
What the Folk Theorem doesn't tell us

In 1968, Sid Winter wrote a wonderful essay inspired by two chapters of Schumpeter's (Winter, 2006). It will not surprise anyone familiar with the next four decades of Sid's work that his concerns in 1968 included mundane change as well as the cataclysmic variety. That is, I read Sid (then and later) as interested in routine production as well as in technological change and industry evolution. It will also not surprise such readers that Sid focused more on what I will loosely call the informational difficulties of mundane change and routine production, rather than on the incentive difficulties. Indeed, in footnote 8 he explicitly sidestepped consideration of "vested interests in the old way of doing things." That footnote (together with its successors across the decades) creates my opportunity.

In this short celebratory essay, I will try to make two points. First, an important message that I have taken from Sid's work is that mundane change and routine production are anything but. Second, I think it is time to bring interests back into our thinking about the difficulties of mundane change and routine production. To make these two points, I begin with a shockingly incomplete review of empirical work that persuades me to conjecture that there may be persistent performance differences among seemingly similar enterprises (PPDs among SSEs). I then sketch one sense of "vested interests" and suggest that we now have some of the tools to explore how such interests might increase the difficulty of organizational change (and hence partially account for PPDs among SSEs). But, in the main focus of this essay, I next sketch another sense of vested interests and suggest both that this is where new tools need to be built and that the payoff from doing so might be quite high. In particular, whereas the first sense of vested interests and the associated collection of existing tools may help us analyze the *political impediments* to change, my hope is that the second sense of vested interests and the associated tools that need to be built will provide important insights into the *organizational capabilities* that may account for some PPDs among SSEs.

The empirical literature I have in mind can be organized along several dimensions, including large- versus small-sample, firm- versus plant-level, between- versus within-firm, and profitability versus productivity as measures of performance. For example, in large-sample investigations, McGahan (1999) documents persistent profitability differences at the firm or business-unit level within narrowly defined industries, and Mairesse and Griliches (1990) find significant productivity differences (in both the intercept and the return to capital) across plants within narrowly defined industries. Work in this vein provides important evidence across broad swaths of the economy, but there is of course a limit to how homogenous a sample one can create even with 9-digit industry codes. I therefore find it useful to complement such large-sample analyses with focused studies of exceedingly similar enterprises (which of course sacrifice the appealingly broad swaths of the large-sample analyses). For

example, Ichniowski *et al.* (1997) show that up-time in 36 steel mini-mills (in 17 firms) is associated with bundles of human-resource practices, and Chew *et al.* (1990) measure 3-fold raw productivity differences across the 41 kitchens of a single firm producing airline meals (and 2-fold differences after detailed controls).

As many readers will know better than I, there are vast literatures in each of these categories: large- and small-sample, between- and within-firm, and so on. While no one paper, and perhaps no one category of papers, will completely persuade a determined skeptic that persistent performance differences might exist among seemingly similar enterprises, I find these literatures compelling enough to warrant thinking about how such differences might both arise and persist.

To suggest some partial answers to these twin questions of how performance differences arise and persist, I will focus on two kinds of incentive difficulties inherent in mundane change and routine production. I choose this incentive focus as a believer in comparative advantage, under the presumption that others will continue to pursue important work concerning what I have loosely called the informational difficulties (although nouns such as “knowledge” and adjectives such as “cognitive” and “tacit” might be more descriptive). To preview the two kinds of incentive difficulties, let me suggest that the first sense of vested interests and its emphasis on political impediments echoes a research agenda of (say) Jim March’s, whereas the second sense and its emphasis on organizational capabilities echoes an agenda of Sid’s.

Parallel to the inadequate outline of the empirical literature given above, my discussion of the political impediments to change will also be extremely brief. The existing tools that I have in mind for exploring this first sense of vested interests come from the last two decades of game-theoretic modeling of lobbying, collusion, rent-seeking, and other political behaviors within organizations. As leading examples, see Milgrom and Roberts (1988), Rajan and Zingales (2000), Rotemberg (1993), and Tirole (1986); see Gibbons (2003) for details and further references. I should note, however, that most of this work concerns inefficiency rather than heterogeneity. That is, these papers offer explanations for why organizations do not perform as well as we would like, but not yet for why a few organizations outperform the others. It seems a short but useful step to apply these ideas to analyze political impediments to change; see Meyer *et al.* (1992) and Schaefer (1998) for nice steps in this direction.

Finally, after all this preamble, I arrive at the main focus of this essay and the connection to Sid’s agenda: a second sense of vested interests and the need for new tools to explore incentive aspects of organizational capabilities and their role in explaining persistent performance differences among seemingly similar enterprises. To link this focus to “What the Folk Theorem Doesn’t Tell Us,” I begin by summarizing what the Folk Theorem does explain. Simply put, where the political literature gave us inefficiency, the Folk Theorem gives us the potential for heterogeneity.

The Folk Theorem of repeated games explores how the shadow of the future can influence behavior today.¹ In a repeated Prisoners' Dilemma, for example, it may make sense to cooperate today, even though today's payoffs are higher from defection, if cooperating today will engender cooperation from the other player in the future, whereas defection today will engender defection in the future. But perhaps the central lesson of this theorem is that the shadow of the future can support almost any behavior today, not just efficient cooperation. As an example at the other extreme, if the other player will surely defect in the future, then it makes sense to defect today. Furthermore, there is a continuum of equilibria between these extremes, and the key feature of these equilibria is that optimal behavior today depends on beliefs about how future behaviors will be conditioned on current ones.

Leibenstein (1987) was the first (that I know) to propose a potential link between this plethora of equilibria in repeated games and PPDs among SSEs. More specifically, using the language of the Prisoners' Dilemma, Leibenstein asks whether laggard plants or firms might be stuck in defect–defect equilibria and so under-perform the few enterprises that managed to build and sustain cooperate–cooperate equilibria. I find two aspects of this approach very appealing. First, in contrast to the work summarized above on political impediments to change, which makes important progress on inefficiency but pays much less attention to heterogeneity, this approach features both: there is a range of equilibria, from maximally efficient to maximally inefficient. And second, this approach accords with decades of work in sociology and management emphasizing that efficient behaviors often depend crucially on “relational contracts” (i.e. agreements too rooted in the particulars of the parties' context to be enforced by a court, but potentially self-enforced by the parties' concerns for their reputations).²

But, much as I like Leibenstein's approach, we are not home free. Specifically, we now have a story that delivers both inefficiency and heterogeneity, but we do not have a story for why different enterprises end up with different relational contracts. To the contrary, most repeated-game models of relational contracts (including those I have co-authored) simply assume that the parties can select the most efficient one. Thus, the Folk Theorem does tell us what equilibria can exist, but it does not tell us anything about building or changing equilibria. Worse yet, by its silence on these issues, the Folk Theorem might be interpreted as implicitly asserting that building and changing equilibria are trivial issues. My view, however, is just the opposite: based on many thick descriptions of real cases—including but far from limited to Lincoln Electric

¹See Gibbons (1997) for a non-technical introduction and Fudenberg and Tirole (1991) for a thorough treatment.

²See Baker, Gibbons, and Murphy (1994, 1999, 2001, 2002, 2006) not only for models of such relational contracts, references to other such models, and references to the sociology and management literatures that emphasize the ubiquity and importance of such relational contracts, but also for models of how such informal relational contracts can be facilitated by prudent choice of the formal design of the organization.

(Fast and Berg, 1975), Hewlett-Packard (Rogers and Beer, 1995), and NUMMI (Brown and Reich, 1989)—I believe that building an equilibrium (especially a cooperative one) is a very tricky business, and changing an equilibrium (especially to a new equilibrium that seems to be renegeing on the old one) is even trickier. Furthermore, not only are building and changing equilibria hard problems in the real world, but we have few existing tools for exploring these problems, so this is where I believe new tools need to be built.

I see two possibilities for new tools, both deserving further exploration. (Fair warning: describing these possibilities requires a different language and tone than the rest of this essay.) The first possibility is not really building an equilibrium, but rather is building cooperation within a non-stationary equilibrium that exists from the start, as in Watson's (1999, 2002) work on "starting small." This approach seems well worth further development, but I expect it to remain somewhat distant from Sid's agenda, for two reasons. First, such models currently offer nice insights about the slow approach to efficiency, but pay less attention to plausible sources of persistent inefficiency. Second, because such models analyze an equilibrium that exists from the start, they emphasize what I think of as the credibility/interests aspects of the problem more than the clarity/beliefs aspects, whereas a second possibility might combine the two. Here I have in mind equilibrium concepts that are weaker than Nash equilibrium, such as Fudenberg and Levine's (1993) "self-confirming" equilibrium, which allows the parties to hold divergent beliefs about events that are not on the equilibrium path. I would like to see a dynamic model that begins in self-confirming equilibrium and then—depending on the parties' beliefs about each other's strategies, which determine what actions the parties take in the early stages of the game, which determine what the parties come to believe about each other's strategies for the remainder of the game—might develop into a Nash equilibrium for the remainder of the game. In fact, in such a dynamic model, a self-confirming equilibrium might develop into an efficient Nash equilibrium or an inefficient one, thereby beginning to answer the question that remains from Leibenstein's approach to explaining PPDs among SSEs: why do different enterprises end up with different relational contracts?

To conclude, let me summarize how I hope this essay relates to Sid's agenda. One of the important things I have learned from Sid is that mundane change and routine production are anything but. As the 1968 paper notes (in an example appropriate to its day), General Motors may know how to produce cars, and General Foods cereal, but getting one to produce the other would not be trivial. Almost forty years later, there remain important issues here, both conceptually (e.g. moving beyond "production sets") and practically (e.g. explaining and perhaps redressing PPDs among SSEs).

Another important thing I have learned from Sid is that there is more to life (and even to economics) than interests. For simplicity, I summarized some of the omitted issues as "informational," but words such as "knowledge," "cognitive," and "tacit" are often more descriptive. It is widely accepted that tacit knowledge is important for individual decision-makers (imagine, e.g., using words to teach someone how to ride

a bike), but it is less thoroughly explored how two or more decision-makers reach or change their collective tacit knowledge. In this essay, I have tried to suggest that a related problem arises regarding players' strategies in a dynamic game.

In short, Sid's work has encouraged me to see the problem of routine production (i.e. making cars or cereal) as the problem of routine production (i.e. building an equilibrium). But building an equilibrium means that interests creep in; one cannot analyze just the evolution of beliefs. Nelson and Winter's (1982: 107–12) discussion of "routine as truce" begins to combine the informational and incentive aspects in something like the way I have tried to suggest here. I hope that we will soon see important progress on both enrichments of the theory of repeated games, by building tools that tell us what the Folk Theorem does not, and applications of these tools as part of the explanation of and solution to persistent performance differences among seemingly similar enterprises.

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