ASQ Forum

Taking Coase Seriously

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In this essay I advance two related theses. First, economic theory predicts that organizations will be a mess but not a mystery. Second, classic case studies conducted by organizational sociologists support this prediction. Fully articulating and defending these theses will require a book, so my goal here is simply to render them plausible. I begin by pointing toward the relevant economic theory and sociological evidence, but I devote the bulk of the essay to a particular example: I develop a formal economic model inspired by a passage from Michel Crozier's *The Bureaucratic Phenomenon.* I conclude by discussing the potential roles of formal economic modeling in organizational research.•

In 1937, Ronald Coase launched the modern economic theory of the firm by asking, "If markets worked perfectly, why would there be firms?" Coase argued that firms will exist only in environments in which firms perform better than markets could. But ever since Adam Smith, the predominant research question in economics had been, In what environments will markets perform efficiently (i.e., produce an allocation of resources that cannot be altered so as to make every individual better off)? To create space for firms, Coase suggested that some environments might be plagued by "transaction costs" that cause markets to perform inefficiently.

For several decades, Coase's paper was "much cited . . . [but] little used" (Coase, 1988: 62). Then Williamson (1975) took two important steps: identifying some of the conditions that create transaction costs, such as imperfect contracts and relationship-specific investments, and suggesting that firms might deal with these conditions more effectively than markets could because firms can use relational contracts, as envisioned in Simon's (1951) theory of the employment relationship. The resulting transaction-cost economics (see also Williamson, 1985, 1996) has made substantial progress on subjects such as vertical integration, supplier relationships, and complex contracts. As these subjects suggest, however, the transaction-cost literature has focused disproportionately on activities at the boundary of the firm, largely ignoring its internal functioning.

In this essay I make three claims about formal economic models of internal organization and their potential relationship to noneconomic organizational research. The first is that Coase's argument has implications not only for the boundaries of firms but also for their internal functioning. If firms arise only in environments in which transaction costs would cause markets to perform imperfectly, then it is one thing to assert that firms may perform better than markets would, but quite another to assert that firms will perform perfectly. That is, firms may be second-best—the best that can be achieved—but their internal functioning seems unlikely to be first-best—the best that can be imagined. After all, why should firms be oblivious to conditions that wreck markets?

Of course, organizational sociologists 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)

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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, "rules are often violated, decisions are often unimplemented, . . . and evaluation and inspections systems are subverted" (Meyer and Rowan, 1977: 343). Moreover, "informal structures deviate from and constrain aspects of formal structure, and . . . the organization's intended, rational mission [is undermined] by parochial interests" (DiMaggio and Powell, 1991: 12).

My second claim is that economics has recently begun to produce formal models of internal organization that fit nicely with this post-Weberian conception of life inside organizations. Consistent with Coase's informal argument, these formal models involve some kind of transaction cost. Some models follow Williamson, emphasizing imperfect contracts and specific investments, but others draw additional inspiration from strands of economic theory developed in the 1970s and '80s such as information economics, agency theory, and the theory of repeated games.[1] These recent economic models of internal organization predict that organizations will be a mess but not a mystery. By "mess" I mean that the predicted organizational outcome is typically not first-best (i.e., the outcome is worse than can be imagined). By "not a mystery" I mean two things—one micro, the other macro. The micro meaning is that in these models each person takes actions that are optimal for him or her, given the (formal and informal) incentives he or she faces. In this sense, the behavior of each individual is not mysterious.[2] The macro meaning is that these models analyze environments in which any organizational design would encounter transaction costs, and the predicted organizational design minimizes these costs. In this sense, the design of organizations is not mysterious. This messiness and these two senses of lack of mystery are illustrated below.

My third claim is that formal economic models may add value to organizational research in several ways, including checking the internal consistency of informal rational-choice arguments, specifying or interpreting empirical tests, providing new explanations for facts previously given non-rational-choice explanations, and allowing analysis of rich environments in which informal analysis would have ground to a halt. Having articulated this third (and most contentious?) claim, let me be absolutely clear about two related issues. First, these potential values-added from formal modeling are not unique to economic modeling; other styles of formal modeling (e.g., Peli et al., 1994; Sastry, 1997) offer analogous contributions. Second, and more important, I do not believe that formal modeling should be the only style of organizational research. 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 organizational economics has too little description and informal theory, but organizational sociology has too little formal modeling.[3] I conclude this essay by nominating two other areas of organizational research (in addition to the functionalist literature on bureaucracy noted above) in which a blend of description, informal theory, and formal economic

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By information economics I mean models involving private information, such as Akerlof's (1970) model of used-car markets, in which a seller knows his or her car's quality better than buyers do, Rothschild and Stiglitz's (1976) analysis of insurance markets, in which drivers know their accident risks better than insurance companies do, and Spence's (1973) theory, in which a worker acquires education to signal his or her productive ability to prospective employers. By agency theory I mean models such as Holmstrom's (1979, 1982a), in which a principal and an agent sign a contract that specifies the agent's compensation as a function of one or more performance measures, and then the agent chooses actions in response to the incentives created by the contract. And by the theory of repeated games I mean "folk theorem" results like Fudenberg and Maskin's (1986) for infinitely repeated games, as well as "reputation" models like Kreps et al.'s (1982) analysis of cooperation in the finitely repeated prisoners' dilemma.

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Some readers 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 as much on its assumptions about the environment as 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 of 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 (or "mess").

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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.

modeling might be especially productive: power and politics (March, 1962, 1994; Pfeffer, 1981) and embeddedness (Granovetter, 1985).

This essay is a companion to a survey, "Game Theory and Garbage Cans" (Gibbons, 1998a), in which I sketched several recent models of internal organization. In that survey I had two goals. The first was to introduce non-economists to the five building blocks mentioned above: imperfect contracts, relationship-specific investments, information economics, agency theory, and repeated games. All the models I discussed involved imperfect contracts, and I used a model of up-or-out promotion rules to illustrate specific investments (in human capital), models of communication and decision making to illustrate information economics, a model of pay for performance to illustrate agency theory, and models of corporate culture and relational contracts to illustrate repeated games. My second goal was to relate the predicted organizational outcomes in these models to evidence from organizational sociology. In particular, for each model I tried to explain why the predicted organizational outcome involved either inefficient or informal or institutionalized behavior and hence departed from the first-best organizational outcome in ways I thought sociologists would find familiar, and possibly even interesting.[4]

In this essay I take a complementary approach, in two respects. First, rather than summarize an arbitrary collection of existing models, I make the much broader claim articulated above: in all current and potential economic models of internal organization, if Coase's argument is correct, then inefficiency will be typical, not exceptional. Figure 1 illustrates this claim.

The figure shows the declining effectiveness of market governance as transaction difficulty increases (e.g., as incomplete contracts and specific assets become more problematic) and also the declining effectiveness of firm governance as transaction difficulty increases.[5] At the critical value of transaction difficulty indicated by the dotted line, markets and firms are equally effective governance structures. Coase's (1937) argument—that firms will exist only where they perform better than markets could—implies that transactions to the right of the dotted line will be governed by firms, those to the left by markets. My claim—that inefficient internal organization is typical, not exceptional—can be seen by comparing the observed effectiveness of firms (to the right of the dotted line) with the observed effectiveness of markets (to the left of the dotted line): the latter is superior, especially as transaction difficulty falls to zero, at which point market governance produces the efficient outcome familiar from neoclassical economics. In brief, it is one thing to explain when and why firms may perform better than markets could, but quite another to assert that firms will then perform perfectly.

The second respect in which this essay complements the survey is that, rather than describe many models and hence fully explain none, I develop a single new model from scratch. For reasons of space, this model is very simple, but I develop it for two reasons: to clarify what an economic

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Miller's (1992) excellent book pursues the same two goals at greater length than my survey, providing nontechnical introductions to building-block models and using these models to interpret case studies from organizational sociology and the business press.

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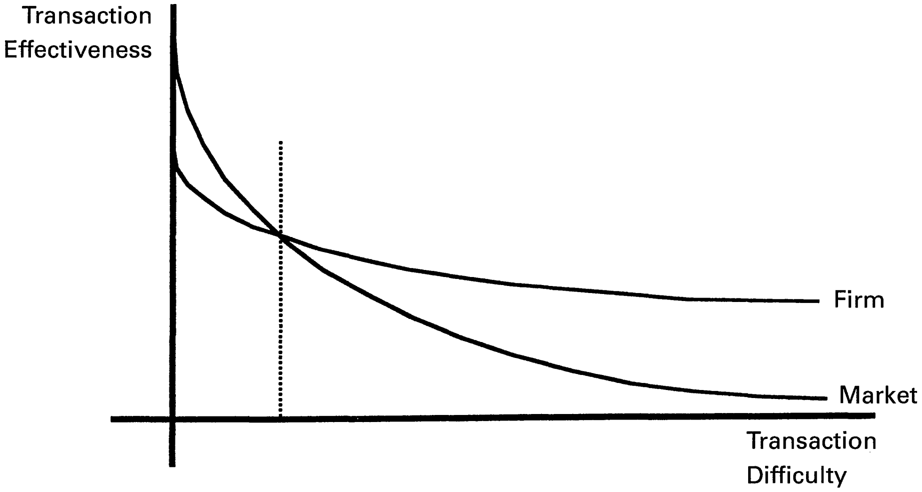
If we define transaction difficulty to be the collection of features that cause the effectiveness of market governance to decline, then it is definitional that the curve labeled "market" in the figure declines as transaction difficulty increases. But it does not follow that the curve labeled "firm" also declines as this definition of transaction difficulty increases. I do not assert that the same collection of features causes both market and firm governance to decline. Rather, I intend the figure to be only suggestive.

Figure 1. Effects of transaction difficulty on the effectiveness of market and firm governance.



model is and to illustrate some of the reasons why such models might be valuable.

AN EXAMPLE OF ECONOMIC MODELING

The model I develop is based on the following page from Crozier (1964: 45). The setting is a large French clerical agency (imagine a division with ten sections, each with 100 employees):

> 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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> 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.
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> 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.[6] In either case, the best feasible organizational design 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. Intermediate alternatives, such as monthly rather than weekly lunches, may be superior to both of these extremes but still cannot achieve the twin goals of informed decision making and no lobbying cost.[7]

Crozier has described an organization that is a "mess." More importantly, for my purposes, his informal theory illustrates why a mess need not be a mystery. Crozier is not surprised that organization members are acting as they are; he is claiming that these actions are a natural consequence of the members acting on their self-interest, given the formal and informal incentives they face. This is the micro meaning of "not a mystery." Furthermore, Crozier's informal theory comes close to illustrating the macro meaning of this phrase: although he does not argue that the firm has created the best organizational design for its environment, he encourages us to conduct the relevant thought experiment, comparing the existing organization (poor decision making but low lobbying costs) with an alternative (improved decision making with high lobbying costs).

I find Crozier's informal theory so persuasive that developing a formal model seems trite. I do so nonetheless, for the two reasons given above. First, I hope 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 hope 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 wish to check the internal consistency of his theory and, in the process, derive boundary conditions for his argument.

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,

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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.

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This argument parallels work by Milgrom (1988) and Milgrom and Roberts (1988) on the "influence costs" associated with job design, promotion rules, compensation contracts, and so on.

and let θ 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 $x = \theta$; the problem is that headquarters does not know θ.

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 θ 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 θ, denoted by s. That is, higher values of θ produce higher values of s, but s is also affected by a random shock (denoted by ε) that is independent of θ. 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

$$s = \theta + L + \varepsilon,$$

where ε is normally distributed with a mean of 0 and variance v_ε. Finally, suppose that headquarters can observe s but not any of its components, so that headquarters cannot tell for certain whether the observed value of s is due primarily to θ, ε, 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 (θ) 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 the utility to headquarters is

$$U_h = -(x - \theta)^2 - L.$$

The precise form of this utility function is not crucial; what matters is that the best outcome for headquarters would be $x = \theta$ and $L = 0$.[8] 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. For simplicity, suppose that the cost function is $c(L) = (1/2)L^2$ and that the utility to the division is

$$U_d = x - c(L).$$

Again, the precise form of this utility function is not crucial; what matters is that the division would like to receive an enormous allocation of capital (x extremely large) without having to engage in any lobbying effort ($L = 0$).

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

$$x = d + bs,$$

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In an economic model, utility is a relative concept, not an absolute one. That is, the proper use of the utility from one outcome is to compare it with the utility from another outcome; a single utility number has no meaning. Thus, it is perfectly appropriate that the best outcome for headquarters yields a utility of zero and that all other outcomes yield utilities that are negative.

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, and that the division cannot bargain over the headquarters' choice of d and b, such as by threatening to take itself private through a management buyout.

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 (but not any of its components—θ, L, or ε); (3) headquarters allocates capital to the division according to the rule $x = d + bs$; and (4) headquarters receives the utility U_h and the division receives the utility 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 $E(U_d)$. That is,

$$\max_{L} E[d + bs] - c(L) = d + bL + bE(\theta + \varepsilon) - c(L).$$

A little math shows that the solution is $L^* = b$.[9] 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 $E(U_h)$, given that the division will choose optimal lobbying effort $L^* = b$ in response. That is,

$$\max_{d,\, b} - E[(d + bs - \theta)^2] - L^* = - E[(d + b(\theta + b + \varepsilon) - \theta)^2] - b.$$

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 θ) and the noisiness of the signal s (as measured by v_ε, the variance of ε). If v exceeds ½ then

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

but if $v \leq 1/2$ then $b^* = 0$.[10] The latter case corresponds to the organization Crozier described: if the chance of taking a

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Those who care to dust off their calculus can compute that the value of L that maximizes $bL - c(L)$ solves the first-order condition $b - c'(L) = 0$. Because $c(L) = (1/2)L^2$, we have $L^* = b$.

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A bit more math is hiding here. Recall that, for any random variable y, $E(y^2) = var(y) + E(y)^2$. Thus, because $d + b(\theta + b + \varepsilon) - \theta$ is normal with mean $d + b^2 - (1 - b)m$ and variance $b^2 v_\varepsilon + (1 - b)^2 v$, we have that headquarters chooses d and b to maximize $-b^2 v_\varepsilon - (1 - b)^2 v - [d + b^2 - (1 - b)m]^2 - b$. The first-order condition for d is therefore $d + b^2 - (1 - b)m = 0$, and substituting this into the first-order condition for b yields $-2bv_\varepsilon + 2(1 - b)v - 1 = 0$, which produces the result in the text.

grossly inappropriate action is sufficiently small ($v \leq \frac{1}{2}$) 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 ($v > \frac{1}{2}$) 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 ½), the optimal weight for headquarters to put on the signal also increases (i.e., b^* increases from zero at $v = \frac{1}{2}$ toward one as v approaches infinity). But even for $v > \frac{1}{2}$, 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.[11] 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.

WHO CARES?

At this point it is completely fair to ask whether there is any value added from formalizing ideas that organizational researchers such as Crozier had long ago. The model above was developed for expositional rather than substantive purposes, but the question should be asked of the organizational-economics literature as a whole. I see four answers, versions of which apply to formal models of any kind, not just the rational-choice models I consider.

First, formal rational-choice models check the internal consistency of informal rational-choice arguments. Of course, far from all arguments in organization theory are informal rational-choice arguments, but some are. For example, I think there are many informal rational-choice arguments in the

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This result follows from the facts that (1) increasing the weight from b^* to B^* would increase the division's lobbying effort, which would be harmful to the organization, and (2) the improvement in decision making from increasing the weight would be small once b^* is close to B^*

functionalist literature on bureaucracy and in the literatures on power and politics and on embeddedness. There is value in formalizing such informal rational-choice arguments, because seemingly airtight informal arguments are sometimes wrong and are sometimes right only if additional assumptions are imposed.[12] In some cases, 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. I see my model of Crozier's argument in this spirit: if we interpret the division head as putting no weight on signals that the section heads might send him ($b^* = 0$), then the model shows that this is the optimal organizational design only if there is sufficiently little uncertainty about the action the division head should take ($v < ½$). Of course, providing this boundary condition ($v < ½$) is not a dramatic addition to Crozier's argument, but the argument and the model were chosen for expositional rather than substantive purposes.

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. For example, consider Granovetter's (1973) argument that weak ties relay job offers more frequently than do strong ties, or Lin's (1982) argument that weak-tie offers are drawn from a better distribution of wages. Bridges and Villemez (1986) found that, after controlling for worker characteristics, tie strength has no effect on wages, seemingly rejecting Granovetter's and Lin's theories. But Montgomery (1992) developed a formal model of network composition showing that Granovetter's and Lin's theories do not imply that observed wages are higher for those who found their jobs through weak ties. That is, Montgomery's formal model showed that a seemingly intuitive empirical test was not an implication of Granovetter's and Lin's theories.[13]

Third, unlike the model developed here, which was intended to mimic Crozier's informal rational-choice account, some formal rational-choice models may provide new explanations for facts previously given non-rational-choice interpretations. In Gibbons (1998a), for example, I explained how a rational-choice model of herd behavior (Banerjee, 1992) has close parallels to institutionalist accounts of mimetic isomorphism (DiMaggio and Powell, 1983). In an ideal world, the presence of competing explanations for a given set of facts would inspire empirical testing of the theories' auxiliary implications.

Finally, 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. Below, I offer the analysis in Baker, Gibbons, and Murphy (1997) as an example of this benefit of formal modeling. But Coleman (1964: vii) made this point much more generally and much more eloquently: "If conceptual elaboration is to progress beyond the proverbs of the

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A few readers of this essay have asked for examples of incorrect informal arguments—perhaps because such examples would improve their understanding of my argument, but perhaps also because such examples make good gossip. The infrequency of formal modeling in organizational sociology makes examples hard to generate in that field, so I will stick to organizational economics. I will also choose authors whose reputations are sufficiently strong that being mentioned in this context will do no harm. One example involves career concerns—that is, a worker's concerns about the effects of his or her current performance on future compensation and employment opportunities. Career concerns were first discussed by Fama (1980), who gave an informal argument that managerial incentive contracts are not necessary, because managers are disciplined through the managerial labor market: superior performances will generate high wage offers; poor performances, low offers. Holmstrom (1982b) then developed a formal model that captured Fama's ideas about labor-market discipline but showed that such discipline is nothing like a perfect substitute for contracts: in the absence of contracts, managers typically work too hard in early years (while the market is still assessing the manager's ability) and not hard enough in later years. I give another example in the next footnote, as part of the discussion of how formal models can help to specify and interpret empirical tests.

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As a second example, consider Spence's (1973) signaling model of education, in which workers choose education levels in an attempt to signal their productive abilities to potential employers. In this model, firms rationally offer higher wages to those who acquire more education, even if education has no effect on a worker's productivity. Taken literally, Spence's model deals only with the wage paid at the beginning of a worker's first job after school, but Layard and Psacharopoulos (1974) suggested that the model could be tested by studying the effect of education on a worker's wages at various levels of labor-market experience, as follows. If firms observe some indication of a worker's performance while he or she is in the labor market, firms should use these indications to update their beliefs about the worker's ability, and hence to update their wage offers to the worker. It would therefore seem that the effect of education on wages should decrease with labor-market experience. But Farber and Gibbons (1996) developed a formal model of such learning in labor markets and showed that, while education does come to play a smaller role in firms' beliefs, the estimated effect of education on wages in a linear regression should be constant at all levels of labor-market experience, because of omitted-variable bias. Thus, once again, a seemingly intuitive but informally derived empirical test was not in fact an implication of the theory in question. Furthermore, in this case, the formal model delivered an empirical test that is an implication of the theory and that is not rejected by the data.

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."

WHAT NEXT?

I have tried to argue that (1) Coase's informal argument dictates not only where we should see firms but also what they should look like inside; (2) economics is now producing formal models of internal organization that predict that organizations will be a mess but not a mystery; (3) classic case studies by organizational sociologists strongly support this prediction; and (4) formal economic modeling can add value to organizational research in several ways, but far from all organizational research should be formal modeling. To conclude, I nominate two other areas of organizational research, in addition to the functionalist literature on bureaucracy, in which a blend of description, informal theory, and formal economic modeling might be especially productive: power and politics, and embeddedness.

I think that much of what I have described here (and in Gibbons, 1998a) will mesh easily with the political perspective on organizations developed by March, Pfeffer, 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 the first economic theory of internal organization, the "theory of teams" developed by Marschak and Radner (1972). Team theory has had limited impact on the way economists think about organizations because the theory assumes away all incentive problems. I therefore think many economists would agree with Pfeffer that the bureaucratic/team approach misses something important; in Gibbons (1998a: 38), for example, I argued that "many organizations look more like garbage cans than like teams." As for the economic approach, Pfeffer's summary is accurate for its time, reflecting early work in agency theory, such as Holmstrom's (1979, 1982a). Again, I think many economists now see important omissions in that approach; in a survey of recent work on incentive contracts (Gibbons, 1998b: 117–123), for example, I focused on problems of distortionary performance measurement and reneging that made it quite unlikely that any 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). Perhaps power and politics haven't disappeared from organizations, only from organizational research; perhaps now is the time to bring them back, by integrating the detailed description and informal theory of the original literature with the kind of formal economic modeling I have illustrated here. Rotemberg (1994) and Rajan and Zingales (1996) have offered nice starts in this direction.

A second area that I think could benefit greatly from blending description, informal theory, and formal economic modeling is the study of social structure, as championed by Granovetter (1985) and others. Granovetter offered two main reasons why personal relations may generate trust and discourage malfeasance: "individuals with whom one has a continuing relation have an economic motivation to be trustworthy, so as not to discourage future transactions; and departing from pure economic motives, continuing economic relations often become overlaid with social content that carries strong expectations of trust and abstention from opportunism" (p. 490). To my ear, the first of these reasons is an informal repeated-game argument. As such, it deserves formal modeling for the four reasons given above. Baker, Gibbons, and Murphy (1997) can be interpreted in this light, as follows.

Baker, Murphy, and I were struck by the importance of "relational contracts" (informal agreements, unwritten codes of conduct, norms) both within organizations (Barnard, 1938; Simon, 1947; Blau and Scott, 1962; Eccles, 1985) and between organizations (Macaulay, 1963; Eccles, 1981; Dore, 1983; Powell, 1990). We therefore sought to revisit Coase's comparison of markets versus firms, taking into account the ubiquity of relational contracts in both domains. To do so, we combined two classic but heretofore independent strands of research: a simple repeated-game model of relational contracts (Simon, 1951; Kreps, 1990) with a simple model of vertical integration based on asset ownership (Grossman and Hart, 1986).[14] We found that it may be optimal to choose an organization's formal structure because of its effects on informal relationships within the firm; similarly,

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See Gibbons (1999) for an informal presentation of this model and a discussion of its relationship to recent work in organizational sociology.

we found that the comparison Coase advocates between firms and markets hinges in part on which of these two governance structures facilitates useful relational contracts.

In brief, Baker, Murphy, and I found that the performance of formal organizational structures and institutions depends importantly on the informal relationships that these structures and institutions facilitate. While I do not expect any sociologists to be surprised by this conclusion, I wonder whether a few may be intrigued (or perhaps dismayed!) that Baker, Murphy, and I reached this conclusion *through our model.* Only later did we realize its consonance with the sociology literature. If this exercise in formal modeling can remove such scales from the eyes of three economists, might future formal modeling deliver insights that are new even to sociologists?

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