

**Team Theory, Garbage Cans, and Real Organizations:  
Some History and Prospects of Economic Research on  
Decision-Making in Organizations**

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This essay attempts to articulate and advance a long-term agenda for organizational economics. The essay begins with a general discussion of four specific items on this agenda, and then moves to a more specific discussion of decision-making in organizations – beginning with two polar-opposite conceptions (team theory and garbage cans) and then describing several recent economic models. Part of the rationale for writing this essay is that this agenda has both substantive and methodological commonalities with the work of James March.

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Twenty years ago, I took a mandatory doctoral course on organization theory from Jim March. It has stuck with me ever since, probably more so as time passes. In this celebratory essay, I try to say a bit about why.

What I remember most *about* the course is that Jim opened each session by cold-calling a student, asking for a fifteen-minute summary and assessment of the session's central paper. These cold-calls were ostensibly aided by a roulette wheel in Jim's head, guaranteed to randomize the terror. But late in the term I became convinced that the wheel was fixed: after I had been either absent or uncharacteristically quiet for several sessions, I recall sitting with my feet on a chair and a banana in my mouth when my number came up.

What I remember most *from* the course is a different story. We read the classics of the day. More importantly, we read papers that have since become classics. I have found both in my files, often with notes in the margins (of confusion, indignation, and even grudging acceptance) showing that I read them. These papers have functioned like little time bombs: some wait for me to uncover them in my files; others go off unbidden, buried in my memory. Regardless of the detonation method, I always get the same feeling – there's Jim, wondering if I have finally gotten the message on this one. It's like the banana all over again.

Beyond these classic papers, there is something else I learned from the course. It is bigger, and more important, but also harder to describe. It has something to do with both style and sensibility, in research and in life. Since I can't describe it, I'll resort to what I consider to be one of the great Marchian aphorisms (which, consistent with its message, I will not only misattribute but also utterly misquote): "T.S. Eliot once said that the value of a poem is in the ideas it inspires in the heads of its readers."

I've had lots of ideas inspired by my adviser and friend, the poet, Jim March. I hope this essay shows how a few of them have come home to roost.

**Team Theory, Garbage Cans, and Real Organizations:  
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by

Robert Gibbons<sup>1</sup>

For two hundred years, the basic economic model of a firm was a black box: labor and physical inputs went in one end; output came out the other, at minimum cost and maximum profit. Most economists paid little attention to the internal structure and functioning of firms or other organizations. During the 1980s, however, the black box began to be opened: economists (especially those in business schools) began to study incentives in organizations, often concluding that rational, self-interested organization members might well produce inefficient, informal, and institutionalized organizational outcomes.

Of course, organizational sociologists (and others outside economics) have long appreciated that organizations are typically not well-oiled machines. For example, the classic case studies by Blau (1955), Crozier (1964), Dalton (1959), Gouldner (1954), and Selznick (1949) depict organizations that differ radically from a hypothetical Weberian bureaucracy, with its “precision, speed, expert control, continuity, discretion, and optimal returns on input” (Merton, 1940: 561). Instead, in the post-Weberian view, “rules are often violated, decisions are often unimplemented, ... and evaluation and inspections systems are subverted.” Moreover, “informal structures deviate from and constrain aspects of formal structure, and ... the organization’s intended, rational mission [is undermined] by parochial interests.”<sup>2</sup>

Since the ‘80s, economics has been making glacial progress on what I hope is a long-term agenda: to document and accelerate the convergence and interplay between new economic models and long-standing non-economic insights about organizations. In this essay, I try to articulate and advance this agenda, first in general terms and then with a specific focus on decision-making in organizations.

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<sup>1</sup> Segments of this essay draw on two published papers (Gibbons, 1998a and 1999) and an unpublished paper (Gibbons, 2000).

<sup>2</sup> Observations in this spirit can be found in dozens of authoritative sources. These quotations happen to be drawn from Meyer and Rowan (1977: 343) and DiMaggio and Powell (1991: 12), respectively.

I begin with an overview of the long-term agenda. More specifically, I unpack “document and accelerate ... convergence and interplay” into four items on the agenda: (1) documenting convergence (between new economic models and long-standing non-economic insights about organizations), (2) proving inevitability (that economic models that take their foundations seriously will deliver a post-Weberian view of organizations), (3) proposing interplay (because economic modeling offers more than just a new language for re-expressing established ideas), and (4) doing something (to improve an organization’s performance and the lives of those who live in it). After describing these four agenda items, I suggest how both the substance and the methodology of this agenda are closely aligned with parts of Jim March’s work.

Of course, a discussion in such general terms leaves one craving particulars. I therefore shift to a more focused discussion of decision-making in organizations. I begin with a summary of two polar approaches to the subject – team theory and garbage cans – and argue that real organizations lie between these extremes. I then describe recent economic models that explore this realistic middle ground, including models of informal authority, power and politics, and lobbying. I choose this focus on decision-making in organizations partly because much of the recent economic research in this area is entering Marchian territory (whether it knows it or not), but also because this literature does a decent job illustrating the four agenda items described above. Patient readers interested in further (and more detailed) illustrations might consult Gibbons (2004).

To shape expectations, let me conclude this Introduction with two caveats: first, I really do mean “decision-making;” second, I really do mean “in organizations.” Thus, I will blithely ignore both (a) important behaviors in organizations that do not conform to my (economist’s) notion of decision-making and (b) important insights into decision-making that apply well outside organizations. For example, Nelson and Winter (1982) on routines and March, Schulz, and Zhou (2000) on rules are excluded by my first test, and Tversky and Kahneman (1974) on heuristics and Ross (1977) on attributions by my second.<sup>3</sup> For the duration of this essay, therefore, I will single-mindedly consider the “logic of consequences” approach described in March (1994), but Akerlof and Kranton’s (2000)

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<sup>3</sup> I trust it is clear why I exclude heuristics and attributions: no matter how important these issues are, they are not inherently organizational issues, and one has to limit the scope of an essay somehow. But it may be less clear why I exclude routines and rules. In this case, the reason is again partly scope, but now also partly style. In terms of scope, routines and rules feel a bit too “macro” in their level of analysis: the models I will discuss consider decisions by individuals, whereas some of the most important applications of routines and rules apply to groups. And in terms of style, routines and rules feel different from the economic models I want to survey: in many discussions of how individuals are guided by routines or by rules, an economists’ conventional focus on calculative decision-making is at least submerged, if not entirely lost. In short, this essay’s focus really is “economic research on decision-making in organizations.”

work on “Economics and Identity” gives me hope that economists will soon begin to grapple with March’s complementary “logic of appropriateness.” I briefly return to these issues in the Conclusion.

### **1. Glacial Progress on a Long-Term Agenda**

During the ‘80s, several prominent economists wondered (in print) whether the time might be ripe for a dialogue between economists and non-economists about organizations.<sup>4</sup> And during the ‘90s, many more economists began to explore this possibility. My own contribution to the latter genre was “Game Theory and Garbage Cans: An Introduction to the Economics of Internal Organization” (Gibbons, 1998a). In this section, I will use this paper and two others that followed to outline a long-term agenda for organizational economics, leading to the conclusion that this agenda has important substantive and methodological commonalities with the Marchian oeuvre.

“Game Theory and Garbage Cans” summarized economic models of problematic pay-for-performance schemes, wasted or non-existent investments in human capital, lobbying and other influence activities, the vagaries of subjective management practices, and herd behavior and group think. I chose these models for two reasons. First, these models showcase the rich and flexible toolkit of organizational economics – not only incomplete contracts and specific investments (tools from transaction-cost economics), but also agency theory, repeated games, and information economics. Second, and more important, these models are consistent with the spirit (if not yet the details) of the post-Weberian view of organizations – one can sense that these are organizations rife with rule violations, unimplemented decisions, subverted inspections, parochial interests, and undermined missions. More specifically, these models deliver inefficient, informal, or institutionalized organizational outcomes, which are three important respects in which non-economist students of organizations often suggest that real organizations depart from economic models.

I hope “Game Theory and Garbage Cans” made some progress documenting the ongoing convergence between new economic models and long-standing non-economic insights about organizations. I continue to believe that existing and potential models in organizational economics can come closer to capturing life in organizations than seems

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<sup>4</sup> See, for example, Kreps (1990), Milgrom and Roberts (1988), Tirole (1986), and Williamson (1990).

widely recognized outside. And I hope to convert not one but two audiences to this view: not only non-economists who assume that economic models predict efficiency, but also black-box economists who assume that real organizations achieve efficiency.<sup>5</sup> But I hasten to add, by discussing “convergence,” I do not mean that disciplinary boundaries will, should, or even could disappear; instead, I mean that the intersection region of the relevant Venn diagram is growing in size, interest, and activity.

There is much more work to be done to document convergence, a bit of which will occur in the discussion below of decision-making in organizations. But there are also important parts of the long-term agenda beyond documenting convergence. In “Taking Coase Seriously” (Gibbons, 1999), I sketched two more: first, convergence is inevitable, not accidental; second, there can be interplay between the disciplines that study organizations, not just convergence.

By “convergence is inevitable,” I mean that it is a logical implication of the seminal paper in organizational economics (Coase, 1937) that organizations will have great difficulty being well-oiled machines of the kind Weber envisioned. That is, economic models that take their underlying assumptions seriously must deliver a post-Weberian view of organizations: rule violations, unimplemented decisions, subverted inspections, parochial interests, and undermined missions will be persistent problems, not exceptions. More specifically, Coase’s famous argument (that firms exist only where they perform better than markets would) has the following long-dormant corollary: the firms we observe will be less efficient than the markets we observe, even though the firms we observe will be more efficient than the markets they replaced. Since this corollary is based on sample selection, I called it “Coase (1937) Meets Heckman (1976).”

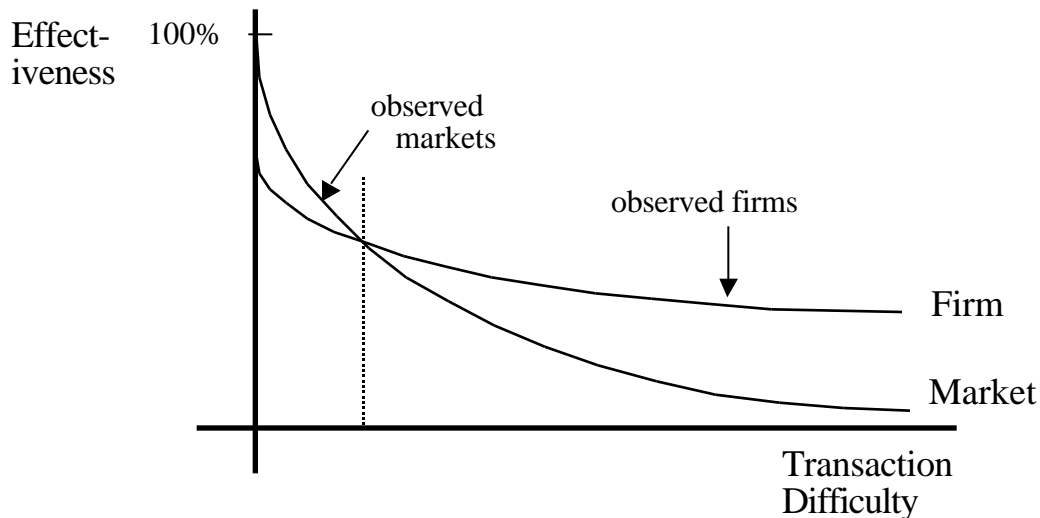
Figure 1 illustrates both Coase’s original argument and its long-dormant corollary, by plotting the declining effectivenesses of market governance and of firm governance as transaction difficulty increases (*e.g.*, as imperfect contracts and specific assets become more problematic).<sup>6</sup> At the critical value of transaction difficulty indicated by the dotted line,

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<sup>5</sup> Some non-economists may harbor the misconception that economic models predict that rational, self-interested people will achieve efficient outcomes. In fact, an economic model’s prediction of efficiency rests more on its assumptions about the environment than on those about the people. In a social dilemma or commons problem, for example, each person’s incentive is to free-ride (*i.e.*, to contribute only as much as is warranted by the resulting increase in his or her own benefit, ignoring the benefits to others), so rational, self-interested individuals are predicted to achieve an inefficient group outcome. The recent economic models of internal organization are like those of social dilemmas: rational, self-interested people are again predicted to produce an inefficient outcome.

<sup>6</sup> For now, I intend this figure to be only suggestive. In this spirit, “effectiveness” means the ratio (expressed as a percentage) of (1) the social surplus actually achieved by the indicated governance structure (firm or market) in a transaction of the indicated difficulty to (2) the maximal social surplus that could be produced from the indicated transaction if governance were flawless and costless. For example, if your value

markets and firms are equally effective governance structures. Coase's original argument is that transactions to the right of the dotted line will be governed by firms, to the left by markets. The corollary follows from comparing the observed effectiveness of firms (to the right of the dotted line) with the observed effectiveness of markets (to the left): the latter is superior, especially as transaction difficulty falls to zero, at which point market governance produces the efficient outcome familiar from neoclassical economics.



**Figure 1. Coase (1937) Meets Heckman (1976)**

In brief, the Coasian corollary asserts that firms will not be oblivious to conditions that wreck markets. As with the first part of the long-run agenda, documenting convergence, there is again much more work to be done in proving inevitability. For example, it will be important to have a formal model from which Figure 1 can be derived. I hope to produce such a model in the near future, probably beginning from the model in Baker, Gibbons, and Murphy (2001). But I will now turn instead to the third part of the long-term agenda, proposing interplay: economic modeling offers more than just a new language for re-expressing established ideas, so there can be interplay between the relevant disciplines, not just convergence.

To suggest how such interplay might occur, in "Taking Coase Seriously" I built a formal model of the argument given on one page of Crozier's (1964: 45) *Bureaucratic*

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from consuming a bag of salt is \$10 and my cost of producing it is \$6 then the maximal social surplus from our transaction is \$4, but if we must spend \$1 on a lawyer to write a contract then effectiveness is 75%. One could define "transaction difficulty" to be the collection of features that cause the effectiveness of market governance to decline, as shown in the figure, but it does not necessarily follow that the effectiveness of firm governance then declines as transaction difficulty increases, as assumed in the figure. For more on these issues, see the discussion of future work below.

*Phenomenon.* Unfortunately, Crozier's one page took me five pages to model! Even I could see "Why bother?" pursed on organization theorists' lips, so I attempted to explain several values that formal modeling might contribute to a research literature.

First, formal models can check the internal consistency of informal arguments. Seemingly airtight informal arguments are sometimes wrong, and are sometimes right only if additional assumptions are imposed. Often, the additional assumptions exposed by the formal modeling can be viewed as boundary conditions for an informal argument that is correct except for its omission of such conditions.

Second, formal models can help to specify and interpret empirical tests. This point is related to the first, but here the potentially troublesome step is the informal translation of a theoretical prediction into an empirical test, regardless of whether the theoretical prediction is derived from a formal or an informal theory: sometimes the translation is wrong, or is right only if additional assumptions are imposed.

Third, developing formal models in rich contexts sometimes allows the analysis to push through to conclusions when informal analysis would have ground to a halt. For instance, even if two informal arguments are both airtight and simple, it may be difficult to think informally about how the arguments interact, yet their interaction may produce new insights or predictions.

This last point was put much more eloquently by Coleman (1964: vii): "If conceptual elaboration is to progress beyond the proverbs of the ancients, special tools are necessary. The most remarkable of these is mathematics. ... The mind falters when faced with a complex system or a long chain of deductions. The crutch that mathematics provides to everyday reasoning becomes essential as sociology moves toward the analysis of complex systems and predictions based on extended chains of deductions."

Lest there be any doubt, let me reiterate that I do not believe that economic models capture all the important aspects of organizational design and performance, nor do I believe that economic models are the only (or best) strategy for organizational research (Gibbons, 1999: 146). To the contrary, I think the most successful literatures are those that blend detailed description, informal theory, and formal modeling. Against this standard, I think that organizational economics has too little description and informal theory, but also that many parts of organization theory have too little formal modeling.<sup>7</sup>

So far, I have described initial progress on three parts of a long-term agenda: documenting convergence, proving inevitability, and proposing interplay. Unfortunately, the

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<sup>7</sup> Recent work on the organizations and institutions of government may be getting the blend about right. For example, on limited government, see North and Weingast (1989) for description and informal theory and Weingast (1995, 1997) for formal models inspired by detailed descriptions, and on government agencies, see Wilson (1989) for description and informal theory and Moe (1997) for a survey of informal theory and formal models.

first two of these parts paint a pretty bleak picture of life in organizations. “Game Theory and Garbage Cans” summarized economic models of problematic pay-for-performance schemes, wasted or non-existent investments in human capital, lobbying and other influence activities, the vagaries of subjective management practices, and herd behavior and group think – all of which bode ill for organizational performance. And “Taking Coase Seriously” argued that unimplemented decisions, subverted inspections, parochial interests, and undermined missions will be persistent problems in organizations. Is there nothing to be done?

In “Why Organizations Are Such a Mess (and What an Economist Might Do About It)” (Gibbons, 2000), I proposed a fourth part of this long-term agenda: doing something. I suggested that a crucial source of superior organizational performance involves the creation and management of “relational contracts” (*i.e.*, informal agreements that are too rooted in the parties’ shared experiences to be enforced by a court, but that can nonetheless be enforced by the parties’ interests in the future of their relationship).<sup>8</sup> Furthermore, in addition to suggesting that relational contracts are important (both within and between firms), I hazarded the hope that they could be managed – both directly and indirectly, as described below.

In making this argument, I was inspired by the many observers who have emphasized the importance of relational contracts both within organizations (*e.g.*, Barnard, 1938; Simon, 1947; Blau and Scott, 1962) and between (*e.g.*, Macaulay, 1963; Dore, 1983; Powell, 1990). Granovetter (1985: 502) summarized this large literature with:

The distinction between the ‘formal’ and the ‘informal’ organization of the firm is one of the oldest in the literature, and it hardly needs repeating that observers who assume firms to be structured in fact by the official organization chart are sociological babes in the woods.

For my purposes, Blau and Scott [1962: 6] went an important step further, asserting that formal and informal aspects not only co-exist but interact:

It is impossible to understand the nature of a formal organization without investigating the networks of informal relations and the unofficial norms as well as the formal hierarchy of authority and the official body of rules, since the formally instituted and the informal emerging patterns are inextricably intertwined.

I think that stellar management of relational contracts is a skill that is both rare in the world and inexpressible in current economic models. A bit more specifically, in “What An

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<sup>8</sup> See Section 2.A of Gibbons (2000) for a six-page unpacking of this terse definition.

Economist Might Do About It,” I argued that conceiving, communicating, and implementing relational contracts are hard tasks, but building, maintaining, and changing relational contracts seem even tougher. There is beginning to be some work in economics along these lines, but delivering big progress will require innovations in the theory of repeated games, not just applications of the existing “Folk Theorem” to new questions. I hope that in a decade or so, theory will catch up with best practice, enabling someone to write a paper here provisionally titled “What the Folk Theorem Didn’t Tell You.” Thus, this research area may become a leading example of Kreps’s observation that “A game-theoretic theory of organizations will do more for game theory than game theory will do for it.”<sup>9</sup>

Although improvements in the theory of repeated games are prerequisites to modeling the direct management of relational contracts, indirect management can be modeled with existing tools. In a series of papers, George Baker, Kevin J. Murphy, and I have explored several interactions between formal and informal organizational structures. For example, our 1994 paper on subjective performance assessments studies the joint use of both objective and subjective performance measures, focusing on how the presence of the latter changes the optimal use of the former. Similarly, our 1999 paper on informal authority asks how the organization’s formal authority structure affects and is affected by the possibility of informal authority relationships. More recently, we have taken analogous approaches to “bringing the market inside the firm” (2001), vertical integration (2002), and strategic alliances (2003). This series of papers implicitly argues that superior organizational performance typically cannot be achieved simply by optimizing the available formal instruments – such as incentive plans, job definitions, reporting relationships, resource-allocation processes, and formal contracts between firms. Instead, one needs both to manage the relational contracts directly and to choose the formal structure to facilitate the relational contracts indirectly.

Let me conclude this overview where I started: trying to snag a little reflected legitimacy by claiming that the four items on this long-term agenda – documenting convergence, proving inevitability, proposing interplay, and doing something – are consistent with pieces of the Marchian oeuvre. First, regarding disciplinary convergence (or at least overlap), consider Padgett’s (19xx: 745) tabulation showing that March’s citations come in large numbers not only from the disciplines I have discussed, economics and sociology/organization theory, but also from political science, psychology, law, business,

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<sup>9</sup> Kreps (1996: 562) says something along these lines. The quotation above is from a pre-publication version, approved for use here by personal communication with the author.

public policy, and education. Second, as a near-proof that organizations will inevitably have great difficulty being well-oiled machines, consider March's (1962) "The Business Firm as a Political Coalition," which needs only a few Greek letters to make it a game-theoretic account of the existence and consequences of politics in organizations. Third, on the role of formal models in advancing a literature, consider Cohen, March, and Olsen's "Garbage Can Model of Organizational Choice," on which much more below.<sup>10</sup> Finally, as for "doing something" (*i.e.*, developing a research literature that improves life in organizations), consider Jim's abiding interest in "decision engineering" (the production of decisions that are intelligent; March, 1994: 222). Appropriately, therefore, I now turn to this essay's main focus, decision-making in organizations.

## 2. Team Theory, Garbage Cans, and Real Organizations

To set the stage for Section 3's discussion of recent economic models of decision-making in organizations, in this section I first summarize two polar approaches to the subject (team theory and garbage cans) and then argue that real organizations lie between these extremes.

### 2.1 Team Theory

Marschak and Radner's (1972) *Economic Theory of Teams* summarized and advanced "team theory," which was the first attempt to develop economic models of decision-making in organizations. Team theory is the application of statistical decision theory to "team" settings, where different agents have different information and control different actions but share a common objective (such as maximization of the firm's profit). Whereas single-person statistical decision theory computes the decision rule that maximizes the decision-maker's expected payoff, team theory computes a set of decision rules (one for each agent) so that the organization as a whole maximizes its expected payoff.

Because team theory envisions different agents controlling different decisions and taking their decisions based on different information, the theory analyzes (some aspects of) decentralized decision-making. But all team-theoretic models share one key feature: they ignore the interests of the team members—there is no shirking, free-riding, lying, lobbying,

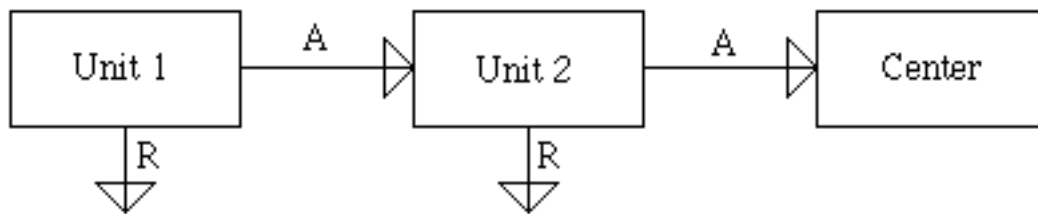
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<sup>10</sup> I could instead have offered Chapter 3 of March and Simon (1958), which contains flow-chart models of three classic theories of bureaucracy – Merton (1940), Selznick (1949), and Gouldner (1954). Good thing Crozier wrote after March and Simon, or they would have modeled his theory, too, thereby scooping my 1999 effort.

or strategizing of any kind. In this sense, team theory takes a Weberian view: the organization is a machine; its parts can be designed (and their interactions controlled).

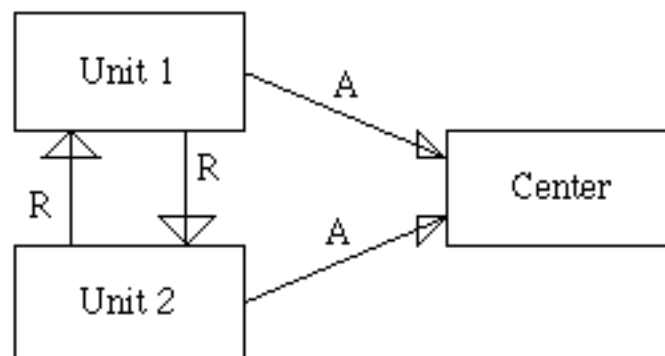
As a simple example in the spirit of team theory, consider Sah and Stiglitz's (1986) comparison of decision-making in two organizational forms that they call hierarchy and "polyarchy." Suppose that the organization consists of a Center and two Units, 1 and 2. The organization is trying to select new projects. A project's value,  $y$ , is either 0 or 1; absent any other information, the probability that  $y = 1$  is  $p$ . Each Unit gets a signal (for free) about each potential project's quality. The signal,  $s$ , is either good (G) or bad (B). Conditional on the true (but unknown) value of  $y$ , the signals observed by the two Units are independent. For example, if  $y = 1$ , then each Unit is more likely to get the G than the B signal, but either or both could get the B signal.

Suppose that each Unit's decision rule is to approve the project (A) if the Unit observes  $s = G$  but to reject the project (R) if  $s = B$ . We can then define two organizational forms, as shown in Figures 2 and 3: one that requires unanimous approval to adopt a project (hierarchy), and another that requires unanimous rejection to stop a project (polyarchy), as shown below.



**Figure 2. Hierarchy**

In a hierarchy, Unit 2 sees only those projects that Unit 1 approves. In a polyarchy, in contrast, either Unit can unilaterally approve a project, and Unit 2 gets to consider projects that Unit 1 rejects (and vice versa).



### Figure 3. Polyarchy

Both a hierarchy and a polyarchy will adopt a project if both Units' signals are  $s = G$ , and both will reject a project if both signals are  $s = B$ . The difference between the two organizational forms is that a hierarchy will reject a project when the Units' signals disagree (one  $G$  and one  $B$ ), whereas a polyarchy will accept such a project.

Suppose the cost of undertaking a project is  $c$ , where  $c$  is small enough that it is optimal to undertake a project when both signals are  $s = G$  but large enough that it is optimal to reject a project when both signals are  $s = B$ . (That is,  $\text{Prob}\{y = 1 \mid G, G\} > c > \text{Prob}\{y = 1 \mid B, B\}$ .) Towards the high end of this range of possible values for  $c$ , it will be optimal to reject a project with mixed signals; toward the low end, optimal to accept. The choice between the organizational forms of polyarchy and hierarchy thus amounts to whether  $c$  is large enough that projects with mixed signals should be accepted or rejected. If  $\text{Prob}\{y = 1 \mid G, B\} > c$ , then projects with mixed signals should be accepted (in which case a polyarchy is the superior organizational form); if  $\text{Prob}\{y = 1 \mid G, B\} < c$ , then such projects should be rejected and so hierarchy is superior.

In this model, as in statistics, there are two types of error: rejecting good projects and accepting bad ones. The optimal organizational form depends (solely) on the losses associated with these two types of errors. If the loss from accepting a bad project is large, then a conservative (or "guardian") decision structure is superior; if the loss from rejecting a good project is large, then a liberal (or "star") structure is superior.<sup>11</sup> With more than two Units, other organizational forms are possible (such as committees and majority rule), reflecting other compromises between the two types of errors.

In the Sah-Stiglitz model, decisions are based on information that Units receive exogenously. Natural extensions could incorporate gathering, communicating, and processing information, each of which may be costly. Furthermore, in a full team-theoretic analysis of the Sah-Stiglitz problem, the Units' decision rule (namely, approve the project if  $s = G$ , but reject it if  $s = B$ ) would be derived as the optimal rule for the organization, given each Unit's information and feasible actions. For example, if hierarchy and polyarchy are

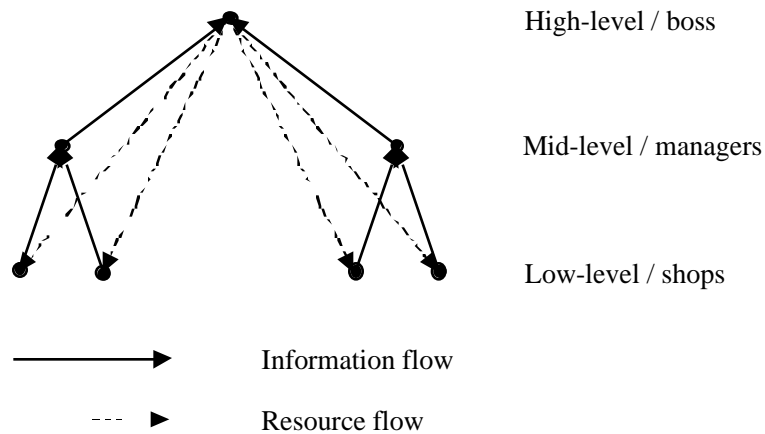
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<sup>11</sup> Jacobs (1981) offers the following definitions of guardian, star, and foot-soldier jobs. In a guardian job (such as piloting a plane), the feasible downside outcomes range from bad to truly terrible (crashing the plane), but the feasible upside outcomes are quite limited (a smooth landing). In a star job (such as a basic researcher), in contrast, the feasible upside outcomes range from good to truly terrific (a world-changing idea), but the feasible downside outcomes are limited (no ideas whatsoever). Finally, in a foot-soldier job, all the outcomes are limited; individual performance cannot produce extremely bad or extremely good outcomes. Jacobs's ideas about jobs have obvious parallels to organizational structures, and to the appropriate matching between structures and environments.

both feasible organizational forms, then the Units' feasible actions apparently include implement (I), discard (D), and transfer (T), where hierarchy uses D (= R) and T (= A) but polyarchy uses I (= A) and T (= R).

Recent work along these lines has distinguished decentralized information processing from decentralized decision-making. Under decentralized information processing, different agents observe different information and communicate subsets of their observations, but a single agent ultimately receives the final communications and makes the decisions. Under decentralized decision-making, different agents observe different information and control different decisions, but there is no communication (and hence no decentralized information processing, in the sense of multiple agents contributing to a final report). Radner (1993), Bolton and Dewatripont (1994), and Van Zandt (1999) study the former; Geanakoplos and Milgrom (1991) and Garicano (2000) study the latter.

Decentralized information processing provides a new perspective on organization structure. As Radner notes, hierarchical structures, "which are usually thought of as the epitome of the centralization of authority, are also remarkably effective in decentralizing the activities of information processing" (p. 1110). For example, consider the problem of finding the maximum of eight numbers: one agent can find the maximum in seven steps (by comparing the first two numbers, then comparing the larger of these two to the third, and so on), but seven agents organized in a hierarchy can find the maximum in three steps (as shown by the solid arrows pointing up the tree in Figure 4). Of course, hiring more agents may be costly, but delay may be costly as well. For example, after one agent takes seven steps to find the maximum, the world may have changed so much that the answer is almost useless. Furthermore, if there is a flow of problems over time (rather than just a one-shot problem), then all the agents in a hierarchy may be busy all the time: on Wednesday, the four low-level agents (shops) take the first step on today's problem, while the two mid-level agents (managers) take the second step on Tuesday's problem, and the single high-level agent (boss) takes the final step on Monday's problem.



**Figure 4. Information Processing and Resource Allocation**

Van Zandt (2001) analyzes both decentralized information processing and decentralized decision-making, and notes that the structure of the latter need not mimic the structure of the former. For example, suppose that information processing occurs via the hierarchy just described (*i.e.*, the four shops take the first step, the two managers take the second, and one boss takes the third), but now suppose that the boss must then decide how to allocate resources for the shops. One possibility is to allocate resources by running the information-processing hierarchy in reverse: the boss allocates resources to the managers, who then use newer information to allocate resources to the shops. But this resource-allocation process takes two steps, so if delay is costly then it may be better for the boss to allocate resources directly to the shops (shown by the dotted lines pointing down the tree in Figure 4), even though such skip-level resource allocation utilizes coarser and older information.

To recap: Team theory was the first attempt to develop economic models of decision-making in organizations. Because different agents control different decisions and take their decisions based on different information, team theory analyzes (some of) the costs and benefits of decentralized decision-making. After three quiet decades, team theory may be beginning something of a resurgence (in part as an appropriate reaction to economists' overwhelming focus on incentive issues). For example, recent work distinguishing decentralized information processing from decentralized decision-making promises to uncover important efficiency considerations in the design of organizations. But team-theoretic models perpetuate a Weberian view of organizations as machines, with parts that

can be designed (and interactions that can be controlled). Thus, team theory may yet have an important story to tell, but it cannot be the whole story on decision-making in organizations.

## 2.2 *Garbage Cans*

I find it doubly ironic that 1972 saw the publication not only of Marschak and Radner's *Economic Theory of Teams* but also of the antithesis of team theory: "A Garbage Can Model of Organizational Choice," by Cohen, March, and Olsen (hereafter CMO). Whereas team theory envisions an organization whose members compute and execute optimal communication and decision rules to maximize organizational efficiency, the garbage-can model envisions "organized anarchy," featuring "collections of choices looking for problems, issues and feelings looking for decision situations in which they might be aired, solutions looking for issues to which they might be the answer, and decision makers looking for work" (p.1). In short, while team theory used statistical decision theory to probe the microanalytics of Weber's theory of bureaucracy, the garbage-can model used a Fortran program to discover the stochastic tendencies of a post-modern organization theory. There are thus two sources of irony: substantive (these polar opposites were published in the same year) and methodological (these opposite substantive focuses were pursued via closely related methodologies – statistical decision theory and computer simulation, both of which were used to explore how organizations respond to change and uncertainty).

Many readers of this Festschrift will understand the garbage-can model better than I do, but in the spirit of Eliot (paraphrased above as "the value of a poem is in the ideas it inspires in the heads of its readers"), I offer the following personal inspirations from CMO's Fortran poetry. More specifically, I will offer two sets of interpretations of the assumptions and the results of the garbage-can model: one set drawn from mainstream economics (although not with team theory), and a second set drawn from edgier economics (such as behavioral economics). I begin by discussing the model's assumptions, and then turn to its results.

The garbage-can model is intended to describe an organization plagued by "problematic preferences," "unclear technology," and "fluid participation." The model is not intended to claim that all organizations satisfy these three assumptions all the time, but I think these assumptions do capture phases that many organizations experience (and some seem stuck in). I will not discuss how the garbage-can model operationalizes these three assumptions, but will instead describe how an economist might model each of them – in both mainstream and edgier ways. The mainstream approaches get only the signs right (*i.e.*,

they deliver the necessary qualitative features, but without really capturing the spirit of the garbage-can model).

Problematic preferences mean that the organization as a whole does not have a preference ordering of the kind assumed in the theory of choice (and in team theory). A bit more evocatively, CMO envision an organization “without consistent, shared goals” (p. 2). Of course, aggregation arguments from Arrow (1951) through March (1962) and beyond show how individuals with consistent preferences may be inconsistent in the aggregate. So it is certainly possible to build mainstream economic models of problematic organizational preferences. In fact, there are recent economic models of managerial “vision” that analyze how life can indeed be easier for organizations with “consistent, shared goals” (Rotemberg and Saloner, 2000; Van den Steen, 2002). But CMO also envision an organization that “discovers preferences through action more than it acts on the basis of preferences” (p. 2). A die-hard mainstream economist might interpret this statement in terms of Bayesian learning, but I doubt that this is all that CMO have in mind. In particular, CMO’s organization seems not to know what it does not know (until it discovers it), whereas the uncertain variables in a Bayesian model have known sets of feasible values (and learning changes the decision-maker’s belief about which values from these known sets are likely). I do not know of a behavioral-economics model in which actors “discover” their preferences, but there are several non-Bayesian in which actors could be said to discover (rather than “learn”) their payoffs.

Unclear technology means that the organization’s “own processes are not understood by its members” (p. 2). Again, one could offer a mainstream interpretation in terms of Bayesian learning: the members know all the ways that the processes might function, and they are learning about which of these ways are likely. (Even team theory fits this description.) But CMO also describe the organization’s understanding as “the residue of learning from the accidents of past experience” (p. 2). Again, this seems like an organization that does not know what it does not know, perhaps along the lines of Mullainathan’s (2002) “Thinking Through Categories,” in which decision-makers use categories as likelihood functions for making inferences, but can get stuck on the wrong category (a result akin to a behavioral version of the bandit problem).

Finally, fluid participation means that “the audiences and decision-makers for any particular kind of choice change capriciously” (p. 2). There are of course mainstream models of optimal mobility between organizations (*e.g.*, Jovanovic, 1979), and it would be straightforward to develop analogous models where organizational events such as promotions and restructurings make it optimal for organization members to shift their

attention from one forum to another, even if they do not leave the organization. But CMO seem to mean something more behavioral when they say “not everyone is attending to everything all the time.” Even this can be given mainstream interpretations, however. For example, in the Aghion and Tirole (1997) model discussed in Section 3, it can be optimal to give the boss a large span of control, so that the probability that the boss becomes informed about any one agent is not too high, so that the agent has an incentive to search for desirable projects (since if the agent proposes a project and the boss does not overrule it then the agent will get to implement the project). But I do not mean that there cannot or should not be a behavioral model of CMO’s observation about bounded (and hence fluctuating) attention; I just do not know of such work to date.

I have thus far discussed the garbage-can model’s three assumptions and argued that (1) they are not inconsistent with mainstream economic models, but (2) these mainstream approaches do not really capture the garbage-can spirit, whereas (3) edgier models may come quite close to capturing this spirit. Given this assessment of the garbage-can model’s assumptions, I now turn to the model’s results.

In the garbage-can model, decisions are made in one of three modes: resolution, oversight, and flight. To describe these decision modes, we need more detail about three of the model’s ingredients: decision-makers, problems, and choices. Decision-makers and problems have energy supplies and demands, respectively, which fluctuate stochastically over time. At a given moment, a particular decision-maker (or problem) is attached to at most one choice. Over time, however, decision-makers and problems move stochastically across choices (perhaps completely randomly, or perhaps influenced by a simple access structure). A choice is made at the first moment when the total energy supplied by the decision-makers attached to that choice exceeds the total energy demanded by the problems attached to that choice. When a choice is made, both the choice and any problems currently attached to it disappear from the garbage can.

In this model, “resolution” means that the choice has been working on a collection of problems (*i.e.*, has had these problems attached to it in the recent past), but only now has more energy supplied by decision-makers than demanded by these problems. In contrast, “oversight” means that a new choice is made quickly, before (m)any problems become attached to it, with a minimum of time and energy necessary from decision-makers. Finally, “flight” means that a choice has not been made for some time (because of the energy demanded by the problems attached to that choice), but then the problems move to another choice, so the original choice is then made but no problems are resolved (since they are now attached to other choices).

These decision modes are not the results of the garbage-can model. Instead, these modes are further assumptions – not macro assumptions about the organization itself, but rather micro assumptions about the processes through which decisions can be made. The results of the model are then the simulation’s findings about which decision modes are likely (and under what conditions). The first major result is that decision-making by flight and by oversight are much more likely than by resolution. Put more evocatively, many choices will be made that do not resolve (m)any problems. Sadly, this seems all too common in many organizations. A second result is that decision-makers and problems track each other across choices. As CMO put it, “one would expect decision-makers who have a feeling that they are always working on the same problems in somewhat different contexts, mostly without results” (p. 11). This, too, seems sadly plausible.

I conjecture that a mainstream economic model could deliver both the decision modes and these two results of the garbage-can model. As already noted, the decision modes are really further assumptions, not results. That is, while a mainstream model of a single decision-maker should have only resolution, to the exclusion of oversight and flight, a mainstream model of multiple decision-makers with problematic (aggregate) preferences and fluid participation, as described above, will generate oversight and flight as long as something akin to the CMO assumptions about decision-makers, problems, and choices are built into the model. So my conjecture is really that such a mainstream model would yield the two garbage-can results described above: flight and oversight are more common than resolution, and decision-makers and problems track each other across choices.

In sum, I find the garbage-can conception of organizations usefully provocative: it may overstate the level of anarchy in many organizations, but it provides desperately needed contrast to the Weberian models from team theory. More specifically, the first lesson I take from the garbage-can model (and from many of March’s other contributions) is that it is often not useful to think of an organization as a single, unified, rational decision-maker—as economics did for 200 years, and as team theory continued to do. A second lesson (which I have taken on board less fully than I perhaps should and someday might) is that it is often not useful to think of an individual as a single, unified, rational decision-maker. Both of these lessons come not only from the formulation and results of the garbage-can model, but at least as importantly from the large literature documenting real decision-making in real organizations. Having described the polar approaches of team theory and garbage cans, I turn next to a brief discussion of these findings on real organizations.

### 2.3 *Real Organizations*

Feldman and March (1981) offer the following summary of many case studies of decision-making in organizations:

- 1) much information that is gathered and communicated by individuals and organizations has little decision relevance;
- 2) much of the information that is used to justify a decision is collected and interpreted after the decision has been made, or substantially made;
- 3) much of the information gathered in response to requests for information is not considered in the making of decisions for which it was requested;
- 4) regardless of the information available at the time a decision is first considered, more information is requested;
- 5) complaints that an organization does not have enough information to make a decision occur while available information is ignored; and
- 6) the relevance of the information provided in the decision-making process to the decision being made is less conspicuous than is the insistence on information.

As Feldman and March note, these findings are at odds with decision theory (for a single, rational decision-maker), which predicts that “relevant information will be gathered and analyzed prior to decision making; information gathered for use in a decision will be used in making that decision; available information will be examined before more information is requested or gathered; needs for information will be determined prior to requesting information; [and] information that is irrelevant to a decision will not be gathered” (p. 172).

Most of the organizational behaviors that Feldman and March catalogue are indeed inconsistent with formal theories of rational choice by *single* individuals—and so are inconsistent with viewing the organization as a single, unified, rational decision-maker. But many of the behaviors they describe are at least partially consistent with (say) simple game-theoretic models of signaling or free-riding. For example:

- a) “ordinary organizational procedures provide positive incentives for underestimating the costs of information relative to its benefits”
 

“the costs and benefits of information are not all incurred at the same place in the organization ... [and] ... post hoc accountability is often required of both individual decision makers and organizations”
- b) “much of the information used in organizational life is subject to strategic misrepresentation”

“information is gathered and communicated in a context of conflict of interest and with consciousness of potential decision consequences; often information is produced in order to persuade someone to do something”

c) “information as signal and symbol”

“when there is no reliable alternative for assessing a decision maker’s knowledge, visible aspects of information gathering and storage are used as implicit measures of the quality and quantity of information possessed and used”

“the arbitrary symbolic use of information is subject to limits imposed by competition for legitimacy and variations in the costs of exhibiting information consumption”

I believe that, if pressed, many economic theorists could build simple (mainstream) models of these ideas.<sup>12</sup>

In sum, it is absolutely correct that “organizational settings for information use differ from those anticipated in a simple decision-theory vision.” But most models in organizational economics have now departed from this “simple decision-theory vision,” preferring instead to take their foundations seriously and so deliver a post-Weberian view of organizations. I now, at last, turn to descriptions of such models.

### 3. Recent Economic Models of Decision-Making in Organizations

This section summarizes models of lobbying (Milgrom and Roberts, 1988; Gibbons, 1999), informal authority (Aghion and Tirole, 1997; Baker, Gibbons, and Murphy, 1999), and power and politics (Rotemberg, 1994; Skaperdas, 1992; Rajan and Zingales, 2000). My hope is that these summaries will convey slightly more detailed understandings of what I mean by the four agenda items described in Section 1: documenting convergence, proving inevitability, proposing interplay, and doing something. And maybe the models themselves will interest a few readers!

#### 3.1 Lobbying

Milgrom and Roberts (1988) introduced the idea of *influence activities* to the economics literature, by which they roughly mean attempts to manipulate information so as

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<sup>12</sup> Indeed, Feldman and March cite some of the classic work in information economics, and presumably would have cited more explicitly organizational models had the organizational economics literature been more developed in 1981.

to influence decisions to one's own benefit.<sup>13</sup> As an example of influence activities, consider Holmstrom's (1982a/1999) model of career concerns in labor markets: workers know that firms will use workers' outputs to draw inferences about workers' abilities, and that these inferences will in turn determine subsequent wage offers, so workers have an incentive to work hard to influence the firms' inference, even if the workers have no private information about their abilities. In Holmstrom's model, the workers' influence activities (hard work) are productive, but in many organizational contexts influence activities either distract organization members from productive tasks or merely change the distribution of organizational resources across members, without improving overall productivity.

Milgrom and Roberts suggest two ways that an organization could respond to the prospect of wasteful influence activities. First, an organization could eliminate influence activities by eliminating opportunities for influence—that is, by closing the relevant communication channels. Naturally, such a response has its costs. Second, an organization may also be able to eliminate influence activities by adjusting its internal structures and/or processes away from what would otherwise be optimal, to eliminate members' incentives to manipulate information. That is, by sufficiently distorting the organizational design, it might be possible to create a Marschak-Radner team, in which all members share a common goal. Of course, an organization could go part way down either or both of these two roads. For example, an organization could commit to limits on its discretion—perhaps by limiting the time given for debate, or by imposing other rules that partially constrain the organization's ability to respond to information provided by its members. In this case, decision makers will have the benefit of some information, but organization members will also engage in some wasteful influence activities.

Crozier (1964:45) lucidly described an organization that went quite far down the first of the two roads that Milgrom and Roberts later proposed—towards shutting down communication entirely. The setting is a large French clerical agency (imagine a division with ten sections, each with 100 employees), in which:

The division head has ... to make all decisions necessary to the functioning of the division--not only the general decisions, such as fixing new goals to co-ordinate the sections' operations ... [but also] the specific decisions pertaining to the daily life of the employees ... [such as] discipline and other personnel problems.

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<sup>13</sup> Economists will recognize that models in the tradition of Spence's (1973) signaling theory also fit this description. The difference is that, whereas in signaling models the signaler has private information that she attempts to signal, in influence-activity models the influencer has no private information to signal, but nonetheless is interested in changing the decision-maker's belief so as to influence the decision-maker's decision.

Placed in such a situation, division heads have neither the capability nor the desire to delegate their responsibilities. Thus they have the final word on a multitude of trivial matters--e.g., whether or not to allow an employee one day off for personal convenience, or whether to blame an employee who has made errors in posting an operation. Since they themselves ... cannot possibly have an adequate knowledge of what is involved, they must rely heavily on the information they receive from the section chiefs. The section chiefs, however, are not in a position to provide reliable information. All ten of them are running parallel identical units that have to compete for scarce resources. Therefore, they have no relationships of interdependence and no common positive interests among themselves, and tend to view each other only as competitors. Thus they are likely, first, to bias the information they give in order to get the maximum of material resources and personal favors with which to run their sections smoothly; and, second, to put pressure on the division head to prevent him from entering into a close relationship with another colleague and to favor the latter over them.

As a result, division heads are condemned to get only unreliable information and to remain isolated from the daily problems of work. Their decisions tend to be impersonal routine decisions--i.e., decisions based on the letter of the rules and not on equity.

I interpret Crozier as giving a persuasive explanation for why it's lonely at the top. The argument applies equally well to academic deans as to division heads, as follows. Suppose a dean announces that she will go to lunch with the faculty every Tuesday. If the dean can sort through the competing claims around the lunch table, she may receive valuable information from the faculty. But there is a hidden cost of collecting information at lunch: the faculty members spend Tuesday mornings preparing to lobby the dean, rather than on more productive activities. The organizational benefit from collecting the information might or might not outweigh this lobbying cost.<sup>14</sup> In either case, the best feasible organizational design (second-best) will not yield the best organizational outcome one can imagine (first-best): either the dean will go to lunch and the organization will incur lobbying costs, or the dean will not go to lunch and the organization will suffer from poor decision making. An intermediate alternative, such as monthly rather than weekly lunches, may be superior to both of these extremes, but it still cannot achieve the twin goals of fully informed decision making and no lobbying cost.

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<sup>14</sup> For example, suppose the dean is about to allocate a prize, such as a faculty fellowship or a chair. It may make little difference to the organization which faculty member gets the prize, and yet it could make an enormous difference to the individuals involved. As a result, lobbying efforts may be high because of the prize, and even higher because of the anticipated lobbying of others. But all the lobbying is a waste for the organization as a whole.

I find Crozier's informal theory so persuasive that developing a formal model seems trite. Nonetheless, I did so in Gibbons (1999), for two reasons. First, I hoped to clarify what an economic model is: a set of simplifying assumptions that allow the model to be solved and yet capture some of the phenomena of interest. Second, I hoped to illustrate one of the reasons why such models might be valuable: by developing a formal model that consciously parallels Crozier's informal theory, I wished to check the internal consistency of his theory and, in the process, derive boundary conditions for his argument. I repeat this modeling exercise here, in the hope that it will be an easy introduction to economic modeling for non-economist readers, allowing terser treatments of informal authority and of power and politics in the following sub-sections.

Consider a multidivisional firm in which headquarters is responsible for allocating capital across the divisions. If headquarters knew the profit that each division could earn on any capital it might receive, then headquarters could solve for the allocation of capital across divisions that maximizes the firm's overall profit. But suppose that headquarters does not know the profit that each division could earn and that each division would like to receive as much capital as possible. Then headquarters faces a problem much like that of the division head or the dean: collecting information would improve decision making, but the process of collecting information may have undesirable side-effects.

For simplicity, we will focus on just two actors: headquarters and one division. Let  $x$  denote the amount of capital that headquarters eventually decides to allocate to the division, and let  $x^*$  denote the capital that headquarters would allocate to the division if headquarters knew the return that the division could earn. That is, headquarters would like to allocate  $x = x^*$ ; the problem is that headquarters does not know  $x^*$ .

To develop the model, we must specify the information structure (who knows what), the utility functions (who cares about what), and the timing (who can do what when). Suppose that publicly available information leads headquarters to believe that  $x^*$  is normally distributed with mean  $m$  and variance  $v$ . Like the dean who goes to lunch, headquarters could collect more information. We model this additional information as a noisy signal about  $x^*$ , denoted by  $s$ . That is, higher values of  $x^*$  produce higher values of  $s$ , but  $s$  is also affected by a random shock (denoted by  $\epsilon$ ) that is independent of  $x^*$ . To capture the idea of lobbying, suppose that the division can engage in activities (denoted by  $L \geq 0$ ) that increase the value of  $s$ . Formally, suppose that  $s = x^* + L + \epsilon$ , where  $\epsilon$  is normally distributed with a mean of 0 and variance  $v$ . Finally, suppose that headquarters can observe  $s$  but not any of its components —  $x^*$ ,  $\epsilon$ , or  $L$ .

We next specify the utilities that headquarters and the division receive, depending on the realized values of the random variables and the choice variables in the model. The utility to headquarters (denoted  $U_h$ ) must capture the ideas that headquarters seeks to allocate capital ( $x$ ) equal to the ideal allocation ( $\bar{x}$ ) but that time the division spends lobbying ( $L$ ) is a diversion from activities that headquarters would find more productive. A simple way to capture these two ideas is to suppose that  $U_h = -(x - \bar{x})^2 - L$ . The utility to the division (denoted  $U_d$ ) must capture the idea that the division would like to receive as much capital as possible. We also assume that lobbying is not free: choosing an enormous value of  $L$  will have an enormous effect on  $s$ , but doing so is costly to the division, perhaps because lobbying takes time away from other activities for which the division is rewarded. A simple way to capture these ideas is to assume that  $U_d = x - c(L)$ ; for simplicity, suppose that the cost function is  $c(L) = (\frac{1}{2})L^2$ .

To complete the model, we specify the timing of moves. To start the game, headquarters chooses a capital-allocation rule as a function of the noisy signal. Formally, suppose that headquarters chooses a rule of the form  $x = d + bs$ , where  $d$  and  $b$  are parameters that headquarters announces to the division. For example, if headquarters chooses  $b = 0$ , then the signal  $s$  is irrelevant to the capital-allocation rule, akin to a dean who never goes to lunch. To keep the exposition simple, assume that headquarters cannot later renege on these choices of  $d$  and  $b$ , no matter how badly it would like to do so.

After headquarters chooses a capital-allocation rule, the remaining moves are as follows: (1) the division chooses lobbying effort,  $L$ ; (2) headquarters observes the signal,  $s$ ; (3) headquarters allocates capital to the division according to the rule  $x = d + bs$ ; and (4) headquarters receives  $U_h$  and the division receives  $U_d$ . To solve this model we work backwards: we first solve for the division's optimal lobbying effort ( $L$ ) for any given capital-allocation parameters  $d$  and  $b$ ; we then solve for headquarters' optimal capital-allocation rule ( $d$  and  $b$ ), taking into account the division's optimal lobbying effort in response to the parameters chosen by headquarters.

Faced with the capital-allocation parameters  $d$  and  $b$ , the division's problem is to choose lobbying effort  $L$  to maximize the expected utility  $E(U_d) = E[d + bs] - c(L)$ , so the solution is  $L^* = b$ . This result is not surprising: the greater the weight ( $b$ ) that headquarters puts on the noisy signal ( $s$ ) in determining the division's capital allocation ( $x$ ), the greater the optimal lobbying effort ( $L^*$ ) by the division.

We can now formulate and solve headquarters' problem: choose the parameters of the capital-allocation rule,  $d$  and  $b$ , to maximize the expected utility  $E(U_h) = -E[(d + bs - \bar{x})^2] - L^*$ , given that the division will choose optimal lobbying effort  $L^* = b$  in response. Let

$(d^*, b^*)$  denote the solution to headquarters' problem. The interesting part of this solution is  $b^*$ , the optimal weight for headquarters to put on the signal in allocating capital to the division. The main result of the model is that, to reduce lobbying costs, headquarters chooses to put little (if any) weight on the signal.

The precise value of  $b^*$  depends on the extent of headquarters' uncertainty about the appropriate capital allocation (as measured by  $v$ , the variance of  $\theta$ ) and the noisiness of the signal  $s$  (as measured by  $v_\varepsilon$ , the variance of  $\varepsilon$ ). If  $v$  exceeds  $1/2$  then

$$b^* = \frac{v - (1/2)}{v + v_\varepsilon},$$

but if  $v < 1/2$  then  $b^* = 0$ . The latter case corresponds to the organization Crozier described: if the chance of taking a grossly inappropriate action is sufficiently small ( $v < 1/2$ ) then it is not worthwhile to incorporate any additional information into decision making, because doing so will cause the organization to incur lobbying costs. The organization is better off committing to ignore additional information but consequently incurring no lobbying costs (i.e., setting  $b^* = 0$  so as to achieve  $L^* = 0$ ).

It is easy to imagine a different organization that fits the first case: if there is sufficiently great uncertainty about what action is appropriate ( $v > 1/2$ ) then it is worthwhile to incorporate additional information into decision making, even though doing so will cause the organization to incur lobbying costs. Such an organization would be worse off if it committed to ignore the additional information, even though doing so would eliminate lobbying costs. Indeed, as the uncertainty about the appropriate action increases (i.e.,  $v$  increases above  $1/2$ ), the optimal weight for headquarters to put on the signal also increases (i.e.,  $b^*$  increases from zero at  $v = 1/2$  toward one as  $v$  approaches infinity). But even for  $v > 1/2$ , the model predicts that information will not be used perfectly, in the sense that the optimal weight  $b^*$  in the capital-allocation rule is positive but still smaller than first-best. To see this, suppose that after observing the signal  $s$ , headquarters was not constrained to choose the capital allocation  $x = d + bs$  but instead could choose any capital allocation it liked. One can show that the optimal capital-allocation is then  $x = D^* + B^*s$ , where

$$B^* = \frac{v}{v + v_\varepsilon}.$$

The key point is that  $b^*$  is less than  $B^*$ : to reduce lobbying costs, headquarters chooses to reduce the weight on the signal. That is, headquarters will sometimes be unresponsive to

important facts that are plain to all participants, but this unresponsiveness will be intentional and optimal.

In summary, this is a model of second-best organizational design: the best feasible organizational design will not yield the best organizational outcome we can imagine; either the dean should go to lunch, even though the organization will incur lobbying costs, or the dean should not go to lunch, even though the organization will suffer from poor decision making. Both the outcome and the mechanism of this model seem broadly consistent with Crozier's observation and informal theory, although the model also reveals a boundary condition (namely,  $v$  must be sufficiently small) that the informal theory obscured.

### 3.2 *Informal Authority*

Aghion and Tirole (1997) develop a lovely model of formal versus real authority. One of their motivations is "rubber stamping" (where the boss has the formal authority, but approves the subordinate's recommendation without inspection or consideration). It seems that shareholders often rubber-stamp the board's decisions, the board rubber-stamps the CEO's decisions, the CEO rubber-stamps the division managers' decisions, and so on. In these settings, one might say that the boss has the formal authority, but the subordinate has the real authority.

From an economist's perspective, it is a bit puzzling why someone with formal authority would cede it. In fact, from this perspective, it is also puzzling *how* one would cede formal authority. That is, if the boss has the formal authority, can't she always take back any delegation of authority to a subordinate? Aghion and Tirole propose answers to both these questions, via the following model.

Suppose there are three possible projects, indexed by  $k = 1, 2, 3$ . Project  $k$  delivers benefits  $B_k$  to the boss and  $b_k$  to the subordinate. One project is terrible for both parties:  $B_k = b_k = -$ . The other two projects deliver benefits of 0 and  $B > 0$  to the boss and 0 and  $b > 0$  to the subordinate. With probability  $p$ , the payoffs from the latter two projects are  $(B, b)$  and  $(0, 0)$ ; with probability  $1-p$ , the payoffs from these projects are  $(B, 0)$  and  $(0, b)$ . Thus, a higher value of  $p$  means that the boss's and the subordinate's interests are more likely to be aligned.

The problem is that, initially, neither the boss nor the subordinate knows which project is which. That is, each party can see the three possible projects, but neither party knows which project is the terrible one, nor which project is the good one for him or her.

Because of this uncertainty (and the severity of the terrible project), if no information is collected about which project is which, then the boss will not allow any project to be chosen.

Both the boss and the subordinate can try to collect information about which project is which, but at a cost. If the boss incurs the cost  $c_B(E)$ , then the boss learns her own payoff on each project with probability  $E$ , but learns nothing with probability  $1-E$ . Similarly, if the subordinate incurs the cost  $c_s(e)$ , then the subordinate learns his own payoff on each project with probability  $e$ , but learns nothing with probability  $1-e$  (where these events for the subordinate are independent of those just described for the boss).

To analyze the parties' incentives to collect information, consider what happens in the following three situations. First, if the boss becomes informed about her payoffs on the three projects, then she will choose the project that pays her  $B$ . Second, if the boss remains uninformed but the subordinate becomes informed, then the subordinate will recommend the project that pays him  $b$ ; even though the boss cannot observe the projects' payoffs (and the subordinate cannot communicate them), the boss will accept the subordinate's proposed project because its expected payoff to the boss is  $pB + (1-p)0 = pB > 0$ . Finally, if neither the boss nor the subordinate becomes informed, then the boss will not allow a project to be chosen, as described above.

From these three situations, we can write the expected payoffs to the boss and the subordinate if the boss incurs the cost  $c_B(E)$  and the subordinate incurs the cost  $c_s(e)$ :

$$U_B = E \cdot B + (1-E)e \cdot pB - c_B(E) \quad \text{and}$$

$$U_s = E \cdot pb + (1-E)e \cdot b - c_s(e),$$

respectively. From these expected payoffs, we can compute the Nash equilibrium choices of  $E$  and  $e$ . Assuming that the cost functions have the usual properties (namely, convexity, zero slope at zero, and infinite slope at one), the Nash equilibrium  $(E^*, e^*)$  satisfies:

$$B(1 - ep) = c_B(E) \quad \text{and}$$

$$(1-E) \cdot b = c_s(e).$$

This equilibrium produces some rubber-stamping: with probability  $E^*$ , the boss becomes informed and chooses the project; but with probability  $(1-E^*)e^*$ , only the subordinate becomes informed, in which case he proposes a project and the boss rubber-stamps the proposal. This equilibrium offers answers to the two questions posed above – namely, why and how would a boss cede formal authority? Strictly speaking, the answers to both questions are that the boss does not cede formal authority, but rather cedes real authority via rubber-stamping. The boss desires to cede real authority (answering the

“why?” question) when the subordinate has superior information and sufficiently similar preferences. (To see the role of similar preferences, imagine revising the model to allow projects with payoffs  $B_k = -$  and  $b_k = b$ .) Furthermore, the boss can cede real authority (answering the “how?” question) when the subordinate has superior information and sufficiently similar preferences.

Aghion and Tirole proceed from this analysis of formal versus real authority to a second analysis of delegation, which they model as the subordinate having the formal authority (but the boss perhaps having the real authority – exactly the opposite of the case just analyzed). In this case, the expected payoffs become

$$V_s = e \cdot b + (1-e)E \cdot pb - c_s(e) \quad \text{and}$$

$$V_B = e \cdot pB + (1-e)E \cdot B - c_B(E),$$

and we can compute a new Nash equilibrium  $(E^{**}, e^{**})$  from these expected payoffs. In this new equilibrium, the subordinate picks the project whenever he is informed, instead of only when he is informed and the boss is not, so the subordinate’s incentive to collect information is stronger than before ( $e^{**} > e^*$ ). In contrast, the boss no longer picks the project whenever she is informed, but instead only when she is informed and the subordinate is not, so the boss’s incentive to collect information is weaker than before ( $E^{**} < E^*$ ). In short, delegation (modeled as giving the subordinate formal authority) increases the subordinate’s incentives, but decreases the boss’s.

Baker, Gibbons, and Murphy (1999) object to Aghion and Tirole’s model of delegation, in which the subordinate holds the formal authority. Instead, Baker, Gibbons, and Murphy (hereafter, BGM) assert that, below the top of an organization, decision rights can only be loaned, not owned. That is, the ability to reject the subordinate’s proposal is a defining feature of being the boss; a subordinate who holds the formal authority is not a subordinate anymore.

But even if delegation cannot occur through formal authority, it may be feasible through informal authority. In particular, BGM envision delegation as a promise (by the boss, not to reject the subordinate’s proposal), rather than a contract (that irrevocably gives formal authority to the subordinate), and they use a repeated-game model to analyze whether this promise is credible. Analogous to other repeated-game models, delegation is credible if the boss’s short-run temptation to reject the subordinate’s proposal is outweighed by the boss’s long-run gains from the subordinate’s increased incentives created by delegation (akin to the increased incentives analyzed by Aghion and Tirole).

But the boss's short-run temptation to reject the subordinate's proposal depends on how well the boss can evaluate the proposal. As an extreme example, if the boss knows nothing about the proposed project beyond the fact that the subordinate proposed it, then the boss is either tempted to reject all proposals or tempted to reject no proposals. And as a less extreme example, if the boss gets a noisy signal about the proposed project's payoff to her, then the boss's temptation to reject the proposal depends on both the project's true payoff to the boss and on the realization of the noise in the signal.

Thus, the repeated-game analysis reveals that changing the boss's information structure has two effects: (1) it changes the decision the boss would like to take, in the usual decision-theoretic way; (2) it changes the boss's temptation to renege on the promise of delegation. This second effect derived from the repeated-game analysis is similar in spirit to some of Feldman and March's observations discussed above, in the sense that decision theory is again seen to be insufficient for understanding decision-making in organizations. Furthermore, the repeated-game's emphasis on credibility (and on the fragility of credibility) accords well with Foss's (2001) analysis of the rise and decline of the "spaghetti" organization at the Danish firm Oticon, where very substantial authority was initially delegated to subordinates but then reclaimed by headquarters.<sup>15</sup>

Several points can be taken from these two models of informal authority, not least of which is that economists have started to consider the subject, rather than restricting attention to formal properties of organizations such as organization charts and job descriptions. This increased attention to informal organization is one way in which organizational economics is adopting a post-Weberian view. More specifically, the Aghion-Tirole analysis (1) articulates a useful distinction between formal and real authority, (2) raises and answers questions of why and how a boss might cede authority, and (3) delivers the reassuring result that delegation increases the subordinate's incentives (but decreases the boss's). The complementary BGM model (1) asserts that formal delegation is impossible (*i.e.*, transferring formal authority makes the subordinate not subordinate), but finds that (2) the promise of delegation can be credible, if the boss's short-run temptation is outweighed by the long-run benefits from increased incentives through delegation, and that (3) changes in

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<sup>15</sup> Crémer (1995) shows a related way in which decision theory is insufficient to understand decision-making in organizations. Crémer's model is similar to Aghion-Tirole, but there are two possible information technologies available to the boss. As an example in the spirit of Crémer's analysis, imagine that in the Aghion-Tirole analysis we allow the boss to pay cost  $K$  to change the cost function from  $c_B(E)$  to  $kc_B(E)$ , where  $0 < k < 1$ . If  $K$  is sufficiently close to zero then decision theory implies that the boss should switch to the cheaper information technology. But reducing the marginal cost of information for the boss increases the boss's optimal choice of  $E$ , which reduces the subordinate's optimal choice of  $e$ , and the net result can be a reduction in total surplus.

the boss's information structure not only change the boss's desired decision, in the usual decision-theoretic way, but also change the boss's temptation to renege on promised delegation.

Not all of these ideas will be new to non-economist students of formal and informal authority, but some are, and there is the prospect of more to come. Similarly, these are not the only issues surrounding informal authority, but I think these are some of the important issues, and there is the prospect that other important issues can be incorporated into future work. In short, in this sub-section, I hope to have (implicitly) conveyed slightly more detailed understandings of what I mean by the four agenda items above: documenting convergence, proving inevitability, proposing interplay, and doing something. I turn next to briefer discussions of recent economic models of power and politics, and of lobbying.

### *3.3 Power and Politics*

Rotemberg (1994) provides a terrific example of how new economic models may converge with established theory and evidence from organizational sociology and organization theory. The paper begins with an overview of classic evidence on who has power in organizations, including Crozier (1964), Hinings et. al. (1974), Pfeffer and Salancik (1978), and Pfeffer (1981). In brief, this evidence supports two related theories: the resource-dependency theory (Emerson, 1962), where power goes to those who control important resources, and the strategic-contingency theory (Hickson et. al., 1971), where power goes to those who can help the organization cope with important problems.

Rotemberg notes that this theory and evidence from organization theory does not match what an economist might predict about who has power in organizations. Instead, an economist might expect power to be held by the organization member who is best informed (as in team theory) or who has the highest willingness to pay for controlling the decision (as in an auction or market). But Rotemberg argues that (a) these predictions rely crucially on the assumption of perfect contracts, and (b) an economic model with (realistically) imperfect contracts will be broadly consistent with the theory and evidence from organization theory. In particular, in an economic model with imperfect contracts, power will be held by those whom the organization would most like to retain. In this sense, Rotemberg shows that it is not economics per se that is inconsistent with organization theory, but rather certain assumptions about the environment that lead economic models astray.<sup>16</sup>

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<sup>16</sup> We have already seen this point in Figure 1 and footnote 5. The figure shows the (obvious, but still sometimes forgotten) point that not all markets are efficient. Or, to put it more in the syntax of the conclusion from Rotemberg's model, economics does not predict that markets all are efficient, but rather

I will present a simplified version of Rotemberg's model and then comment on several extensions. Imagine that a boss must decide which of two subordinates will be allowed to make a key decision. (For simplicity, we will ignore the issues discussed in the previous sub-section – namely, why and how the boss would delegate decision-making authority to a subordinate rather than keeping it for herself. One answer to these questions would use the repeated-game approach of the BGM model.) Each subordinate would value receiving this decision-making authority, but the subordinates might value this authority differently. To capture such a difference, let  $b_i$  denote the benefit that subordinate  $i$  would receive if given the decision-making authority, where  $i = 1$  or  $2$ .<sup>17</sup> If this difference in benefits (*i.e.*,  $b_1$  vs.  $b_2$ ) were the only consideration, an economic model would prescribe that subordinate 1 be given the decision-making authority if  $b_1 > b_2$ , but subordinate 2 if  $b_2 > b_1$ .

In addition to this difference in the value each subordinate places on having decision-making authority, suppose there are two other differences between the subordinates: in their value to the firm (denoted  $V_i$ ) and in their best alternative employment offer (denoted  $r_i$ ). Furthermore, suppose that the precise value of  $r_i$  is known only to subordinate  $i$ ; all other parties know only that  $r_i$  is uniformly distributed on  $[0, R_i]$ . Assume that  $V_i > R_i$ , so that it is always efficient for the subordinate to work in the current job, rather than accept the best alternative offer. Note that  $V_i/R_i$  is a measure of how much more valuable subordinate  $i$  is in the firm than outside – a concept similar to those invoked in the resource-dependence and strategic-contingency theories discussed above. Finally, suppose that the only contract that the boss can offer the subordinate is a take-it-or-leave-it wage offer, denoted  $w_i$ . That is, the boss makes the offer, and then the subordinate compares the offer to  $r_i$  and chooses the larger of the two.

The key feature of Rotemberg's model is that the boss can offer a lower wage to the subordinate who gets the decision-making authority, so it becomes worthwhile to allocate authority to the subordinate whom the boss would most like to retain, as follows. If the boss gives the authority to subordinate  $i$ , then the boss's optimal wage offers to subordinates  $i$  and  $j$  solve the following problems:

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that markets can be efficient in the right environments. Similarly, the footnote makes the (again, obvious, but sometimes forgotten) point that not all interactions among rational actors are efficient. Instead, much depends on the environment.

<sup>17</sup> In an economic model, this benefit is assumed to arise because subordinate  $i$  would use the authority to take the decision in his self-interest. Of course, there may be moral, political, and formal constraints on how self-interested the decision-maker can be, and these will reduce the size of  $b_i$ , but the assumption is that there is still some scope for self-interest, and that this scope generates this benefit from having power. For simplicity, suppose that a self-interested decision by one subordinate yields no cost or benefit either to the other subordinate or to the boss.

$$\max_{w_i} (V_i - w_i) \text{ Prob}\{r_i < w_i + b_i\}$$

and

$$\max_{w_j} (V_j - w_j) \text{ Prob}\{r_j < w_j\} .$$

The solutions are  $w_i^* = (V_i - b_i)/2$  and  $w_j^* = V_j/2$ , and they yield expected profits to the boss of  $E_i = (V_i + b_i)^2/4R_i$  and  $E_j = V_j^2/4R_j$ , so the boss's total expected profit from allocating decision-making authority to subordinate i is  $E = (V_i + b_i)^2/4R_i + V_j^2/4R_j$ . Alternatively, the boss could allocate authority to subordinate j, in which case the resulting optimal wage offers would yield the total expected profit  $E = (V_j + b_j)^2/4R_j + V_i^2/4R_i$ . Therefore, the boss should allocate authority to subordinate i if

$$2b_i \frac{V_i}{R_i} + \frac{b_i^2}{R_i} > 2b_j \frac{V_j}{R_j} + \frac{b_j^2}{R_j} .$$

Note the role of  $V_i/R_i$  vs.  $V_j/R_j$  in determining the optimal allocation of decision-making authority. For example, if  $b_i$  and  $b_j$  are approximately equal, then the  $V$ 's and the  $R$ 's (and especially the  $V/R$ 's) are the key determinants of who gets the authority. In short, there are sound economic reasons to give power to those one hopes to retain.

Rotemberg analyzes three extensions of this basic model. The first is a two-period model, where the authority and a first wage are given in the first period, but the second-period's wage cannot be determined until the second period. The second allows power to be split, so that some authority can be given to each subordinate. And the third is a repeated-game model, where the firm can try to use its reputation to pay different wages than the one-period optimal wages computed above. In all of these extensions, the basic result above continues to hold: there are strong reasons to give power to those one hopes to retain.

From power, it is a short step to politics. Formally, suppose that two organization members have some power (rather than one holding all the power, as in Rotemberg's model). If the powerful organization members disagree, there may be something like a fight; see Skaperdas (1992) and Rajan and Zingales (2000). More generally, when there are gains to be had, interested parties who possess appropriate instruments may engage in gaming over the surplus. I will call such gaming "politics," because of the similarity between this account and the political perspective on organizations developed by March (1962, 1994), Pfeffer (1981), and colleagues. Consider Pfeffer's (1981: 28) comparison of the "bureaucratic" and "economic" approaches to the political approach:

In bureaucratic theories of organizations, the presumption is that through control devices such as rewards based on job performance or seniority, rules that ensure fair and standardized treatment for all, and careers within the organization, the operation of self-interest can be virtually eliminated as an influence on organizational decision making. Economic or incentive theories of organizations argue that through the payment of wage, particularly when compensation is made contingent on performance, individuals hired into the organization come to accept the organization's goals. Political models of organizations assume that these control devices, as well as others such as socialization, are not wholly effective in producing a coherent and unified set of goals. ... *To understand organizational choices using a political model, it is necessary to understand who participates in decision making, what determines each player's stand on the issues, what determines each actor's relative power, and how the decision process arrives at a decision* [emphasis added].

Pfeffer's summary of the bureaucratic approach, in which the operation of self-interest is eliminated as an influence on organizational decision making, is reminiscent of team theory. I certainly agree with Pfeffer that the bureaucratic/team approach misses something important (hence my characterization of the garbage-can model as "desperately needed contrast to the Weberian models from team theory"). As for the economic approach, Pfeffer's summary is accurate for its time, reflecting early work in agency theory, such as Holmstrom's (1979, 1982b). Again, I agree that there are important omissions in that approach; I much prefer the "multi-task" or "get what you pay for" agency models launched by Holmstrom and Milgrom (1991) and summarized in Gibbons (1998b), in which no contract could cause individuals to "come to accept the organization's goals."

Although I agree with Pfeffer's assessments of the bureaucratic and (old) economic approaches, I think the most exciting source of agreement concerns the political approach. To me, Pfeffer's summary of the political approach is fundamentally game-theoretic, if informally so. Compare his italicized statement to the following: an "extensive-form representation of a game specifies (1) the players in the game, (2a) when each player has the move, (2b) what each player can do at each of his or her opportunities to move, (2c) what each player knows at each of his or her opportunities to move, and (3) the payoff received by each player for each combination of moves that could be chosen by the players" (Gibbons, 1992: 115). In short, I read Pfeffer as a closet game-theorist!

More generally, although power and politics have almost disappeared from recent non-economic research on organizations, it seems unlikely that they have disappeared from organizations themselves, so perhaps now is the time to bring them back into the research agenda by integrating the detailed description and informal theory of the original literature

with the kind of formal economic modeling summarized above. Rotemberg (1994) has made a good start on power, and Skaperdas (1992) and Rajan and Zingales (2000) on politics, but much remains to be done.

#### 4. Conclusion

As I warned in the Introduction, I have blithely ignored important insights into decision-making that apply well outside organizations. I hope that it will soon be possible to survey economic research on decision-making in organizations that remedies this omission. To conclude this essay, let me point to a few issues and papers that will likely loom large in such a survey.

I excluded from this essay the original work on heuristics and attributions, such as Tversky and Kahneman (1974) and Ross (1977), but there is already work co-authored by economists that applies these ideas to organizational contexts, such as Babcock and Loewenstein (1997) and Weber et. al. (1999). Turning from cognition to preferences, I assumed self-interested preferences, but reality may be social utility (such as where I care about your payoff; Kelley and Thibaut, 1978), and I assumed exogenous preferences, but reality may be contingent preferences (such as where how I feel about your payoff depends on how I think you feel about mine; Dawes, McTavish, and Shaklee, 1977). Again, there is already work by economists that applies these ideas, such as Fehr and Gächter (2000) and Rabin (1993), respectively.

By citing some recent work in this connection, some of which is even by economists, I hope to suggest that these issues are slowly moving onto some economists' radar screens and may be incorporated into models in organizational economics in the finite future. In short, organizational economics may follow finance in incorporating behavioral economics into its thinking.

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