

## **Transaction-Cost Economics: Past, Present, and Future?**

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### **Abstract**

Oliver Williamson is the founder and chief developer of transaction-cost economics (TCE). In this brief essay, on the occasion of his Nobel Memorial Prize, I offer a partial discussion of Williamson's contributions by first summarizing some of the accomplishments of TCE-past and then sketching some of the opportunities for TCE-future. Most of the topics in both sections (TCE-past and TCE-future) start with a quotation from Williamson's early work that I think still speaks volumes today. That is, while fellow travelers and successors have acted on one set of Williamson's insights, helping to produce the accomplishments of TCE-past, another collection of Williamson's insights has gone relatively unremarked, creating some of the opportunities for TCE-future.

Keywords: Oliver Williamson, Nobel, transaction costs, boundary of the firm  
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# **Transaction-Cost Economics: Past, Present, and Future?**

by Robert Gibbons

## **1. Introduction**

In 2009, Oliver Williamson shared the Nobel Memorial Prize in Economics Sciences “for his analysis of economic governance, especially the boundaries of the firm.” More generally, Williamson is the founder and chief developer of transaction-cost economics (TCE), with five books, seven edited volumes, and more than 170 papers on TCE and related topics. In this brief essay, therefore, I cannot offer more than a partial discussion of Williamson’s contributions (emphasizing that “partial” can mean both incomplete and biased). In particular, the first part of the essay summarizes some of the substantial accomplishments of TCE-past, and the second part sketches some of the intriguing opportunities for TCE-future. To conclude, I then briefly describe TCE-present as at a crossroads: celebrating its past and charting its future.

Both of the essay’s parts, on TCE-past and TCE-future, are broken into a handful of topics. For example, one of the topics in the accomplishments of TCE-past concerns the boundary of the firm, and one of the topics in the opportunities for TCE-future concerns internal organization. But almost every topic in both parts, whether accomplishment or opportunity, starts the same way: with a quotation from Williamson’s early work that I think still speaks volumes today. The difference I see between TCE-past and TCE-future, then, is not that Williamson launched the former but ignored the latter; to the contrary, Williamson can be seen as trying to launch both. Instead, the difference I see is that fellow travelers and successors have acted on one handful of Williamson’s insights, producing the accomplishments of TCE-past, whereas another handful of Williamson’s insights has gone relatively unremarked, creating some of the opportunities for TCE-future.

Before diving in, I must issue an overarching caveat and apology: space constraints all but preclude discussion of contributions besides Williamson’s. Thus, this essay is more a celebration of an individual than a literature review. In particular, in Section 2 on TCE-past, I

hope to clarify why the dedication of Williamson (1985) counts Coase and Simon as “teachers,” but I barely mention Williamson’s fellow travelers and successors in TCE. Similarly, in Section 3 on TCE-future, I give only hints about recent developments in organizational economics that complement rather than directly build on TCE. For each section, a full discussion will require another outlet.

## **2. Some Accomplishments of TCE-Past**

Both 2009 laureates, Williamson and Elinor Ostrom, are cited for their analyses of “economic governance,” but what does this mean? Dixit (2009: 5-6) defines the term as “the structure and functioning of the legal and social institutions that support economic activity and economic transactions by protecting property rights, enforcing contracts, and taking collective action to provide physical and organizational infrastructure. ... Good economic governance thus underpins the whole Smithian process whereby individuals specialize in different tasks and then transact with one another to achieve the full economic potential of the society.”

The 2009 Nobel Memorial Prize was a special moment for the study of economic governance, and there have been others (such as the earlier prizes to Buchanan, Coase, and North), because the institutions of economic governance explicitly affect or implicitly underlie huge parts of both economic activity and economics literature. We need institutions not only to support simple exchanges in markets, but also to conduct both non-market transactions (in firms, communities, governments, and elsewhere) and complex transactions in markets (via contracts and other governance structures that are not simple exchanges). When considering settings beyond simple exchange, including those analyzed by Ostrom and by Williamson, one sees why Buchanan (1975: 229) argued that “economics comes closer to being a ‘science of contract’ than a ‘science of choice’.”

To summarize the accomplishments of TCE-past, I focus on three topics: (1) methodology for studying particular governance structures and comparing them to others; (2) assumptions about the nature of certain important economic transactions and the devices available for governing them; and (3) applications, mostly to the boundary of the firm, broadly construed. Some of these accomplishments may now have become taken for granted,

so that not everyone recognizes that there is an accomplishment there, not to mention who produced it.<sup>1</sup>

As primary sources for several of these accomplishments of TCE-past (and several of the opportunities for TCE-future discussed below), I draw heavily on Williamson's 1971, 1973, and 1979 papers, as well as on chapters 4 and 5 from his 1975 book. With the benefit of thirty years of hindsight (as well as the enormous body of Williamson's subsequent writing noted above and the huge empirical literature on TCE discussed below), I have come to see these key writings from the 1970s as expressing the core theoretical ideas of TCE.<sup>2</sup> As Hahn (1961: 204) said about Debreu's *Theory of Value*, I find the returns to re-reading these key Williamson writings to be "very high indeed and probably increasing."

## 2.1 Methodology

Even before Williamson's prize cited his analysis of "the boundaries of the firm," his best-known contribution concerned the make-or-buy problem: which upstream inputs should a downstream business purchase and which should it manufacture? (Or, as Coase (1937: 393-4) put it, "Why does the entrepreneur not organize one less transaction or one more?")

To analyze the make-or-buy problem, one might focus on the firm's production function. After all, production functions are a standard topic in microeconomics courses, from introductory to advanced. As Demsetz (1983: 377) notes, however, "It is a mistake to confuse the [neoclassical] firm ... with its real-world namesake." In particular, Williamson (1971: 112) argues that production functions are not the way to analyze the make-or-buy problem.

"[T]he substitution of internal organization for market exchange is attractive less on account of technological economies associated with production but

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<sup>1</sup> This notion of being taken for granted parallels a story about my MIT colleague Barbara Liskov, an Institute Professor (the highest rank at MIT), who won the Turing Award in 2008 for her work in the 1970s. In response to the award, someone (much younger?) apparently wrote, "What did she get this award for? Everyone knows this, anyway." <http://web.mit.edu/newsoffice/2009/liskov-event.html>

<sup>2</sup> Two enormous omissions in what follows are that (1) I focus on what Williamson (1985: 24) calls the "governance" branch of TCE rather than the "measurement" branch, thus entirely omitting important papers such as Alchian and Demsetz (1972), and (2) I focus almost entirely on Williamson's arguments (both who made them and who used them), thus only barely mentioning Klein, Crawford, and Alchian (1978) in Sections 2.3 and 3.4. See Baker and Gil (2010) for more on Klein et. al.'s role in launching active discussion of the central case study in this field, GM-Fisher Body.

because of what may be referred to broadly as ‘transactional failures’ in the operation of markets for intermediate goods.”

The question, then, is how to analyze the sources and remedies of “transactional failures.” In this section, I discuss two aspects of the methodology that Williamson proposed and practiced: (a) microanalysis of transaction detail and (b) comparative institutional analysis.

*(a) Microanalysis of transaction detail*

Williamson’s approach to analyzing transactional failures is “microanalytic” (1976: 74), meaning that it

“examine[s] the contracting process in [great] detail ... to discern the types of difficulties which market mediated exchange encounters and, relatedly, to establish in what respects and why internal ... organization offers an advantage.”

In this microanalytic focus on how governance structures reshape parties’ incentives and opportunities, Williamson reflects ideas from his doctoral training at Carnegie Mellon, such as March (1962: 662) on “the business firm as a political coalition” and Cyert and March (1963 [1992]: 202) on “the organization as a decision process” (which we revisit in Section 3.3).

This microanalytic approach contrasts in two respects with the production-function approach. First, a production function is a reduced form, abstracting from transaction detail. But, as the press release for Coase’s Nobel Prize stated, endorsing the spirit of microanalysis, “the power and precision of analysis may be enhanced if it is carried out in terms of rights to use goods and factors of production instead of the goods and factors themselves.”<sup>3</sup> Second, the standard optimization problems involving production functions are single-person problems, whereas studying transaction detail typically reveals contending interests among the parties to a transaction; the microanalytic approach explores the parties’ political and strategic actions as a game rather than a single-person problem. As a result of these two differences, TCE can have a lot to say about production (see Section 3.2 for a start) but little reference to production functions.

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<sup>3</sup> [http://nobelprize.org/nobel\\_prizes/economics/laureates/1991/press.html](http://nobelprize.org/nobel_prizes/economics/laureates/1991/press.html)

Before leaving production functions, however, I find it interesting to note that, whereas Kreps (1990) treated organizational issues not only in Chapter 7 on “The Neoclassical Firm” but also in Chapter 19 on “Theories of the firm” and Chapter 20 on “Transaction cost economics and the firm,” Mas Colell, Whinston, and Green (1995) included only a chapter on “Production” and placed it in their Part I—on *individual* decision-making. Given the literature in 1995, I can understand the choices that Mas Colell et al. made, but I hope it will someday be taken for granted that (a) the external boundaries and internal structures and processes of organizations should receive at least as much attention as production functions in leading micro texts and (b) these aspects of organizations should be analyzed from a microanalytic perspective (i.e., as reshaping the incentives and opportunities of the parties to the transaction in question). In short, I await textbooks that embody Cyert and March’s (1963 [1992]: 30) view of organizations: “People (i.e., individuals) have goals; collectivities of people do not.”

*(b) Comparative Institutional Analysis*

As a methodological point, Williamson (1973: 316) has long argued and practiced that

“the problems of efficient economic organization need to be examined in a comparative-institutional way.”

For example, Coase (1937) and Williamson (1971) analyzed the make-or-buy problem as the efficient choice from a discrete set of alternative governance mechanisms (namely, integration and non-integration). Furthermore, such comparative institutional analyses can be conducted not just of the boundary of the firm but also of its internal organization and other topics, as in the following three early examples. First, Simon (1951) and Williamson, Wachter, and Harris (1975) compared an “employment” relationship (where the boss can choose the worker’s task) to various contractual relationships (e.g., where the task is agreed in advance). Second, Coase (1960) and Williamson (1976) compared regulation to relevant alternatives (integration for Coase and franchise bidding for Williamson). Finally, Chandler (1962) and Williamson (1981) compared functional organizations (where each function reports directly to headquarters) to multi-divisional organizations (where each division contains each function and each division reports to headquarters). In each of these early

studies, Williamson's microanalytic approach considerably deepened the institutional comparisons initiated by his predecessors.

Simon (1978: 6) provided an early articulation of the motivation and methods of the comparative-institutional approach, noting that “[a]s economics expands beyond its core of price theory ..., we observe in it ... [a] shift from a highly quantitative analysis, in which equilibration at the margin plays a central role, to a more qualitative analysis in which discrete structural alternatives are compared.” Today, large literatures (not just TCE) have conducted comparative institutional analyses of a wide range of topics. For example, there are analyses of the optimal extent of horizontal integration (not just vertical), the optimal control structures inside organizations (not just functional and divisional forms), the optimal contracts and other governance structures between firms (such as joint ventures), and the optimal governance structures for the political economy (such as different constitutional and legislative structures).<sup>4</sup> But the point here is to identify early sources of this comparative-institutional approach, not to catalogue its many subsequent applications.

## 2.2 Assumptions (about Difficult Transactions)

In order to conduct microanalysis of alternative governance institutions, Williamson needed to reconceptualize the environment in which much important economic activity occurs. In this section, I focus on three environmental conditions that collectively can create “transactional failures” (whether “in the operation of markets for intermediate goods” or elsewhere): (a) unprogrammed adaptation because ex ante contracts are incomplete, (b) lock-in arising from the “fundamental transformation” (1985: 61), and (c) haggling (i.e., inefficient bargaining) because ex post contracts are incomplete.<sup>5</sup>

The following section begins by focusing on vertical integration in markets for intermediate goods, explaining that this combination of unprogrammed adaptation, lock-in,

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<sup>4</sup> See, for example, Ménard's (2004) *International Library of the New Institutional Economics*.

<sup>5</sup> Williamson (1973, 1975, 1985) emphasizes not only environmental conditions but also individual ones, especially bounded rationality and opportunism. I proceed slightly differently but to the same basic effect. Replacing bounded rationality, I assume that contracts can be incomplete even if the parties are rational. And endorsing opportunism, I assume that individuals are opportunistic in all economic models, even if in some settings, such as general-equilibrium models, such actors have so few opportunities that they “do not buy more than they can pay for, ... do not embezzle funds, ... [and] do not rob banks” (Diamond, 1971: 31). In short, opportunistic actors in constrained environments behave well, but our interest is in other environments.

and haggling creates Williamson's earliest rationale for integration. The remainder of the section then discusses other settings, beyond intermediate goods, where these three environmental conditions again have important effects.

*(a) Unprogrammed Adaptation Because Ex Ante Contracts Are Incomplete*

In the 1960s, many general-equilibrium models assumed the existence of a complete set of state-contingent claims, as introduced by Arrow (1953) and Debreu (1959). In contrast, Williamson (1971: 113) was then considering "unprogrammed adaptations" resulting from ex ante contracts that are incomplete.

"[O]nly when the need to make unprogrammed adaptations is introduced does the market versus internal organization issue become engaging."

Simon's (1951) model has a similar spirit: in an "employment relationship," the boss chooses the worker's task after the state of the world has been realized, and Simon compared this governance structure for adaptation to a "sales" contract where the task is agreed before the state is realized (in which case there is no adaptation). The key point is that Simon did not allow the parties to write a contingent contract (prescribing the task as a function of the state) before the state is realized; that is, he ruled out what might be called programmed adaptation. In endorsing Simon's incomplete-contract approach to adaptation (for studying the make-or-buy problem), Williamson can again be seen as reflecting his Carnegie roots, but it is worth noting what a departure this endorsement was from Arrow and Debreu's complete-contract approach that was so prominent throughout the 1960s.

The possibility of unprogrammed adaptation is important because different governance structures can have different influences on the parties' response to the need for such adaptation. As Williamson (2000: 605) later summarized three decades of work, TCE

"holds that maladaptation in the contract execution interval is the principal source of inefficiency."

For example, one way that such maladaptation can arise is from privately optimal but collectively inefficient haggling. Whatever the sources and sizes of such maladaptations, the core TCE idea is that the efficient governance structure minimizes the resulting inefficiency.

*(b) Lock-In Arising from the "Fundamental Transformation"*

If there are many equivalent upstream or downstream parties, then competition seems likely to produce an efficient outcome, even if *ex ante* contracts are incomplete. Thus, a second ingredient in Williamson's rationale for integration is lock-in via "bilateral monopoly" (1971: 115) or "small numbers" (1973: 318).

Williamson's contribution on this point is not to focus on settings that have small numbers from the beginning, but rather to explain why large numbers in the beginning often become small numbers over time, through specific investments, learning by doing, and so on.

"Although a large-numbers exchange condition obtains at the outset, it is transformed during contract execution into a small-numbers exchange ..."  
(1975: 29).

Thus, for whatever reason, it often becomes more efficient for the parties to continue to deal with each other rather than change partners.

Although specific investments have played a large role in the TCE literature as a leading source of such lock-in, it is important to consider the possibility of lock-in even in the absence of specific investments. In this spirit, Masten, Meehan, and Snyder (1991: 9) describe "temporal specificity" as follows. "Where timely performance is critical, delay becomes a potentially effective strategy for extracting price concessions. ... Even though the skills and assets necessary to perform the task may be fairly common, the difficulty of identifying and arranging to have an alternative supplier in place on short notice introduces the prospect of strategic holdups." This issue of alternative sources of lock-in, beyond specific investments, reappears below.

*(c) Haggling Arising from Incomplete Ex Post Contracts*

In 1971, Williamson had access to the Nash Bargaining Solution, which assumes efficient bargaining rather than inefficient haggling, but not to Tullock's (1980) model of collectively inefficient rent-seeking behaviors or to models of inefficient bargaining under asymmetric information such as Myerson and Satterthwaite (1983). On the other hand, as Williamson (1975: 73) notes (citing Schelling (1960) and (1971) here and elsewhere in the book), "To observe that the pursuit of perceived individual interests can sometimes lead to defective collective outcomes is scarcely novel."

Whatever his thoughts about the sources of inefficient haggling, Williamson (1971: 115) clearly envisioned inefficient bargaining, asserting that

“[a]lthough this haggling is jointly (and socially) unproductive, it constitutes a source of private pecuniary gain.”

As we will discuss in Section 3, the literature is still debating how to formalize the idea of haggling in a way that produces a formal theory of vertical integration. Nonetheless, as we will see in Section 2.3, even without a formalization of inefficient haggling, Williamson’s rationale for vertical integration inspired a great deal of empirical work.

### *2.3 Applications*

For environments that satisfy the three assumptions in Section 2.2, Williamson could perform microanalytic comparisons of alternative governance institutions, beginning with the make-or-buy problem. This section summarizes some of the ensuing applications, in four parts, largely concerning the boundary of the firm: (a) TCE’s theory of vertical integration; (b) TCE evidence on vertical integration; (c) TCE theory and evidence on contracts between firms; and (d) further applications.

#### *(a) TCE’s Theory of Vertical Integration*

As suggested above, the combination of unprogrammed adaptation, lock-in, and haggling create Williamson’s earliest rationale for integration. To avoid inefficient haggling under non-integration, it may be more efficient to concentrate control, producing decision-making by fiat. As Williamson (1971: 114) argued,

“fiat is frequently a more efficient way to settle minor conflicts ... than is haggling or litigation.”

A more detailed argument appears in chapters 4 and 5 of his 1975 book. Specifically, Chapter 4 argues that the need for unprogrammed adaptation implies that many *labor* transactions are more efficiently conducted in a firm (under something like Simon’s “employment relationship”) instead of in a market (under something like Simon’s “sales

contract”).<sup>6</sup> Having made labor transactions the centerpiece of Chapter 4, Williamson then makes an explicitly parallel case for intermediate products in Chapter 5: “The argument here really parallels that of Chapter 4 in most essential respects” (1975: 99).

In short, Williamson’s earliest rationale for vertical integration concerned choosing a governance structure to minimize the inefficiency of unprogrammed adaptations. While Simon shared this focus on unprogrammed adaptation, there are three important advances in Williamson’s argument. First, whereas Simon analyzed a setting with only two parties, Williamson endogenized the small-numbers context via the fundamental transformation. Second, Simon analyzed a setting with only one decision (the worker’s task), so Williamson’s interests in non-integration and its associated haggling could not arise. And third, in a bold analogy, Williamson argued that issues akin to those in employment also arise for intermediate goods.

For both theoretical and empirical purposes below, let me reiterate that, although this rationale for vertical integration does require lock-in from small numbers, it does not require specific investments to be the source of that lock-in. (In particular, Simon says nothing whatsoever about investments.) To put this differently, one could say that Williamson’s earliest rationale for integration requires specificity but not specific investments. I make this point not because the 1975 argument was incomplete, but rather the reverse. Given the strong emphasis on specific investments in later work, a completely coherent argument in the 1975 book (based on specificity without specific investments) largely got lost.<sup>7</sup>

Increased emphasis on specific investments occurred in Klein, Crawford, and Alchian (1978: 297-8), who analyzed “post-contractual opportunistic behavior” (i.e., haggling) over “appropriable specialized quasi rents ... [created by] a specific investment,” and in

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<sup>6</sup> Williamson (1975: 71-72) critiques both Simon’s (1951) sales contract and his employment relationship, the former for ignoring contingent claims and sequential spot contracting and the latter because it is ill suited for large-scale adaptations (e.g., where the boss’s and worker’s interests diverge substantially). In my view, however, these critiques under-appreciate further contributions in Simon’s paper: importantly, Simon also considered an alternative governance structure (namely, letting the worker decide, p. 304) and the role of repeated interactions in moving a decision-maker’s decisions away from short-run self-interest and towards Pareto-efficiency (p. 302).

<sup>7</sup> As one way to assess the increased emphasis on specific investments, compare the indexes of the 1975 and 1985 books. In 1975, the words “asset” and “investment” and “specific” do not appear, but “small-numbers exchange condition” appears ten times; whereas in 1985, “small numbers” does not appear, but phrases related to “asset specificity” or “transaction-specific” assets and the like now appear 77 times.

Williamson's 1979 paper, where he focused on "economic activity that involves transaction-specific investments in human and physical capital" (p. 234). Having thus identified a leading cause of lock-in, Williamson (1979: 252-3) then returned to the rationale for vertical integration sketched above.

"The choice of organizing mode then turns on which mode has superior adaptive properties. ... The advantage of vertical integration is that adaptations can be made ... without the need to consult, complete, or revise interfirm agreements."

In sum, by 1979, Williamson had made the argument that specific investments cause small numbers and, under non-integration, unprogrammed adaptations invite inefficient haggling. A large empirical body of TCE literature on vertical integration followed, to which we turn next.

*(b) TCE Evidence on Vertical Integration*

So far, I have spent this entire essay discussing theory. On one hand, this seems appropriate, because Williamson's main direct contributions have been theoretical. On the other hand, this seems odd because the huge body of TCE literature is overwhelmingly empirical. To bridge this disjuncture, I now consider Williamson's largest indirect contribution: operationalizing TCE so that empiricists sought to test it. In particular, I focus in this subsection on the empirical TCE literature on vertical integration and in the next subsection on the theoretical and empirical TCE literature on contracting.

For empirical work on both of these core TCE issues--vertical integration and contracting--the key step was the 1979 *JLE* paper, which began by asserting that

"[f]urther progress in the study of transaction costs awaits the identification of the critical dimensions with respect to which transaction costs differ" (p. 234)

and then nominated uncertainty, investment idiosyncrasy, and frequency as the critical dimensions of transactions. Relative to the three ingredients in the rationale for integration described above, Williamson argues that uncertainty creates unprogrammed adaptations and that investment idiosyncrasy creates lock-in. Haggling, the third ingredient in the rationale

for integration, is assumed to be omnipresent, or at least very widespread.<sup>8</sup> Finally, the third transaction dimension nominated in the 1979 paper, frequency with which the transaction recurs, is beyond the scope of this section's focus on vertical integration and contracting.<sup>9</sup>

Among the first empirical papers testing TCE's predictions concerning vertical integration were Monteverde and Teece (1982) and Masten (1984). Monteverde and Teece studied 133 automobile components used by Ford and General Motors and found that components requiring greater engineering development effort are more likely to be produced in-house. Engineering development effort can be interpreted as specific human capital, creating lock-in; and the other two ingredients in Williamson's rationale for integration--unprogrammed adaptations and haggling--are assumed to be present. For his part, Masten (1984) analyzed a large aerospace project, constructing measures of specificity and complexity for each input and finding that the combination of these two measures is especially important in explaining which inputs are produced in-house. Thus, Masten's analysis not only attempts to measure two of the three ingredients in Williamson's rationale for integration but also recognizes that all three ingredients must be present for the rationale to apply (and, like Monteverde-Teece and successors, assumes that haggling is present).

Many other (perhaps 100!) empirical TCE papers on vertical integration followed. Because this space-constrained essay focuses on Williamson's contributions, I cannot survey these ensuing papers and so will resort instead to surveying the subsequent surveys. Shelanski and Klein (1995) and Macher and Richman (2008) focus on the breadth and quantity of empirical work broadly in keeping with TCE insights, not limited to vertical integration. Lafontaine and Slade (2007) and Bresnahan and Levin (2010) focus on vertical integration, consider theories beyond TCE, and pay increased attention to measurement and econometric issues. David and Han (2004) and Carter and Hodgson (2006) offer more critical

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<sup>8</sup> More specifically, Williamson assumes that opportunism is widespread (p. 234) and creates haggling if parties are given the chance to haggle (such as under non-integration). Given later models of inefficient bargaining such as Tullock (1980) or Myerson and Satterthwaite (1983), I would restate this assumption as opportunism and ex post contracts that are incomplete create haggling if the parties are given the chance.

<sup>9</sup> Williamson (1979) argues that frequency provides a boundary condition separating private ordering (where only the parties themselves are involved in governance), which is efficient for high-frequency transactions, from trilateral governance (where a third party such as an arbitrator is involved in governance), which is efficient for low-frequency transactions. This section's focus on private ordering can thus be interpreted as assuming high-frequency transactions.

assessments of the power and interpretation of TCE tests concerning vertical integration and contracting.

(c) *TCE Theory and Evidence on Contracts*

Although Williamson and TCE may be best known among economists for analyzing the make-or-buy problem, there is a sense in which the main issue in TCE is actually contracting, where integration is a special case. More specifically, Williamson's early work (e.g., the 1971 and 1973 papers and the 1975 book) focused on markets versus hierarchies, where the vision of non-integration emphasized the hazards more than the advantages of contracting between firms. But the title of the 1979 *JLE* paper is "Transaction-cost economics: The governance of *contractual* relations" (emphasis added), and the preface of the 1985 book argues that

"any issue that either arises as or can be recast as a problem of contracting is usefully examined in transaction cost terms" (p. xii).

Moving from titles and prefaces to analysis, the two main governance structures studied in the 1979 paper (pp. 250-3) are bilateral governance (i.e., contracting under non-integration) and unified governance (i.e., contracting under integration); and the 1983 paper focused solely on "the use of bilateral governance structures (private ordering) to implement nonstandard contracts where the adaptation and continuity needs of the parties are especially great" (p. 537). In short, by the early 1980s, contracts had become at least as central to TCE as integration.

As Joskow (1985) emphasizes, the first question to be asked about contracts between firms is not which contract is chosen but rather why the parties are contracting at all. Furthermore, in choosing whether to contract, the parties are comparing this option to two alternatives: arm's-length transacting on one hand, and integration on the other. It would be good to see more explicit treatment of both these alternatives in future theoretical and empirical work. See Masten and Saussier (2000) and Lafontaine and Slade (2010) for surveys on the empirical side.

Having decided to contract, the parties then need to decide on terms, such as contract duration, pricing and price adjustment, vertical restraints, and so on. TCE empirical work on

contracting often studies how contract terms depend on key drivers from the TCE theory, such as unprogrammed adaptation and haggling (or, as they are sometimes proxied, uncertainty and appropriation hazards). For example, see Joskow (1987) on duration and Masten and Crocker (1985) on the role of take-or-pay clauses in adaptation. As with the TCE empirical work on vertical integration, the TCE empirical literature on contracting is sufficiently large that I again resort to surveying the surveys: in addition to Masten-Saussier and Lafontaine-Slade, which are specifically about contracts, see also the contracts sections of the aforementioned Shelanski-Klein and Macher-Richman empirical TCE surveys.

Empirical surveys necessarily omit theory, and it is interesting to note that recent theory by Bajari and Tadelis (2001) on the effect of complexity on contract design is very much in the spirit of TCE's informal arguments about contracts. As we discuss in Section 3.4, building on the Bajari-Tadelis approach is a promising method for trying to formalize TCE's arguments about integration.

Finally, there is one last body of empirical literature (again with many papers—perhaps 140 when combined with the contracts papers above), not exactly on integration and not exactly on contracting, but relevant nonetheless. This is the literature on “hybrid” governance structures, the simplest of which is perhaps a joint venture (e.g., Pisano, 1989), but others of which involve many parties, many assets, and many contracts (e.g., Ménard, 1996). See Ménard (2010) for a survey dedicated to hybrids, as well as the hybrids sections of Shelanski-Klein and Macher-Richman.

*(d) Further Applications*

Williamson has long argued that the study of organization should combine law, economics, and organization theory. Indeed, he argues (1985: 2-7) that one can discern these three sources as far back as the 1930s in Llewellyn (1931), Coase (1937), and Barnard (1938), respectively. Furthermore, in addition to the comparatively abstract arguments about vertical integration and contracting in the 1971, 1973, and 1979 papers, Williamson also performed more applied analyses on topics relating law and economics, such as antitrust (1968) and regulation (1976), and edited a book of contributions (including one of his own) on Chester Barnard and organization theory (1990).

In keeping with this breadth in Williamson's interests and background, among his most cited publications are articles in sociology, law, and organization journals (1981, 1984, and 1991, respectively). More importantly, and to go beyond simple citations, perhaps the most striking fact documented in the Macher-Richman survey is that TCE has inspired research on a wide variety of subjects in a wide variety of fields. These include areas within economics (such as industrial organization, law and economics, and regulation), management disciplines (such as marketing, strategy, and international business), and other social sciences (such as political science and sociology). This broad set of applications (and others not mentioned here) extends well beyond the boundary of the firm, even if integration, contracts, and hybrids may be the original core of TCE theory and evidence. We return to the interplay between Williamson and sociologists below.

### **3. Some Opportunities for TCE-Future**

Having spent most of this essay on the accomplishments of TCE-past, I now briefly discuss some opportunities for TCE-future. Just as space constraints limited the discussion of Williamson's fellow travelers and successors in TCE, they also allow only a mere mention of recent developments in organizational economics that complement rather than directly build on TCE.

I focus on four opportunities for TCE-future: (1) costs of integration, (2) identification, (3) internal organization, and (4) formal models. As with many of the accomplishments of TCE-past, several of these possible topics for TCE-future are consistent with observations Williamson made in the 1970s.

#### *3.1 Costs of Integration*

Williamson articulated the main point of this subsection long ago:

“A complete treatment of vertical integration requires that the limits as well as the powers of internal organization be assessed.” (1971: 113)

Unfortunately, a more recent assessment suggests only partial progress on this agenda.

“The main benefits of vertical integration ... are discerned by examining the problems that attend autonomous contracting when the parties to a trade are operating in a bilateral exchange relation. The main costs of vertical integration are more difficult to discover, however.” (1985: 153)

Williamson (1985) discussed several possible costs of integration, including “accounting contrivances” (p. 138), transfer prices that are determined “unilaterally” (p. 139), and “the strategic propensity to use the resources of the organization to pursue subgoals” (p. 149). Many of these “costs of bureaucracy” (p. 148) might be summarized as abuse of fiat (i.e., opportunistic use of concentrated control). The corporate governance literature (e.g., Shleifer and Vishny (1997), Core, Holthausen, and Larcker (1999), and Gompers, Ishii, and Metrick (2003)) takes abuse of fiat very seriously, so it is easy to imagine opportunistic use of concentrated control under integration. One concern, however, is that the factors that make haggling very inefficient under non-integration seem likely to be correlated with those that make abuse of fiat very inefficient under integration. Indeed, Williamson suggested the following early intuition.

“Substantially the same factors that are ultimately responsible for market failures also explain failures of internal organization” (1973: 316).

Unfortunately, if one is to preserve the original interpretation of the large body of empirical TCE literature on vertical integration, it is difficult to argue that abuse of fiat is a chief cost of integration. That is, the standard and simplest interpretation of this empirical literature is that the costs of integration are in the error term and orthogonal to the regressors that proxy for costs of non-integration, but this orthogonality assumption is violated if the factors that make haggling very inefficient under non-integration are correlated with those that make abuse of fiat very inefficient under integration. As Masten, Meehan, and Snyder (1991: 1) put it, “recognition that variations in internal organization costs may also play a role in the decision to integrate exposes an inherent weakness in ... [the existing] tests.”

In short, TCE theory does not provide as clear an explanation for *variations* in the costs of integration as it does for the costs of non-integration. Without a theory of variations in the costs of integration, it is difficult to know which factors responsible for market failures are correlated with (or even identical to) factors responsible for organizational failure. One could simply ignore these issues and assume that the costs of integration are orthogonal to the proxies for the costs of non-integration. Furthermore, one could choose to study the effect on

vertical integration of the costs of non-integration rather than the costs of integration. Indeed, from Monteverde-Teece and Masten onward, essentially all TCE empirical papers on vertical integration have asked whether proxies for unprogrammed adaptation and lock-in make integration more likely, with very few asking instead whether factors that increase the costs of integration make non-integration more likely. For example, an attempt to separate papers cited in Shelanski-Klein, Macher-Richman, and Lafontaine and Slade (2007) on this basis suggests that fewer than five percent of the papers are in the latter category.

As I suggest in Section 3.4, we are moving towards theories in which “substantially the same factors that are ultimately responsible for market failures also explain failures of internal organization.” As such theories mature, it will be important both to derive their implications for the interpretation of estimated coefficients in the traditional TCE regressions on vertical integration and to discover new estimation strategies that help us understand not only the benefits but also the costs of integration. See Masten, Meehan, and Snyder for probably the most serious attempt to address these issues thus far.

### *3.2 Identification*

Besides the econometric issues associated with the possible failure of orthogonality between the costs of non-integration and the costs of integration, there are various other econometric issues that deserve attention. I begin by collecting four long-standing but still important observations. First, it would be helpful to measure inefficient haggling directly, rather than use asset specificity as a proxy for the possibility of haggling. As Winter (1988: 172) notes, “progress has been achieved not by the development of techniques for measuring transaction costs directly but by the development of operationalizing hypotheses to suggest where transactional difficulties are likely to be severe.” Second (and related), in addition to the incidence of vertical integration, it would be helpful to know more about its effects. As Joskow (1991: 81-82) put it, “we need more than an ordinal ranking of the efficiency of different organizational arrangements. We would like to know how much we lose by going from the best to the next best.” Third, Williamson’s rationale for vertical integration requires all three ingredients (unprogrammed adaptation, lock-in, and haggling), so the appropriate regression estimates the effect of the interaction of these ingredients, not their individual

effects (unless, of course, one or more of these ingredients can be presumed to be present throughout the sample). As Lafontaine and Slade (2007: 660) note, “[g]iven the importance of the interaction between specificity and contractual incompleteness in the theory, it is surprising that it is rarely tested directly.” Finally, as Kogut and Zander (1992: 394) observed, the evidence in Monteverde-Teece—one of the most famous empirical papers supporting TCE—is at least as strong for a “capabilities” view of the firm.<sup>10</sup>

In addition to these long-standing observations, there is also a general trend in applied microeconometrics that is important to acknowledge and adopt: in a wide range of fields including labor, public finance, development, and beyond, there is an increased emphasis on quasi-experiments and causal inference, such as Angrist (1990) and Card (1990) in labor. While it is natural that empirical work from the 1980s (in any field) did not adopt this quasi-experiment approach, and while there are some papers on vertical integration and contracting that exploit plausibly exogenous variation in the costs of alternative governance structures (such as Forbes and Lederman (2009) on integration of regional airlines with majors), it remains the case that empirical work in TCE (and in organizational economics more generally) could stand to pay more attention to identification strategies.

### *3.3 Internal Organization*

Interestingly, even though Williamson is best known and celebrated for his work concerning the boundary of the firm (on both integration and contracts), from the beginning he showed an equal interest in internal organization. For example, the 1973 paper is equally divided between analyses of (a) the integration decision and (b) alternative structures for internal organization. Likewise, each of the 1975 and 1985 books has about the same number of chapters devoted to internal organization as to the boundary of the firm. Among the internal-organization topics are the employment relationship, the organization of work, peer groups, hierarchy, multi-divisional firms, and corporate governance. Finally, while some

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<sup>10</sup> More specifically, while Monteverde and Teece is widely cited for its finding that automobile components requiring greater engineering development effort are more likely to be produced in-house, the t-statistic on the firm dummy variable is higher than those on any other independent variables, including engineering effort. See Winter (1988) and Langlois and Foss (1999) on the capabilities view, including its emphasis on production rather than exchange, heterogeneity rather than inefficiency, and path-dependence rather than stationarity. Interestingly, these issues are (finally) surfacing in organizational economics; see Gibbons (2010, Section 4) for an overview; Gibbons and Henderson (2010) for a survey, and Chassang (2010) for an initial model.

readers recall the full title of the 1975 book as *Markets and Hierarchies: Analysis and Antitrust Implications*, only a few seem to know that the subtitle (visible only on the title page) is *A Study in the Economics of Internal Organization*.

For some reason, Williamson's ideas about internal organization did not inspire empirical follow-up the way his ideas about integration and contracting did. Certainly in the early going and even to this day, TCE empirical work tilts very significantly towards studies of the boundary of the firm, with little attention to internal organization. (This point is related but not identical to the earlier observation that TCE empirical studies of vertical integration predominantly explore the costs of non-integration rather than the costs of integration.) For example, an attempt to separate the papers cited in Shelanski-Klein and Macher-Richman by their focus on the boundary of the firm versus internal organization suggests that only a small minority of the papers are in the latter category (although there are papers on multinationals, and it is not clear how to categorize some papers on hybrids).<sup>11</sup>

Even though Williamson's ideas about internal organization have not received much empirical attention, he did point in some interesting directions. For example,

“it is opportunism in conjunction with both a small numbers and an information impactedness condition that accounts for the transactional disabilities that internal organization experiences. ... Internal opportunism takes the form of subgoal pursuit—where by subgoal pursuit is meant an effort to manipulate the system to promote the individual and collective interests of the affected managers. Such efforts generally involve distorting communications in a strategic manner. ... The upshot of this is that distortion-free internal exchange is a fiction and is not to be regarded as the relevant organizational alternative in circumstances where market exchange predictably experiences nontrivial frictions” (1975: 124-5)

These ideas clearly relate to the discussion in Section 3.1, on costs of integration. Indeed, the “subgoal pursuit” Williamson envisions within firms may be related to the “haggling” he envisions between firms, so it may well be possible to develop a unified theory in which “substantially the same factors” are responsible for the costs of both integration and non-

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<sup>11</sup> One might wonder whether empirical work on internal organization is simply harder to produce than empirical work on the boundary of the firm. This could be true, but papers like Baker (1992), Baker, Gibbs, and Holmstrom (1994), Baron, Burton, and Hannan (1996), Bertrand, Mehta, and Mullainathan (2002), Bloom and Van Reenen (2007) Henderson and Cockburn (1996), Ichino and Maggi (2000), Ichniowski, Shaw, and Prennushi (1997), Lazear (2000), and Mas and Moretti (2009) demonstrate many different styles of such work; see Baker and Gil (2010) and Ichniowski and Shaw (2010) for more.

integration. In this subsection, however, my focus is not on the costs of integration or the make-or-buy problem but rather on developing a theory of internal organization (under the assumption, for the moment, that the parameters are such that integration performs better than non-integration).

Given this focus, I have three reactions to Williamson's ideas on internal organization. First, these ideas are quite different from the prominent approach to the economics of internal organization in the early 1970s: Marschak and Radner's (1972) team theory, which ignored incentive conflicts so as to focus on information gathering, communication, and decision-making. Second, as suggested in Section 2.1, this microanalytic approach to internal organization echoes the theory and evidence of the Carnegie School's political view of organizations (summarized above as "the business firm as a political coalition" and "the organization as a decision process"). More specifically, Cyert and March (1963 [1992]) argued that "[w]here different parts of the organization have responsibility for different pieces of information ..., [we would expect] some attempts to manipulate information as a device for manipulating the decision" (p. 79). Indeed, Cyert and March went further, anticipating game-theoretic arguments such as Crawford and Sobel (1982) by noting that "we cannot reasonably introduce the concept of communication bias without introducing its obvious corollary – 'interpretive adjustment'" (p. 85). Third, all of this (Williamson and Cyert-March) has a very current feel. For example, there is now active research in how parties choose firm boundaries and internal control structures in part to affect incentives to gather and communicate information. See Milgrom and Roberts (1988) and Holmstrom and Tirole (1991) for early work; Alonso, Dessein, and Matouschek (2008), Rantakari (2008), Dessein, Garicano, and Gertner (2010), and Friebel and Raith (2010) for recent work; and Gibbons (2010, Section 3) for more on this literature.

### *3.4 Formal Models*

Williamson's contributions to TCE are a counter-example to Krugman's (1995: 27) dictum (originally composed for economic geography but equally applicable across most of economics): "Like it or not, ... the influence of ideas that have not been embalmed in models

soon decays.” Recognizing that his ideas had not been “embalmed in models,” Williamson argued that TCE was moving through a

“natural progression ... [from] (1) informal analysis ... [to] (2) preformal and (3) semiformal stages, ... [culminating] with (4) fully formal analysis.” (1993: 38)

Cautioning against

“[p]rematurely formal theory [that] purports to deal with real phenomena without doing the hard work of making serious contact with the issues” (1993: 43),

Williamson nonetheless clarified that

“[s]ome might infer the progression begins with big ideas and ends with marginal refinements. That would be wrong. Important conceptual advances [are needed] at each stage of the process.” (1993: 39)

This section attempts to illustrate both the kinds of “conceptual advances” that are necessary for “fully formal analysis” and why they can be important.

We economists are so used to seeing our theory in formal models that we don’t often ask why this is or when it should be. There are three familiar benefits of formal modeling. First, formal models can check the internal consistency of informal arguments, including providing boundary conditions under which the informal argument holds. Second, formal models can help specify and interpret empirical tests of informal arguments. And third, formal models sometimes deliver results that few of us could have anticipated without the model (as discussed below).

A fourth benefit is perhaps less familiar but equally germane: formal models can help diagnose what is amiss in a theory when new evidence proves the original to be flawed, as in the following example. In his 1975 book (p. 108-9), Williamson largely dismissed the “hybrid” governance structures mentioned at the end of Section 2.3(c). Then, in his 1985 book (p. 83), he seems to have seen new evidence and (sensibly) changed his mind: “Whereas I was earlier of the view that transactions of the middle kind were very difficult to organize and hence were unstable, ... I am now persuaded that ... [they] are much more common.” Finally, in his 1991 paper, hybrids are on par with markets and hierarchies as outcomes driven by asset specificity (Figure 3, p. 292). To me, this progression shows

Williamson having the right reaction to new evidence, but I am left not knowing how to amend the original theory. Where did the original argument go astray? Was there a logical error, or was a boundary condition violated? If so, does this imperfection in the original theory have implications for other applications? At least in my experience, there is a better chance of amending the argument if one can see the original formally explicated.

An interesting side point, in connection with the multi-disciplinary roots and applications of TCE discussed in Section 2.3(d), is that theory and evidence of the kind developed and synthesized by sociologists such as Granovetter (1985), Stinchcombe (1985 [1990]), and Powell (1990) seem to have played a role in swaying Williamson's opinion about hybrids. Freeland (1996) is another example of cross-disciplinary feedback, in his sociological comparison of TCE theory on multi-divisional firms to detailed evidence from the early decades of General Motors.

In short, for all four of the reasons given above, it would be very helpful to have a formal theory that captures Williamson's TCE arguments. It may be that Tadelis (2010), building on Bajari and Tadelis, has started on a path towards this goal. In particular, Tadelis's model can be interpreted as covering the case of specificity without specific investments, as discussed above. And, digging more deeply into the possible micro-foundations of haggling arising from incomplete ex post contracts, it may be that Hart and Moore's (2008) "reference points" approach is a productive path. Time will tell for both of these agendas.

At the same time, however, we have seen various ways in which there is also clearly room for alternative theories. For example, as noted in Section 3.1, TCE theory does not provide as clear an explanation for variations in the costs of integration as it does for the costs of non-integration. To conclude this subsection, therefore, I briefly describe an alternative approach, which delivers a compelling and unexpected account of variations in the cost of integration (perhaps better seen as variations in the benefit of non-integration). The reason for including this discussion here, however, is that, as far as I can see, this new view of the benefit of non-integration was discovered through the process of formal modeling.

The model in question is Grossman and Hart's (1986), which explores an alternative to Williamson's (2000: 605) emphasis that "maladaptation in the contract execution interval is

the principal source of inefficiency.” Instead, in the Grossman-Hart model, there is zero maladaptation in the contract execution interval, and the sole inefficiency is in endogenous specific investments.<sup>12</sup>

It is striking how different the logic of inefficient investment can be from the logic of inefficient haggling. In their pure forms envisioned here, the two can be seen as complements. For example, the lock-in necessary for Williamson’s focus on inefficient haggling could result from *contractible* specific investments chosen at efficient levels. But by assuming efficient bargaining and hence zero maladaptation in the contract execution interval, Grossman and Hart focused attention on *non-contractible* specific investments and hence discovered an important new determinant of the make-or-buy decision: in the Grossman-Hart model, an important *benefit* of non-integration is that both parties have incentives to invest; in Williamson’s argument, an important *cost* of non-integration is inefficient haggling. In short, the two theories are simply different.

Interestingly, both Williamson (1971: 116) and Klein, Crawford, and Alchian (1978: 301) can be seen as suggesting the idea of endogenous investments, but this benefit of non-integration does not appear in either informal account. Rather, as an example of the third benefit of formal models described above, it took the Grossman-Hart model to discover unexpected implications of the endogenous-investment argument. In this spirit, one can hope that formalizing Williamson’s TCE arguments will also produce unexpected results, as well as deliver the other kinds of benefits noted above.

#### **4. TCE-Present: Celebrating the Past, Charting the Future**

I see TCE-past as a 40-year journey of innovation and accomplishment: from the microanalytic instincts and methods of the Carnegie School; to a Coasean focus on integration, antitrust, and regulation; to startling regularities in the data on firms’ boundaries and contracts; to applications across many fields of economics and management disciplines. Put differently, if the core of TCE is (1) microanalytic comparison of alternative governance structures under (2) conditions of incomplete ex ante contracts, lock-in, and incomplete ex post contracts in the study of (3) transactions between and within firms, then, to paraphrase

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<sup>12</sup> See Gibbons (2005) for a less cryptic exposition of this and related theories of integration.

Richard Nixon, maybe all economists who study organizations are transaction-cost economists now. That being said, TCE-future would be dull if it did not have opportunities, and I believe it does.

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