Whatever Happened to Critical Mass Theory? A Retrospective and Assessment*

Pamela E. Oliver and Gerald Marwell
University of Wisconsin

Between 1983 and 1993 the authors published a series of articles and a book promulgating and explicating "Critical Mass Theory," a theory of public goods provision in groups. In this article we seek to trace the growth, change, or decline of the theory, primarily through an analysis of all journal citations of the theory. We find that the majority of citations are essentially gratuitous or pick a single point from the theory, which may or may not be central to the theory. However, we identify four lines of theorizing that creatively use substantial parts of Critical Mass Theory in their own development: (1) theories relevant to issues in communication studies such as interaction media and shared data bases; (2) Macy's work on adaptive learning models; (3) Heckathorn's models of sanctioning systems; and (4) theories that are centrally concerned with issues of influence in collective goods processes. A few additional, less developed lines of work are also discussed. None of this work identifies itself as being itself "Critical Mass Theory," but many of the innovations and assertions of the theory are important bases for its development.

The lives and deaths of social theories are often presented as linear and triumphal or are set forth as tales of empires built by conquest and later conquered in their turn or crumbling from within. In the real world of social theory, however, theories not only come and go or rise and fall, but they may transmute into truncated or distorted collections of ideas that are only loosely grounded in the original statements; they may become embedded as crucial parts of "other" theories or suffer fates too hideous to mention (Hargens 2000). This is because theories never remain the property of their original authors but are taken up by other authors with new agendas. Rarely do we attend to these real dramas in the life course of a theory. In this article we attempt to look more closely at what has happened over the period of a decade to a particular theory first published in the late 1980s as it was taken up by others. We have been paying attention because it is a theory of our own.

Beginning in approximately 1983, and culminating in the publication of *The Critical Mass in Collective Action*, in 1993, the intellectual project that took most of our time and attention (and the time and attention of several graduate students, as well), was developing what we called the "Theory of the Critical Mass," or what others have called "Critical Mass Theory." The project involved writing and running simulations, as well as more mathematical and logical analyses and, obviously, writing papers from this work. For various reasons, neither of us has been working on Critical Mass Theory for the past few years. But we have wondered about its fate, and, seeing in the citation index that it has received a fair amount of play, we could not help but wonder who was citing us, and why, and how. So we decided to search for all the citations to this work that we could find in the literature. This paper presents the results of that process. It can be read as a case study of theory evolution, not in the old-fashioned celebratory meaning of evolution as teleologi-

*Address correspondence to: Pamela E. Oliver and Gerald Marwell, Department of Sociology, University of Wisconsin, 1180 Observatory Drive, Madison, WI 53706-1393; e-mail: oliver@ssc.wisc.edu, marwell@ssc.wisc.edu.

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cally rising toward perfection but in the modern understanding of evolution as involving path dependence, dead ends, and vestigial residues, as well as growth. But the evolution metaphor has limits, for part of the story involves construction processes in which complex ideas become distilled into catch phrases.

Considered formally, "Critical Mass Theory" has never been a single theory at all, although it is a deeply theoretical enterprise. In most of the ways it is cited, Critical Mass Theory is treated as a number of discrete, unconnected assertions. And yet, there is a solid core of interconnected and cumulative work that has built on our prior publications and has developed some distinctive approaches and findings. Since many scholars have worked on closely related problems using "rational choice" approaches like ours, it is hard to isolate causal effects. However, if we were to pick the one possible contribution of our work for which we would most like to take credit, it would be the change from simple monocausal theorizing about "collective action," as if it were a unitary entity, toward a disciplined search for the distinctions among different types of collective action and the factors that distinguish them. For example, the political scientist Elinor Ostrom says in citing our work, "The kind of theory that emerges from such an enterprise does not lead to the global bivariate (or even multivariate) predictions that have been the ideal to which many scholars have aspired" (1998) and then quotes us as saying: "This is not to say that general theoretical predictions are impossible using our perspective, only that they cannot be simple and global. Instead, the predictions that we can validly generate must be complex, interactive, and conditional" (Marwell and Oliver 1993:25).

In this paper, we begin by sketching the key ideas and insights that led us to start thinking about the critical mass, and we then review the major concepts and arguments we developed. In the core of the paper, we review the works that have cited Critical Mass Theory, as a way of assessing the character of its impact. Unhappily, but perhaps not surprisingly, we discovered that the vast majority of these citations are either gratuitous or incomplete and frequently misrepresent what we would have taken as the central point of our contribution. However, we do find a small number of works, many of them important and representing sustained intellectual attack, that have seriously engaged the issues we posed and have developed new theory to address the implications or failures of our writings. In a more detailed review of these works, we show where we think there has been genuine development and where we believe there are unnecessary blind spots. The failure we most often point to is a failure to think in terms of controlled comparisons and "complex, interactive, and conditional" propositions.

BEGINNINGS

Like most early work in the resource mobilization/rational choice tradition, Critical Mass Theory began as a conversation with Mancur Olson's *Logic of Collective Action* (1965). For students of social movements in the 1960s (like us, e.g., Demerath, Marwell, and Aiken 1971; Oliver 1983, 1984, 1989; Oliver and Furman 1990; Oliver and Marwell 1992) the most compelling argument in *Logic* was Olson's assertion that "rational, self-interested individuals will not act to achieve their common or group interests" (1965:2) without private or selective individual incentives that reward cooperators or punish noncooperators. Prior to Olson, social scientists assumed that there was a natural tendency for people with shared interests (interest groups) to act together in pursuit of those interests. Economists, however, had long argued that coercive taxation is necessary because rational individuals in a competitive market would not voluntarily contribute money to pay for public goods such as armies, legislatures, parks, public schools, or sewage systems. Olson argued that all group goals or group interests were subject to the same dilemma. He defined a collective good as one which, if provided to one member of a group with an interest in that good, cannot be withheld from any other member. This is generally called nonexcludability or "impossibility of exclusion" (Hardin 1982:16). Collective action was thus defined as any action that provides a collective good. Olson argued that if the benefits of a collective good cannot be withheld from nonparticipants, rational members of interest groups are motivated to free ride on the contributions of others. Furthermore, he argued, this temptation would be greater the bigger the group, where the benefits of a contribution would have to be divided up among more people and where any one person's contribution would be less likely to make a noticeable difference in the outcome. Thus, he said, collective action is "irrational" unless people are given private or selective incentives as inducements to make contributions to collective goods. Olson's argument had a major influence on early resource mobilization theory in the study of social movements. He problematized collective action and, thus, opened the door to studying the conditions under which collective action can occur.

Ironically, Olson's theory arguing that collective action is irrational appeared in the midst of one of the great historical periods of social movements. The irony was lost on no one. The data and the theory seemed at least somewhat at odds. Marwell's experimental work, widely cited in economics, added fuel to the fire by showing that under a variety of conditions in which Olson would predict no public goods production, groups in fact produced very substantial amounts of public goods (e.g., Marwell and Ames 1979, 1980, 1981). Everyone who seriously engaged Olson's arguments mathematically or experimentally (or both) rapidly recognized that his arguments were much too general and unconditional to be generally true, and research articles rapidly proliferated that either presented his core claims in a different mathematical format that could more unambiguously represent them, or used a different mathematical format to show why his claims were not correct, or provided experimental evidence that real people's behavior did not follow his claims. (Early examples include Bonacich et al. 1976; Chamberlin 1974; Frohlich and Oppenheimer 1974; Frohlich, Oppenheimer, and Young 1971; Hardin 1982; Schofield 1975; Smith 1976.)

In Marwell's experiments (1979) he created real-life large-group collective dilemmas by setting up situations in which high school students (and other kinds of subjects in later work) were asked if they wished to make contributions to a fund that would be doubled but then would be distributed equally to all members of the group, whether they contributed to the fund or not. Marwell found that students did contribute much more in these experiments than economists' theories would predict, although their contributions were suboptimal. In fact, the only group of subjects studied whose behavior actually fit Olson's predictions were economics graduate students (Marwell and Ames 1981)!

Oliver (1980) joined these debates by focusing on the side payments or incentive issue. It had quickly been recognized (by Frohlich, among others) that private or selective incentives could not logically "solve" the collective action problem, because paying for the incentive was itself a kind of "collective action" that merely created a second order public goods problem. Oliver's particular contribution to this issue (besides explaining it clearly in a venue where many sociologists saw the argument) was to show that rewards and punishments were structurally different as incentives because they would necessarily have different cost structures. She argued that rewards were more efficient when a small group could provide the good for everyone, while punishments were more efficient for enforcing unanimous cooperation. Even though our study of citations revealed that her particular contribution was rarely engaged, the "second order" problem of paying for incentive structures has been an important subsequent line of research that has merged with Critical Mass Theory. For Oliver, the selective incentives work involved engaging the issues that led to our later emphasis on production functions and structures of organizing. In particular, she encountered economists' critiques of their standard "convexity" assumptions and the beginnings of the literature (now much more prominent and developed) on nonlinearities and increasing marginal returns or economies of scale.

CRITICAL MASS THEORY

Our intention in producing Critical Mass Theory was to develop a theory that would encompass both Olson's argument and the fact of collective action, one that would allow us to make predictions about the conditions under which collective action would and would not emerge. We began by confronting a basic assumption of Olson's that students of actual social movements find quite unrealistic. Like most economic theories of markets, Olson's theory assumes that individual interest group members make their decisions *independently* of one another. Although they have complete information about the "game" they are "playing," or the decision they must make, they have essentially no information about each other. Since no social movement actually looks like this, our key decision was to try to model a situation in which decisions by group members are *inter*dependent. Experimentalists studying prisoner's dilemmas had long previously established that subjects who could communicate with each other almost invariably locked into cooperative solutions.

The second issue we wanted to confront was the idea that there were different kinds of collective action. We wished to extend and elaborate the initial recognition that sometimes a few can provide the good for many, while at other times unanimous action is needed. From this, and from economists' discussions of convexity assumptions, we developed our way of talking about accelerating and decelerating production functions as special cases of the economists' general S-shaped ~~(convex)~~ curves.[1] This permitted us to show that decelerating production functions fostered at least initial levels of action but created problems of optimization, strategic action, surpluses, and free riding. By contrast, accelerating production functions had daunting start-up costs and fostered inaction but opened the door for contractual solutions or solutions in which actors could reasonably assume that their own actions would motivate later contributions by others. Subsequent authors, especially Heckathorn (1996), linked production functions to games, showing that the prisoner's dilemma occurs in a fairly small portion of a game space with linear production functions, while accelerating production functions create assurance games and decelerating production functions create chicken games.

Putting production functions together with interdependence led us to one of our innovations. There are a variety of ways in which one can model interdependence. To talk about interdependence and production functions together, we postulated a situation in which group members made decisions *sequentially*, with full knowledge of what had been done previously and, in some cases, with the ability to calculate the effects of their actions on the action choices of subsequent actors. This way of modeling the decision process was, as far as we know, novel at the time, and it gave us and subsequent researchers a way of addressing issues that had previously been ignored, partly for lack of tools. In subsequent work, we worked with the more common model of contractual solutions, but we never thought these would just "happen" and instead explicitly modeled how they would be created with organizer-centered theory. An "organizer," a figure we knew well from social movements, would incur costs to contact others and seek to form a contract.

Our fourth core insight was that the behavior of heterogeneous groups could not be predicted from models of one individual at a time, that heterogeneous groups would not

[1] In fact, the manuscript that was reviewed called these two types u-concave and u-convex functions. It was a fortunate insight that let us rename them to accelerating and decelerating in the copyediting stage.

generally behave the same way as homogeneous groups, and that larger groups could not be assumed to act like very small groups (dyads or triads). Thus, we insisted on incorporating group heterogeneity and larger groups into our models right from the outset, even though this often made them less elegant. Our emphasis on heterogeneity quickly led to an emphasis on the critical mass, the subset of highly interested and/or highly resourceful people who play a crucial role in the early phases of collective action. The idea of the critical mass in exactly this sense was common in social movements; in fact, *Critical Mass Bulletin* was (and is) the name of the newsletter of the Collective Behavior and Social Movements section of the American Sociological Association, which predated the formation of the section. We saw our attempts to formalize the decisions of the critical mass and the consequences of their actions for the total group's outcome as directly related to core issues in the study of social movements. Interestingly, one line of citations to our work treats it as a species of threshold models and considers the "critical mass" as specifically the threshold. However, this conception holds only for the accelerative phases of collective action, where the critical mass overcomes the start-up costs and creates the conditions for others' involvement. Although we certainly talked about this kind of role of the critical mass, these citations ignore the parts of our work that explicitly argued that the early contributors play a very different role in the decelerating cases, where they provide the good and give everyone else the opportunity to free ride. In our models, interest heterogeneity generally improved group outcomes, and resource heterogeneity sometimes did, although extreme heterogeneity could be harmful under some conditions. We explicitly argued that the effects of heterogeneity depended heavily on the mean level of the variable in question and on the specific kind of collective action process, as well as the levels of other variables. Nevertheless, our emphasis on the critical mass definitely tended to stress the people who were different from the others.

Our rebuttal to Olson's "group size" argument was one of the most widely cited (and misunderstood) of our specific claims, even though our argument had been anticipated many times in the previous literature. To recapitulate, Olson simply *defines* a "large group" as one in which no individual makes a noticeable contribution to the collective good. We never disputed Olson's claim that actors assumed to be acting according to the principles of rational means-end decision making would not make contributions with no noticeable effects, and, to our knowledge, no one else disputes this claim, either. However, Olson also advanced the empirical claim that groups with large numbers of individuals in them would generally be "large groups" in his sense, that is, would be groups in which no individual could make a noticeable difference in the collective good. It is with this empirical claim that we disagreed (and disagree). We have said, and still say, that the whole thing hinges on the production function, that is, on the way in which contributions translate into units of the collective good. There are, in fact, many different "types" of production functions with many different properties, and the significance of individual contributions in each varies tremendously. The whole matter of collective action is a subset of the more general economic problem of externalities, in which individuals' actions affect other people. Our bottom line is that *there are no general principles of collective action:* You have to set some parameters of particular kinds of actions first, and then you can examine the effects of other factors such as group size. To make the point strongly, we showed that in cases of high jointness of supply and heterogeneous groups, a collective good could actually be provided by fewer contributors in a larger interest group than in a smaller one, assuming that the two groups had the same distributional properties.

We then investigated the effects of network centralization as well as network density on the prospects for collective action. Our unexpected finding was that, when groups are heterogeneous, network centralization increases the rate of collective action by increasing the probability that an organizer will be tied to the few large contributors. This finding is clearly specific to the particulars of this model, although it both encouraged further study of networks in collective action and pointed to the way particular mechanisms affect the results. This result is particularly contingent on the assumption we made that organizers do not randomly choose from people in their networks but, rather, choose those who will make the largest contributions to the contract, a process we called *selectivity* and to which we devoted attention in subsequent analyses.

The remainder of our findings, one first published in the *Journal of Mathematical Sociology* and the other published only in our book (Marwell and Oliver 1993), have received very little play. This is probably mostly due to where they were published, but also because they have less sound bite value. In the first, we assumed that organizers have finite resources that can be used in some mix of contacting people or in gaining information about who is likely to contribute more, and we measured the cost of information as the decrease in the number of people who could be contacted. The question, then, is, when is the information worthwhile? We explored the problem with two different models, each hinging on the fact that the mean of some top fraction of a distribution must be higher than the overall mean of the distribution. We found that information is worth more as group heterogeneity increases, that there is an optimum information level that increases as group heterogeneity increases, and that there is always a point at which it is more worthwhile to expand networks and gain information rather than mobilize more people from within the existing networks.

In a different model with a former graduate student (Prahl, Marwell, and Oliver 1991), we explored the trade-offs between reach and selectivity in recruitment campaigns. Using an organizer-centered model of simultaneous coordinated action and an accelerating production function, we developed an equilibrium equation for the expected total contribution from a heterogeneous group with a given distribution of interest and resources, and we showed how this expected contribution varies as the parameters vary. The *reach* is the total number mobilized; the *selectivity* is the mean interest or resource level of those mobilized. (The "shape" of the distribution is held constant as a lognormal with standard deviation equal to the mean.) Both reach and selectivity have thresholds that must be achieved if any of the collective good is to be obtained. Once all the necessary thresholds are reached, further increases in reach or selectivity for resource are more efficacious than further increases in selectivity for interest.

Apart from these "findings," and our development of sequential decision models as an approach to interdependence, it seems to us that our use of experimental design in simulation modeling has also been important. This was particularly evident in the analysis of network effects. We held constant an accelerating production function and the process of an organizer-centered mobilization of a contractual agreement. There were five independent variables: interest and resource heterogeneity, organizing cost, network density, and network centralization. The core of the analysis was an experimental design and a Monte Carlo simulation. There were 6 possible values each for the two heterogeneity terms, 10 for costs and density and 19 for centralization, which taken together define a $6 \times 6 \times 10 \times 10 \times 19$ design with 68,400 cells. Since it was impossible to generate all possible combinations of parameters (2,794 cases were generated across several months' time), parameters were themselves randomly chosen from uniform distributions across the ranges of interest, thus yielding a representative random sample of the full design. A further random component was the generation of heterogeneous groups of size 400 with the indicated heterogeneity and network parameters. We analyzed output from the simulation with standard regression techniques. We found generally positive heterogeneity effects and the expected negative effect of organizing costs and positive effect of network density.

In any complex mathematical model, anything that is part of the specification of the model could be material in generating its results. Seemingly minor variations in assumptions can often generate large differences in results. In our own work, we tried to ferret out all these ancillary assumptions to determine how consequential they were, but, of course, we did not always succeed. As we reviewed others' works, we often spotted seemingly minor operational differences that produce huge differences in the results. We will mention two. First, we assumed that organizers would select the "best" potential contributors, that is, those who would make the highest contributions. Models that make similar assumptions generate very different results regarding the effects of heterogeneity than those that assume that the selection of participants from a pool of eligibles is random.

Second, our algorithms permitted individuals to contribute only some of their resources, so "resourceful" individuals with low or moderate interest levels would make partial contributions; this specification is the underlying reason for the generally positive effects we obtain from resource heterogeneity. Many other modelers have required that a contributor give all of his or her resources or none of them, regardless of interest level; such models often find a *negative* "resource heterogeneity" effect because highly resourceful persons with lower interest levels give zero, rather than the lower amount they would be willing to give based on their interest if partial contributions were permitted. This all-or-nothing assumption applies to movements that require recruits to give away all their worldly goods, and others' theoretical findings that the wealthy are less likely to join such movements (e.g., Kim and Bearman 1997) seem plausible, but such cases are obviously very different from the less-extremist secondary associations that permit partial contributions, as our models assumed. Despite the substantial sociological significance of these competing assumptions, few of the authors who used the all-or-nothing specification called attention to this difference when they contrasted their results with ours (an exception is Heckathorn 1993). We continue to believe that the assumption of partial contributions is more generally applicable.

HOW CRITICAL MASS WORK HAS BEEN USED

We know now that tracing and analyzing the citations to one's own work can be a humbling and frustrating experience, and we recommend it only to those whose interest in the workings of the discipline is greater than their commitment to an exalted vision of themselves. Our data source was the Web of Science (the online version of the Social Science Citation Index), which contains citations in articles published in the journals it tracks; this database excludes all books and book chapters, as well as some journals. In this way we identified 223 citations to our work. We reviewed these citations and then attempted to locate copies of the articles to see how our work was used.

Table 1 shows the substantive area or purpose of the papers that cited our work. As the table indicates, the largest share of citations are in works centering on formal theory or protest studies, but there are significant numbers of citations in general theory articles, organization studies, communication studies, and political studies. ~~Nineteen citing articles are about protests or social movements. In 15 the central purpose is to develop a formal model of some type of collective action, and 7 are general political theory papers with some emphasis on collective action. Of the rest, 6 are about work organizations; 3 are in communication studies; 3 are generally about politics; and 2 are specifically about simulations.~~

Insert:
We distinguish "nontrivial' citations as those that mention at least something specific from the article.

In Table 2, we group the citations according to the content of the citation. As the table indicates, the vast majority of these citations—about 66 percent—could be characterized as gratuitous or passing at best. That is, the citation is just one in a long list of works cited

Table 1. General Substantive Area of Articles Citing CMT (across citation types)[a]

	All	Nontrivial[b]
Formal theory, collective action theory, or experiments (mathematical)	61	28
Protest studies, including social movements, ethnic conflict, etc.	56	21
General theory (verbal) or history of theory including political theory	27	7
Organizations	20	6
Information, communication	15	6
Politics	9	4
Environment	7	
Voluntary action	7	
Animals	2	
Simulations	2	2

[a]These tallies were done using a less-than-rigorous methodology, and the numbers do not quite add up, but they still accurately convey the general patterns we found in the citation counts.
[b]At least some specific finding or more detailed discussion.

for some very general claim, for example, "uses mathematical models" or "discussed collective action." There were 57 articles, about 26 percent, that mentioned some specific claim within our work, usually just in a phrase, sometimes in a sentence or two, but that did not relate that claim to the larger theoretical structure or address the complex or contingent nature of the results: Seventeen mentioned us in connection with networks (including at least one that cited the wrong article in this respect), 14 mentioned the group size issue, eight mentioned production functions, 7 mentioned group heterogeneity, 4 said that our models predicted thresholds. There were 2 citations that implied we said something we never said and another incorrectly characterized our central argument. Two articles referenced the point that our models predict a variety of results, which include thresholds and heterogeneity effects, and another fairly correctly summarized our different results but blurred the distinctions among the different analyses. One person stressed what we think is a central point—that we can get results but they must be complex and conditional. There were also 3 references to the fact that we were modeling the cost of organizing contractual solutions and 2 references to the fact that we had to spell out assumptions in our simulation, including one who noted that we said this led us to recognize new theory. (Numbers do not add up because a few articles mentioned more than one specific finding.)

Only a handful of articles have engaged our theory, as opposed to specific predictions, in empirical work. We found 19 articles, about 9 percent of the total, that included detailed summaries of Critical Mass Theory arguments that could be said to use or build on our work, although Critical Mass Theory is not necessarily central to every one of these enterprises. Thirteen of these articles are formal models. We will discuss these 19 articles more extensively.

First, however, we would like to comment on the general relationship between what we thought we were doing and how it has been interpreted and received in the literature. One of the most important contributions of our work seems simply to have been our use of the term "critical mass." However, there is divergence in how even this has been interpreted. We tended to stress that the problem of collective action should be understood not as the problem of obtaining unanimous participation but as the problem of getting enough people organized to contribute that some or much of the collective good could be provided. How-

Table 2. Content of Citations to Our Work[a]

Passing References	Total: 147
Article not read: abstract made it seem unlikely our work could be central, or unable to locate article	60
Rational action or resource mobilization	28
Critical mass or threshold	28
Second order Problem	31
Some Specific reference to a variable or finding (no information on larger theory, or scope conditions)	Total: 57[b]
Networks	18
Group size	15
Production functions	8
Group heterogeneity	7
Variety of effects or results	5
Thresholds	4
Contractual solutions, cost of organizing	3
Patently incorrect attributions	3
Spelled out assumptions in simulations	2
Any summary of the work longer than two sentences and/or analysis that builds upon the work	19

This last line is a total, should be brought out

If possible, swap the order of "patently incorrect" and "spelled out" lines in table

Change to: "Total: 19" to eliminate ambiguity

[a]These tallies were done using a less-than-rigorous methodology, and the numbers do not quite add up, but they still accurately convey the general patterns we found in the citation counts.
[b]Totals will not sum to 57 because some items were mentioned more than once.

ever, perhaps the majority of those who cite us seem to understand the “critical mass” as a species of threshold model, which is about getting enough initial cooperators so that a tipping point can be passed and unanimous cooperation can be achieved. And, of course, another group of those who cite the “critical mass” formulation do so to reject it entirely as a cop-out, believing that the only “real” solutions to collective action require that large numbers of individuals participate simultaneously.

COMMUNICATION STUDIES

One area where Critical Mass Theory has been taken most seriously is completely outside our original domain of interest. Theorists of communication have adapted and applied our theory to their own concerns, in particular, the evolution of computer-based information systems, for example, the Internet. We feel that they have, in so doing, exactly appreciated the core spirit of the theory, which is the need to analyze a particular situation rather than expect simple bivariate patterns to hold. They have recognized that information is a public good and that different structures for sharing information have different properties that affect people’s willingness to participate. One of the very interesting things about information as a public good is that no actor benefits from the contribution of his or her own information to the common pool; individuals benefit only from others’ contributions. Some scholars might have taken this as a sign that our models could not possibly apply. But to the contrary, communication theorists have used our theory the way we would hope, as a framework pointing to the crucial factors to study.

Markus (1987) relied upon our first (Oliver, Marwell, and Teixeira 1985) paper to develop a "critical mass" theory of interactive media, such as telephone, paper mail systems, electronic mail, voice messaging, or computer conferencing. The characteristics of interactive media are (a) widespread use creates universal access, so that people can use it who have not contributed to creating the system and (b) there is reciprocal interdependence, so the benefits and costs of using the media are affected by those who adopt later as well as those who are prior adopters. Interactive media are thus subject to problems of start-up costs and discontinuance and to the problem of "who will go first," issues that "diffusion of innovations" theory does not handle well. In extending Critical Mass Theory into this new area, Markus first summarizes the key points and then modifies assumptions appropriately for the new empirical setting. He argues that the production functions for interactive media are generally accelerating and that there are only two stable states: everyone uses a medium, or no one does. He predicts that our findings for accelerating production functions will generally hold, including the importance of costs of use and the prediction that heterogeneity will increase universal access, with predicting high interest/resource persons being the early users.

Thorn and Connolly (1987) also rely on the first article, as well as the experimental literature on collective action (including Marwell and Ames 1979, 1980, 1981) and study contributions of information to databases. Such contributions can only benefit other people and not the contributor. Based on Critical Mass Theory, they predict that reduced contributions arise from higher contribution costs, larger groups, lower values of information to participants, and greater asymmetries in information value and benefits across participants. Although they explicitly cite our emphasis on nonlinear production functions, and their own arguments about information value seem to imply an accelerating production function, they operationalize their experiments with a linear production function in which everyone's dominant strategy is not to contribute but where there is the possibility of side payments or incentives. When they discuss heterogeneity, they argue that the people with the most or best information to give will have no interest in worse information from others, so the early contributions will not attract later contributions; but this hinges on the assumption of a linear production function: The question would be what kind of database is being constructed. In their experiment, players acted as managers of a country's agricultural output and also had information about demand in their country for all the products. They could pay a cost to contribute their demand information to a database accessible to all players. Results: (1) contributions declined as costs increased; (2) asymmetries (heterogeneity) in either how useful the information was or how much they would benefit from the information lowered contributions; (3) bidding arrangements so players could compensate others for information raised contributions; (4) group size had no effect.

Monge et al. study the creation and maintenance of "interorganizational communication and information systems" (ICIs), in which different firms pool information. Such information goods include "*connectivity*, the ability of partners to directly communicate with each other through the information and communication system" and "*communality*, the availability of a commonly accessible pool of information to alliance partners" (Monge et al. 1998:411). They extensively quote Critical Mass Theory, but as a framework for analysis, not as a set of static propositions. Thus they examine the characteristics of the goods, of the participants, of the group, and of the action processes. Then they carefully distinguish the dimensions of connectivity and communality in ICIs and the ways these are produced, and then they explicitly bring in the distinction between decelerating and accelerating production functions, quoting Markus's argument that connective goods generally have accelerating production functions, and then carefully weigh the conditions under which this would be true, specifying particular conditions under which the production function would be decelerating. For participants, they begin with interests, quote Klandermans as well as us that interests can change over time, and give explicit arguments why interest in the collective goods will rise as more and more people participate, in addition to why participants will experience increasing net gains after the start-up costs have been paid. Our specific discussions of how heterogeneity works are used as the basis for a very careful discussion of the ways in which heterogeneity will affect the systems under discussion. Size and coordination issues are similarly explicitly discussed. They specifically consider the possibility that the resource-rich members of a collectivity will share information only among themselves, and they consider the conditions that will lead the resource rich to be willing to share with the resource poor.

ADAPTIVE LEARNING MODELS

Macy (1989, 1990, 1991a, 1991b) has explored the effects on collective action models of replacing the forward-looking rational actor with the backward-looking adaptive learner. Adaptive learners repeat behaviors that have had positive consequences and change behaviors that have had negative consequences. Although Macy's first articles stressed the superiority of his assumptions over rational decision assumptions, he and others now recognize that different decision models produce similar results under wide ranges of circumstances and that, when they do not, the "best" model varies depending on circumstances. We believe that assumptions about which way actors look are less important than two other elements of Macy's work. First, he treats actors' choices as probabilistic or stochastic, rather than determinate. In Macy's models, the probabilistic luck of multiple actors happening to do the same thing at the same time plays a crucial role in outcomes. At the cooperative equilibrium, populations divide into permanent contributors and permanent noncooperators. Second, Macy emphasizes the importance of an aversive privativistic baseline that leads actors to experiment with prosocial behavior. This is in contrast with rational action models that treat the baseline as a neutral or zero point. He stresses the very important point that the level of satisfaction or dissatisfaction with the status quo is an important motivator. Macy uses a general S-shaped production function and does not directly compare adaptive learning to rational decision models with the same production functions, nor does he identify where on the S-shaped curve the outcomes land, so it is very difficult from his published work to determine just how much difference adaptive learning makes in the final outcome of a process. This problem is exacerbated when he addresses group heterogeneity and compares his results with ours. Despite our wish for more controlled comparisons, we think that Macy's work has offered major advances in our understanding of collective action.

SANCTIONING SYSTEMS

Heckathorn (1988, 1989, 1990, 1991, 1992, 1993, 1996) has built a complex formal model that takes off from the rather basic discussions of Oliver (1980) and Coleman (1988) about the "second order problem" and ~~combined~~ it with insights from Critical Mass Theory and other theories to develop a broad-based theory of collective action. He adds sanctioning systems to a collective goods situation. In his basic case, an external agent imposes a collective punishment on the group if anyone defects, but the actors would otherwise prefer defection. The question is whether group members will impose internal sanctions on each other to force cooperation. The short answer is that sanctions can cut either way: Group members may either enforce compliance or use their sanctions to enforce rebellion

combines

and resistance to the external control agent. Heckathorn's conclusions are complex and contingent. Interactions among sanction strength, group cohesion, and the mix of individual and collective sanctions determine whether a group is indifferent to the external agent, compliant, or rebellious. He finds divisions of labor within groups. Group cooperation often arises through "hypocritical compliance," using sanctions to make others comply while defecting oneself, until there are enough sanctions to make everyone cooperate. Many groups retain this division of labor in equilibrium: Some members cooperate with the external agent, while others bear the cost of the sanctions to enforce their cooperation.

Heckathorn's sanctioning system has a particular production function, and he does not discuss its consequences for his results. However, when he tackles the question of group heterogeneity, he constructs careful controlled comparisons between "voluntary" systems with no sanctions, "compliance" systems with sanctions to enforce cooperation, and "balanced" systems with sanctions both for and against cooperation. He finds that when the average/mean interest is low, heterogeneity increases cooperation in all regimes, most in the compliant control system, next in the oppositional control, and least in the voluntary system. However, there is a transitional range after which heterogeneity produces even higher cooperation for voluntary compliance systems but produces lower cooperation for compliant control systems and drastically lower cooperation for oppositional control. Resource and cost heterogeneity improve voluntary compliance when conditions are otherwise unfavorable but have little effect on systems with sanctions.

This line of work has been extremely productive and has influenced subsequent scholars. Macy (1993) has modified Heckathorn's model to add an adaptive learning component and to investigate the effects of sanctioning systems with different types of production functions. Flache and Macy (1996) show that actors in sanctioning systems can become more oriented toward exchanging bilateral approval than toward coercing each other to contribute to the collective good. Heckathorn has also shown that his compliant control model can be an effective basis for an AIDS abatement program that uses group peer pressure to discourage needle sharing. Heckathorn and Broadhead (1996), Heckathorn et al. (1999), and Brown and Boswell (1995) have used it to derive predictions concerning interracial solidarity versus strikebreaking in the 1919 steel strike.

We also want to mention, but cannot begin to summarize, Heckathorn's work in integrating collective action and game theory approaches, showing how payoff functions define a complex space in which, to quote Heckathorn, "In addition to the trust problem arising in the prisoner's dilemma, collective action also confronts the bargaining problem of the chicken game, the coordination problem of the assurance game, the overcooperation problem of the altruist's dilemma, and the absence of a problem in the privileged game. Hence, studies of collective action should explore the full range of possible games" (Heckathorn 1996).

INFLUENCE MODELS

The sanctioning system models have shown one way in which people can shape other people's behavior, but a number of modelers argue that collective action is created and sustained less by attention to the collective good than by mechanisms by which cooperative action directly affects others' future actions. Critical Mass Theory argued that actors would attend to their influence on others in accelerative cases, in which there were increasing marginal returns to contributions and high rates of participation were necessary to provide the collective good. All of these models that focus on influence implicitly assume some sort of accelerative dynamic, at least insofar as they construct systems in which cooperation fosters more cooperation, and there is a general assumption that more collective action is better, although they are not necessarily grounded in our analysis of production functions. All of these authors cited Critical Mass Theory, although only Kim and Bearman (1997) and Lohmann (1994) compared their results with ours.

Although we have several disagreements with the way that Kim and Bearman (1997) represent Critical Mass Theory,[2] they develop an interesting model that emphasizes opinion change as a key Their model assumes that people respond to the decisions of people around them to whom they have a network tie and that people assess the likelihood that their own actions will affect others. Actors increase their interest if connected to others with higher interest levels who contribute, and they decrease their interest if connected to others with lower interest levels who defect. The key results of the analysis of their model are that collective action occurs only if there is a positive correlation between interest and power/centrality and that collective action cannot occur at all if they are negatively correlated. They also find that the degree of interest heterogeneity has positive effects (if the regime permits any action at all); but this effect doubtless arises because it leavens the population with some people with higher initial interest rates that can "pull up" others more effectively (if the network ties are in place).

The core of Lohmann's (1994) model is a "signaling" process. Others' actions signal the extent of dissent from the regime. People protest to influence others, not to bring the regime down directly. Lohmann uses a great deal of data on the timing and size of protests in Leipzig, as well as on the opinions of protesters and the general population at different points in time, to directly challenge our claim that extremists are important for the critical mass. She instead argues that protest accelerates when moderates are involved early in the process. Lohmann does not "test" her model with the data in any direct sense but instead uses her model as a framework for discussing the data. Nevertheless, this kind of link between theorizing and empirical data has been very rare, and it would be interesting to take her findings back into the "collective action" modeling tradition.

Gould's (1993) work on collective action and networks introduces two different kinds of interdependence effects in a model of public goods provision. First, Gould suggests that norms of fairness play a role in determining collective outcomes. Since people do not like being exploited, Gould reasons, they also do not wish to be viewed as exploiters. Therefore any contribution to a collective good by one person is subject to some matching function where others will contribution some fraction of the contribution made by the first person. When each person contributes, this changes the total contribution and the average contribution per person, which in turn invokes the norms of fairness, again causing each individual to reevaluate his or her contribution level and move it up a fraction more. Thus, the provision of the public good results from an iterative, interdependent process, rather than from a set of individual, independent decisions. Gould assumes that people will try to "match" their contributions to the average contribution everyone else has made. To model this, he assumes that people change their decisions over time. He has one person who starts

[2] They begin by stating that they want to develop a collective good solution that is a large-group solution where no contribution is noticeable and they do not want implausible assumptions. They quote us as saying the critical mass triggers action without mentioning the accelerating production function, which we list as the condition for this result, then criticize us for saying that only a few provide the good, which is the predicted outcome of a steeply decelerating production function, thus entirely missing the key distinction in our work. They then get us exactly backward by saying that there are order effects in the accelerative case (it is the decelerative case that produces order effects), and they say we argued that the most interested would necessarily go first. What we actually said was that contributions would be maximized if the *least* interested would go first, but this was a psychologically improbable scenario, and it would seem rather that the most interested would tend to go first. Thus what we actually said is what Kim and Bearman also assume in their model. Finally, they ignore our discussion of projecting others' future contributions in the accelerative case, despite its close parallels with their own model.

contributing when no one else does. Everyone else starts at zero, but their subsequent behavior is determined by this equation:

$$c_i(t) = \frac{\lambda}{N-1} \sum_{j}^{N} c_j(t-1), \qquad i \neq j.$$

This equation indicates that i's contribution at time t equals the average of everyone else's contributions at time $t-1$ multiplied by λ, a parameter that ranges between 0 and 1, where 1 means one matches the average perfectly and 0 means one stays at zero no matter what else others do. From this starting point, Gould derives an equation for the equilibrium contribution from a group.

Second, Gould examines the effects of network density and the position within the network of key (initial) contributors by assuming that the fairness equation above considers only the people to whom one has network ties. From there, he works deductively to derive a variety of network effects. Gould's model predicts that network ties have big effects. Generally speaking, the greater the network density (i.e., the greater the number of ties between individuals), the greater the total contribution to the public good will be. However, this effect is conditioned on the position of the initial contributor within the network. If the first contributor is randomly positioned in the network structure, network density monotonically increases the level of total contribution. However, if the most central actor in the network is the initial contributor, then increases in network density through low levels of density increase the total contribution (to much higher levels than the random actor scenario), but continued increases in density beyond those low levels result in decreases in the total contributions because these ties would be to those who started as noncontributors and thus would lower the average for the "fairness" equation. If the least central actor is the first to contribute, then network density also monotonically increases contribution levels but at a much slower rate of increase.

Glance and Huberman (1993) develop a stochastic model in which someone intending to participate does so with probability p and may defect with probability $1-p$; similarly, someone intending to defect does so with probability q. Benefits are linear with actual contributions, and each individual can estimate the number of other cooperators using the utility function, but there will be error in actual cooperation. The future expected utility of ongoing interactions affects decisions but with time discounting. Actors assume their actions will affect others' future actions, with these effects decreasing with group size and increasing with the overall level of cooperation. Then using mean field theory (assumptions that the group is large and that the average value of a function of a variable is well approximated by the function at the average of the variable), they derive equilibria for this model. They take several other steps, including finding critical group sizes (all of which follow from the prior assumption that effects of actors' actions on others decrease with group size). This model produces a wide variety of outcomes, including persistence of nonoptimal outcomes, flip-flop strategies, and sharp transitions from cooperation to defection.

Bahr and Passerini (1998a, 1998b) develop statistical mechanical models of collective behavior processes from analogies to physical systems. We are cited as a touch point only: They are doing something different, making no assumptions about decision algorithms, providing "an alternative basis for threshold and critical mass models." Instead of saying how individuals decide, they just assign probabilities and work with them. They cite Latané's (Latané 1981; Latané, Nowak, and Liu 1994) theory of social impact as a basis for transition from physicslike to truly sociological models of collective behavior and critique Macy for failing to give a mathematical foundation for his models. They begin by listing the constraints an influence function should have, and then they discuss the problem of finding a theoretically grounded probabilistic distribution that satisfies the constraints and fits the empirical data fairly well. They set up a model in which probability of choosing an opinion is proportional to the number who hold that opinion, where each person is weighted by a "strength" factor (of opinion or of persuasiveness) and the probability of one person changing an opinion can be affected by the "group" distribution of opinion. This model can then be adapted to include a group M who will not change opinions (e.g., a critical mass) and then to find the size of M necessary for n to change opinions. The model generates a threshold effect, that is, the tendency for opinion change to be most rapid near a critical point. They argue that the critical mass and S-shaped production function are thus products of their model, rather than inputs. Social "temperature" can be understood as the volatility of opinions; there can also be social forces that bias the direction of change, or noise in the process. Specific models of these ideas are offered, and these are combined into a general model of opinion change. Some fitting to data is given. Part II considers cellular automata, in which individuals interact only with those near them according to well-formed rules and then are put together in large numbers to generate large-group patterns. For example, in a model of water molecules in a river, each molecule interacts with others nearby according to microlevel rules, and then a cellular automaton puts them together to form macroscopic river flow. Such models can be used to show how opinions or action can spread across a large population according to reasonable rules about how people behave in response to those nearby. These models show abrupt transitions from consensus to near consensus with well-ordered pockets of opinion at low social "temperatures," to less-ordered nonconsensus at higher temperatures. Infusing a small group into the situation could produce chaos.

OTHER MODELS

We also found a handful of articles that addressed different issues related to Critical Mass Theory. Cortazar (1997) emphasizes Olson's "intermediate" groups in which contributions are noticeable but no one person alone can provide the good and works in the game theory tradition, building principally on Sadler. He defines the concept of an n-group, a nonredundant group such that all members' contributions are needed, which then resolves to an assurance game, and he mentions our discussion of the accelerating case, in which the initial group motivates contributions from others and thus people may decide to act. He stresses that the subjective feeling of being needed may arise from identities other than the one at stake and could be improved by group homogeneity, and he concludes: "Finally, the structure of payoffs described by the 'Assurance Game' (AG) explains why collective action frequently arises in a rather abrupt way. The *coordination* required for collective action to begin may be triggered by various circumstances, any incident or situation that 'signals' to an 'n-group' that all of its members are about to take action."

Ohlemacher (1996) works with networks, developing the concept of relays, which are mobilization-mediating social networks. The general context of this work is the importance of weak ties. His summary of Critical Mass Theory stresses importance of the critical mass, as well as homogeneity, heterogeneity, and our self-comparisons to Granovetter. His own contribution is to define a "social relay." Social relays connect previously unconnected networks, acting as brokers or transmitters of contacts between strangers or groups of strangers; form the immediate environment, organizational background, or institutional grounding of several face-to-face networks; and in some cases generate new networks by charging preexisting contacts in a new way. Social relays thus spread mobilization to networks outside themselves. Structurally, relays are heterogeneous, they generate subnetworks (the critical mass) that create new networks, and they need a rich body of weak ties that link as much of the population as possible. He has empirical data on citizens' campaigns in Germany that demonstrate the importance of these distinctions. This work thus builds on and extends our initial work on network effects but goes way beyond it in specifying the elements that are crucial in a rich empirical case.

In the most recent work using Critical Mass Theory that we have found, Jones and colleagues (Jones et al. 2001) also focus on networks. They build directly on our critical mass arguments to develop a theory of coalition formation in the mobilization networks of a variety of protests. They argue that coalition forms are central to mobilization and draw on our work to stress that the critical mass in this process is the need for a few highly productive organizers to be brought together. They stress the central importance of locating the few high-contributing individuals who can form a coalition. In discussing the factors that might impede organizers from working together, their essential point is that mobilization has a different production function from that of planning and framing. Our distinction between accelerative and decelerative production functions is central to their argument, as they contend that protest forms have accelerative production functions, while leadership functions have decelerative functions. They suggest that there are factors that can impede leadership, that is, that there are negative production functions in leadership. Framing also involves decelerative production functions. Thus they argue that the dynamic between mobilization, with its accelerative function, and framing, with its decelerative function, is crucial in understanding coalition dynamics. Small groups are important in initial phases of mobilizing but are important in all phases of framing. In addition to the critical mass, the authors also refer to our older work on mobilizing technologies (Oliver and Marwell 1992) that emphasizes the incompatibilities of mobilizing money and mobilizing personnel.

Yin (1998) analyzes threshold models to show how tipping over into revolution can happen. His reference to us is only passing. But he has a detailed analysis of how distributions affect results. He notes that although many people use normal distributions, bimodal, multimodal, or skewed distributions make perfect empirical sense. He constructs three families of logistic functions to characterize a wide variety of distributions, and he analyzes the equilibria of each, where equilibria are points at which the proportion protesting just matches the threshold, so no more are joining in. A normal curve has three equilibria, but the first and third are stable attraction basins (very low, very high), while the middle one is an unstable tipping point that can go either way. Whenever there are multiple equilibria, starting points or shocks affect the outcomes. In a bimodal distribution, there are a pro-government group and an opposition group with different thresholds. In the skewed or multimodal distributions, one mode is bigger than the other, and they vary in how far apart they are. Unimodal distributions can exhibit sudden tips, although they can also be quite stable despite widespread discontent; bimodal distributions are less tranquil but do not show the sudden tips. Yin offers propositions about how the opposition "should" behave in the complex situations. All of the published work on heterogeneity in collective action has used only unimodal distributions. Taking account of these more complex distributions would be an important extension.

Szilagyi (2000) develops a formal way to relate production function ideas to prisoner's dilemma (PD) payoffs, using a formulation in which the S-shaped, decelerating (convex), accelerating (concave), and linear production functions are special cases. A PD can be expressed as two payoff functions specifying the payoff to a cooperator and defector as functions of the ratio of cooperators to total number of participants. The PD total payoff function is a scaled version of a production function for collective action. There are x cooperators, and $n - x$ defectors; $y1(x)$ is payoff function for cooperators, and $y2(x)$ is payoff function for defectors. In a PD, $y2(x) > y1(x)$ for all x, but $y1(n) > y2(0)$. Quadratic

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functions describe most of the cases. Szilagyi defines points in PD functions that can provide reference points for defining a quadratic function, and he solves for the quadratic coefficients. He then defines the average payoff to each member of the collective and allows this to represent the payoff function for the collective (e.g., it is the collective production function divided by n). The production function is a cubic and connects the lower end of the defectors' curve with the upper end of the cooperators' curve. It is impossible to go backward, from production function to payoff functions, without additional information. The PD specification permits nonmonotonic payoff functions. These equations can prove useful for linking collective action and game theory traditions.

CONCLUSIONS

It is clear that most social scientists have finally moved away from trying to develop "the theory of collective action" to recognizing that there are many different issues and many different kinds of collective action and that one can shade into the other depending upon the structural characteristics of the situation. Sociologists rarely use the term "response surface," but it is a very helpful concept for thinking about the complexities involved in collective action. A response surface is simply a k-dimensional graph of an outcome variable as predicted by $k - 1$ independent variables. In the kinds of models we have been considering, the outcome is the total contribution to the collective good (or the total number of contributors), and the independent variables have included such factors as group size, the cost of the good, the degree of jointness of supply, the shape of the production function, the mean resource and interest levels, the degree of interest and resource heterogeneity, the presence or absence of sanctioning systems, and so on. There are large regions of the response surface in which a few variables are at levels that make collective action impossible, so that other variables have no effect, and other large sections in which collective action should never be problematic. All the other variables make a difference only in the regions of the response space where cost/benefit relations do not overwhelm other factors. It is obviously impossible to study all possible independent variables at once, but when we write models, we should be envisioning the location of our model within the full response space, recognizing what is being held constant (and at what level) and what is being varied (and within what ranges). Simply explicitly listing the factors held constant and comparing them to other models might more readily call to attention seemingly unimportant operational decisions that turn out to make big differences in the results, for example, whether actors must "spend" all their resources or can make partial contributions changes how "resource heterogeneity" affects collective action.

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Envisioning the full response surface should be linked with a search for controlled comparisons and thoughtful experimental designs to clarify complex interdependencies. This is all too rare. Instead, most of us seem to approach modeling so that we can say something like, "See, I can make my model do something different from what your model did." Our own work is as subject to this criticism as that of those who have followed us.

All of these various formal models should also be subjected to some empirical assessment. For example, Lohmann specifically argued that we, and most other formal modelers, are wrong to stress the importance of highly interested actors in the initial stages of action: She calls them "extremists" and says, to the contrary, that in the opinion data that form her empirical case it is the participation of moderates that is crucial. With the increasing amount of protest event data becoming available, it is possible to subject collective action models to very different kinds of tests, looking at whether they produce shapes and patterns of protest event distributions over time that look like empirical data.

Despite the lack of empirical data supporting the models of Critical Mass Theory in detail, we stand by many of the assumptions that animated them. We think that the claim that it is important to analyze the specific production function of a particular case has stood the test of time, although analysts often downplay this factor when they want to stress others. We believe the general argument that there are different kinds of or phases of collective action that have the properties we pointed to in our contrast between the decelerative and accelerative cases has stood up, although this contrast is often downplayed. We think that Heckathorn's further specification of the differing dynamics of collective action in different cases will further advance attempts to clarify the different kinds of dynamics of collective action. We stand by the empirical claim that relatively small groups of people are often at the core of action. Research also often reveals the importance of a small initial cadre even in protest waves that become very big. Whether we correctly identified the particular dynamic whereby the initial participants motivate future participants seems open to question, but we have seen very little empirical evidence that supports the idea that huge protests "come from nowhere." However, our proposed mechanism of the contractual solution does not seem to have garnered much empirical support. Instead, theory and data seem to point more to the creation of implicit contracts through signaling, or considerations of what we called "indirect production" through considering the effects of one's actions on the future actions of others.

Within the broader terrain of rational choice theory, there has been serious and illuminating discussion of the differences among forward-looking, backward-looking, and sideways-looking models, as well as discussions of the kinds of factors people attend to in their decisions. Empirically, it is quite clear that real people do all of these, although they do them in different circumstances and do not necessarily follow the posited decision rules accurately. It is also clear that in many circumstances, different assumptions about individual decisions lead to essentially the same results at aggregate group levels. In our own work, we came increasingly to focus on the larger structural factors of the collective action problem itself that shaped action contexts, rather than individual decision rules. The shift from determinate to stochastic decision models seems to us to be a definite advance, and we are well aware that stochastic "matching rules" fit empirical behavior patterns much better than the kind of determinate decision models we developed. However, it should be said that it is not clear that we would have been able as easily to understand the dynamics of production functions and to explore the effects of group heterogeneity and network centralization if we had not started with the simpler determinate decision rules. We suspect that in the next wave of theorizing researchers will move increasingly toward "statistical mechanical" approaches that treat behavior probabilistically, in which the underlying decision engine is less consequential.

Stepping back from the specific questions about "whither collective action theory?" what about the lessons for theory development more broadly? We have offered only a single case study, but we suspect the case is far from unique or even unusual. Theory books tend to treat theory development subsequent to initial statements as if there were some kind of triumphal march to greater and greater clarity and/or generality and/or verification. Or, alternatively, as a grand conflict with alternative theories, and/or negative findings, either credible or mistaken. Our experience seems more mundane. Not everything that happens in social theory is about clashes of dominant paradigms or even about the spread of new ideas. Much of the preliminary phase of theorizing involves locating oneself in a social/intellectual space rather than actually engaging ideas. We were initially dismayed to realize that we received so many essentially gratuitous citations to our work that either ignored its content completely or abstracted some single claim from its larger complex and interactive context. However, this is consistent with one of the major patterns of citation found in a recent comparison of disciplinary areas (Hargens 2000). At the same time, we take no small amount of pride (and perhaps undeservingly claim some small share of the credit) in the few works that built on our beginnings and crafted elegant and complex models capable of integrating a large number of factors that built upon each other and truly expanded our understanding of the dynamics of collective action.

REFERENCES

Bahr, D. B., and E. Passerini. 1998a. "Statistical Mechanics of Collective Behavior: Macro-sociology." *Journal of Mathematical Sociology* 23:29–49.

———. 1998b. "Statistical Mechanics of Opinion Formation and Collective Behavior: Micro-sociology." *Journal of Mathematical Sociology* 23:1–27.

Bonacich, R., G. H. Shure, J. P. Kahan, and R. J. Meeker. 1976. "Cooperation and Group Size in the *n*-Person Prisoners' Dilemma." *Journal of Conflict Resolution* 20:687–706.

Brown, C., and T. Boswell. 1995. "Strikebreaking or Solidarity in the Great Steel Strike of 1919: A Split Labor-Market, Game-Theoretic, and Qca Analysis." *American Journal of Sociology* 100:1479–1519.

QCA

Chamberlin, John. 1974. "Provision of Collective Goods as a Function of Group Size." *American Political Science Review* 68:707–16.

Coleman, James S. 1988. "Free Riders and Zealots: The Role of Social Networks." *Sociological Theory and Methods* 6:52–57.

Cortazar, Rene. 1997. "Non-redundant Groups, the Assurance Game, and the Origins of Collective Action." *Public Choice* 92:41–53.

Demerath, N. J., Gerald Marwell, and Michael T. Aiken. 1971. *Dynamics of Idealism: White Activists in a Black Movement*. San Francisco: Jossey-Bass.

Flache, A., and M. W. Macy. 1996. "The Weakness of Strong Ties: Collective Action Failure in a Highly Cohesive Group." *Journal of Mathematical Sociology* 21:3–28.

Frohlich, Norman, and Joe A. Oppenheimer. 1974. "The Carrot and the Stick: Optimal Program Mixes for Entrepreneurial Political Leaders." *Public Choice* 19:43–61.

Frohlich, Norman, Joe A. Oppenheimer, and Oran R. Young. 1971. *Political Leadership and Collective Goods*. Princeton, NJ: Princeton University Press.

Glance, N. S., and B. A. Huberman. 1993. "The Outbreak of Cooperation." *Journal of Mathematical Sociology* 17:281–302.

Gould, Roger V. 1993. "Collective Action and Network Structure." *American Sociological Review* 58:182–96.

Hardin, Russell. 1982. *Collective Action*. Baltimore: Published for Resources for the Future by the Johns Hopkins University Press.

Hargens, Lowell L. 2000. "Using the Literature: Reference Networks, Reference Contexts, and the Social Structure of Scholarship." *American Sociological Review* 65:846–65.

Heckathorn, Douglas D. 1988. "Collective Sanctions and the Creation of Prisoners-Dilemma Norms." *American Journal of Sociology* 94:535–62.

———. 1989. "Cognitive Science, Sociology, and the Theoretic Analysis of Complex Systems." *Journal of Mathematical Sociology* 14:97–110.

———. 1990. "Collective Sanctions and Compliance Norms: A Formal Theory of Group-Mediated Social-Control." *American Sociological Review* 55:366–84.

———. 1991. "Extensions of the Prisoner's Dilemma Paradigm: The Altruist's Dilemma and Group Solidarity." *Sociological Theory and Methods* 9:34–52.

———. 1992. "Collective Action and Group Heterogeneity: Cohesion and Polarization in Normative Systems." *Advances in Group Process* 9:41–63.

———. 1993. "Collective Action and Group Heterogeneity: Voluntary Provision versus Selective Incentives." *American Sociological Review* 58:329–50.

———. 1996. "The Dynamics and Dilemmas of Collective Action." *American Sociological Review* 61:250–77.

Heckathorn, Douglas D., and Robert S. Broadhead. 1996. "Rational Choice, Public Policy, and AIDS." *Rationality and Society* 8:235–60.

Heckathorn, Douglas D., Robert S. Broadhead, Denise L. Anthony, and David L. Weakliem. 1999. "AIDS and Social Networks: HIV Prevention through Network Mobilization." *Sociological Focus* 32:159–79.

Jones, Andrew W., Richard N. Hutchinson, Nella van Dyke, and Leslie Gates. 2001. "Coalition Form and Mobilization Effectiveness in Local Social Movements." *Sociological Spectrum* 21.

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207-231 peo

Kim, HyoJoung, and Peter S. Bearman. 1997. "The Structure and Dynamics of Movement Participation." *American Sociological Review* 62:70–93.

Latané, Bibb. 1981. "The Psychology of Social Impact." *American Psychologist* 36:343–56.
Latané, Bibb, Andrzej Nowak, and James H. Liu. 1994. "Measuring Emergent Social Phenomena: Dynamism, Polarization, and Clustering as Order Parameters of Social Systems." *Behavioral Science* 39:1–24.
Lohmann, Susanne. 1994. "The Dynamics of Informational Cascades: The Monday Demonstrations in Leipzig, East-Germany, 1989–91." *World Politics* 47:42–101.
Macy, Michael W. 1989. "Walking out of Social Traps: A Stochastic Learning Model for Prisoner's Dilemma." *Rationality and Society* 1:197–219.
———. 1990. "Learning-Theory and the Logic of Critical Mass." *American Sociological Review* 55:809–26.
———. 1991a. "Chains of Cooperation: Threshold Effects in Collective Action." *American Sociological Review* 56:730–47.
———. 1991b. "Learning to Cooperate: Stochastic and Tacit Collusion in Social-Exchange." *American Journal of Sociology* 97:808–43.
———. 1993. "Backward-Looking Social-Control." *American Sociological Review* 58:819–36.
Markus, M. L. 1987. "Toward a Critical Mass Theory of Interactive Media: Universal Access, Interdependence, and Diffusion." *Communication Research* 14:491–511.
Marwell, Gerald, and Ruth E. Ames. 1979. "Experiments on the Provision of Public Goods, I. Resources, Interest, Group Size, and the Free-Rider Problem." *American Journal of Sociology* 84:1335–60.
———. 1980. "Experiments on the Provision of Public Goods. Part II—Provision Points, Stakes, Experience, and the Free Rider Problem." *American Journal of Sociology* 85:926–37.
———. 1981. "Economists Free Ride, Does Anyone Else? Experiments on the Provision of Public Goods. Part IV." *Journal of Public Economics* 15:295–310.
Marwell, Gerald, and Pamela E. Oliver. 1993. *The Critical Mass in Collective Action: A Micro-Social Theory*. ~~Edited by J. Elster and M. S. McPherson.~~ Cambridge University Press. AU: Which location? New York
Monge, P. R., J. Fulk, M. E. Kalman, A. J. Flanagin, C. Parnassa, and S. Rumsey. 1998. "Production of Collective Action in Alliance-Based Interorganizational Communication and Information Systems." *Organization Science* 9:411–33.
Ohlemacher, T. 1996. "Bridging People and Protest: Social Relays of Protest Groups Against Low-Flying Military Jets in West Germany." *Social Problems* 43:197–218.
Oliver, Pamela E. 1980. "Rewards and Punishments as Selective Incentives for Collective Action: Theoretical Investigations." *American Journal of Sociology* 85:1356–75.
———. 1983. "The Mobilization of Paid and Volunteer Activists in the Neighborhood Movement." *Research in Social Movements, Conflicts and Change* 5:133–70.
———. 1984. "If You Don't Do It, Nobody Else Will: Active and Token Contributors to Local Collective Action." *American Sociological Review* 49:601–10.
———. 1989. "Bringing the Crowd Back In: The Nonorganizational Elements of Social Movements." *Research in Social Movements, Conflicts and Change* 11:1–30.
Oliver, Pamela E., and Mark Furman. 1990. "Contradictions between National and Local Organizational Strength: The Case of the John Birch Society." *International Social Movement Research* 2:155–77.
Oliver, Pamela E., and Gerald Marwell. 1992. "Mobilizing Technologies for Collective Action." In *Frontiers in Social Movement Theory*, edited by A. M. Mueller and C. M. Mueller. New Haven: Yale University Press.
Oliver, Pamela E., Gerald Marwell, and Ruy Teixeira. 1985. "A Theory of the Critical Mass. Part 1—Interdependence, Group Heterogeneity, and the Production of Collective Action," *American Journal of Sciology* 91:522–56.
Olson, Mancur, Jr. 1965. *The Logic of Collective Action: Public Goods and the Theory of Groups*. Cambridge, MA: Harvard University Press.
Ostrom, Elinor. 1998. "A Behavioral Approach to the Rational Choice Theory of Collective Action." *American Political Science Review* 92:1–22.
Prahl, R., Gerald Marwell, and Pamela E. Oliver. 1991. "Reach and Selectivity as Strategies of Recruitment for Collective Action: A Theory of the Critical Mass .5." *Journal of Mathematical Sociology* 16:137–64.
Schofield, Norman. 1975. "A Game Theoretic Analysis of Olson's Game of Collective Action." *Journal of Conflict Resolution* 19. AU: Page numbers? 441-61
Smith, Jan. 1976. "Communities, Associations, and the Supply of Collective Goods." *American Journal of Sociology* 82:291–308.
Szilagyi, M. N. 2000. "Quantitative Relationships between Collective Action and Prisoners' Dilemma." *Systems Research and Behavioral Science* 17:65–72.
Thorn, B. K., and T. Connolly. 1987. "Discretionary Databases: A Theory and Some Experimental Findings." *Communication Research* 14:512–28.
Yin, C. C. 1998. "Equilibria of Collective Action in Different Distributions of Protest Thresholds." *Public Choice* 97:535–67.