“JUST WHAT IS GOING ON HERE?” AN HOMAGE

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I

INTRODUCTION

Oliver Williamson frequently told his students to ask, “What is going on here?” That question, for him, epitomized the open-ended, pluralist, exploratory research method that he advocated and often contrasted against self-confident versions of academic orthodoxy. It became so closely associated with Williamson’s approach to economic research that it was the title of an obituary written by one of his students.1

Williamson offered his most elaborate framing of “what is going on here”—which I’ll refer to as “Williamson’s charge”—in a 2003 unpublished manuscript, “Pragmatic Methodology and Economic Organization”:

Roy D’Andrade’s discussion of different scientific research traditions (1986) distinguishes between imperative and inquiring research orientations. Whereas the former is characterized by an advanced state of development, is self-confident, and works out of the imperative “This is the law here,” the latter is more tentative, pluralist, and exploratory and poses the question “What is going on here?” Physics is widely regarded as the exemplar of the imperative tradition, but parts of economics also have these aspirations— as witness Solow’s observation that “there is a lot to be said in favor of staring at the piece of reality you are studying and asking, just what is going on here? Economists who are enamored of the physics style seem to bypass that stage”.

Like most dichotomies, this one is overdrawn. Few economists who are enamored of the physics style have no curiosity whatsoever with the phenomena. The readiness, however, to impose preconceptions—rather than to get close to the phenomena by asking and attempting to answer the question “What is going on here?”—is nevertheless widespread, as John McMillan noted in contrasting his research strategy and that of others:

To answer any question about the economy, you need some good theory to organize your thoughts and some facts to ensure that they are on target. You have to look and see how things actually work or do not work. That might seem so trite as not to be worth saying, but assertions about economic matters that are

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based more on preconceptions than on the specifics of the situation are still regrettably common.

The upshot is that those who have an abiding interest in economic organization will combine detailed knowledge of the phenomena, to which the “look and see” contributions of organization theorists are frequently pertinent, with a focused lens.2

This Article explores the roots, context, and legacy of Williamson’s charge. It begins by describing what Williamson called the “Carnegie Connection” and his early years at Carnegie Mellon, which provide the greatest context and most seminal inspirations for Williamson’s charge. In multiple papers, Williamson credited his graduate education at Carnegie for advancing an economics that laid the groundwork for his notable contributions to transaction cost economics and for encouraging an embrace of multiple disciplines to understand social phenomena. It then describes Robert Solow’s 1997 paper, which Williamson frequently credited for coining the phrase, and explores the context of that originating Solow remark. Notably, Solow was not the only of Williamson’s intellectual contemporaries to express the thrust of Williamson’s charge, and the paper explores other instances in which the phrase was used. Finally, the Article aims to explain how Williamson’s signature approach to research, embodied by his charge, influenced his most significant work and the work of his progenies. It closes with some concluding thoughts, both on the influence of asking, “What is going on?” and its role in contemporary scholarship.

II
THE CARNEGIE CONNECTION

Williamson’s first written piece that asked, “What is going on here?” was a brief article in 1990 born out of a conference discussion entitled Different Approaches to the Economics of Institution.3 He then repeated his charge, with slightly varying words and contexts, in articles in 1996,4 2003,5 2007,6 2010,7 and

Williamson’s most significant invocation of his charge might have been in his 1996 article *Transaction Cost Economics and the Carnegie Connection*, in which he reflected on his scholarly values and delved into their origins.\(^8\) The article echoed remarks he made at a September 1993 conference honoring Richard M. Cyert, a professor, dean, and President of Carnegie Mellon University from 1948 through the late 1990s. Its introductory section was entitled *Carnegie in the 1960s*, and it was no accident this discussion about Carnegie, where he spent intellectually formative years, crept into a musing about research methodology. Williamson described Carnegie as “an infectious place” that “bubbled with research excitement, much of it of an interdisciplinary kind.”\(^11\) He frequently credited Carnegie for the interdisciplinary roots of transaction cost economics, which rested on the analytical tools of economics, incorporated behavioral assumptions grounded in psychology, and used sociology and political science to understand group dynamics. He emphasized that effective institutional analysis relied on multiple disciplines, and he was proud to have encouraged the growth of both the study and *The Journal of Law, Economics, & Organization*—emphasis on the “and”—“because, as a student of Carnegie, it could hardly be otherwise.”\(^12\)

Williamson described two features that made Carnegie a unique place to study economics. The first was its ability to marry two distinct paths in economics:

The astonishing thing about Carnegie is that it joined two very fundamental and seemingly incompatible strands of research. One dealt with bounded rationality, organization theory, and behavioral economics. The leading members of that group were Herbert Simon, Richard Cyert, and James March. The second strand dealt with rational expectations and efficient markets. Members of that group include Franco Modigliani (who, unfortunately, left Carnegie just as I arrived), John Muth, Merton Miller, and Allan Meltzer, to be joined later by Robert Lucas (who arrived as I was graduating), Thomas Sargent (who was my first research assistant), and Edward Prescott.\(^13\)

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And the second was its integration of economics with the other social sciences. In a tribute to Dick Cyert, Williamson remarked that the Carnegie leaders fought to “open up the world of economics.”

Thus, while the other social sciences – political science, organization theory, aspects of sociology and social psychology, parts of the law – have regularly availed themselves of economics, economics was always special: it was self-contained; it was the queen of the social sciences; it played hardball. Dick Cyert did not think that any of the social sciences – economics included – were self-contained; and he and others at Carnegie were determined to correct this misconception. . . . Those who witnessed the 1960s will recall that the world of economics was not overjoyed with these intrusions. But a beach-head was established from which further excursions could be launched.

From this dedication to interdisciplinarity—or, to put it more actively, this dedication to requiring different areas of social science to engage with each other—came a certain methodological agnosticism. Williamson said Carnegie was “a permissive place – the test being not methodological rectitude but whether a formulation deepens our understanding of complex issues. . . . [D]iscipline-based property rights were alien to Carnegie.”

And this was the lead-in. Remarking that he learned from Kenneth Arrow that “research problems that do not fit into orthodox boxes should be addressed on their own terms,” Williamson, closed with a paraphrasing of his charge: “As between the question ‘What is going on here?’ and the imperative, ‘This is the law here,’ the enterprise is inspired mainly by the former.” With this pithy summation, Williamson encapsulated the pillars of his approach to research and the foundations of his transaction cost economics.

III

THE SOLOW STATEMENT

Williamson did not credit Robert Solow in his 1996 article on Carnegie because Solow had not yet made the comment, (or at least not that specific comment in writing). Solow, a Nobel Prize winner in 1987, was already known as a gadfly to economic theorists. In 1978, Solow plainly stated that one of price theory’s central assumptions—that markets clear—was nonsense (“It is plain as the nose on my face that the labor market and many markets for produced goods do not clear in any meaningful sense”), and he was particularly critical of

14. Williamson, supra note 4, at 149.
15. Id. at 149–50.
16. Id. at 151.
18. Id. His 1996 article was the basis for an introductory chapter to the third of Williamson’s three cornerstone books, THE MECHANISMS OF GOVERNANCE, in which he made the same point with slightly different language: “rather than being preoccupied with the imperative, ‘This is the law here,’ the enterprise is mainly inspired by the question: ‘What’s going On here?’”. WILLIAMSON, supra note 13, at 25.
19. Lars P. Syll, Solow on post-real Chicago economics, LARS P. SYLL BLOG (Sep. 25, 2016),
prevailing macroeconomic models (“I find that fundamental framework ludicrous”).

Solow and Williamson thus were kindred spirits in criticizing an economics that relied heavily on mathematical models and was short on humility. It is likely the two communicated, either directly or through other media, on the direction of economic methodologies throughout the latter decades of the twentieth century. So, when Solow wrote in 1997: “Just what is going on here? Economists who are enamored of the physics style seem to bypass that stage,”

which Williamson quotes in 2003,

it was possible that it was not the first time Williamson heard him say it.

Interestingly, Solow made his statement in an almost offhanded way at the conclusion of a broader narrative that charted how the field of economics developed as a science. His How Did Economics Get That Way and What Way Did It Get? was a brief essay for a Daedalus symposium entitled American Academic Culture in Transformation: Fifty Years, Four Disciplines.

The symposium’s motivation was, as described by its preface,

to consider the changing nature of American academic culture—a “culture” too rarely acknowledged or investigated—whose character and complexities it behooves us to understand. . . . The world of scholarship is generally oriented towards the production of new disciplinary knowledge. It rarely pauses to take itself as its subject, to examine its own transformations.

Entries included three on economics (Solow’s included), three on literature, and two each on philosophy and political science, with opening and closing comments by five different historians.

Within this context, Solow aimed to describe how economics has changed from the 1940s to the 1990s. For the most part, Solow tells a happy story. He describes how the growth of data availability and computational capabilities allowed economists to transform a mostly descriptive discipline into one that develops and tests hypotheses. However, a primary objective of his essay is to counter those who have criticized economics for descending into “formalism.” Explaining both the nature of the criticism and a brief accounting of his defense, he wrote:

The intended implication is that economics has lost touch with everyday life, that it has become more self-involved and less relevant to social concerns as it became more formal


22. Williamson, supra note 2, at 1.

23. Solow, supra note 21, at 56.

(and more mathematical). I think that this view of the discipline rests on a misconception about the change in the way mainstream economists go about their work. Barking may well be justified, but not up the wrong tree.

Solow then explains that though “the past fifty years has indeed seen formalist economics grow and prosper[, it] has not grown very much.” And he clarifies that “what the outsider sees is really model-building, which is an altogether different sort of activity.” The critical distinction, according to Solow, lies in the research orientation:

If ‘formalist economics’ means anything, it must mean economic theory constructed more or less after the model of Euclid’s geometry. One starts with a few axioms, as close to ‘self-evident’ as they can be, . . . and then tries to work out all the logical implications of those axioms. Formalist economics starts with a small number of assumptions . . . and goes on to study what can then be said about the resulting economic system . . . .

A model is a deliberately simplified representation of a much more complicated situation. . . . The idea is to focus on one or two causal or conditioning factors, exclude everything else, and hope to understand how just these aspects of reality work and interact.

Solow then proceeds to defend the discipline’s dedication to model-building, while also recognizing its shortcomings and its tendency to misinterpretation.

Lastly, in a series of appended thoughts towards the end of his essay, Solow discusses the relationship economics has with the other social sciences. In what sounds like a lament, he remarks, “I am tempted to guess that economics has drawn further away from the other social sciences in the past half-century.” He observes that some economists have imported ideas from social psychology, sociology and political science, often “as a way of escaping from the narrow idea of rationality.” And similarly, some sociologists and political scientists have drawn from economics, especially its use of rationality, as an organizing principle. But these have been peripheral developments, and Solow surmises that:

This failure to connect may arise because the other social sciences have not adopted the model-building philosophy that motivates and guides economists. Experience has taught me that I should say explicitly that I have no neocolonialist designs: sociology may be right to stay away from model-building as a mode of thought. Adjacent territories may adopt different track gauges for good and sufficient reasons, but their railroads will have problems at the border crossings.

And then, finally, comes the moment we—in this article—have been waiting for. In contrast to his hope that economics would engage more with the social sciences, Solow—in what he calls “an uncomfortable task”—offers some

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25. Solow, supra note 21, at 42.
26. Id. at 43.
27. Id.
28. Id. at 42–43.
29. Id. at 54.
30. Id.
31. Id. at 55.
32. Id. at 55.
thoughts “about the analogies between economics and the natural sciences.”

This is where he warns economists who “have the ambition to behave like physicists” of two “dangerous pitfalls”:

The first is the temptation to believe that the laws of economics are like the laws of physics: exactly the same everywhere on earth and at every moment since Hector was a pup. That is certainly true about the behavior of heat and light. But the part of economics that is independent of history and social context is not only small but dull.

[A] second pitfall [is] a tendency to undervalue keen observation and shrewd generalization, virtues that I think are more usually practiced by biologists. . . . I am making a much weaker point, that there is a lot to be said in favor of staring at the piece of reality you are studying and asking, just what is going on here? Economists who are enamored of the physics style seem to bypass that stage, to their disadvantage.

Solow concludes his essay by reiterating his main argument: economics has not—absent some exceptions—become formalistic, it has become technical (“which is quite different”).

This reading of Solow’s 1997 essay—in which Williamson derived his charge—offers two immediate discoveries. First, though Williamson routinely credited Solow for the phrase “just what is going on here,” Solow made the remark in a 1997 publication whereas Williamson began using similar phraseology as early as 1990. Second, the Solow remark was a casual appendage in a paper that was largely inconsistent with the thrust of the comment, or at least Williamson’s affection for the comment. Though Solow was a frequent critic of economics that rested on formal syllogisms rather than common sense and curiosity, his essay mostly defended the field’s transition to technical methods. Nonetheless, both Solow and Williamson exhibited an unusual interest in methodology and occupational self-inquiry, and one can see in Solow’s 1997 essay much that echoed Williamson’s enthusiasm for Carnegie. Perhaps this underlying consonance, more than the specifics of the article, is why Williamson grabbed onto the phrasing in Solow (1997), used it to replace—or sometimes supplement—his original wording, repeated it frequently, and integrated it into his other thoughts about economic methodology.

IV

WILLIAMSON’S CONTEMPORARIES

In urging “what is going on here,” Williamson was not just channeling Solow and Carnegie. He was both echoing and influencing other economists of his generation.

Chief among Williamson’s influences was Ronald Coase, and it is not surprising that Coase had his own version of the Williamson charge. In a short
essay, entitled How Should Economists Choose? — the same essay in which Coase famously said, “if you torture the data enough, nature will always confess” — Coase criticized reductive theoretical thinking. Encouraging scholarship that is both unconstrained by rigid frameworks and receptive to broader curiosities and perspectives, Coase wrote:

[Between] a theory which predicts well but gives us little insight into how the system works and one which gives us this insight but predicts badly, I would choose the latter, and I am inclined to think that most economists would do the same.

This was part of an extensive and not-so-subtle critique of his colleagues in the University of Chicago economics department. Some of his critiques were directed at the very core of economics scholarship. In a 1962 unpublished essay, Coase criticized prevailing economics at Chicago — and Milton Friedman’s economics in particular — for offering “a misleading account of the scientific process.” He further mused that “it is much nearer to the truth to say we test a theory because we believe in it than we believe a theory because we have tested it.”

Stephen Medema, who has pored over Coase’s private archives, says that Coase urged economists to focus on tangible phenomena, not esoteric theory. He wrote:

A theory that is applicable to the real world, then, must have reasonably realistic underlying assumptions to facilitate analysis, to elaborate the causal chains that explain economic activity, and allow one to properly evaluate the potential effects of policy proposals. Absent this, Coase argued, we are left with “blackboard economics,” Coase’s disparaging term for analysis that exists only on the economist’s blackboard and has little actual bearing on the world in which we live. To assume that the competitive markets model provided a reasonable approximation of actual markets was, for Coase, simply the Chicago variant of what he regarded as the profession’s misguided approach to economic reasoning.

Frank Knight — who some would call a founder of the Chicago School and whose students included Milton Friedman, George Stigler, and James Buchanan — offered a similar perspective on both the dangers of theoretical orthodoxy and the benefits of attuning to real-world observations. In a series of

37. Id. at 6.
39. Id.
essays, Knight distanced himself from the methodological approach that emphasizes developing and testing economic hypotheses, an approach that is commonly associated with the Chicago School, and instead argues that economics is little more than common sense: “The serious fact is that the bulk of the really important things that economics has to teach are things that people would see for themselves if they were willing to see.”

Williamson’s methodological preferences counted their ancestors among Chicago royalty, not just its critics.

Williamson’s charge is also evident in the work of one of his methodological compatriots, Paul Joskow, the longtime chair of the economics department at MIT. In a 1991 essay in which Joskow applies transaction cost economics to specific public policy debates, Joskow tellingly invokes Williamson’s charge to illustrate the dangers of overconfidence and the need for scholarly circumspection. In observing that antitrust policy in the 1970s was overzealous in prohibiting conduct that seemed unfamiliar—or ‘nonstandard’—to enforcers (a statement with which the vast majority of economists agree), Joskow wrote:

[Transaction cost economics] teaches us that nonstandard vertical contracts are not inherently mysterious, suspect, and indicative of market power. . . . . . It also provides a framework for understanding why particular types of contractual arrangements have evolved for reasons other than market power. Overall, it can provide a much more productive way of thinking about nonstandard vertical contracts and understanding ‘what is going on here.’

He similarly critiqued formal and inflexible approaches to merger review and again invoked Williamson’s charge. “[A]s part of the general need to understand ‘what is going on here,’ it makes good sense for enforcement agencies routinely to try to understand the underlying efficiency motivation for mergers.”

Mancur Olson, another prominent economist who embraced Williamson’s interdisciplinary and institutional lenses, accepted Williamson’s charge in his 2000 volume, coedited with Satu Kähkönen, A Not-So-Dismal Science. In an introduction to a volume in which Williamson contributed a chapter that offered a version of his charge, “‘What is going on here?’ and ‘Is it remediable?’” Olson argued that economics has migrated from the central city of formal and quantitative study to the suburbs of law, history, political science, and sociology—a migration that caused expansion and dynamism in the suburbs while the downtown declined. Significantly, Olson argues that the interdisciplinary suburbs made economics less dismal by recognizing the empirical realities and mysteries

43. Id. at 66.
that show themselves when markets prove to be far from efficient. With characteristic flair, Olson observes that this necessitates challenging economic dogma—such as recognizing that there are indeed free lunches and “big bills left on the sidewalk.” Finding faults in economic theory is good both for the scholar and for the field.

Some of these variations of Williamson’s charge were published after Williamson started publishing it regularly, and some predated Williamson’s earliest invocation. There certainly is not enough evidence to track the causal spreading of the phrase. But there is enough to indicate that these giants in economics all shared a common methodological fear that the field’s orthodoxy could stifle curiosity and scholarly value, that the field’s myopia would preclude the rich benefits of interdisciplinarity, and the field’s technical complexity would preclude scholarly humility. That these giants held common views and reinforced them through interactive exchanges indicates that Williamson’s charge was both the reflection of a larger intellectual urge and a contributor to a broader scholarly critique.

V

THE CHARGE IN ACTION: TRANSACTION COST ECONOMICS

No one took Williamson’s charge more seriously than Williamson himself. It was reflected heavily in Williamson’s primary scholarly contribution, transaction cost economics, and in turn Williamson routinely referenced transaction cost economics as a product of that particular scholarly approach.

Williamson’s first written piece asking “What is going on here?” was an essay that contrasted transaction cost economics to competing theories of the firm, particularly to property rights and agency theory. Williamson used his charge to distinguish the utility of transaction cost economics over these alternative frameworks:

Transaction cost economics is an interdisciplinary undertaking that joins law, economics, and organization. The approach is unabashedly interested in interpreting organizational variety and unpacking puzzles. Thus whereas physical scientists (and


47. An honorable mention goes to James J. Heckman & Burton Singer, Abducting Economics, 107 AM. ECON. REV. 298 (2017), who encourage scientific research to embrace and learn from “surprising findings,” what they call “abductive economics.” Id. at 298. The authors elaborate: [t]he Abductive approach to empirical economics advocates a process and a mindset. It privileges no source of data, style of research, or mode of inference for learning about the economy provided the analyst produces useful knowledge that survives critical public scrutiny. It values factually-rich descriptions as major sources of knowledge. It favors using every piece of available information, despite varying trustworthiness of parts of it. Id. at 301.

some economists) ask ‘What’s the law here?’, transaction cost economics is preoccupied with ‘What’s going on here?’. It subscribes to the following modest research objective: ‘to organize our necessarily incomplete perceptions about the economy, to see connections that the untutored eye would miss, to tell plausible... causal stories with the help of a few central principles, and to make rough quantitative judgments about the consequences of economic policy and other exogenous events.’

Looking back to Williamson’s 1971 article The Vertical Integration of Production: Market Failure Considerations, perhaps his earliest articulation of transaction cost economics, one can also see an early articulation of Williamson’s charge. The article begins:

The study of vertical integration has presented difficulties at both theoretical and policy levels of analysis. That vertical integration has never enjoyed a secure place in value theory is attributable to the fact that, under conventional assumptions, it is an anomaly: if the costs of operating competitive markets are zero, ‘as is usually assumed in our theoretical analysis’, why integrate?

One can see the charge at work in this seminal article. Williamson does not ask the “why integrate?” question rhetorically. It instead forces economic theory to confront the truth that integration is, in fact, a common reality. In this battle between theory and reality, reality wins in a knockout, and theory must conform.

Transaction cost economics continued along this methodological path. It evolved from a markets-vs-hierarchies dichotomy to a spectrum that included an assortment of complex bilateral arrangements, and its evolution is credited to Williamson’s curiosity and attentiveness to real-world phenomena. Williamson gradually extended transaction cost theory to nonmarket institutions, such as federal bureaucracies, regulatory agencies, and government contracting, and although the enterprise met consistent empirical support, Williamson always remained methodologically humble, as if he would soon encounter unusual phenomena that would require him to adjust his theoretical priors.

This same theoretical reticence and intellectual humility also informed his time as a government regulator. As an economist in the U.S. Department of Justice’s Antitrust Division, Williamson encountered policy that was hostile to vertical integration—which he and others named “the inhospitality tradition.” Williamson, with transaction cost economics still percolating in his head, recognized that certain vertical mergers can achieve certain efficiencies and thus should be treated with less suspicion. At the same time, Williamson also resisted the Chicago School’s categorical embrace of vertical restraints and thus carved out a middle-ground approach that balanced an open mind and a demand for further study.

Decades later, after Williamson’s third book expounding transaction cost

49. Id. at 65 (footnote omitted).
economics and his framework thoroughly formulated, we can see the same scholarly values reflected in the complete version. Transaction cost economics remains both theoretically robust but circumspect. It has been shown to explain a broad array of phenomena yet remains highly contextualized. It explains some phenomena better than others—its theory of the firm seems to perform better in static rather than technologically dynamic markets—but Williamson always instructed his students and readers to recognize its limits in addition to its strengths. It is a theory that vividly mirrors its architect, who in turn remained faithful to his core intellectual roots and academic principles.

VI
THE CHARGE’S LEGACY: RESEARCH BY WILLIAMSON’S PROGENY

It should come as no surprise that Williamson’s charge deeply influenced my own research. I was barely two pages into the preface of my book on the diamond industry, STATELESS COMMERCE, when I quoted Williamson’s charge and its origination from Solow. I added:

This book begins with asking what is going on, and then proceeds to ask why it is going on. I have spent a lot of time, in Solow’s words, staring at this piece of reality, and hopefully I offer both an accurate description of that reality and some lessons that accompany it.52

The charge is not just quoted verbatim in the book; its substance pervades the entire study. As I examined the commercial behavior of diamond merchants, I found unusual features that resisted conventional wealth-maximizing models: low-income brokers were in possession of million-dollar diamond caches but never presented a risk of flight; the trade association’s headquarters featured an array of social events that often distracted from commerce; the rhetoric surrounding diamond sales were sharp reflections of certain community values while were sharp violations of others. The merchant community defied off-the-shelf models, and this is precisely why Williamson encouraged its examination. I ultimately harnessed an approximation of a club-good model that recognized preferences for psychological constructs such as community and inclusion, but the underlying lesson was quintessential Williamson: theory must be informed by real-world phenomena, and if observed behavior presents surprises, then those surprises offer more intellectual rewards than predictable conventions.53

Building on my own experience, I explored whether the charge affected other Williamson students.54 My search of the literature suggests that Williamson’s

53. See Heckman & Singer, supra note 47 (defining “extreme instances”).
54. In pursuing this investigation, I might have committed one of Coase’s sins, in that I was testing a theory only because I believed it. See supra note 39. Or maybe I was taking Williamson’s charge to heart by pursuing a curiosity and simply investigating whether scholars explicitly embraced both the language and spirit of his charge, assuming that deeper lessons will follow.
charge appears to have lived on,\textsuperscript{55} though admittedly not always stated as explicitly as I did in my book’s preamble. One prominent example is work by Jackson Nickerson and coauthors that pursue what they call “problem formulation,” which serves as the exercise that underlies the development of research questions.\textsuperscript{56} In an elaboration of Williamson’s charge, they develop a framework that rests on curiosity, epistemic humility, self-criticality, courage, and reflection.\textsuperscript{57} They aptly call this an “Oliver Williamson inspired approach” and encourage scholars to employ problem formulation to explore and pursue theoretical frontiers.\textsuperscript{58}

Two former Williamson students, Nick Argyres and Joanne Oxley, channel a form of Williamson’s charge in rethinking economic relationships that were at the center of Williamson’s theories. In \textit{Evidence of the Role of Firm Capabilities in Vertical Integration Decisions}, Nick Argyres observes that contrary to Williamson’s transaction cost logic, different firms pursue vertical integration decisions in different ways.\textsuperscript{59} Argyres examines an assortment of manufacturing sectors and suggests that firm capabilities—qualities that are internal and specific to individual firms—affect how these firms engage in similar economic environments.\textsuperscript{60} This conclusion appears modest, but it offers a potent constraint to transaction cost economics. Williamson’s directive to remain actively curious

\textsuperscript{55} So does Solow’s, though Solow’s comment is more strictly interpreted as a need to depart from mathematical rigidity and theoretical insularity. \textit{See, e.g.}, Meir Kohn, \textit{Value and Exchange}, 24 CATO J. 303, 305 (2004) (citing Solow, supra note 21) (arguing that “the devotion to mathematics and the adherence to the value paradigm have not been without cost. Mathematization has promoted a kind of sterile armchair theorizing. Many theorists see little need to be acquainted with the details of real-world economies: almost exclusively, they study each others’ models.”); W. G. Runciman, \textit{Culture Does Evolve}, 44 HIST. & THEORY 1, 4 (2005) (citing Solow, supra note 21) (warning against reflexively using evolutionary and game-theoretic models to understand cultural dissemination).

\textsuperscript{56} Akhil Bhardwaj et al., \textit{Problem Formulation for the Theorizing at the Frontier: An Oliver Williamson Inspired Approach}, STRATEGIC MGMT. REV. (forthcoming 2022).

\textsuperscript{57} Id. at 6. Bhardwaj, et al., connect Williamson’s dual emphasis on curiosity and humility with other famous methodologists. They note that Karl Popper observed that “social sciences always start from problems, from the fact that something inspires amazement in us.” \textit{Id.} at 1 (citing KARL POPPER, \textit{ALL LIFE IS PROBLEM SOLVING} 3 (1999)). They also highlight that psychologist and philosopher David Robinson wrote “[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.” \textit{Id.} at 19 (citing David N. Robinson, \textit{Paradigms and the Myth of Framework}: \textit{How Science Progresses}, 10 THEORY & PSYCH. 39, 41 (2000)).

\textsuperscript{58} A similar effort is advocated in Gelman & Imbens, “Why ask Why? Forward Causal Inference and Reverse Causal Questions,” NBER Working Paper No. 19614, available at: https://www.nber.org/papers/w19614 [https://perma.cc/JTL4-9XS7]. In addition to sporting a title that invokes the spirit of Williamson, this paper suggests that economics should deemphasize the careful measurement of exogenous effects and instead should pay more attention to the mechanisms that drive those targeted effects. The search for causal mechanisms, the authors lament, are too often dismissed as “cocktail party chatter;” instead they encourage asking “What is going on?” suggesting that such questions drive the scientific effort.


\textsuperscript{60} Id. at 148.
and intellectually flexible led to a limitation—or a qualification and an extension—to his own theory. In a similar vein, Joanne Oxley, with coauthor Fabrice Lumineau, argues for a rethinking of the colloquial view of dispute resolution.61 They find that litigation and the end of a trading relationship are not predicted by the introduction of lawyers.62 Rather, these costly dispute resolution outcomes are better predicted by features of the disputed transaction and the shadow of future relationships.63 Contrary to the view that lawyers either escalate disputes or precipitate legal actions, they find instead that lawyers are directed by the same economic factors identified in transaction cost economics.64

Other Williamson students, similar to my approach in STATELESS COMMERCE, examined peculiarities in specific industries and, inspired by Williamson’s charge, asked basic questions that led to meaningful scholarly insights. Scott Masten examined the famous antitrust lawsuit against United Shoe that successfully argued that the company only leased, rather than sold, its sophisticated machinery to sustain an illegal monopoly.65 Masten and coauthor Edward Snyder observed that United Shoe only leased its machinery to some shoemakers and then did so through a variety of complex contracts that varied in duration, price, and warranties.66 After delving into the specifics of the industry, noting that “shoe machines were remarkably complex and heterogeneous instruments, costly to develop and prone to failure,” they concluded that United Shoe’s conduct was economically benign and was designed to promote “the supply of quality machines and the generation and dissemination nonpatentable innovations and know-how without the need for comprehensive contracting.”67 What policymakers thought was harmful conduct was actually pro-competitive, suggesting that antitrust law should delve into industry specifics before using generalisms to condemn behavior.

Williamson students Brian Silverman, Jackson Nickerson, and John de

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62. Id.
63. Id.
64. Id. at 821.
66. Id. at 56.
67. Id. at 35. The authors further note that

[the efficient operation of a shoe factory required a large number of these complex devices and an assortment of services that included guidance on the selection and configuration of shoe machines, training in their operation, timely and effective repairs, and the dissemination of technical advice on shoes and shoe manufacturing generally. The complexity of shoe machines and of related services would have left conventional contractual arrangements hopelessly incomplete and susceptible to conflicts. By making the proceeds from machinery transactions contingent on the retention and use of machines, leasing promoted the supply of quality machines and the generation and dissemination of nonpatentable innovations and know-how without the need for comprehensive contracting.

Id.
Figueiredo take a similar approach in focusing on peculiar and unexpected elements in particular industries to derive broader theoretical insights. In an examination of the U.S. trucking industry, Nickerson and Silverman remark that while theory would suggest widespread user ownership of transport trucks, most drivers instead work for large trucking companies. They use this insight to discover that the industry is characterized by significant agency and transaction costs that instead prevent self-employment. And in an investigation of government contracts, de Figueiredo and Silverman show how seemingly byzantine and convoluted government contracts are actually efforts to reduce transaction costs through the limited devices available to public agencies. Understanding the mechanics of government contracting thus constrains hypothesized reform efforts and should inform political critiques.

Sometimes, Williamson’s charge was invoked explicitly. David Teece, both a student and later colleague of Williamson, used the charge to describe how Williamson’s approach both embraced reality while encouraging the pursuit of theoretical concepts:

Oliver was also always adamant that any proposed theory of the firm must be able to scale up from ‘toy model’ status—the penchant of contemporary academic economists—to approximate the phenomenon of interest. He would constantly ask ‘what is going on here?’ to force his students to come to grips first with reality, and only then with theory.

Williamson’s students would agree with Teece that the charge to ask what is going on had a lasting effect on their research orientations and scholarly curiosity—fittingly, Williamson’s Nobel Prize autobiography included, “Teaching can be learning, especially if student curiosity with the question, ‘What’s going on here?’ can be elicited.” It was central to both Williamson’s economic framework and his pedagogy. That said, relatively few academic papers make specific reference to his charge.

The greatest enthusiast of Williamson’s charge is probably Mikko Ketokivi, who authored two pieces that embraced the phrase. Ketokivi’s *Transaction Cost Economics as a Constructive Stakeholder Theory*, coauthored with Joseph Mahoney, invokes Williamson’s charge three separate times: first, posing the paper’s overarching question of why transaction cost economics was not being

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69. Id.
71. Id.
incorporated effectively into managerial scholarship; second, illustrating how transaction cost economics offers a better framework for complex economic structures than the Supreme Court’s confused antitrust rulings; and finally, quoting Williamson’s Nobel Prize speech. The article is a reflection of Williamson’s charge in both word and spirit.

The second piece is a tribute named What Is Going on Here? that Ketokivi wrote following Williamson’s death. The tribute included an explanation for its title:

I cannot think of a better title than What is Going On Here? to my homage to Oliver Williamson. This deceptively simple question, which kept Williamson busy throughout his career, refers to the idea of being curious about the world and looking for ways to understand it on its own terms, not through conventional formulations or overly simplifying assumptions.\(^7^4\)

VII

CONCLUSION

Tellingly, Williamson’s charge also appears in other tributes written for him after his death. Ranjan Ghosh and Yugank Goyal, writing in Mumbai’s Economic and Political Weekly, close their homage with a reference to the Carnegie Connection and the methodological influence it had both on Williamson and on the field. They emphasize Williamson’s methodological convictions because, they argue, his Nobel Prize award served as a propitious rescue of the field of economics:

What was so timely about this annual ritual? It was also the year of the great economic downturn which saw big banks and hedge funds fail, and a time when the faith in model wielding, invincible economists was shaken. Paul Krugman, the 2008 prize winner, had recently remarked in New York Times that ‘. . .the economics profession went astray because economists as a group, mistook beauty, clad in impressive-looking mathematics, for truth.’\(^7^5\)

As this essay suggests, Williamson clearly was not alone. In addition to his co-recipient Elinor Ostrom, he was joined by other scholars—his predecessors at Carnegie, contemporaries like Solow, Coase, Joskow, Olson, and an army of imitators and students—who adhered interdisciplinary curiosity and resisted methodological orthodoxy. And though it is probably hyperbole to say that this team of innovative scholars saved economics, it is safe to assert that the field is much better because of them.

\(^7^4\) Ketokivi, supra note 1, at 492.