Recycling the Garbage Can: An Assessment of the Research Program

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The garbage can theory of organizational choice is one of the best-known innovations in modern organization theory. It also has significantly shaped a major branch of the new institutionalism. Yet, the theory has not received the systematic assessment that it both deserves and needs. We evaluate the early verbal theory and argue that it fails to create an adequate foundation for scientific progress. We then analyze and rerun Cohen, March, and Olsen’s computer model and discover that its agents move in lockstep patterns that are strikingly different from the spirit of the theory. Indeed, the simulation and the theory are incompatible. Next, we examine how the authors have built upon these incompatible formulations in developing the theory further. We assess this larger program, which includes the March-Olsen version of the new institutionalism, and find that many of the problems that attended the original article have intensified over time. We conclude that a fundamental overhaul is required if the theory is to realize its early promise.

The garbage can theory of organizations (Cohen, March, and Olsen 1972) has led a charmed life—to its great disadvantage. Although proponents have effectively advanced their ideas, potential critics have done little to bring about a productive intellectual exchange. This one-sidedness impedes scientific progress. And it surely has hurt the garbage can theory, which, despite its prominence, and despite the optimism and enthusiasm that mark its history, has never been able to overcome its inherent weaknesses and make the kind of contribution that, nearly thirty years ago, seemed its destiny.

This is unfortunate. Cohen, March, and Olsen’s first paper is brimming with provocative insights that offer a promising basis for theory, but thus far their potential has largely gone untapped. Indeed, the theory that has grown up over the years is so complex and confusing, and some of its components are so seriously flawed, that there is little reason for thinking that it can look ahead to a more fertile future. For fundamental reasons, the theory lacks the rigor, discipline, and analytic power needed for genuine progress. We want to explain why this is so and what might be done to get it back on track.

Why expend so much energy examining an “old” article and its progeny? Such an exercise would be eminently worthwhile if only because some of the ideas are well worth salvaging. But there is more to be gained than that. The 1972 article has had considerable influence on political science and on institutional theory more generally. Some of this influence is directly due to the original article, whose ideas have shaped thinking about organizations ever since, and which presents one of the most famous computer simulations in all of social science. Perhaps most of its influence, however, is now exercised indirectly, through more recent works that have moved into new terrain and gained much attention in their own right but are grounded in the original theory. A few of the latter have been carried out in the work of other authors, notably Kingdon’s Agendas, Alternatives, and Public Policies (1984), one of the most highly regarded treatments of policymaking in the last two decades. But virtually all the basic theoretical work in this tradition has been done by the original authors, particularly March and Olsen.

Consider the lineage. Ambiguity and Choice (March and Olsen 1976a) expanded on the 1972 theory, received considerable attention, and solidified the garbage can’s new status as a major school of organization theory. Subsequently, the American Political Science Review published March and Olsen’s (1984) hugely influential overview article, “The New Institutionalism . . . .”, which officially ushered the new institutionalism into political science and offered a perspective on institutional theory that gave a prominent role to the garbage can and its progeny. Henceforth, political scientists would routinely consider the March-Olsen line of theory as a main variant of the new institutionalism (Peters 1996). This article, in turn, developed into March and Olsen’s (1989) longer statement on the new institutionalism, Rediscovering Institutions, which, as we will show, was strongly influenced by the original garbage can theory and its extensions. This book met with much acclaim and has been judged a “contemporary classic” by Goodin and Klingemann (1996) in their recent survey of the discipline.

This is an impressive hit parade; clearly, the garbage can theory has had an important impact on the field. Yet, remarkably, there have been no systematic at-
tempts to assess the theory or even to clarify its logic and content so that everyone can agree on what it actually says. This is not true for the other two major approaches to institutional analysis—theories of rational-choice (e.g., Weingast 1996) and of bounded rationality (Cyert and March 1963; March and Simon 1958; Simon 1947)—that are its main rivals. Scholars are quite clear about what these theories say, and both traditions have been the subject of debate and criticism (e.g., Green and Shapiro 1994). The garbage can theory has escaped all this. Even if it were a shining example of social science theory, this would be unhealthy, for no theory and no field of study can do without criticism, clarity, and debate. The situation is all the more worrisome given the intellectual influence the garbage can has had over the years. If we are right about its deficiencies, this influence cannot help but propagate problems that the discipline should avoid in its pursuit of truly productive theories.

We offer a critical assessment of the garbage can theory that is as comprehensive as possible, befitting a research program that has gone unchallenged for its entire lifetime. We also seek to clarify what the theory actually says, which is absolutely necessary given the pervasive confusion that surrounds it. Finally, we try to map out in a bit more detail the theory’s intellectual lineage in order to show how the original work—and its problems—are reflected in the new institutionalism. This is a lot to do in one article; there is not enough space to do more, although we would like to. Our immediate goal is to stimulate an intellectual exchange that will promote a better understanding of the garbage can approach and its role in institutional theory. In the longer term, we hope to be part of a collective effort to reconstruct the theory around some of its more promising (and still exciting) ideas and thus help revitalize the research program and set it on a more productive path.

We cannot delve into the details of this reconstruction here. That remains for future work. But we do think—for reasons we will cover below—that the garbage can’s key ideas can best be developed by anchoring them firmly in theories of rational choice or bounded rationality, most likely the latter, whose analytic apparatus for exploring decision making under cognitive constraints is particularly appropriate. Understood in this way, a revised garbage can theory would become a parametric variant within the bounded rationality program. This would clarify the garbage can’s theoretical logic, give it a clear microfoundation, and provide it with real deductive power for pursuing its insights—all of which it now lacks.

THE VERBAL THEORY AND THE COMPUTER MODEL

What is the garbage can theory (GCT)? This is a simple question with no simple answer. The original article sets out two distinct formulations. The first is a set of ideas about how decisions get made in “organized anarchies.” These ideas, because they are expressed in ordinary English and their logical relations are not rigorously spelled out, constitute a verbal (or informal) theory. This is Cohen, March, and Olsen’s (1972) general statement of the garbage can theory as originally conceived. The second formulation is the computer model, which is intended to capture in simplified, mathematical form the key features of the verbal theory and to provide a rigorous logical structure that can generate predictions. We will examine both of these.

The two formulations should be kept separate. The verbal theory is fundamental. The computer model is derivative: It is only one of many possible ways to formalize key features of the theory and draw out its implications. But we must remember that any particular formalization is not unique. We must also remember that the sophistication and rigor of the simulation do not in themselves guarantee that it faithfully reflects the theory; depending on the details of its design, so much may be lost in the translation that the model could turn out to have little value or even prove misleading.

The place to start if we want to understand the GCT, then, is with the verbal theory. The original article, however, sheds surprisingly little light on its content. Only two pages discuss it, and little is done to develop, connect, or clarify its central ideas. Instead, almost all the article is devoted to the computer model, whose properties are discussed in detail, and whose implications are explored at length and then applied to a specific empirical domain (universities).

Most of what we know about the informal theory comes not from the original article but from later work, particularly March and Olsen’s Ambiguity and Choice (1976a), March and Weissinger-Baylon’s Ambiguity and Command (1986), and March and Olsen’s Rediscovering Institutions (1989). (See also March’s A Primer on Decision Making [1994].) This later literature does more than fill in the basic contours of the original theory, however. It also dramatically extends the scope and complexity of garbage-can-like thinking and ultimately develops a new theory, the March-Olsen variant of the new institutionalism, which makes frequent, although imprecise, use of GCT ideas.

Unfortunately, the authors and their colleagues rarely distinguish between the informal theory and the computer model when specifying what GCT is and what it tells us about organizations. This holds for most of the more recent literature on the GCT and is even true of the original article. There the authors begin with a clear distinction between the two, but by the concluding section they have tossed both into the same idea-pool, characterizing garbage can processes by properties that come sometimes from the verbal theory and sometimes from the computer model—but without any recognition that this is being done. GCT thus emerges as an undifferentiated blend of the two.

We will sort through all this by proceeding as follows. First, we clarify and evaluate the ideas that appear most central to the verbal theory—emphasizing those in the original article but including some introduced in more recent work when these seem appropriate. Second, we turn to the computer model, clarify its
structure and operation, and assess its contribution to the larger enterprise. Third, we examine GCT's post-1972 theoretical development, including its influence on the March-Olsen variant of the new institutionalism, and assess these newer lines of research. Finally, we briefly illustrate how the GCT might be revitalized by using some classical ideas of bounded rationality to analyze one of the garbage can's most provocative insights.

THE EARLY VERBAL THEORY

The verbal theory is about organized anarchies, a class of organizations or decision situations marked by problematic preferences, unclear technology, and fluid participation. As the definition suggests, organized anarchies face certain kinds of ambiguity. This is what makes them interesting. GCT is an effort to explain how organizations make choices and solve problems under conditions of ambiguity so troubling they would appear to render decision making extremely difficult or impossible.

The various branches of rational choice theory have long had sophisticated tools for modeling individual choice under uncertainty. But GCT is founded on a radically different approach to choice. Its premise is that choice in an organized anarchy cannot be understood via the intentions of organizational participants, and imposing a rational explanation on organizational behavior can only distort what is really going on. Choices often just happen, with no clear connection to what participants want. They arise from dynamic organizational processes that are complex, highly contextual, and unpredictable, driven more by accident and timing than individual intention. This gives much of organizational life an almost chaotic appearance.

At the heart of GCT is the notion that organizational outcomes arise from independent "streams" of problems, solutions, participants, and choice opportunities, whose random intersection generates decisions. In this scheme, choice opportunities are the garbage cans. As problems, solutions, and participants move independently about the organization, various combinations find themselves dumped into these cans, and the decisions coming out (if any) depend on whatever mixtures the intersecting streams happen to generate. The organization's basic structure (notably, its rules) channels and constrains this multifarious action, thereby shaping the organization's patterns of choice and problem solving. But the driving force of the garbage can explanation is process.

The process-driven world of the garbage can is more than just dynamic. It is also strange and even pathological by conventional standards, and the authors make much of this. Alice has gone through the looking-glass, and nothing is as it seems. Choices happen for no apparent reason. Outcomes are divorced from intention. Solutions are disconnected from problems. People wander aimlessly in and out of decision arenas. In this welter of loosely coupled activity, some decisions do get made, and some problems do get solved. There is a chaotic brand of "intelligence" at work. But it is largely due to the time-dependent confluence of events, not to the rational effects of plans or goals.

The organizational literature often calls the GCT a metaphor. Even the authors have used this term (e.g., March and Olsen 1986). In a sense this rings true: The notion of different inputs being mixed together in a garbage can, with output depending on what is mixed with what, is clearly metaphorical. But we must be careful with this kind of language, because calling a set of ideas a metaphor may inappropriately shield it from criticism. A metaphor is, after all, a literary device that need not meet the same standards as social science theories. Yet, the GCT's basic ideas are obviously intended to be a theory and should be treated as such.

Any effort to clarify and appraise the theory could highlight a large number of issues. We think the following are among the most important.

Individual Choice and the Level of Explanation

GCT is a theory of organizational choice. In developing it, the authors naturally move back and forth between individuals and organizations. They discuss how individuals make choices and how these choices depart from standard notions of rationality. They also talk about how, as an organization's dynamics work themselves out, the choices of individuals are aggregated into organizational choices. The impression is that GCT explains organizational choice via a logically coherent, if unorthodox, theory of individual choice.

But this is not really so. Although the authors make clear that they are abandoning conventional rational choice approaches, they do not develop their own model of individual choice that can function as a component of GCT. Indeed, the original article offers no theoretical treatment of individuals at all; and in the computer model individuals are little more than automatoms, barely touched by motivations or beliefs. The article fashions a theory of organizational choice based on assumptions about process and structure—but without any underlying theory of individual choice. The explanation is at the macro level.

In more recent work, which moves toward a larger theory, the authors do talk theoretically about individuals and offer arguments about how decisions are made in the face of ambiguity. In these discussions, however, everything inherent to individual choice is left endogenous to the choice process. Among other things, it is not assumed that preferences exist prior to choice or that they determine actors' evaluation of alternatives; rather, people discover their preferences only after they have decided, which begs the question of how decisions are made in the first place.

Empirically, it might seem attractive to relax every assumption of classical rational choice theory and argue that all these aspects depend on experience. Analytically, however, with everything treated as endogenous and nothing taken as given, there is no logical basis for deriving behavioral expectations or for moving from individual to organizational properties. The authors try to fill in the gaps by referring to other
possible foundations of individual behavior—symbols, rules, duty, obligation, myths, and the like—but these complications, which may or may not be empirically warranted, do not add up to a coherent model of individual choice that can generate testable implications. If anything, they hinder a serious effort to construct one.

However one might evaluate these more recent efforts to build a theory of individual choice, the early GCT is essentially a macro theory of organizations. It talks a lot about choice and individuals, but the theory really focuses on process and structure—organization-level phenomena—and does not arise from individualistic foundations.

**Independent Streams**

The hallmark of GCT is its attempt to understand organizations by reference to streams of problems, solutions, participants, and choice opportunities, which are independent of one another and exogenous to the system. On the surface, this formulation seems to be a simple, straightforward way of characterizing the internal dynamics of organization. It is memorably provocative, widely cited—and crumbles upon closer examination.

First, in the real world problems are typically identified and put on organizational agendas by organizational participants. Similarly, solutions find their way into organizations because they are pushed by certain participants in light of the problems they face. There is nothing mysterious about this. It is obvious. People are the carriers of problems and solutions. Indeed, when Cohen, March, and Olsen discuss garbage can processes, they often say exactly that. Their original article (1972) even included this point in its list of "basic ideas": "To understand processes within organizations, one can view a choice opportunity as a garbage can into which various kinds of problems and solutions are dumped by participants as they are generated" (p. 2, emphasis added). This sort of reasoning is repeated elsewhere. But if problems and solutions are dumped by participants, then how can streams of problems and solutions be independent of the stream of participants? Clearly, which problems and which solutions get dumped into a particular garbage can depend on which participants happen to go there. They are not independent streams.

There is a second important source of confusion that surrounds the notion of independent streams. This one arises from an odd use of language. Take, for instance, the concept of "solution." *Webster’s Collegiate Dictionary* defines this term as "an action or process of solving a problem" (p. 1123), which is surely the way it is commonly used and understood. By definition, then, solutions do not exist on their own, independent of specific problems. Nor does it make sense to call an object (like a computer) or an idea (like decentralization) a solution without indicating what problem it is meant to address. Objects are objects. Ideas are ideas. They qualify as solutions, and we are justified in applying that concept to them, only when we show that they somehow address particular problems. It follows that streams of solutions and problems cannot possibly be independent of one another—unless commonly accepted definitions are abandoned.

The 1972 article defines a solution as "somebody's product" (p. 3), using a computer as an example. By this definition, there is no logical connection between solutions and problems, and the two can be "decoupled" into independent streams. GCT proponents can then talk provocatively about solutions chasing problems and problems chasing solutions, a standard ingredient of garbage can chaos. But all this turns, as we have suggested, on a definitional distortion. Their concept of solution is not the one everyone else uses; it is the same word invested with an entirely different meaning. Hence, the theory is not about solutions as we understand them. It is about something else. Had this something else been given a label of its own, this source of confusion would have been eliminated. But GCT would have appeared much narrower and less interesting, and its claims, of course, would have been very different.

This linchpin of GCT's framework, then, does not stand up to scrutiny. The notion of independent streams may appear at first blush to be an inspired simplification, but we are hard put to see how it can be justified.

**Organizational Structure**

The original article emphasizes organizational dynamics, but it also recognizes a significant role for structure, which influences how people, problems, and solutions get matched up in choice situations and what outcomes are generated as a result. What it says about structure, however, is very limited—and what it leaves out is crucial. Two problems stand out.

The first arises because the early GCT takes structure as exogenous, thus ignoring where structure comes from or why it takes the forms it does. The theory's focus is on the effects of structure, not on its origin, design, or change. All else equal, we would not object to this. Something must be taken as exogenous; not everything can be explained. But in this case there is a heavy price to pay.

Precisely because structure does have a major effect on organizational processes and outcomes—and therefore on its effectiveness and its distribution of costs and benefits—decision makers are likely to regard it as a

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1 This has been reported as an empirical finding by Stephen Weiner (1976), who contributed a case study to March and Olsen's (1976a) edited volume on GCT: "The San Francisco study stimulates a view of the flows of problems, solutions, and energy, when the choice is subject to a deadline, that varies from the theoretical predictions. Problems and solutions, on the one hand, and participants, on the other hand, are intertwined. Problems and solutions are carried by participants" (p. 245, emphasis added). But his observation was virtually ignored in the book's theoretical chapters. (For an extended examination of the idea of independent streams, see Heimer and Stinchcombe 1998.)

key means of shaping organizational behavior. This is unavoidable. To say that structure has important effects is to imply that it can be used to achieve certain ends. And participants, especially those in authority, have incentives to do just that. When leaders act on these incentives, then, they need not accept the garbage can's chaotic dynamics as uncontrollable facts of life. If GCT is correct in claiming that garbage can processes work very differently in different structures, then the theory's own logic suggests that leaders could choose structures with this in mind.

It is difficult to deny that the instrumental use of structure by leaders is pervasive and fundamental to an understanding of organizations. Yet, GCT essentially ignores it. The result is that GCT is a theory of short-run dynamics that fails to account for how structure would adjust over time to garbage can processes flowing within—and it thus fails to explain how organized anarchies would ultimately behave. The thrust of GCT is to highlight the chaos, perversity, and ineffectiveness of organized anarchies—and what the theory explicitly omits, it turns out, are the structural adjustments that could create more orderly and productive organizations. Absent structural choice, the deck is stacked.

The second problem with the authors' treatment of structure concerns the kinds of structure they consider relevant to an understanding of organization. It turns out that essential components of real organizations are simply omitted and thus can never figure into the garbage can's short-term dynamics. The result is a perspective that misses and mischaracterizes much of organizational behavior.

The most significant omissions bear on issues of authority, delegation, and control. Whether or not organizations have neat hierarchies, virtually all—including universities, the quintessential organized anarchies—have authority structures of some sort. In these systems, superiors can tell others what to do, hire subordinates to do their bidding, and delegate authority to them. They thereby create control opportunities: They can get much more done by extending their reach into far-flung decision arenas, even if they cannot personally participate. But they also create control problems: Subordinates will not necessarily do what is in the superior's best interests. Other structures, such as incentive schemes and monitoring systems, are used to promote as much congruence as possible. The structure of an organization largely reflects efforts to control and coordinate the actions of separate but interdependent individuals.

These issues are at the heart of organization and, not coincidentally, have long been at the core of organization theory. GCT, however, is built around a simplified framework of independent streams that largely ignores them. Although the authors recognize that some participants may be more "important" than others, in having more access to "important" decision arenas, there is no systematic attempt to recognize that participants are connected—notably, that many are agents of others—and that these connections are not accidental: They are designed to extend the reach of superiors. Instead, the world of the garbage can is made up of anomic individuals (the notion of a "stream" of participants says it all), and organizational decisions turn on who happens to wander into particular decision arenas.

Structure can induce order and productivity, but GCT does not allow participants to recognize as much and simply removes structure from their realm of choice. Were these issues of structure seriously integrated into the theory, it is hard to see how the garbage can's basic claims could be maintained.

Organized Anarchy

A fundamental GCT claim is that in a certain domain, organized anarchy, garbage can processes prevail. To test this hypothesis one must identify the domain. What, then, are organized anarchies (OAs)? Cohen, March, and Olsen (1972, 1) define them as "organizations characterized by problematic preferences, unclear technology, and fluid participation." But this definition, though widely cited, suffers from serious ambiguities that subsequent work has done little to clarify.

The first problem arises from the 1972 article's ambiguous statements about whether OAs must have all three properties or just some of them. Much of the discussion suggests that each property is necessary. Yet, in the conclusion, OAs are described as situations that depart from classical decision models "in some or all of three important ways: preferences are problematic, technology is unclear, or participation is fluid" (p. 16, emphasis added); that is, each property suffices to identify an organized anarchy.

This confusion between the logic of "and" versus "or" creates two problems. First, the size of the concept's domain is unclear: It is much larger if only one property is needed to identify an OA than if all three are required. Second, it opens up a theoretical can of worms for the research program. For instance, under the "some or all" stipulation, an agency with clear goals, rigid participation, and unclear technology and one with clear goals, clear technology, and fluid participation would both be OAs. But why would we expect either to behave like garbage cans? In both cases preferences are unproblematic, so solutions could not chase problems; options that fail the (clearly) relevant criteria will simply not be considered solutions. Furthermore, what are the theoretical reasons for assign-

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3 "Organized anarchies interfere with fundamental tactical principles that depend on tight control and coordination... adaptations employed by naval commanders include the use of SOPs, operational plans, and strong centralized authority acting through a chain of command" (Weissinger-Baylon 1980, 51, emphasis added).

4 A subsequent article (March and Romelaer 1976) does recognize an important role for authority, particularly in allowing superiors to delegate and thereby participate in many decision arenas simultaneously, but this interesting line of analysis has not been integrated into the larger structure of the GCT.

5 See, for example, the preceding quote, where the three properties are linked by "and."
ing both agencies to the same set? Would not an organization with unclear technology and rigid participation behave differently from one with clear technology and fluid participation?

Finally, the meaning of each property in isolation is open to question. Consider universities, which are held up as paradigmatic organized anarchies. In what sense is the technology of teaching unclear? True, it is uncertain how to teach graduate students to become creative researchers. But it is known how to teach statistics, chemistry, and many other subjects: Certain sequences of readings, lectures, problem sets, and discussion sections produce a stable percentage of students who grasp the material, as verified by exams. Thus vital aspects of teaching are understood rather well. So is this an "unclear technology" or not? The garbage can literature gives us no guidance. Much the same could be said for problematic preferences and fluid participation.

GCT and the March-Simon Tradition

It is a common perception (e.g., Goodin 1999, pp. 71–2) that GCT, though now important enough to be considered a research program of its own, evolved from the earlier behavioral theories of March and Simon and thus can be considered an offspring of the decision-making school of organization theory. On the surface, this makes sense. After all, James March is a founder of both the Carnegie school and GCT, so one expects a strong continuity of ideas. Moreover, Cohen, March, and Olsen (1972) go to some lengths to describe their work in exactly this way, emphasizing that they "build on current behavioral theories of organizations" (p. 1), that their ideas about organizations "have a broader parentage" (p. 2) in that literature, and that their intentions are "conservative," aiming "to continue a course of theoretical development" with roots in the decision-making tradition (March and Olsen 1976c, 22).

With effort, one might make a case for this view. On the whole, however, it mischaracterizes the garbage can. The March-Simon tradition is grounded in a theory of individual choice. Although it relaxes classical rational choice assumptions to account for human cognition, it borrows heavily from economic methodology (e.g., Cyert and March 1963; Simon 1947), and its explanations are individualistic. It sees behavior as intendedly rational, it sees organizations as products of boundedly rational choice, and it seeks to model all this as clearly and simply as possible.

At the heart of the March-Simon tradition is an analysis of how cognitively constrained individuals solve problems and what this entails for organization. The theory forges a connection between bounded rationality and organizational structure, demonstrating that, precisely because individuals are limited in knowledge and computational abilities, they must rely on organizational routines and other forms of programmed behavior to solve their problems. Above all, the theory provides an explanation of structure, arguing that it arises out of the cognitively limited yet intendedly rational behavior of very human problem solvers.

The garbage can's approach is very different. Three contrasts stand out.

First and most fundamentally, GCT rejects the key tenet of the entire Carnegie tradition: that individuals are intendedly rational. Indeed, it does not even portray individuals as caring about solving problems.

Second, GCT does not relax rational choice assumptions selectively, as March and Simon do, in order to retain some of the power and rigor of the methodology. It rejects all the relevant choice-theoretic assumptions and abandons the entire program of methodological individualism. In contrast, what the March-Simon tradition says about organizations is firmly rooted in a coherent model of individual choice. That is its analytic foundation.

Third, the basic aspects of organization that March and Simon seek to explain—structure (especially) and process—are treated by GCT as exogenous. No explanation is given. The aim of GCT is to explain outcomes, not organization. The aim of the March-Simon tradition is to explain organization.

It is important not to confuse GCT with its authors, who have surely been influenced by the March-Simon tradition. Analytically, however, GCT is only tenuously connected to that tradition; indeed, it rejects the decision school's core components. So it is misleading to think of the current formulation of GCT as an outgrowth of bounded rationality conceptions of organization. A clear, cold look at the theory suggests that it has much more in common with the institutional school of organization theory (e.g., Meyer and Rowan 1977), which also rejects the idea that organizations can be understood in terms of the rational behavior of their members. Institutional theory emphasizes other influences—symbols, myths, legitimacy—that are reflected, nonrationally or even irrationally, in the foundations of organization. As the GCT authors develop their larger theory, it is evident that this is the branch of organization theory where the garbage can is most at home.

THE SIMULATION MODEL

The computer simulation formalizes the verbal theory and is widely regarded as the research program's scientific core. We agree that formal modeling is desirable, but the specific model they develop represents the theory and exacerbates the GCT's problems. To show this, we need to describe the model's basic assumptions and how they generate outcomes.

A Description of the Model

The computer model simulates the behavior of an organized anarchy over twenty periods, in which the various organizational streams interact to produce outcomes. The choice stream consists of ten choice

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6 For a more detailed analysis of the simulation, see Bendor, Moe, and Shotts 1996.
arenas (the garbage cans), the participant stream of ten decision makers, and the problem stream of twenty problems.

This much follows the informal theory. But notice, right at the outset, what is missing. Whereas the verbal theory posits a central role for streams of choices, participants, problems, and solutions, the computer model has no stream of solutions. No explanation is ever offered as to why this stream is missing. Given the theory, the temporal pairing of problems with solutions ought to be fundamental. And it goes without saying that any notion of "solutions chasing problems and problems chasing solutions"—perhaps the most famous property of the verbal theory—cannot be part of the model at all if it does not include solutions in the first place.

Let us put this aside for now, however, and return to describing the computer model as it was constructed. The temporal sequencing is as follows. During each of the first ten periods, one new choice arena and two new problems enter the simulation, where they may be acted upon by the organization's decision makers, all ten of whom are assumed to be present from the start. By the tenth period, all choices and problems have been introduced. There are no new inputs during the last ten periods.

During each period, each problem and each decision maker attaches itself to (at most) one choice arena. They "decide" where to go, subject to two constraints. First, they cannot go to a choice that has not yet been introduced or has already been made. Second, their options are limited by the organization's "access structure" (for problems) and "decision structure" (for participants).

The access structure determines which problems have access to which choice arenas. In an unsegmented access structure, the problems can go anywhere: They can attach themselves to any active choice arena. In the hierarchical structure, problems are ranked by importance: The two most important problems can go to any choice arena, the next two can go to nine of the ten choice arenas (the off-limits arena being "too important" for them), and so on, down to the two least important problems, which can go only to one of the ten arenas. In the specialized access structure, each problem can go only to one particular choice arena: Two of the problems can go only to arena #1, two can go only to arena #2, and so on.

Similarly, the decision structure determines where decision makers can go. In the unsegmented decision structure, any agent can work on any choice. In the hierarchical structure, important decision makers have access to more choice arenas than do less important ones. And in the specialized structure, each agent can work in only one particular arena.

Within these constraints, each problem and decision maker follows a simple rule in selecting a choice arena: Go to the choice that is closest to being made. This rule is a crucial assumption that drives many of the results. We will discuss how it works in more detail below, after describing how choices are made.

How Choices Get Made

In the model, the key concept for understanding how choices get made is "energy"—a concept that has no role in the informal theory. The model, unlike the theory, secures choices as the product of an energy balancing mechanism. It works as follows.

Each problem requires a certain amount of energy to be solved. This requirement is the same for all problems in an organization but can vary across organizations depending on the "load" they face. (The higher the load, the more energy is required per problem.) The energy required to make a choice is the sum of the energy requirements of the problems attached to that arena.

Decision makers supply the energy that gets things done. In each period a participant supplies a fixed amount of energy to the choice on which he is currently working. Not all this energy is assumed usable, however, in solving problems. The usable energy is derived by multiplying the available energy by a fraction, set in the model at 0.6. The authors interpret this fraction as representing "solutions" in the simulation. Presumably, larger fractions mean more usable energy and thus more solution power. To reiterate, however, there is no stream of solutions, as the verbal theory requires. Indeed, apart from this fixed fraction, solutions play no role in the model.

In any period, the energy supplied to a choice arena is the sum of the amounts brought by each of the participants to that arena. Curiously, this energy supply is assumed to cumulate over time: The total energy in a choice arena is the sum of all the energy anyone has ever brought to that arena since that choice was first introduced. This creates the possibility of "ghost" energy (our term): A choice arena may have usable energy in a period even though no one is in the arena at that time.

If the total usable energy equals or exceeds the energy required by the problems currently attached to the arena, then the choice is made and the problems are solved. Because of ghost energy, however, problems may get solved even though no one is present to work on them. More perplexing still, a choice can be "made" even when there are no problems in the choice arena at all. Indeed, this is likely, because when there are no problems the energy requirement is zero, and the presence of any agents or (even without participants) any ghost energy automatically satisfies the energy requirement. By the simulation's logic, then, a choice can be "made" even though no problems are solved, no one is present—and, quite literally, nothing happens.

This is not, of course, what is normally meant when we say a choice has been "made." To claim, as the model does, that a choice is "made" when nothing is happening is an odd use of language and ultimately a source of serious confusion. It is better to say, under these circumstances, that the choice arena closes down, or the meeting adjourns without any decisions being taken. This accurately describes what actually happens, and it does not imply that choices are being made when
they are not. We will return to this peculiar form of decision "making" below, for it plays an important part in GCT analysis.

Finally, we want to be clear about how energy relates to the movement of people and problems. As noted, both follow a simple decision rule: Go to the choice that is closest to being made. Reinterpreted in energy terms, this rule says: Go to the choice arena with the smallest energy deficit, that is, the smallest gap between the energy required to solve problems and the energy supplied by agents (including ghost energy). In each period, problems and participants look over all choice arenas, calculate each arena's energy deficit, and move in the next period to the open arena with the smallest deficit (subject to the constraints of the access and decision structures).

The Verbal and Formal Theories: Different Worlds

These are the model's essential components, which largely determine how it simulates the behavior of organized anarchies. We have already pointed out some serious design problems: the absence of a solution stream, for instance, and that choices can be "made" even when no people or problems are present. Before we go on, we want to add four general observations.

First, the informal version of the GCT is clearly an information-processing formulation. It refers to intelligence in the face of ambiguity, to technologies that are poorly understood, to the allocation of attention. All these ideas are fundamentally cognitive or informational. The simulation is markedly different. It is built around the concept of energy: information and beliefs of decision makers are not explicitly represented at all. Instead, participants are simply carriers of energy. In addition, problems are defined in terms of energy requirements, solutions are coefficients that convert available energy to usable energy, people and problems move around in response to energy deficits, and choices are made when energy deficits are overcome. Nothing here resembles the verbal theory.

Second, the model's assumptions about participants are especially odd. They are basically bundles of energy—virtually automata. They have no explicit objectives and are essentially unmotivated. They do follow a decision rule: They go to choices that are closest to being made. But the authors do not suggest that this reflects an underlying goal, nor do they explain why decision makers might want to do this. What we do know about participants is that they do not care in the slightest about solving problems. They are perfectly happy to go to an arena that contains no problems and participate in the "decision." Indeed, because such a can will have a light energy load, their decision rule implies that they actually prefer it to one with problems. It is hardly surprising, then, that the simulation ultimately shows that organizations are bad at solving problems. No one is interested in solving them.

Third, the verbal theory is about a stochastic world filled with ambiguity, chaos, and randomly intersecting streams of activity. Its depiction of organizational dynamics is built around randomness and the uncertain confluence of events. But the simulation is fundamentally deterministic. People and problems move at the same time, using the same deterministic rule. And the technology assures that choices will always get made in the same way when certain energy requirements are met. The world created by the computer simulation is dynamic—people and problems move around over time—but it is clearly not the stochastic world the verbal theory describes, because it omits the very essence of what that world is supposed to be about.

Fourth, if we accept the interpretation that the three properties of problematic preferences, unclear technology, and fluid participation are individually necessary and jointly sufficient for something to be an organized anarchy, then this central concept of the informal theory is poorly represented by the simulation. Compare these defining properties of an OA to the simulation's nine structural variants, shown in Figure 1.7 When the access structure is hierarchical (versions corresponding to cells 4, 5, and 6), problems are ranked by importance. Thus, the organization's preference ordering is completely coherent; its preferences are not problematic at all.8 Regarding technology, versions in cells 7, 8, and 9 have specialized access structures: Problems are coded by type and go to appropriately coded choice arenas. This is a clear technology: The organization knows enough about problems to categorize and route them correctly. Finally, participation is completely rigid in cells 3, 6, and 9, for these have specialized decision structures that restrict each agent to a single choice arena; it is semirigid in cells 2, 5, and 8, where lower level decision makers are sharply constrained. Thus, to remain true to the verbal theory's central ideas, the simulation should have highlighted a single variant: the unsegmented-unsegmented version. This is not merely one version of the model; it is the prototypical organized anarchy.

A World of Remarkable Order

Given this design, it is not surprising that the computer model leads to very odd and inappropriate implications for the behavior of organized anarchies, implications that clash with the informal theory. This is not obvious from the original article, which does not present detailed information on how the simulated organizations function or what outcomes are produced. Instead, it gives summary statistics about things like "decision style" and "problem activity." But when the simulations are rerun (as we did, using the original FORTRAN program), the gulf between model and theory is strikingly clear.

This disconnect can be illustrated in many ways, for the underlying design problems leave their fingerprints

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7 Figure 1 is a graphical representation of text information in Cohen, March, and Olsen 1972.
8 Moreover, as we have seen in the analysis of choice behavior in the simulation, to the extent that individuals have goals, they all "want" the same thing: to go to arenas that are close to finishing up. Hence their interests do not conflict at all.
FIGURE 1. The Simulation’s Nine Structural Variants

<table>
<thead>
<tr>
<th>Decision Structure</th>
</tr>
</thead>
<tbody>
<tr>
<td>Unsegmented</td>
</tr>
<tr>
<td>1</td>
</tr>
<tr>
<td>Unsegmented</td>
</tr>
<tr>
<td>4</td>
</tr>
<tr>
<td>Hierarchical</td>
</tr>
<tr>
<td>7</td>
</tr>
<tr>
<td>Specialized</td>
</tr>
</tbody>
</table>

Access Structure

Note: The decision structure maps decision makers onto choice arenas. The access structure maps problems onto choice arenas. Each cell represents a variant of the simulation.

almost everywhere. We will focus on one problem that is absolutely fundamental—and fatal.

The heart of the verbal theory is the notion of independent streams of participants, problems, and solutions intersecting unpredictably in organizational garbage cans—choice arenas—to generate organizational outcomes. This is basic. Perhaps the most important question we can ask of the computer model, then, is whether it reflects this central feature of the informal theory. The obvious place to look for this brand of chaotic behavior is in the paradigmatic organized anarchy, where both the access and decision structures are unsegmented. In these “unsegmented-unsegmented” organizations, anyone can work on any choice, any choice arena can be burdened by any problem, and virtually any permutation of choices, participants, and problems seems possible. If the model can produce garbage-can-like behavior at all, then one would surely expect to find it in this prototypical case.

To see, we re-ran the purely unsegmented version under all three energy loads: light, medium, and heavy. The results are illustrated in figures 2, 3, and 4. To simplify, we have scaled these figures down by half in every dimension: Each depicts only five choice arenas, five decision makers, ten problems, and ten periods. This scaling loses no essential information and makes the figures easier to read. The text’s descriptions of the simulation follow the figures and so are also scaled down by half.9

Consider the light load first (Figure 2). In the initial period, one choice arena and two problems enter the simulation.10 Because only one arena is open, both problems and all decision makers move—as a pack—to that one. The sum of the participants’ energy inputs exceeds the problems’ energy requirements, so the choice is made (via the energy balancing mechanism described earlier), and the problems are resolved. In each of the next four periods, exactly the same thing happens: Decision makers and problems travel in a pack from arena to arena, so that each choice is made, and each problem is resolved, immediately upon entering the simulation. The organization is extremely orderly and totally effective.

Under a medium load, with more energy needed to solve each problem, the organization does worse. Nonetheless, the main behavioral pattern is much the same: From period three on, all agents and all active problems move together in a pack (Figure 3).11 Here is what happens. When a new choice enters the simulation after the second period, all players go to that arena. There they supply energy, but because it is less than the problems’ energy requirements, the choice is not made. In the next period, the participants and problems all go to the next new garbage can that opens up. The old choice that they left behind is now “made,” even though it contains neither decision makers nor

9 For example, when we say “at the end of period five all five choices are active,” this is exactly what the corresponding figure (Figure 4) portrays. In the full-scale simulation, all the choices (all ten) are not activated until period ten.

10 The order in which choices and problems enter is irrelevant. Our figures use the following orders: for choices it is 3, 1, 5, 4, 2; for problems it is 8, 6, 7, 3, 2, 9, 4, 1, 5, 10.

11 In period 2, the decision makers separate from the problems due to a slight difference in how energy balances are calculated. The details of this assumption are unimportant.
FIGURE 2. An Unsegmented-Unsegmented Organization Facing a Low Load

Choice Arena

Period

#1

#2

#3

#4

#5

Period

Prob
7,3

DM
All

Period

Prob
8,6

DM
All

Period

Prob
2,9

DM
All

Period

Prob
4,1

DM
All

Period

Prob
5,10

DM
All

Period 6

Period 7

Period 8

Period 9

Period 10

Key

Not yet active, has not yet entered the stimulation.

Prob
2.7

DM
4.35

Active, does get made this period. The attached problems (2 and 7 in this example) are solved.

No longer active; already made.

Note: Each box shows the status of a particular choice arena in a particular period. Each column shows how an arena's status changes over time. A choice arena must be in one of four states: (1) not yet activated; (2) active, choice not yet "made" in the current period (none in this figure); (3) active, choice does get "made" in the current period; (4) deactivated (after a choice was made).
FIGURE 3. An Unsegmented-Unsegmented Organization Facing a Medium Load

Choice Arena

Period 1

#1

Prob

8,6

DM

All

Period 2

#2

Prob

8,6,7,3

DM

None

Period 3

#3

Prob

None

DM

None

Period 4

#4

Prob

8,6,7,3

DM

All

Period 5

#5

Prob

8,6,7,3

DM

None

Key

Not yet active, has not yet entered the simulation.

Active, does not get made this period. Problems 2 and 7 (for example) are attached to this choice in this period. Decision makers 4, 3, and 5 are working on this choice in this period.

Active, does get made this period. The attached problems (2 and 7) are solved.

No longer active; already made.
FIGURE 4. An Unsegmented-Unsegmented Organization Facing a High Load

<table>
<thead>
<tr>
<th>Choice Arena</th>
<th>#1</th>
<th>#2</th>
<th>#3</th>
<th>#4</th>
<th>#5</th>
</tr>
</thead>
<tbody>
<tr>
<td>Period 1</td>
<td></td>
<td></td>
<td></td>
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<tr>
<td></td>
<td>Prob</td>
<td>DM</td>
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<tr>
<td></td>
<td>7,3</td>
<td>All</td>
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<td></td>
<td></td>
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<tr>
<td></td>
<td>Prob</td>
<td>DM</td>
<td></td>
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<tr>
<td></td>
<td>7,3</td>
<td>None</td>
<td></td>
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<td></td>
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<tr>
<td></td>
<td>Prob</td>
<td>DM</td>
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<tr>
<td></td>
<td>7,3</td>
<td>None</td>
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<tr>
<td></td>
<td>Prob</td>
<td>DM</td>
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<td></td>
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<tr>
<td></td>
<td>4,1</td>
<td>All</td>
<td></td>
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<td>Period 5</td>
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<td></td>
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<tr>
<td></td>
<td>Prob</td>
<td>DM</td>
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<tr>
<td></td>
<td>7,3</td>
<td>None</td>
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<tr>
<td>Period 6</td>
<td></td>
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<tr>
<td></td>
<td>Prob</td>
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<tr>
<td></td>
<td>5,10</td>
<td>All</td>
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<tr>
<td>Period 7</td>
<td></td>
<td></td>
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<tr>
<td></td>
<td>Prob</td>
<td>DM</td>
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<tr>
<td></td>
<td>8,6</td>
<td>None</td>
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<tr>
<td></td>
<td>Prob</td>
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<tr>
<td></td>
<td>8,6</td>
<td>All</td>
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<tr>
<td></td>
<td>Prob</td>
<td>DM</td>
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<tr>
<td></td>
<td>4,1</td>
<td>All</td>
<td></td>
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<td></td>
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<tr>
<td></td>
<td>Prob</td>
<td>DM</td>
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</tr>
<tr>
<td></td>
<td>2,9</td>
<td>All</td>
<td></td>
<td></td>
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</tr>
</tbody>
</table>

Key

- Not yet active, has not yet entered the simulation.
- Active, does not get made this period. Problems 2 and 7 (for example) are attached to this choice in this period. Decision makers 4, 3, and 5 are working on this choice in this period.
- Active, does get made this period. The attached problems (2 and 7) are solved.
- No longer active; already made.
problems. This happens because its ghost energy (energy cumulated over past periods) now exceeds the energy demanded of it—which is zero, simply because all the problems have left. In the second half of the simulation, no new choice arenas enter the process, so all the participants and problems must remain in one arena (the last one that entered) for the rest of the simulation. They have no place to go, because all the other choices have been "made" (with no problems or people present). In each period the decision makers exert more energy in that arena, but the simulation ends before enough has accumulated to solve the problems. Thus, the organization facing a medium energy load is completely ineffective. It solves no problems.

In a bizarre turnabout, however, going from a medium to a high load makes the organization totally effective again (Figure 4). In the first period two problems enter and go to the one available arena. All participants go to this choice—again traveling in a pack—but they lack the energy to solve the problems. In the second period a new arena enters the simulation, and two new problems enter and go to this choice. The old problems remain at the old garbage can. The entire pack of decision makers then goes to the new arena but again lacks the energy to solve the attached problems. In periods three, four, and five, exactly the same thing happens: All the participants and two new problems go to the newly entered arena; all old problems stay at the old cans. At the end of period five all five arenas are active, each with two attached problems. Each choice has exactly the same energy balance because each has received one period's worth of energy from decision makers. In period six, all decision makers return in a pack to choice arena #1. There they exert additional energy, make the choice, and resolve the attached problems. All participants then go en masse to arena #2, arena #3, and so on, in order. Hence, the pure unsegmented system facing a heavy load is completely effective, just like the organization facing the light load.

Thus, the three load versions present a peculiarly nonmonotonic pattern. Increasing the load initially makes the organization completely ineffective, but increasing the load once more sends problem-solving effectiveness soaring back to 100%. Sometimes nonmonotonic patterns are bold insights, but we doubt that this one is. The patterns bear no resemblance to expectations based on the informal theory. The latter certainly does not lead us to expect that a heavily burdened organized anarchy will be extremely effective at solving problems.

This is troubling, but the more profound problem is that the computer model generates clockwork dynamics that do not remotely resemble the decision processes associated with a disorderly garbage can. The simulation's hallmark in the prototypical organized anar-

chy is that decision makers always move together in a single pack. This pattern, moreover, is not confined to the unsegmented-unsegmented case highlighted here. It holds for all simulations with unsegmented decision structures, regardless of the other parameters of the model. The reason is straightforward. In these simulations, all decision makers start in the same place (the first can that opens) and use the same rule to decide where to go in every period. These features, together with synchronous adjustment, imply that they virtually always stay together.

It is difficult to imagine a more fundamental disjunction between model and theory. The computer model is supposed to represent the disorderly world of garbage can decision processes, but even in the prototypical case it generates an incredible degree of order. This is so clearly incompatible with the independent, randomly intersecting streams of the verbal theory that the simulations can offer no real insight into the workings of organized anarchies. They are from another world.

A New Look at Well-Known Properties of the Garbage Can

Given the problems at work here, there is little point in examining the model's implications at length. They inevitably reflect the model's basic design, which is seriously at odds with the informal theory. We do, however, want to focus briefly on two well-known claims about organized anarchy that emerged from these simulations. Over the years, they have been presented by the authors and others as basic properties of organized anarchies, and as central components of the theory itself.

The first is that "resolution of problems is not the most common style of making decisions" in organized anarchies (March and Olsen 1989, 13); decisions are more often made by flight or oversight. Decision making by flight and oversight are by now famous properties of the garbage can, conjuring up visions of a paradoxical organizational world in which many decisions get made, but problems are not resolved.

In fact, there is nothing paradoxical here. These properties are simply artifacts of the model's inappropriate assumptions. Flight occurs when all the problems attached to a choice arena leave, and the choice is subsequently "made." Oversight occurs when a choice is "made" before any problem attaches itself to the

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12 The old and new problems go to different cans because the computer program assumes that new problems, when they first appear, calculate energy deficits somewhat differently than existing problems do. The details of this assumption are not important.

13 It is possible for agents to separate in simulation variants in which they have unequal amounts of energy. In simulations with unsegmented decision structures, however, all decision makers travel in a single pack more than 99% of the time.

14 Indeed, not only do the participants move simultaneously, but the problems do so as well: Being synchronized with the movement of decision makers, they all move at the same time. It is true that the assumption of synchronous adjustment is often used in computer simulation. But this is in general a risky modeling strategy: Synchronicity can produce artificial results that disappear if even small asynchronies are introduced (Huberman and Glance 1993). Furthermore, in the special context of organized anarchies, it is substantively inappropriate to assume that decision makers and problems adjust synchronously.
arena. In neither case are decisions actually made; indeed, nothing happens. In ordinary English, decisions involve selecting an alternative. The model, however, uses its definitional fiat to brand truly vacuous events as "decisions." Because its own assumptions guarantee that empty choice arenas have zero energy requirements, its own logic of energy balancing guarantees that all these "decisions" will get "made." The much-touted prominence of flight and oversight, therefore, is literally built into the model, an artifact of unwarranted assumptions; it says nothing about how organizations make decisions under ambiguity.

A second famous property associated with the garbage can is that decision makers and problems tend to track one another. This is presented as an interesting pattern in the otherwise mysteriously confusing complexity of garbage-can dynamics. We make two points here. First, even if the model and its implications are taken at face value, the tracking phenomenon cannot properly be regarded as a general pattern of the simulation, for it does not and cannot hold for five of the nine variants. In systems with specialized access structures (versions 7, 8, and 9 in Figure 1) problems can go to only one choice arena, so they cannot track participants. In systems with specialized decision structures (versions 3, 6, and 9) participants can only go to one choice arena; they cannot track problems. Only in the other four versions is it even theoretically possible for decision makers and problems to track one another. So claims that the tracking phenomenon is a central feature of the simulation would be misleading even if the model were acceptable.

But it is not. The model assumes (with a few minor wrinkles) that all decision makers and all problems make the same calculations simultaneously: They go to the arena with the smallest energy deficit. In general, therefore, when the access and decision structures allow them to go to the same, smallest-deficit arena, that is what they do. They go together, in a pack, which is another way of saying that they track one another. This, then, is our second (and more important) objection to the famous claim that tracking is a central property of organized anarchy: Like the prominence of flight and oversight, it is simply guaranteed by the model's inappropriate assumptions about how participants and problems calculate where to go. It is an artifact of the design, and it tells us nothing about what we should really expect from organized anarchies in the informal theory, which clearly does not lead us to expect pack behavior.

Summary Comments

Formalization often helps science progress, but it is not a panacea. In this case a heavy price was paid, for the verbal theory and the computer model do not represent the same phenomena at all. Decision makers in the verbal theory confront a chaotic world in which they, solutions, and problems dance around one another, meeting by chance in choice arenas. But in the simulation, packs of decision makers—and often problems as well—march in lockstep from arena to arena. And solutions never move at all.

This is ironic. The informal theory of the garbage can is famous for depicting a world that is much more complex than that described by classical theories of organizational choice. The latter's tidy image of goal specification, alternative generation, evaluation, and choice is replaced by a complex swirl of problems looking for solutions, solutions looking for problems, participants wandering around looking for work, and all three searching for choice opportunities. Yet, the simulation depicts almost none of this and in fact creates a world of remarkable order.

Garbage Cans and Universities

In the final section of the 1972 article, the authors apply their analysis to what the literature now routinely spotlights as the quintessential organized anarchy: universities. They aim to show that GCT is more than just an abstract line of reasoning and can be used to shed new light on an important class of real organizations. This analysis has been influential (foreshadowing Cohen and March 1974 [1986] and March and Olsen 1976a) and is thus worth discussing here, if briefly.

The authors begin by asserting that universities conform to the basic implications of the theory, which they summarize as follows:

University decisionmaking frequently does not resolve problems. Choices are often made by flight or oversight. University decision processes are sensitive to increases in load. Active decision makers and problems track one another through a series of choices without appreciable progress in solving problems. Important choices are not likely to solve problems (1972, 11).

Note that these implications are based without modification on the simulation, not on the verbal theory. This is an early, important, and very clear example of what has been going on in the literature ever since: The computer model and its implications have been bonded to the theory. The two have become one.

Applying the simulation is only the first step in the authors' attempt to develop a garbage-can analysis of universities. To derive more detailed predictions they extend the model by adding auxiliary assumptions, which posit relations between the model's properties (e.g., access structure) and other variables, such as a university's size and wealth.

In principle, of course, it is legitimate to introduce auxiliary assumptions, particularly when applying a general theory to a specific domain. But it is important to recognize that in this case the new assumptions are not part of GCT in any of its guises. They belong neither to the informal theory nor to the computer model. They are freestanding assertions, introduced casually and with relatively little discussion. Here are the sorts of claims they entail:

1. In bad times, the access structures of large rich schools are hierarchical; those of small rich ones are unsegmented (Cohen, March, and Olsen 1972, 15, Figure 4).
2. As times improve, the access structures of large rich schools change from hierarchical to specialized; the same environmental change has no effect on the access structures of small rich universities or poor ones (p. 15).

3. As times worsen, large rich schools and small poor ones change their decision structures; large poor schools and small rich ones do not (p. 15, Figure 5).

No justification, theoretical or empirical, is given for these assumptions. They are simply employed, along with the simulation, to generate predictions about university decision making, predictions that are supposed to represent what GCT says about this important class of organizations.¹⁵

The predictions cannot do that, however, because they are based on premises of sand. This is not a meaningful application of GCT to universities. The assumptions and implications of the simulation are largely unwarranted and have little to do with the verbal theory. And the auxiliary assumptions about universities are questionable, unrelated to the theory, and offered with scant justification.

RECENT DEVELOPMENTS

We have now taken a close look at both key branches in the early development of GCT, the informal theory and the computer model. As our analysis suggests, this early work left much to be desired. Yet, such a story is not unusual in the early stages of a research program. Path-breaking work is often crudely developed. Its real function is to shake up conventional ways of thinking. With that done, the kinks, gaps, rough edges, and unexplored implications can eventually be worked out to yield a coherent, well-defined theory. But has this happened with the garbage can? Nearly three decades have gone by, and a substantial literature has expanded upon the original research. Let us see what this follow-on work has produced.

The Computer Model as a Source of Confusion

The computer model became a source of confusion almost immediately. Although the 1972 article begins with a clear distinction between the “basic ideas” and the attempt to model them via simulation, it later conflates the two. The GCT is summarized by statements such as “it is clear that the garbage can process does not resolve problems well,” or that, quite often, decisions get made only “after problems have left a given choice arena or before they have discovered it (decisions by flight or oversight)” (Cohen, March, and Olsen 1972, 16). It is as though these simulation results, rather than being one set of implications from one possible model, were central to an understanding of garbage can processes. The model was fused to the theory.

In more recent work, this hybridized GCT, an undifferentiated blend of informal theory and computer model, is the norm rather than the exception.¹⁶ Decisions by flight and oversight, access structures, decision structures, the tendency of decision makers and problems to track one another across choice arenas, the sensitivity of the system to variations in load—these and other components of the computer model tend to be presented as central, even defining features of garbage can processes in general, not as the highly specific assumptions or findings of one particular model.

This conflation of theory and model is especially debilitating because the computer model formalizes the theory so poorly. As the model's findings and building blocks have been tightly woven into the fabric of GCT, the “theory” itself has been transformed by claims and properties that are not legitimately part of the theory at all and that distort its real message. This would not have happened had the theory been granted a life of its own, separate from the model. Nor would it have happened had critics pounced on the model from the outset, exposed its inadequacies, and prevented it from gaining such prominence and influence. As things have developed, however, the garbage can literature has been profoundly and subtly shaped by an invalid model, to the point that GCT and its central ideas have become the model's illegitimate offspring. They are what they are, in good measure, because the computer model is what it is.

The Computer Model, Science, and the Liberation from Science

We refer, here and elsewhere, to the computer model. This is because Cohen, March, and Olsen did not develop other garbage can models after their 1972 article. A few others have tried their hand at it (Anderson and Fischer 1986; Carley 1986; Padgett 1980).¹⁷ Unfortunately, their models have not gained anything like the attention accorded the original, and their modifications have not been assimilated into mainstream organization theory's understanding of GCT.¹⁸

¹⁵ These predictions are reproduced in toto in Cohen and March 1986 (Table 28, p. 88). By this point, however, the extended model's auxiliary assumptions not only are unjustified but also are invisible: The book omitted the 1972 article's original figures that had represented the auxiliary assumptions.


¹⁷ Interestingly, each of these works abandons key components of the GCT. Padgett (1980) and Carley (1986) drop the assumption that participation is fluid. Anderson and Fischer (1986, 145) modify the assumption of independent streams: "Individuals are the vehicles which carry solutions and parochial problems to choice opportunities." Most important, all three emphasize the instrumental nature of individual and organizational action (Anderson and Fischer 1986, 153-4; Carley 1986, 165, 177; Padgett 1980, 585, 591).

¹⁸ We are not sure why this happened. One colleague (Carley, personal communication, 1996) has suggested that the problem is that simulation is not part of the standard repertoire of doctoral students. (See Masuch and LaPortin 1989, 38, for other explanations.) Thus, mainstream organization theorists have overlooked technical challenges and new simulations, attending instead to Cohen, March, and Olsen's own verbal accounts of their simulation.
Apart from this small body of technical work, the field has not benefited from a proliferation of formal models. The non-technical literature on the garbage can has continued to center on just the original model, which has survived for nearly thirty years without challenge or change from the mainstream.

Throughout, this computer model has served as GCT’s scientific core. However perplexing the authors’ discussions of the garbage can, underpinning it all was a literature-wide recognition that the informal ideas could be rigorously formalized, implications deduced, and tests conducted. The computer model was the justification, for it demonstrated, by sophisticated analytical means, that all this was indeed possible. It thereby bestowed scientific legitimacy on the entire GCT enterprise, no matter how unscientific it might otherwise seem. Thus, the computer model has liberated the rest of GCT to throw off the shackles of science and go its own way. The result has been an expanding, eclectic collection of loosely coupled ideas that have somehow seemed relevant to organized anarchy. Much of this is mind-bogglingly complex and vaguely expressed, and it is never explained how all these elements can be organized into a coherent and powerful theory.

**The Framework Expanded**

Since 1972 the authors (primarily March and Olsen) have moved beyond their initial formulation to assemble a larger, more general theoretical apparatus. All of the more recent work is rooted in the GCT and has inherited, as a result, its fundamental problems. Indeed, in important respects the newer work is actually more complex and confusing than GCT ever was.

At the heart of this entire research tradition is a general theme that is a hallmark of the GCT’s verbal theory: the juxtaposition of ambiguity and socially constructed order. The centrality of the first pillar was well reflected in the title and content of March and Olsen’s 1976 book, *Ambiguity and Choice in Organizations*, and of many other publications. But the second pillar is also central. Their work consistently emphasizes an interplay between the two. As March and Olsen (1984, 743) put it in their seminal statement on the new institutionalism, “institutional thinking emphasizes the part played by institutional structures in imposing elements of order on a potentially inchoate world.”

In the 1972 article, ambiguity meant organized anarchy’s triad of properties (ambiguous goals, unclear technologies, fluid participation), and socially constructed order—what they would later call temporal order—was that allegedly produced by garbage can decision processes. This particular juxtaposition between latent chaos and institutional order is often referred to in later work. But they have also added new types of ambiguity and of order. On the first dimension they have added the ambiguities of experience and of the past (Cohen and March 1986, 199–201; March and Olsen 1975), of power and of success (Cohen and March 1986, 197–9, 201–3), of relevance (March and Olsen 1976a, chap. 2 and p. 226, respectively), and of intelligence and of meaning (March 1988, 391–5). On the second they have added, among other things, symbolic order (March and Olsen 1984, 744), normative order (p. 744), and, most prominently, interpretive order (March and Olsen 1976a, chaps. 4 and 15; 1989, chap. 3).

This sprawling theoretical framework is so complicated and ill-specified that there is very little chance of recasting it as a coherent formal structure. Consider, for instance, its treatment of individual choice. March and Olsen have often argued that their work is fundamentally about individual choice, with deep roots in the bounded rationality tradition. Presumably because the 1972 article did not actually use or develop any theory at the micro level, a defining feature of their post-1972 work has been an effort to map out a theory of individual choice that could be a micro foundation for the new research program.

How have they done this? Although they claim they are engaging in the “gradual relaxation of rigid assumptions in classical theories of choice” (March and Olsen 1986, 28), and thus following in Simon’s footsteps, they actually abandon the framework of bounded rationality—and of rational choice, too—and strike out on their own. The result is a free-floating discussion of decision making that has no clear framework. Here are some of the factors that *Ambiguity and Choice* argues should be included in a theory of individual choice.

First, it must recognize that “beliefs and preferences appear to be the results of behavior as much as they are the determinants of it. Motives and intentions are discovered post factum” (March and Olsen 1976c, 15). Hence, the bedrock components of decision theories are to be treated as endogenous. The problem is that, although exogenous parameters are vital to the generation of predictions in any testable theory, nothing about individual choice is taken as exogenous here.

Second, it must recognize that individual preferences are shaped less by self-interest or problem solving than by roles, duties, and obligations, and by “the definition of truth and virtue in the organization, the allocation of status, the maintenance or change of friendship, goodwill, loyalty, and legitimacy; [and] the definition and redefinition of ‘group interest’ ” (p. 16).

Third, it must recognize that beliefs are shaped by


21 This newer work has a postmodernist flavor. The emphases on language and symbols, on meaning and its social construction, and on the chaos that lurks beneath our delicately maintained interpretations—are all postmodernist themes. These ideas are only touched on in the 1972 article but are given much more attention in *Ambiguity and Choice*. Indeed, the latter may well be the first sustained postmodernist study of organizations—a fact recognized early on by Perrow (1986, 137–8).

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19 See, for example, Cohen and March 1986, 195–203; March 1978, 1988, 1994, chap. 5; March and Olsen 1975; and March and Weissinger-Baylon 1986, chaps. 1–2.

20 See, for example, Cohen and March 1986, xv, 91, 195–215; March

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decision makers' interpretations and by "myths, fictions, legends, and illusions" (p. 18). All this and more would be part of a larger theory of learning that is needed to explain the evolution of beliefs.

Fourth, it must recognize that people do not attend to all issues. Their involvement is selective, often for reasons unrelated to substance, and any explanation of this behavior—a theory of attention—must emphasize "duty, tradition, and routine"; the "educational, ideological, and symbolic role of choice situations in organizations"; and how each person's opportunities depend on what everyone else is doing (March and Olsen 1976b, 45).

This book's perspective on choice, which was complicated even further in their later work, may be a plausible description of the countless forces that affect people's decisions. But description is not theory. And we do not see how this enormously complicated tangle of possibly relevant factors can ever lead to a rigorous, productive theory. Theories are supposed to reduce complexity, not surrender to it.

Despite these difficulties, Ambiguity and Choice quickly had a substantial effect on the field of organizations. Its influence on political science lagged a bit, but in the 1980s several works that were highly visible to political scientists disseminated its ideas in a major way. In 1983 the American Political Science Review published March and Olsen's article on administrative reorganization, in which garbage can concepts figured prominently. The research program got an even bigger boost the following year when the APSR published March and Olsen's "The New Institutionalism." This much-cited work followed Ambiguity and Choice in "deemphasizing metaphors of choice and allocative outcomes in favor of other logics of action and the centrality of meaning and symbolic action" (1984, 738). Furthermore, among the article's six "institutional conceptions of order," three are clearly linked to the 1976 book or the 1972 paper: temporal order (pure GCT), normative order, and symbolic order. And when they suggest three "examples of possible theoretical research" (1984, 744-6), it is no accident that one of these is the garbage can model.

The 1984 article, then, brought substantial attention within the discipline to their own variant of institutional thinking, which they presented as an integral part of the new institutionalism. What solidified its status, however, was the more elaborate presentation of these ideas in Rediscovering Institutions (March and Olsen 1989). It was quickly hailed as a major contribution and (as noted above) has come to be regarded as a "contemporary classic" (Goodin and Klingemann 1996, 16).

Obviously, we cannot analyze this complex book in detail. But its anchoring in March and Olsen's earlier work is evident throughout, and we will simply offer a few (of many possible) illustrations of its intellectual lineage.

1. Chapter three, "Interpretation and the Institutionalization of Meaning," begins with a section on how people make sense of their world. This was a central concern of Ambiguity and Choice. But the similarity goes far beyond common themes: Much of this section in the new book (pp. 41-5) is taken word-for-word from the earlier book.

2. One of the most-noted themes in Rediscovering Institutions is the contrast between "the logic of appropriateness associated with obligatory action and the logic of consequentiality associated with anticipatory choice" (1989, 23). This same theme was developed in Ambiguity and Choice as well, in its analysis of "attention as obligation" (pp. 48-50).

3. Most of the chapter on institutional reform is based on March and Olsen's (1983) APSR article, which in turn essentially restates the argument developed years earlier in Olsen's chapter, "Reorganizations as Garbage Cans," in Ambiguity and Choice.

4. The references section of the 1989 book gives individual citations to all but one of the 17 chapters from the 1976 book, and Ambiguity and Choice is far and away the most cited item of their references.

In short, the GCT of the 1972 article led to the interpretivist vision of the 1976 book, which in turn led to the new institutionalism of the 1984 article and the 1989 volume. There has been growth and elaboration (if not progress) throughout this evolution, but the continuity in themes and substance is striking. It is clearly a single research program, which carries the genes of the garbage can (Sjoblom 1993).

This research program does have intriguing ideas and assertions, but the problems remain and are much the same. The formulations are overly complex, the arguments unclear. Consider one of the best-known themes of Rediscovering Institutions, the importance of the "logic of appropriateness" in politics. The core hypothesis is that "most behavior follows such a logic of appropriateness, that rules are followed and roles are fulfilled" (p. 161). This may seem to be a straightforward and empirically falsifiable claim about politics, but in fact it is neither. Whatever the logic of appropriateness refers to, it clearly involves rule-governed behavior. To know what it implies and how it might be tested, then, we must know what rules are. Early in the book, the authors deal with this explicitly. What they do, however, is take a concept with clear meaning in ordinary language and transform it into a conceptual morass: "By rules we mean the routines, procedures, conventions, roles, strategies, organizational forms, and technologies around which political activity is constructed. We also mean the beliefs, paradigms, codes, cultures, and knowledge that surround, elaborate, and contradict those roles and routines" (p. 22).

22 See especially section four, "Reorganization as Garbage Cans" (1983, 285-7), and the concluding observations (p. 292).
23 For example, the discussion in the 1984 article on "normatively appropriate behavior" (p. 744) is completely consistent with March and Olsen's earlier analysis of how attention is organized (1976b, 49).

24 This idea is also central to March and Olsen 1996. See also March and Olsen 1995, chapter 2.
25 The first definition of "rule" in Webster's Collegiate Dictionary (9th ed., 1030) is simply "a prescribed guide for conduct or action."
This staggering expansion of the concept trivializes the claim that political behavior is rule governed. Since their definition leaves so little out, what else could drive institutions? This expanded definition also makes it impossible to figure out what the logic of appropriateness refers to in concrete terms, and thus what kinds of behaviors are consistent or inconsistent with it. There is no clear content here to test.

In addition, Rediscovering Institutions never makes clear how rule-driven behavior differs from instrumental behavior. The predominance of the former, and the relative insignificance of the latter, is one of their central theoretical claims; yet, at times they seem to recognize that rule-following can be instrumentally based, and that the two are not at all distinct.

To say that behavior is governed by rules is not to say that it is either trivial or unreasoned. Rule-bound behavior is, or can be, carefully considered. Rules can reflect subtle lessons of cumulative experience, and the process by which appropriate rules are determined is a process involving high levels of human intelligence, discourse, and deliberation. Intelligent, thoughtful political behavior, like other behavior, can be described in terms of duties, obligations, roles, and rules (p. 22).

Any notion that there is a clear distinction between rule-following and instrumentalism, moreover, is contradicted by cognitive science. The logic of appropriateness involves the matching of actions to situations (p. 23)—as in, “if you are in situation x, then do y”—but cognitive scientists argue that this is a broad rubric, and very often involves rules that are cognitive guides for instrumental action, encoding or summarizing the problem-solving effectiveness of skilled (although boundedly rational) agents (Anderson 1995). In a larger social setting, some of these instrumentally based rules may emerge as social norms, such as standards of professionalism.26

The confusion surrounding the logic of appropriateness could have been dispelled had the authors anchored their variant of the new institutionalism in a coherent micro theory. Ironically, one was right at hand: March and Simon (1958, 139–50) analyzed how cognitively limited decision makers would be guided by rules and routines, or what they called performance programs. There is now a substantial literature, some of it quite formally sophisticated, that seeks to explain organizational behavior on these individualistic foundations. Using the same foundations would have encouraged March and Olsen to examine individual choice in a clear, rigorous way and would have given them a framework of demonstrated analytic power that

26 To be sure, some institutions (e.g., religious ones) have rules that encode sacred rituals. These differ in many ways from the problem-solving operators analyzed by Anderson. In particular, sacred rituals need not have a “logic of consequentiality,” so their logic of appropriateness may be independent of secular notions of instrumentality. But Rediscovering’s (1989, 160) argument that “politics is organized by a logic of appropriateness” is unqualified by any distinction between the sacred and the profane. Unquestionably, the claim is intended to cover ordinary behavior in ordinary governmental institutions. In this domain, the fatal ambiguity identified above—that rule-governed behavior may be infused by the consequentialism of problem-solving operators—is quite evident.

Chaos, Order, and Causality

Work on GCT has been much cited in organization theory, political science, and sociology. Yet, apart from work done by the authors themselves and some of their colleagues, the garbage can and its descendants have been put to little empirical use in these fields. For a theory often considered one of the major perspectives on organizations (e.g., Perrow 1986; Scott 1992), this neglect might seem odd, but we do not find it so. As it stands the theory is almost impossible to test, and this can only discourage serious empirical research. Many applications of the approach do little more than describe some parts of an organization as garbage cans, offering descriptions—usually detailed ethnographic accounts—that emphasize GCT’s central themes. These case studies are, as Anderson and Fischer (1986, 141) note, often “only loosely coupled to the theory” (for a similar criticism see Perrow 1977, 297). They describe events using garbage can terminology, but it is not at all clear that they provide real GCT explanations. Authors of such applications (e.g., Sproull, Weiner, and Wolf 1978) tend not to see it this way; they seem to believe that, by merely labeling and describing organizations along these lines, they have

27 In one section (March and Olsen 1989, 40–5), the authors did construct an argument with individualistic foundations. But this was an exception to the book’s central tendency, and they made no attempt to introduce or summarize their overall approach via an explicit micro theory.

28 A major exception is Kingdon (1984), whose work is distinguished by a careful empiricism tied to theoretical concerns.
somehow helped us understand them— that description has produced explanation. But it has not.

Latent here is a methodological issue of great importance: What counts as an explanation, and what kind of explanation do Cohen, March, and Olsen aim to provide? Clearly, description is not explanation; to equate them is a mistake. But what is the answer to the larger methodological question? If GCT and its offshoots did their job properly, what kind of explanations would they provide? How would they help us understand organizations?

Addressing this issue is crucial if the authors are to put their work in a scientific perspective. And in fact they do address it, although in less depth than it deserves. They argue that their type of explanation is methodologically very different from those of conventional theories: It rejects the “consequential order” of conventional theories, substituting a “temporal order” in which “problems, solutions, and participants are joined together more by the timing of their arrivals than by other attributes” (March and Olsen 1986, 12).

Here, the authors are emphasizing that the theory is not only about chaos and disorder, the feature that has caught the attention of most social scientists, but also about order. Through the lenses of conventional theories, organized anarchies look chaotic, but they have an underlying order that the theory can help us see. This is good to point out, especially given how others have interpreted their work. But they still need to tell us how temporal and consequential orders differ, and in what sense the two give different types of explanations. So far, these issues remain unclarified.

We are reasonably sure what the authors mean by a “temporal ordering”: It links events—“orders” them—based on the timing of the various streams’ intersections. We are less sure what “consequential ordering” means. It seems to be a perspective that explains organizational outcomes via the intentions of individuals. It is “consequential,” presumably, because actors intentionally make choices—and thus generate organizational outcomes—by assessing consequences.

From this vantage point the authors’ methodological claim seems straightforward: GCT explains organizational outcomes on the basis of timing rather than intentions. Yet, if this is all there is to the argument, no new language is needed; and without the new language there would be no confusion. If we take the new concepts and the accompanying (brief) discussions seriously, however, there is much to be confused about and much to pay serious attention to—for it appears that what the authors are really doing is distancing the garbage can framework from causal explanations in general.

This is reflected in a second, more general way of thinking about “consequential order” that sometimes seems to orient the authors’ analysis. In this interpretation any causal sequence involves a consequential logic; that is, certain outcomes are effects of certain determinants. X causes Y, hence Y is a consequence of X. Intentionality rests on such a logic, but this does not distinguish it from other theories. All causal theories use a consequential logic, in this sense.

Had the authors meant only intentionality when using the term “consequential order,” as in the first interpretation, then presumably they would have said so explicitly. But they did not. We suspect this is because they were referring to more than intentionality—directing their criticism, at least implicitly, to causal approaches in general.

This inference is reinforced by the way the authors discuss temporal ordering. The essence of a temporal ordering, in their portrayal, is not just that timing determines outcomes; it is that events are not driven by an identifiable causal structure. Never, in fact, do Cohen, March, and Olsen present such a causal structure as the foundation of their theory, and they rarely say that identifying and modeling it is the key to explanation, as it would be in conventional theories.\(^{29}\) In their view, the flow and intersections of events are largely driven by random occurrences, accidents, and a huge variety of complex institutional, social, psychological, economic, and political forces that cannot be represented by a causal model. The upshot is a perspective in which outcomes are explained not by a well-specified causal structure but by the timing of events and their random, unspecified determinants. As a statistician might put it, all the action is in the error term.

Because the authors’ methodological discussions are so brief, we cannot be sure that we have correctly described their position. Yet, in a recent book March seems to confirm our judgment, for he explicitly states that his framework entails “the orchestration of decisions through temporal orders rather than causal orders” (1994, 180, emphasis added). In any event, if GCT and its extensions are ever to be legitimate scientific theories, they must set out a causal logic of some kind. That timing is central to their explanations has no bearing on the causality issue. A proper dynamic theory differs from other theories only in having a time component; all theories worthy of the name, including dynamic ones and those allowing for randomness, identify some causal structure as the basis for explanation. To suggest that their temporal order is relieved of this responsibility, that it can explain events without delineating causation, creates a fundamental confusion.

Finally, this methodological confusion ties into the garbage can’s empirical applications in an ironic and unproductive way. It is hard to read this literature without encountering a convoluted line of reasoning that goes roughly as follows. What we do not understand about certain organizations—because their behaviors appear horribly complex, seemingly random, and impossible to disentangle—is actually just what the garbage can theory tells us to expect. In this sense we do understand it. Although we have almost no idea why certain events occur, we “understand” them because they are precisely the kind of incomprehensible phenomena that garbage can processes produce. That organizations are so confusing, then, is actually strong evidence of the theory’s great explana-

\(^{29}\) For an exception see Cohen, March, and Olsen 1976, 36.
atory power. The less we can figure anything out—the less, that is, we can identify a causal structure—the more the theory seems to be working, and the better a theory it seems to be.30

This reasoning may seem far-fetched, but anyone familiar with the literature knows it is quite common. The solution is simple: GCT, if it is to become a genuine theory, must play by the same rules as all other theories in social science. The job is to identify the causal structure of institutional behavior. That is how behavior is explained and understood.

REVITALIZING THE GCT

Despite the many problems identified above, we believe that the garbage can program can be revitalized. Because we cannot carry out a full reconstruction here, we merely provide an example to show that basic themes of the GCT can be grounded in theories of bounded rationality. Our example will show how the well-known property of temporal ordering—that solutions may be linked to problems more by chance than design—can be derived as a natural implication of a classical (Simon 1957) model of adaptive search. This is a significant test case because temporal ordering looms large in the GCT literature.31

We begin with Simon’s satisficing model and assume there is an organizational superior who decides whether to accept an alternative to solve a particular problem. She has three subordinates, trained in different professions. (Imagine a diplomat, a military officer, and an economist working for the secretary of state.) Due to the “trained incapacity” of specialists, the diplomat sees the problem as one of diplomacy, the officer sees a military problem, and the economist sees an economic one. They craft their solutions accordingly. The quality of proposals and how long it takes to generate them are random variables. Once a proposal is done it goes to the boss. She has an aspiration level that acts as a stopping rule: If she gets exactly one proposal that exceeds her aspiration, she selects that one, and the process ends. If no proposal satisfies, her staffers keep working. If more than one does, she picks the one she thinks is best.32 Thus, the superior is picking samples from three different distributions, produced by the three specialists.

The value of the superior’s aspiration level powerfully influences this process, as the following (easily proven) implications show. (1) If the superior’s aspiration level is very demanding, then the randomness of proposal generation will have little effect on which type of solution is accepted (although it will affect how long it takes to find a satisfactory option). If her aspiration level is so high that only one type of proposal can satisfy it, then random generation has no qualitative effect on the outcome. (2) The lower the superior’s aspiration level, the more the process exhibits temporal coupling, that is, the more the proposals’ chance arrival times influence what type of solution is picked. If her aspiration is below the worst possible alternative, then choice is driven completely by chance.

This shows how a central GCT property can easily be generated by a classical model of behavioral search. But it also suggests how the prediction of temporal coupling may be empirically limited. The preceding sketch fixed aspirations exogenously, as they were in Simon’s model. But many have argued that aspirations adjust to experience, rising in good times and falling in bad (e.g., Cyert and March 1963). If aspirations were thus endogenized, then temporal coupling would tend to diminish over time. The superior, discovering that her payoffs beat her initial standard, would tighten her requirements, which would make her less prone to accept poor solutions.

Temporal coupling has been held up by garbage can theorists as a finding of almost mystical significance. But it turns out to be very compatible with choice-theoretic formulations. So, we believe, are all the other GCT ideas that are worth keeping.

CONCLUSION

The philosopher Max Black (1962, 242) once remarked that science moves from metaphor to model. In part he meant that science should move. As he went on to say, metaphors are invaluable at the start of an inquiry. Ideas often come to us first as metaphors, a vague notion that x is like y. These can be creative insights; they may revolutionize a field. But they should not remain as they are born, for as scientific formulations, metaphors are flawed (Landau 1972). Their logic is obscure. How do claims relate to each other? Which are premises and which conclusions? And the empirical content of metaphors is often thin: Few, if any, of their claims may be empirically testable. Thus, both to clarify their reasoning and to provide targets for empirical scrutiny, scientists should—and typically do—transform metaphors into models.

The garbage can research program has done just the opposite: It has moved from model to metaphor. A model was presented first, in the same pathbreaking article that presented the metaphorically inspired informal theory. Later, instead of critiquing and revising the original formal model—which certainly needed attention—the research program focused mainly on embellishing the verbal theory. And it did so in ways that not only failed to clarify GCT but also actually made it more metaphorical.

Because the initial computer model misrepresented the informal theory, perhaps tinkering with it would...
have been a mistake. It probably should have been replaced by a new model. This did not happen. Instead, the original simulation was frozen in time.

The fault should not be laid only at the feet of the authors. Their 1972 article included a copy of the FORTRAN program, and everyone had a chance to critique it. The article’s text described the simulation well enough so that the discrepancy between the informal theory and formal model could be discerned. So the authors discharged their obligations by giving readers plenty to work with. By and large, readers passed up this opportunity. Thus, the verbal theory and the simulation remained so deeply conflated that few could tell where one left off and the other began. That is no way to create a research program with ever clearer logic and growing empirical content.

We believe it is possible to revitalize the GCT. Doing so requires radical surgery. Because this operation would make the framework’s logic transparent to all, it would deprive the GCT and the March-Olsen variant of the new institutionalism of a certain mystique. Without this bold move, however, there is little chance that these ideas will shed much enduring light on institutions.

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