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A Response to “Transaction Cost Economics on Trial Again”

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Introduction

We appreciate the careful and thoughtful commentary on Schermann et al. (2016) by Lacity and Khan (2016). Their commentary caused us to reflect on some of the underlying assumptions guiding our research. In the spirit of Lacity and Khan (2016), we do not attempt to defend our research and ‘win’ the conversation. Instead, we build on their insights to contribute to this potentially fruitful conversation among IS scholars started by Lacity and Khan (2016). We are grateful for the invitation from the Journal of Strategic Information Systems (JSIS) to do this.

Two reviews in JSIS investigate inconsistencies among the empirical findings for predictions based on Transaction Cost Economics (TCE) in the IT outsourcing (ITO) literature (Karimi-Alagheband et al., 2011; Lacity et al., 2011). Both studies review the same literature using the same methodology. They present similar findings but draw very different conclusions.

Karimi-Alagheband et al. (2011) conclude that TCE is an appropriate theoretical framework for ITO research. To resolve the inconsistencies, they call for IS researchers to apply TCE more rigorously and to adopt more powerful methodologies. In contrast, Lacity et al. (2011) conclude that “the ITO phenomenon is more complex than can be accommodated by TCE” (p. 139). They call for the ITO research community to search for an endogenous ITO theory to replace TCE in future ITO research.

Our motivation in Schermann et al. (2016) is to contribute to this debate. We present a meta-analysis of the relationship between the choice of contract type (CT) and task uncertainty (TU) in the ITO literature. We chose to investigate this relationship for two reasons. One is that the choice between fixed-price contracts and time-and-material contracts is a major research stream in the ITO literature. The other is that TCE “makes specific and unambiguous predictions about the choice of CT as a function of TU” (Schermann et al., 2016, p. 2). Hence, the results

would be directly relevant to the debate on the accumulation of knowledge and its implications for future research.

In the preface to their seminal textbook on meta-analysis, Hunter and Schmidt (2004) state: “[t]here are two steps to the cumulation of knowledge: (1) the cumulation of results across studies to establish facts, and (2) the formation of theories to organize the facts into a coherent and useful form” (p. xxvii). In Schermann et al. (2016), we adopt this structure. First, we ‘let the data speak’. Second, we discuss the implications of that data for the on-going debate on the relevance of TCE to ITO research.

The commentary by Lacity and Khan (2016) also adopts this structure. First, they assess the contribution from accumulating empirical facts on the relationship between CT and TU in Schermann et al. (2016). Second, they develop three arguments to establish what constitutes a fair appropriation of theory. Considering each argument in turn, they critique the theoretical implications presented in Schermann et al. (2016).

To respond to their commentary, we follow their structure. First, we consider the facts: the accumulated empirical evidence on the relationship between CT and TU in the ITO literature. Second, we consider each of the three arguments raised by Lacity and Khan (2016), both in the specific context of Schermann et al. (2016) and more generally.

The facts and nothing but the facts

In Schermann et al. (2016), we consolidate empirical results from 28 studies from the ITO literature. We report the expected relationship between CT and TU, aggregating across five different measures of task uncertainty: technological uncertainty, requirements uncertainty, technological complexity, organizational complexity, and project size.

Frequently, the relationship between CT and TU is significant and consistent with TCE for studies in which the data collection started prior to or during 1999. In contrast, the relationship is non-significant and inconsistent with TCE for studies in which data collection started after 1999. Disaggregating across the five operationalizations of TU, we find significant relationships for requirements uncertainty, organizational complexity, and project size in the earlier subsample. None of the operationalizations of TU yield significant results in the more recent subsample.

In step one, Lacity and Khan (2016, p.1) conclude that “[s]omething interesting is clearly happening empirically” in Schermann et al. (2016). On that basis, they judge the contribution of accumulating facts to warrant publication. Naturally, we would agree. Others, who believe that ‘theory is king’¹, would not.

¹ See the debate about whether “[t]heory is [k]ing” (Straub, 2009, p. vi). This quote has sparked a number of commentaries (Avison and Malaurent, 2014; Gregor, 2014; Markus, 2014; Silverman, 2014). Despite critiquing the role of theory as ‘king’, none of the commentaries argue for the publication of empirical facts on their own.

Indeed, we doubt that the accumulation of empirical facts on the relationship between CT and TU on its own would have been judged by many to have made a contribution that warranted publication in a top IS journal. Our experience tells us that reviewers and editors would have asked us to comment on the theoretical implications of those facts.

Against this belief, the Academy of Management has just started the new journal “Discoveries” to publish “empirical information [...] that can be used to stimulate subsequent theory building papers and hypothesis-testing research” (Van de Ven et al., 2015, p. 2). Perhaps, the facts and nothing but the facts will be published in IS journals in the future. We would encourage this.

A fair appropriation of theory

To motivate their discussion of what constitutes *a fair appropriation of a theory*, Lacity and Khan (2016) begin by asking an intriguing question: “Who created the theory of evolution by natural selection?” (p. 2) Their exploration of this apparently straightforward question is pertinent to their critique of Schermann et al. (2016). All successful theories have many parents and many siblings. Naturally, unsuccessful theories are orphans.

This complexity about the “true” ownership of theories motivated Lacity and Khan (2016) to develop three arguments for what constitutes a solid foundation on which to build a ‘bridge’ between empirical facts and theory. Their first argument is to adopt a well-specified version of the to-be-appropriated theory. Their second argument is that “the main constructs, relationships, and contextual interplays of a theory” (p. 4) should be comprehensively appropriated instead of appropriating isolated relationships out of context. Their third argument is that an appropriation of a theory in the context of a meta-analysis would be “cleaner and stronger” when the individual studies selected for inclusion in the meta-analytical sample also “invoked the theory’s logic” (Lacity and Khan, 2016, p. 6).

First, Lacity and Khan (2016) accept that we present a well-specified version of TCE. Second, they conclude that our claim for TCE to be an ‘obsolete’² theoretical basis for future ITO research does not constitute a fair appropriation of TCE because the appropriation is limited to the review of the relationship between CT and TU. Third, they suggest that we should have restricted our meta-analysis to studies that explicitly reference TCE to derive hypotheses and to justify the inclusion of the relationship between CT and TU as a control variable.

Below, we respond to each argument. They proved to be powerful vehicles to critically reflect on choices that we made during the research process in Schermann et al. (2016). We share the reasoning behind our choices to help others to make appropriate choices when they appropriate theories.

² We acknowledge that Lacity et al. (2011) do not claim that TCE is ‘obsolete’. We chose the term. On reflection, we should have selected another.

A well-specified version of the to-be-appropriated theory

The first argument by Lacity and Khan (2016) is: “A fair theoretical appropriation requires that one clearly establishes which version of the theory is being appropriated” (Lacity and Khan, 2016, p. 2). We agree that a well-specified version of a theory is fundamental to a fair appropriation of the theory. However, over time, researchers refine theories, and add or remove relationships. For example, Williamson and his take on “his own” TCE theory has evolved substantially since its first formulation (Williamson, 1991, 2010). So, the question is: Which version of the theory to appropriate?

Despite initial skepticism, Lacity and Khan (2016) conclude that we do establish a well-specified version of TCE in Schermann et al. (2016). While this is important, their primary contribution is in their exploration of the argument. Their careful research of the relevance and logic of the references that we choose to motivate our choice of TCE as the theory to be appropriated is a role model for others to adopt when critiquing their own appropriation of a theory.

Our initial submission to JSIS drew on both TCE and Principle Agent Theory (PAT). We had not focused on the issue of *a fair appropriation of a theory*. The reviewers pointed this out. Indeed, one reviewer suggested that we appropriate PAT instead of TCE. Our response was to inspect the theoretical frameworks used to describe the relationship between CT and TU in the studies that we had selected for inclusion in our meta-analysis and develop the argument presented in Schermann et al. (2016) appropriating TCE theory as the focus of our analysis.

Our reflections on the Lacity and Khan (2016) comments have increased our understanding of the challenges in making a fair appropriation of a theory. Fortunately, these insights have not caused us to doubt our appropriation of TCE theory. The studies included in our meta-analysis sample invoke a variety of theoretical frameworks, dominated by TCE and PAT. In addition, we found that the studies frequently draw from multiple theories to derive the hypotheses that they test and to justify the control variables that they include.

Of the 28 papers in our meta-analysis sample, 20 directly or indirectly reference the same two papers. These are Banerjee and Duflo (2000) and Bajari and Tadelis (2001) (see Figure 1). Both papers invoke TCE and PAT, and Bajari and Tadelis (2001) attempt to build an overarching theory of contracting integrating the two theories. These two papers also reference each other. Banerjee and Duflo (2000) reference the working paper (Bajari and Tadelis, 1999). This was published as Bajari and Tadelis (2001), in which the authors reference back to Banerjee and Duflo (2000). The remaining eight papers do not provide theoretical references for CT.

[Insert Figure 1 about here]

We then re-examined our understanding of Williamsons work and specifically Williamson (1979). In this, he differentiates between three modes of governance: market, hierarchy, and hybrid (Williamson, 1979). Uncertainty, the independent construct in our meta-analysis is only a critical issue in the hybrid mode (Williamson, 1991). The form of the contract prescribes

which party bears the costs of monitoring and/or of renegotiating the contract (Osei-Bryson and Ngwenyama, 2006; Susarla et al., 2009). In Williamson's terms, CT is an "adaptive mechanism[...] to effect realignment and restore efficiency when beset by unanticipated disturbances" (Williamson, 1991, p. 272).

In addition, in a recent meta-analysis, Crook et al. (2013) show that task uncertainty is the dimension of uncertainty that "comes from the type of work that is performed" (Crook et al., 2013, p. 70). It is the only form of uncertainty that exhibits significant predictive validity for the mode of governance chosen. Combining this argument with the above interpretation of CT as a governance mechanism provides a powerful logic to justify our appropriation of TCE. It supports our claim that TCE "makes specific and unambiguous predictions about the choice of CT as a function of TU" (Schermann et al., 2016, p. 2).

However, even if we were to rewrite our original paper, this theoretical argument would not be our primary logic for appropriating TCE. Dominating the theoretical justification, the critical motivating factor for appropriating TCE instead of PAT was, and remains, our intent to contribute to the on-going discussion on the future role of TCE in ITO research. To do this, we positioned Schermann et al. (2016) as a contribution to the conversation in the IS community on empirical 'anomalies' when TCE is used as the theoretical lens.

We didn't want to start a new conversation around PAT. While we agree with Lacity and Khan (2016) that our use of the word 'obsolete' may have pushed our analysis a bit too far, we believe that the adoption of TCE as one of the two dominant theoretical frameworks for the analysis of ITO has reached a tipping point and that our results tip the balance in favor of the Lacity et al. (2011) call for the development of an endogenous ITO theory over the Karimi-Alagheband et al. (2011) call for more rigorous research within the TCE framework.

We acknowledge the limitations associated with our process of theory appropriation in Schermann et al. (2016). We thank Lacity and Khan (2016) for the opportunity to reflect on this process. In general, explanations of authors' theorizing are missing from the publications that the three of us read. Instead, authors (us included) attempt to present a polished, streamlined and hopefully compelling justification for the theory appropriated, as we attempted to do in Schermann et al. (2016). Any doubts about the appropriation of a theory are typically suppressed. Perhaps this is a practice that should be reviewed. Certainly, access to such theorizing could be of value to researchers at the beginning of their careers.

The comprehensive appropriation of theory

The second argument by Lacity and Khan (2016) is that "[a] fair theoretical assessment of a theory requires that the key ideas of the theory are assessed" (p. 4). They conclude that our claim for TCE being 'obsolete' for future ITO research does not constitute a fair appropriation of TCE for two reasons. One is that investigating the relationship between CT and TU in isolation of other critical TCE-based relationships is not sufficient to generalize potential validity threats to the whole theory. The other reason is that we did not investigate interaction effects (e.g., with

asset specificity). They state: “[I]solating the effects of uncertainty from asset specificity makes little sense” (Lacity and Khan, 2016, p. 5).

We agree with Lacity and Khan (2016) that claims of generalizability should be qualified when they are based on the investigation of a single relationship within a theory compared to a comprehensive review of that theory. However, it is not our intention in Schermann et al. (2016) to assess TCE. Instead, we are investigating two competing positions on the future role of TCE in ITO research. Although we side with Lacity et al. (2011) and call for the development of an endogenous theory of ITO, we accept that TCE is still applicable in a variety of specific contexts. For example, these would include research into ITO in countries or industries with limited experience of ITO³.

We do not claim to test TCE in Schermann et al. (2016). Instead, we assess its relevance to ITO research. Hence, we felt it was not necessary to test interaction effects as in a comprehensive meta-analysis (Crook et al., 2013). Furthermore, the papers that we included did not study make-or-buy decisions as a function of asset specificity. Rather, they investigate the effects on ITO performance of a variety of governance mechanisms. Typically, these papers describe hybrid modes of governance in ITO projects. We assumed that the contexts of these projects exhibited sufficient and consistent levels of asset specificity. However, this is an assumption that we should have articulated.

Our reading of Kuhn (1962) suggests that the shift from TCE as the dominant paradigm, or one of the two dominant paradigms, in ITO research requires an alternative paradigm that could take the place of the ‘old’ paradigm(s). We see the call for the development of an endogenous theory of ITO by Lacity et al. (2011) as the call for developing that alternative paradigm. Kuhn (1962) further argues that increasing evidence inconsistent with the dominant paradigm persuades some researchers to work on an alternative paradigm before the ‘old’ paradigm is replaced. We hope that our results contribute to that effect.

We agree with Lacity et al. (2011) that the ITO research community has matured. At the same time, the ITO industry has matured into a major worldwide industry (Manning, 2013). This increased industry maturity is supported by our conversations with ITO practitioners for whom CT has been, and still is, an important governance mechanism.

However, in their search for competitive advantages, ITO vendors and clients have adapted their behavior and the purpose of CT has evolved. As a professional as well as a theoretical discipline, IS research should not only “advance [as] a scientific discipline [.. but also] enlighten practice” (Van de Ven, 2007, pp. 1, citing Simon, 1976). We would add that the research community should also listen to the professionals to explore the evolution of the theory in use.

³ Insight based on a private conversation with Leslie Willcocks.

Choosing a contract type that is appropriate to the context of an ITO project is a frequent practical application of ITO research (e.g., Gopal and Koka, 2012; Hoermann et al., 2015; Susarla et al., 2009). While this means that IS research is relevant to the ITO practice, it also means that the relevance may fade as practitioners learn, adapt and innovate. Our conversations with practitioners indicate that this process is well advanced in the practice of ITO.

Studies that investigate specific governance mechanisms, including CT, could help us to identify other research areas in which to develop new and relevant guidance for ITO practice. Schermann et al. (2016) is an example of the search for 'expiration dates' not only of theoretical frameworks but also of the practical guidance that we teach and offer as consultants.

Empirical versus theoretical sampling

The third argument by Lacity and Khan (2016) is that “[a] meta-analysis claiming to review a theory is stronger when one only selects articles for the meta-analysis that invoked the theory’s logic.” (p. 6). In making this argument, they differentiate between two types of sampling. One is a theoretical sampling, where “all interpreters [in the individual studies and in the meta-analysis] are using one theoretical lens” (p. 6). The other type is an empirical sampling, where any study may be part of a meta-analysis “as long as the reality is measured in the relevant constructs” (p. 6). They conclude that our appropriation would have been “cleaner and stronger” (p. 6) if we had restricted the meta-analysis to studies that invoke TCE.

Initially, we were surprised by this argument. We agree with Hunter and Schmidt that meta-analysis is a technique to “estimate what the results would have been had all the studies been conducted without methodological limitations or biases” (Hunter and Schmidt, 2004, p. xxv)⁴. Thus, in Schermann et al. (2016), we include studies that meet three empirical requirements: ITO is the unit of analysis; at least one operationalization of TU is investigated; and the correlation between CT and each measurement of TU is reported.

Again, we see the contribution of Lacity and Khan (2016) not in the conclusion but in the exploration of the argument. Does our empirical sampling justify the claim we make in Schermann et al. (2016)? In our analysis, each data point on the relationship between CT and TU is a potential application of TCE. Excluding data that was not collected within the dominant paradigm might make the analysis cleaner but it would weaken the generalizability of the findings rather than strengthen the meta-analysis.

The only criteria for the inclusion of an observation in the meta-analysis sample in Schermann et al. (2016) is that the data satisfies the three requirements specified above. We acknowledge potential limitations in our coding procedures in the limitation section of Schermann et al. (2016) and, as noted by Lacity and Khan (2016), we provide the codes that we use in an Appendix, which makes the analysis transparent and capable of being replicated.

⁴ We acknowledge that our meta-analysis falls short of this ideal.

To exclude studies from our meta-analysis because they do not adopt a specific paradigm, for example, one unrelated to Banerjee and Duflo (2000) Banerjee and Duflo (2000) or Bajari and Tadelis (2001) in Figure 1, would not only have severely limited our meta-analytical sample but would have subjected us to a potential major validity threat: the file-drawer problem (Rosenthal, 1979). Unlike the traditional narrative review process, meta-analysis requires an exhaustive search in journal papers, conference publications, theses, and unpublished work, where available, unconstrained by the reason for which the data was collected.

However, we agree with Lacity and Khan (2016) that empirical sampling may limit the generalizability of the results. While Hunter and Schmidt (2004) argue “that there is nothing objectionable about mixing apples and oranges if the focus of the research interest is fruit” (p. 470), we are aware that it is sometimes very difficult to differentiate “apples” from “oranges”. It is also challenging to ensure that the research interest is fruit. Again, we must acknowledge the contribution of our reviewers in helping us to make the appropriate distinctions.

Conclusions

Why is the issue of a fair appropriation of a theory critical to the contribution made by Schermann et al. (2016)? After all, as researchers, we measure the empirical world and collect data through the lens of theoretical frameworks. However, as shown above, in meta-analysis different researchers have collected the data employing different lens and, in some cases, they evoke multiple theories or no theory at all.

In other data-driven research, it is very unlikely that data collected by practitioners, business associations and government bodies, for example, would explicate their theoretical frameworks (Lazer et al., 2014). By separating theory from the cumulative results, meta-analysis and other structured research reviews help to establish the facts. The next step, the fair appropriation of theory to explain those facts, is critical to advancing theory. Thus, we hope that this conversation about a fair appropriation of theory continues to advance both theory development and evidence-based practice.

In addition, the commentary by Lacity and Khan (2016) and our response could point to a potential, major research opportunity in the IS discipline. They argue that ITO research has reached a deeper and more inclusive level of analysis “than can be accommodated by TCE” (Lacity and Khan, 2016, p. 139). We believe that ITO research is not the only field in the IS discipline in which the levels of depth and maturity warrant a reanalysis of the dominant theoretical framework, where that framework was imported from a reference domain when the IT context was different.

For two reasons, meta-analysis is one of the critical tools to examine these potential opportunities. One reason is that meta-analysis is a powerful technique to consolidate empirical evidence across multiple studies. The most common purpose is to provide an overview of empirical evidence. For example, Schermann et al. (2016) illustrates how meta-

analysis can help to investigate inconsistencies between theoretical frameworks and empirical data. Separating reliable from unreliable hypotheses is a critical step both to refine existing theories and to provide the initial building block to develop endogenous theories.

The other reason is that meta-analysis could be used to challenge potentially outdated or flawed practice by investigating the underlying arguments. In Schermann et al. (2016), we investigate the role of CT, which is the basis of various practical governance mechanisms. From the data, we believe that meta-analysis would be a powerful tool for systematically searching for 'expiration dates' of the practical guidance that we offer as a discipline. The fair appropriation of theories would be a critical step in providing new guidelines.

We benefitted a lot from reflecting on the Lacity and Khan (2016) exploration of what constitutes a fair appropriation of theory. We have enjoyed this opportunity to share our reflections on those arguments and how we muddled through the research presented in Schermann et al. (2016). We hope this conversation encourages others to explore meta-analysis as a mechanism to consolidate and move IT research forward.

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⁵ * = Study included in the final meta-analytical dataset of Schermann et al. (2016).

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Figure

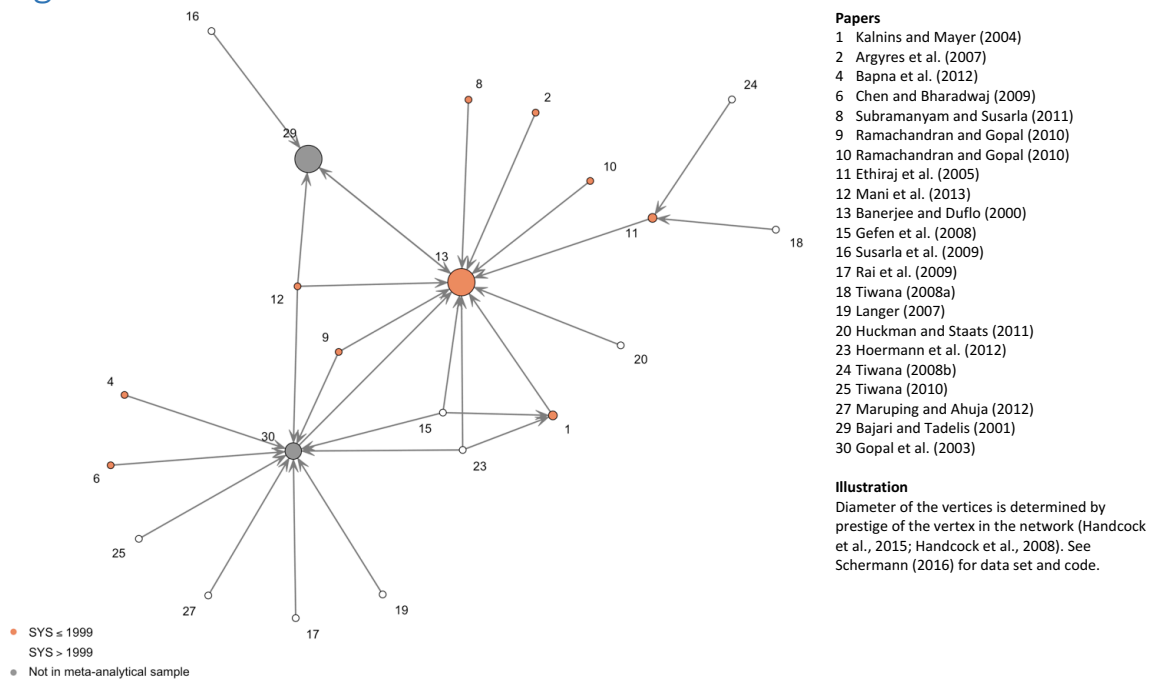


Figure 1: Banerjee and Duflo (2000) and Bajari and Tadelis (2001) as the core references for choice of CT