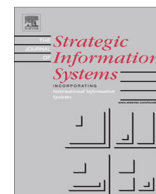




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Theory appropriation and the growth of knowledge

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The two studies – “The Role of Transaction Cost Economics in Information Technology Outsourcing Research: A Meta-Analysis of the Choice of Contract Type” by Schermann et al. (2016) and “Transaction Cost Economics on Trial Again: A Commentary on “The Role of Transaction Cost Economics in Information Technology Outsourcing Research: A Meta-Analysis of the Choice of Contract Type”” by Lacity and Khan (2016) – provide rich empirical material for the scholarly observer interested in theorizing about how scientists (here, information systems researchers) go about building knowledge. Philosophers and historians of science observe scientists at work doing science, just as sociologists and anthropologists observe people in their everyday lives in communities and villages. In this commentary, I make observations from the same perspective.

What is theory appropriation? Why do Lacity & Khan raise it as an issue? How is it important to the growth of knowledge not only regarding transaction cost economics, but in general?

The logic of scientific reasoning is one thing; its implementation, quite another. The logic of empirical science is an application of the logic of the syllogism in the form of *modus tollens* (Lee and Hubona, 2009). The major premise is “if p is true, then q is true”; the minor premise, “q is not true”; and the conclusion, “therefore, p is not true.” In this logic, “p” is the theory being tested and “q” is the theory’s implications to be observed, as in (but not limited to) an experiment. Observations can be consistent with the theory (i.e., “q is true”) but, logically, they may not prove the theory true. At best, observations may only prove the theory false (i.e., observations that “q is not true”), thereby eliminating it while other theories, “left standing,” provide grist for the mill of continuing scientific research. The implementation of this logic, however, is hardly as clear as the logic itself. How, for instance, does one state what the theory, “p,” is in the first place? Lacity & Khan refer to this problem as “theory appropriation.”

Lacity & Khan ask, “Which version of Transaction Cost Economics [TCE] did Schermann et al. (2016) claim to appropriate?” Lacity & Khan refer to the historical development of the theory of TCE, noting that “theories have messy origins and TCE is no exception.” They note that TCE can be traced back to the scholarly works of Coase, of Commons, of Cheung, and of Williamson, where they also note that Williamson in turn credits the scholarly works of Macneil, of Simon, of Cyert, of Alchian, and of Demsetz, “to name but a few.” Given the presence of so many different hands involved in the creation of a theory, the documentation of which is spread out over a vigorous stream of articles and books, how may a researcher, interested in testing the theory, know that his or her statement of the theory (i.e., “p” in “if p is true, then q is true” and “therefore, p is not true”) captures or *appropriates* the past literature satisfactorily, whether all of it or even just a segment of it, such as Williamson’s? A single scientist building a theory anew (if this is possible) and testing it would not face this daunting challenge; however, this challenge must be accepted by a scientist who is at work doing science as a member of a larger scientific “community” or “village” and who, importantly, sees his or her work as contributing to a cumulative tradition of growing already existing knowledge. Science is not a series of one-off studies; science progresses step by step, where the next step requires the faithful if challenging appropriation of what the previous steps have already accumulated. To argue otherwise would be to argue against knowledge being cumulative.

I offer two ways by which to frame how to think about appropriation: the juridical frame and the structural-cultural frame.

The juridical frame refers to the Anglo-American legal manner of reasoning, which is based on *stare decisis* (i.e., “the legal principle of determining points in litigation according to precedent; properly as *v.phr.*, to be bound by precedents”

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(Oxford English Dictionary)), for which, in turn, the distinction between the “holding” and the “dicta” is illuminating. In a judge’s opinion, those words which may serve as a binding precedent in subsequent cases are the “holding.” The remaining words are the “dicta,” which refers to “an expression of opinion by a judge which is not essential to his or her decision as to the particular case” (Oxford English Dictionary). The holding is binding on subsequent decisions, but the dicta are not. In writing a decision, however, a judge does not neatly specify which part of the decision is the holding and which, the dicta; such is left for future decisions, typically by other judges. Over the course of many cases that attempt to apply the original precedent, where the cases follow procedures of legal reasoning, what is considered to be the holding that constitutes the “precedent” can eventually emerge.

Unfortunately, in scientific reasoning as opposed to legal reasoning, there is no comparably definitive juridical method or other set of explicit and accepted procedures of reasoning by which to develop a consensus on the “holding,” i.e., to appropriate what is to be regarded as the “theory” from a vigorous stream of research in which many scientists have had a hand in shaping. Even in the research of a single statistical positivist study, little attention has been paid to the process by which to operationalize a theory from its propositions (stated in prose) into mathematics (for instance, should the equations be linear or nonlinear and must the variables be continuous or discrete?). Indeed, is the “theory” what the propositions state in prose or is it a mathematical representation of the prose (and, if the latter, *which* mathematical representation?).

Turning to meta-analysis, how may one know that the constructs and relationships considered in a meta-analysis appropriate a theory faithfully? Lacity & Khan forthrightly proceed to assess whether the meta-analysis by Schermann et al. satisfactorily appropriated Williamson’s TCE, which Lacity & Khan accomplish by innovating three criteria: “Argument 1: A fair theoretical appropriation requires that one clearly establishes which version of the theory is being appropriated,” “Argument 2: A fair theoretical assessment of a theory requires that the key ideas of the theory are assessed,” and “Argument 3: 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.” Furthermore, Lacity & Khan operationalized each criterion and applied it; for example, for the first one, regarding Schermann et al.’s appropriation of TCE theory to include the proposition, “TCE predicts that Task Uncertainty determines Contract Choice,” Lacity & Khan offered this conclusion after a detailed analysis: “Schermann et al. (2016) provided a sufficient logical chain of citations to TCE-related works that considered some type of uncertainty or complexity to be determinants of contract choice.” Lacity & Khan are to be lauded for rising up to the challenge of formulating criteria by which to identify whether Schermann et al. properly identified the “precedent” by providing “a sufficient logical chain of citations”; however, by definition, their effort of making up criteria is *ad hoc* and can (and should) be challenged by other scientists who would purport to offer better criteria. In a sense, will Lacity & Khan’s three criteria, and their ways of operationalizing them, themselves survive as its own “precedent,” whether in their current form or in a form modified by the hands of other scientists in the future? Unfortunately, unlike juridical reasoning, reasoning in science has no explicitly established and well accepted procedures to guide such development. In the juridical frame for examining theory appropriation, the recommendation would be for such procedures of reasoning to be explicitly established.

Next, the structural–cultural frame refers to a perspective that draws on anthropology and sociology; in particular, it draws on the social construction of reality. In contrast to the juridical frame, it highlights how theory appropriation is also implicit, not just explicit.

A hallmark of the structural–cultural frame is that human individuals do not and cannot exercise complete free will in what they do and think; rather, human individuals are subject to the constraints imposed on, and the opportunities allowed to, the role they occupy in the social structure(s) of which they are members or in which they happen to find themselves. An illustrative case I often use for pedagogical purposes is the kinship structure of the Fox Indians, a Native American people in North America, and its contrast to the kinship structure of white or European Americans (Gearing, 1970). For instance, for white Americans, one’s father’s brother is one’s uncle, but for Fox Indians, one’s father’s brother is also one’s father, where the latter (the “uncle” in white American parlance) has the same responsibilities to one as the man of whom one was born. The three people – whether one, one’s father, and one’s uncle in the white American social structure, or one and one’s two fathers in the Fox social structure – have no choice in this matter; they are all literally born into the situation, and behave and think according to their respective roles in their social structure. Furthermore, worthy of our explicit attention is that, in addition to the social structure itself (the array of different roles, such as father, brother, mother, niece), there is the shared knowledge of how the occupant of each role may behave in the social structure. Each person in the social structure shares a common body of knowledge about not only how the occupant in his or her own role may behave, but also how occupants in different pairs of roles (and even larger combinations) may behave with regard to each other in the interactions among them. I define such shared knowledge in a social structure as its “culture.”

We scientists are not above the people whom we study in our behavioral research. We too are human individuals captured in a social structure, one of our own scientific community. The roles in our social structure include, to name just a few, professor, reader, lecturer, associate professor, assistant professor, doctoral student, editor, reviewer, dean, department head, provost, vice president for research, conference chair, and program chair. And there is shared knowledge – culture – regarding how one may and may not engage in research. The culture of any community of scientists includes the explicit and implicit rules by which they may conduct their work, i.e., their science.

Berger and Luckmann state in the classic book, *The Social Construction of Reality: A Treatise in the Sociology of Knowledge* (1966, p. 61): “Society is a human product. Society is an objective reality. Man is a social product.” The social construction of reality – where reality includes social structure and culture – is the ongoing process where society is continually being produced and reproduced by the humans in it, where society continually sustains its quality of an objective existence beyond

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