A FUTURE FOR THE SCIENCE OF ORGANIZATION DESIGN

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Rather than *the* future of organization design (academics make justifiably reluctant futurists), I want to discuss *a* possible future for the science of organization design – one that I hope will come to pass.

I understand organization design to refer to a particular form of human problem solving in which the problem is one of getting multiple individuals with diverse knowledge and interests to collectively achieve something that they could not by acting individually. Because bounded rationality affects not only the members but also the designers of organizations, solutions may be imperfect and unsuccessful, and many may have arisen almost unintentionally. But clearly there are better and worse solutions, and given the predominantly organizational nature of our economy, many good solutions exist in the form of the organizations that surround us.

Is organization design an important field of study? If we judge importance in terms of potential impact on human affairs, then the answer is a resounding "yes". Further, the importance of improving our knowledge of organization design is likely to remain high in the foreseeable future because of several trends. These trends include advances in information technology that encourage experimentation with new organizational designs, large economies like India and China attempting to rapidly transform the organizational infrastructure of their public administration, the professionalization of the NGO and charity sector, and multinational corporations' increasing attempts to exploit globally distributed intellectual resources.

Can a normatively oriented field such as organization design be amenable to scientific study? Simon's (1996) statement remains the authoritative one on the epistemology of a science of design, and indeed the field made considerable scientific progress through the contributions of academic stalwarts such as Lawrence, Lorsch, Thompson, Tushman, Nadler, Mintzberg, Ghoshal, Doz, and others. Yet as my co-authors and I discuss elsewhere (Gulati, Puranam, & Tushman, 2012), for a variety of reasons there has been a hiatus in the study of organization design, which is only now showing signs of lifting.

So what would organization design as a rejuvenated and useful branch of organization science look like? I believe the field would have three main characteristics. First, the field would be characterized by a high degree of *consilience*. As described by the biologist E.O. Wilson (1998), consilience advocates the importance of scientific explanation at one level of aggregation based on scientific knowledge about lower-order phenomena (e.g., organizations as aggregations of individuals or individual actions occurring as a result of cognitive structures). Consilience requires not only scientifically derived knowledge of lower-level phenomena but also a theory of aggregation.

It is well known that it is sometimes possible to construct theories of higher-level aggregates with only scant knowledge of lower-level elements (Simon, 1996) – in other words, without consilience. However, if the purpose is to develop theories that improve how organizations work (and not only describe how they behave), then it seems unlikely we can progress far in this way. Put simply, useful theories of organization design are likely to emerge from knowledge (rather than assumptions) about how individuals interact in organizational contexts. Thus, there are likely to be many useful things we can learn from cognitive and social psychologists to help construct better theories of organization design.

Second, the field would see *a revolution in empirical methods*. Greenwald's (2012) recent analysis of Nobel prize awards highlights the importance of methodology in opening up new areas for theory development, and this seems particularly relevant to organization design.

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Obtaining large-scale data on the design of organizations has always been difficult, but if the field is to progress, then rich and reliable data on the workings of organizations are essential. Creative ways to get at organizational data will have to be found. One approach involves returning to methods that used to be mainstream: laboratory experiments have contributed significantly to the field in the past (e.g., Cyert & March, 1963) and could do so again. A second approach is to adopt appropriate methodologies from adjacent disciplines, such as methodologies that allow the analysis of social network data or the conduct of field experiments. A third approach involves looking for data in unusual places (e.g., methods to reliably code and analyze textual and linguistic data on governance arrangements in alliance contracts, post-merger integration plans, email records, annual statements, accounting results, and CEO reporting relationships).

Third, a sophisticated applied branch of the field would develop which goes well beyond providing general advice to *prototyping new organizational designs*. This could happen either *in silico* through computational agent-based models or in the behavioral lab – with new proposed organizational arrangements being tested for unanticipated consequences before being implemented.

A dash of humility is appropriate when discussing the future of the science of organization design. It may be that organizations prove to be such formidably complex systems that we make little progress on any of these dimensions. A science of organization design requires at least some degree of consilience by synthesis (Wilson, 1998), and this may prove to be just too difficult. However, I do not think the evidence and progress to date warrant such pessimism; in any case, the enterprise is too important to not even try.

In conclusion, the technology of organizing is the mother of all "general purpose technologies" (Bresnahan & Trajtenberg, 1995). It provides the framework within which we make progress on other technologies (and is sometimes in turn shaped by them). Organization design is too important a field of social science to suffer another long hiatus given its potential to be in Pasteur's quadrant (Stokes, 1997), an arena where the synergies between practice and theory are likely to be very high.

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