level.

JN: So, the paradigm shift is that the regulated national railway system fractured into a lot of companies with size dispersion. That could be the reason why knowledge was lost. I'm trying to make a connection between what Joe said in terms of the loss of knowledge and a paradigm shift for which the governance structure isn't designed to compensate and retain the knowledge.

JM: It occurred to me that that part of the formulation of the problem might be to think about the interplay. To us, like Williamson's picture of the interplay between the institutional changes and governance and that the governance may be well adapted under one institutional regime, but then, as the regulatory institutional regime changes, the governance may not adapt in a way that achieves the same functionality, as the original combination of institutional governance arrangements allowed.

AB: The governance structure that existed was operating under different set of rules of the game, and you have a shift in the rules of the game. But I want to continue to play the game, and there's no reason to believe that continuing to play the game in the same way when your rules have changed is going to lead you to good outcomes. And, as the evidence seems to show, it doesn't.

JM: What is really needed for a solution is to deal with the clear governance issues and then with institutional changes, the need to solve problems with maladaptation that can occur in adapting the governance system, but also, it seems to me like a lot of atrophying of capabilities are independent of the incentive system.

AB: Yeah, I think that's the other thing which is important to realize. To try to explain this accident entirely with incentives, I think is too narrow.

JM: Another way of expressing it is through Williamson's work which suggests that there are two problems. There's the opportunism problem and the bounded rationality problem. So, his ideas are that organizations economize on bounded rationality and attenuate opportunism. I think both problems are at play here. It's also related to the deep composition of the problems coping with the complexity of the system. But then as Jackson notes, over time there's a price to pay for that decentralization in the form of lost knowledge and lack of know-why and lack of systems knowledge. Then, if you incrementally change too many things in the system, then there's the failure that occurs.

Now, the one concept that I think needs to be unpacked more is this idea of safety drift, and so I guess I would phrase it a little bit differently than Jackson. I think the safety drift has to do with the time pressure. It has to do with saving of money.

JN: Maybe that's the same thing as the normalization of risk. Are those two things the same thing? [Head shaking] Okay, I just want to make sure that I understand. In that case, I agree with you, Joe; although I think that the coupling is much broader and tighter between the capabilities, the cognitive aspect of it, and governance.

JM: I've written about it consistently over the years, I think I'm much more on the side as Williamson would describe it, rather than the Kogut and Zander way of describing it.

JN: There are a couple of things that I want to throw out just to see if they matter. We haven't talked at all about double loop learning. In what we're talking about, we kind of lose the outer loop from these paradigm shifts for which our governance system hasn't adapted. Is that comment giving us any insight into the phenomenon or explaining it in some way?

JM: One comment I would make on the thesis of double loop learning is that one of the major impediments to it is “don't tell me what I don't want to hear.” When you have an organization where there's not a culture of trust and there's fear, there is no learning. You know academia can get like that, too, by the way.

JN: What I heard you say is that double loop learning may be a topic for the discussion section, but it doesn't provide a mechanism.

JM: When we talk about the impact, I mean, if we come to the conclusion that that we need to be in a continuous learning organization, then we do want to talk about double loop learning, which is needed. But most of the Argyres and Schön book in 1978 on organizational learning is about the impediments of that happening.

AB: There's a book called “pre-accident investigations.” I think it's written by a guy called Conklin. And this whole idea was to learn using counterfactual reasoning. One of the things he identified that impedes it from happening is people getting defensive, not wanting to be told what to do, because they're not interested in changing the way they do things.

JN: Isn’t it a failure of imagination, which leads to underinvestment in safety?

JM: This systemic underinvestment occurs at the university too. People involved in coordinating and making the system work hardly ever get more than an intrinsic reward for preventing glitches from happening. So, all the benefits from non-events don't yield credit or rewards.

JN: Unless these non-events are baked into the governance structure over the long run, which it's likely not to be or it's easy to decay, you don't have an incentive to engage in prevention.

AB: One last point: the thing that also strikes me for this case is that when the engine was failing, it was abnormal. You know straight away something is wrong when oil droplets are falling 25 feet away and on the windscreen of the taxi. It’s a screaming anomaly. Yet, no one paid real attention to the anomaly, so there's failure of foresight or imagination. I haven't seen anyone focus on how organizations can become better at correctly identifying these moments.

JN: Two things are needed for organization to deliver this capability. The first is attention that something is amiss. The second is imagination that simulates what might happen under various scenarios. This simulation is a matter of an individual and cannot be readily routinized.

JM: Let me ask one question about how to position the paper. At the most basic level of the story about this disaster, is it a management problem or a technical problem? And are our current business school curriculums and management teaching the management skills that are needed to avoid such problems?

JN: I think the issue is about governance.

JM: So, I think we're on the same page. I actually think one of the central aspects of management is governance. From a publishing standpoint, we can use the rich case study to generate theory. Yet, we will need to have a few other illustrations to show that the theory has the plausibility of explaining other catastrophes. I think we first have to (re-)conceptualize what the problem is. It'd be great if we can reformulate and come to an agreement on the formulation of the problem. Then we can think about the details of how to write it up.

JN: Joe's offering very good advice on this point. But first, I'd like to see if we can come to an agreement on what the canonical problem is.

AB: How do you prevent system decay…how do you prevent governance decay over time? In a way what we are looking at is really a sort of governance entropy.

JM: Although another way to rephrase the question is, what are the skills needed to have a good architecture of design of an organization? I'm with you, with the idea of maintaining an effective system. But I think an antecedent question is, what exactly are we maintaining?

JN: The system is designed for redundancy. But a shock occurs and somehow the shock leads to a decaying of these redundancies. How can the governance structure renew the resiliency of the system, after some sort of shock?

JN: As we've now explored a bunch of different ways to formulate, and we are zeroing in on a new formulation, maybe we should take a break and digest our conversation. I think that we did concurrently and empathetically seriously engage in four to six dialectics and in an open-minded way. We focused near and far, we talked about what's happening with those frontline workers, but we also thought at a high level of conceptualization. We relied on pre-PhD and post-PhD imprinting. I think all of us did, although I don't know if Joe talked about his pre-PhD imprinting. We had not only a dialectic between two theories but among perhaps six theories. We've talked about the practical as well as the academic and the three of us coming at the context from different perspectives. And we're focused on getting it right yet also we put a time limit around ourselves so that we can get this out and have some sort of practical output. We also recognized different language and tried to define terms and created a concordance. And I think the protocol got us to reveal our conflicting assumptions about knowledge and the difference between management and governance. And we discovered the potential antecedent of paradigmatic shocks that are not just technological, which informs the operating environment connected to second step of the protocol is all about.

The third step is to identify these conflicts and tensions among perspectives, and I think we went through various aspects of governance versus incentives and technology versus organizational and managerial factors. Near decomposability and the notion of administrators versus designers' dimension in which we found tensions and conflicts. And finally, the fourth protocol step sought to formulate and reformulate until a truly novel question emerges, and that it holds the promise for creating new value. We haven't finished this last step. We've been talking for two hours, and we may be cognitively overloaded. We have a bunch of clay, and we have to squish it around to figure out which questions make sense. We also came up with a few title possibilities that should inform the specific language we use to reformulate. That's the final step.

Our conversation seems to be consistent with the values of humility, motivation, courage, and curiosity with a learning mindset. I think we worked through the framework in thorough way. We can write up the conversation to say here's what we started with, here's how we did it, and here's what we ended up with, which is why we must eventually reformulate the problem in a precise way.

Here's your opportunity to make me feel good [Akhil]. Was this a transformational conversation for you?

AB: It certainly was. In fact, I might even grow some hair on my bald head tomorrow! [laughter].

Epilogue: After a few additional email exchanges and video calls, Akhil, Jackson, and Joe unanimously agreed to a revised canonical problem formulation. The initial formulation was:

> *Using the theories of accidents (e.g., defense-in-depth), how could have the Lac-Mégantic accident occurred, and what could be done to mitigate such events?*

This formulation led to a technical perspective that offered a midrange theory of faultless accidents. The theory was motivated and applied to the specific Lac-Mégantic accident.

The new formulation is:

> *Are catastrophes the inevitable outcome of day-to-day operations of complex socio-technical systems, and why are regulators unable to mitigate their likelihood of occurring?*

This new formulation led us to recognize a general phenomenon that we call "fragility drift" in which the safety redundancies found in the operations of complex socio-technical systems are incrementally removed or made less robust.

The reformulation also led to a novel theory that focuses attention on post-arrival (demand or innovation) changes to a complex socio-technical system and political influence over decisions by a regulatory administrator. These decisions enable localized innovations or the acceptance of new routines that, while locally beneficial and do not immediately precipitate accidents, increases system fragility at precisely the time when system safety may need to be enhanced ore redesigned because of the shock. Assuming that politicians cannot credibly keep from attempting to influence regulatory administrators, and assuming the administrators take into account both technical and political recommendations, the probability of fragility drift is expected for all complex socio-technical systems in the aftermath of demand and innovation shocks. While a catastrophe is not specifically predicted to happen under such conditions, our theory predicts that the likelihood of one increases.

APPENDIX D
Applying the framework

	Principles	Dialectic	Protocol
Examples*			
	Curiosity: the question *what's going on here* acted as a driver for AB, JM, and JN as they sought to develop a plausible explanation for catastrophe.	*Ideological:* this dialectic is evident in AB's pre-PhD engineering training and industry experience and post-PhD governance imprinting.	*Empathetically seek out multiple perspectives:* in seeking out differing theories from across paradigms (safety science, economics), AB, JM, and JN examined the problem from multiple perspectives. They also sought to employ different perspectives such as technical and organizational.
	Humility: AB, JM, and JN approached the case through multiple perspectives, such as theories of accidents, governance, and learning. They recognized that a perspective that integrates insights might be useful.	*Collegial:* the dialectic between AB and JN lead to a shift in the approach in that JN offered a more "systems" perspectives, with an explanation centered on innovations stripping the system of critical safety features. JM drew attention to the importance of knowledge and decomposability of systems.	*Engage in multiple dialectics:* AB initially employed the dialect of theories of accidents and safety science while JM and JN employed the dialect of theories of governance. AB also employed the dialect of the railroad industry. They also borrowed from popular literature (e.g., Anti-fragile) to clarify their arguments as needed.
	Motivation: In an email to AB and JM after the conversation, JN conjectured several necessary conditions that may result in similar catastrophes, at the same time admitting that he may be "missing" something, or the conjectures may need revision.	*Paradigmatic:* the dialectic between AB, JN, and JM spanned disciplinary paradigms in that AB drew on theories of accidents (safety science) while JN and JM drew on theories of governance (economics). Mutual understanding was enabled by having some language (governance) that they shared and also an effort to better explain terms (e.g., artifact).	*Create a new dialect as needed:* In formulating the term "fragility drift," AB, JM, and JN created the basis for a new dialect. The dialect of system fragility links to failures of governance in that system fragility seems to be an outcome of failures of governance. The link needs to be explored further, and the logic fleshed out.

	Courage: The dominant paradigm to explain accident is Normal Accident Theory, and theories of safety drift are also frequently applied. JN challenged the latter, while AB, JM, and JN later (not in transcript) have decided to challenge normal accident theory.	*Theoretical Practical:* AB's previous work experience in the industry aided in keeping the theorizing relevant. JN's work experience as an engineer in NASA also played a critical role in promoting practical relevance. JM was able to draw on his organizational experience to highlight the practical challenges of coordination.	*Create (essential) tensions:* during their conversation, it became evident that the explanation developed by AB, JM, and JN would challenge the work on system drift by safety scientists (e.g., Dekker, 2016). In their correspondence following this conversation, AB, JM, and JN have zeroed in on challenging Normal Accident Theory with a perspective and theorizing based on the literature on governance (with insights from theories of accidents).
	Reflection: In an e-mail following this conversation, AB e-mailed JM and JN informing them that his stance of no evidence of opportunism was misplaced. During the conversation, JN was quick to reflect and change his perspective about the possible role of knowledge.	*Micro-macro Bridging:* The dialectic between AB, JM, and JN exhibited the concern of establishing the micro-macro bridge by considering the link between operational decisions taken during day to day working (play of the game) and industry wide regulation (rules of the game).	*Repeatedly reformulate in search of a Canonical question:* AB's reformulated question centering on prevention of system/governance decay was reformulated by JM, and then by JN. The most recent version of the reformulated question is: *Are catastrophes the inevitable outcome of day-to-day operations of complex socio-technical systems, and, if so, why are regulators unable to mitigate their likelihood of occurring?*
		Getting it right and getting it out: AB, JM, and JN discussed a path forward in terms of where the study may fit and how to move towards publication.	

* *see key to abbreviations below*
AB: Akhil Bhardwaj
JM: Joseph Mahoney
JN: Jackson Nickerson