THE FACTS, NEIL SAID, NOTHING BUT THE FACTS...WHOSE FACTS? ANSWERED DAVID

Bruce Kogut

Joel Baum and Frank Dobbin were perceptive in choosing these two articles, both of which have made important, if not defining, contributions to the academic literature. The additional remarks of Neil Fligstein and David Teece provide a chance for those who wish for the re-emergence of a social science community over fortress sub-specialities to pose the question if the triangulation, rather than opposition, of these two contributions is the more interesting direction for future research. I will argue that the camps represented by Fligstein and Teece are individually stronger as members of a common community. Surely, to ignore the contributions of either these two lines of thought is intellectually untenable.

In reviewing the design of the book, I thought that Baum and Dobbin probably meant the labels of economic sociologists and organizational economists, the latter who might be better associated with notions of pragmatic action than the sort of calculative rationality of orthodox economics. Whereas the arena of business is surely as susceptible to power, symbol, and culture as any other field, I take it as self-evident that as much as artists believe themselves to be creating art, business

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people see themselves as producing things in line with certain objectives. To eliminate these distinctions is not to take the facts seriously, as Fligstein so carefully and artfully claims is the stronghold of his field. What are these facts?

FLIGSTEIN AND THE MULTIDIVISIONAL FIRM

Fligstein's commentary is a lucid and sympathetic presentation of recent contributions in economic sociology. I agree strongly with his point of view, especially the emphasis on understanding behavior as historically situated. Teece, however, is on the mark in his insistence on the evidence that suggests that *economic* sociology needs to incorporate economic considerations into their theoretical concerns. The example of the diffusion of the multi-divisional structure is a good case in point, as Teece and Fligstein have both written on this subject, as have many others. If we look at the totality of the evidence, and not just selective results, the overall inference is that economics matter a great deal to business decisions.

David Parkinson and I (Kogut & Parkinson, 1998) analyzed data on the diffusion of the multidivisional structure from 1919 and found that Alfred Chandler's account was not only misunderstood by many organizational sociologists, but is largely in accordance with the statistical findings. Chandler is too enamoured of associating size with efficiency, and this is the Achilles heel at which Fligstein most effectively directs his arrow. Yet, there is much more to Chandler's history than this pithy claim. It is a history of pragmatic efforts of people situated in particular times to identify problems and to solve them in reference to their positions of power and to what they know about other firms in their organizational field. Chandler, who divided up his histories into 4 periods similar to Fligstein, paid considerable attention to the role of power, of imitation within industries, along with the pragmatic search for better practice. Imitative industry effects matter, exactly as Chandler discussed at length, and as others, such as Palmer et al. (1993), have found. Efficiency matters, as Teece's earlier work so effectively demonstrated. I don't see anything in the economic sociology field that has proven Teece's finding wrong. It remains a fact.

Sometimes, simple economics helps pose the right test. There is the hypothesis in sociology that efficiency first dictates the innovation, then imitation (or institutional) effects matter (Tolbert & Zucker, 1983). Teece's (1981) article showed that early adoption of the divisional structure lead to remarkable increases in the rate of return, but these increases were not evident for late adopters. That this finding is entirely ignored in the economic sociology literature is nothing short of scandalous given its support for a basic claim in institutional theory. Perhaps the failure to cite this finding is coupled with the discomfort over the competing explanation that the competitive equilibrium changes with adoption; why should late adopters get a bang for adoption when they are, after all, late in the game? Economic sociology needs to confront (and absorb) this sensible alternative.

Fligstein's speculations that cartels lead to smaller and less diversified firms butts up against the fact that German firms acquired actively and were more diversified (depending on the level of product aggregation) than American firms (Kocka & Siegrist, 1979). French capitalism was far less cartelized or consolidated than American or German prior to World War I, and the firms were small (Kogut, 1999). They remained dramatically smaller than American firms until the 1960s. If one turns to the data in Chandler, we can show statistically that Germany, United Kingdom, and the United States nevertheless had all generated *relatively* large firms in the same industries (Kogut, 1992) despite very large institutional differences. These too are facts that deserve to be confronted.

Business history is, among other aims, the study of why firms accumulate, or fail to do so, capabilities to produce, to survive, to influence their environments. Chandler has the theory that bigness leads to capabilities; others (including myself) see bigness usually as the outcome of capabilities. Sociologists have needless trouble with capabilities, for they are skeptical of the notion of progress. They look at publications, and see fads associated with ideas. They don't realize that people stop talking about something when it becomes, very much in the sense of Schutz, Goffman, and Garfinkel, part of the common sense understanding of the world. As a consequence, sociology refuses to admit its own importance. Here we have unmistakable evidence that the economic life in many countries is dramatically better and cannot be explained by physical or human capital accumulation only, that institutional improvements and organizational innovations matter to growth. Since the tendency of economic sociology is to deny the goal-oriented nature of human pragmatism, it deprives itself of a contribution.

In this vein, it is possible to see ideologies, or conceptions of control, as problem-solving approaches. Fligstein (1990) has a pragmatic theory that firms choose CEOs from the functions best able to solve the problems of the day. Barley and Kunda (1992) imply that ideologies have a problem-solving character by showing they are correlated with economic cycles. Pareto (1983) said the same, though Barley and Kunda do not pose his more cynical theory of elites. Too bad, because looking at the strategies of knowledgeable actors represents an interesting terrain, often explored, in economic sociology (see Evans, 1992; Stark & Bruszt, 1998; Spicer, 1998). But the more important point is that beneath these problem-solving orientations, lies the accumulation of "practice," of organizational innovations that allows a worker to produce more today than yesterday. Fligstein should not be puzzled that managers of the 1950s did not adopt organizational practices of the 1970s, just like we should not be puzzled that the radios of the 1920s did not use semiconductors.

this mean that in imitating, these robots of the Zeitgeist did not studiously examine if adopting innovation A would improve performance?

Giddens is far more reasonable about this than my stereotyped sociologist response. "Why is it," Giddens (1984, p. 178) asks, "that some social forces have an apparently 'inevitable' look to them?" It is because in such instances there are few options open to the actors in question, given that they behave rationally—"rationally" in this case meaning effectively aligning motives with the end-result of whatever conduct is involved. There are many social forces that actors, in a meaningful sense of that phrase, are "unable to resist." It does not contradict sociology to hold that action is influenced by the means-end anticipation in making an economic choice, for example, the profitability of adopting an innovation.

Fligstein endorses this pragmatic actor perspective, and yet remains unnecessarily wedded to a vision of script-obeying actors, or conceptions of control that unilaterally dictate action. It is the best of sociological traditions to begin by understanding cognition as contextually grounded in individual identification with social categories (e.g., worker, gender) that reflect a social structure and stratification.[1] Because cognition—even when non-reflexive—is pragmatically sensitive to the probabilities of success within a given order, individuals are heedful of structure that tends, consequently, towards self-replication. But self-replication of order does not mean that people do not make means-ends choices.

Understanding individual action by the concepts of identity and social categories that are situated in, and reinforced by structure provides a proposal for constructing an economic sociology that is attentive to the pragmatism of everyday life. Here is where sociology will find the resources to deflect the calculative rationality of economics. But protesting that social facts matter without seriously engaging in the terrain of individual choice brings us no further than Durkheim's concluding reflections on the difference between economic and social beings in his study on the division of labor in 1895. The work by Coleman, White, Burt, Granovetter, and Zelizer among others (to focus on the United States only) presents a considerable theoretical advance since then, but the conversation is not yet coherent. It is a good time to confront this legacy by a wider intellectual embrace than suggested by either of these two engaging contributions.

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NOTE

1. I adopt this approach in analyzing the conflict of identities and changing social categories in the debate on national models of development (Kogut, 1997).

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