

REINVIGORATING ZMRM:
ZALD/MCCARTHY RESOURCE
MOBILIZATION

by
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- ◇ Charles Tilly, "Social Movements Here and Elsewhere, Now and Then"
- ◇ W. Richard Scott, "A Call for Two-Way Traffic: Improving the Connection Between Social Movement and Organizational/Institutional Theory"
- ◇ Elisabeth S. Clemens, "How Shall We Organize? Privatizers, Volunteers, and Policy Innovation in the 1990s"
- ◇ Charles Perrow, "The Rationalist Urge in Sociology and Social Movements: Zald as History"
- ◇ Gerald F. Davis and Doug McAdam, "Corporations, Classes, and Social Movements After Managerialism"
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- ◇ Yeheskel (Zeke) Hasenfeld, "Human Service Organizations and the Production of Moral Categories "
- ◇ John D. McCarthy, "Reinvigorating ZMRM: Zald/McCarthy Resource Mobilization"
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**Reinvigorating ZMRM:
Zald/McCarthy Resource Mobilization**

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Reinvigorating ZMRM

I have chosen as my title Reinvigorating Zald/McCarthy Resource Mobilization. An alternative title I entertained was Resuscitating Resource Mobilization, but I decided that it suggested that RM was closer to the deathbed than I believe is the case. You might be surprised, however, to know that I believe that RM is sickly. Some might argue, on the other hand, that RM is alive and well, and some of the agendas that have emerged inspired by early statements by a number of us have grown quite vigorous. But a number of the research agendas that Mayer and I thought implicit in our early statement of the key lines of resource mobilization analysis, and which we still believe deserve serious attention, have not drawn the research attention we believe they merit. What I want to do today is to recall some of those lines of analysis, try to put them into broader context of work that has been accomplished in the twenty-five years since we laid out our version of resource mobilization, and ponder why some of them have not developed. But I want to begin by noting some of the main lines of criticism of what we did that have been offered by some of our critics. What I have to say today benefits from lengthy discussions with Mayer, but, unfortunately, I cannot hold him responsible for everything that I have to say.

Main Lines of Criticism

While Mayer and I have been accused of a multitude of sins, many of which I will ignore today, I want to briefly note several of the more important lines of criticism that have been leveled at our **earlier, foundational, statements**, and, then, to respond to them briefly. Both Mayer and I have continued to write about social movements for the ensuing twenty-five years, extending and modifying our early statements, contradicting one another, and, consequently, we have bedeviled the efforts of the most serious of the newer theory-bashers who hope to hold us to some earlier orthodoxy.

An Impoverished Conception of Resources. We pointed to the importance of material resources, labor and legitimacy as key factors in understanding how social movement organizations turn preference pools or solidary groups into challenging groups, and then how resources figure in efforts by such groups

to pursue challenges. A litany of criticism from both our scholarly friends and foes, which I must hasten to add continues, emphasized how little attention we paid to thinking about kinds of resources beyond the three we named, and, especially, our emphasis upon material over human resources. We were, indeed, almost silent on the key concept of resources. We had been deeply influenced by Bill Gamson's *Power and Discontent*, where he developed a convincing brief for assessing the power of groups through their control of slack resources. We followed him (as did Chuck Tilly in *From Mobilization to Revolution*) by emphasizing the use of resources rather than developing a more sophisticated conception of kinds of resources, which groups control which kinds, their fungibility and their transferability. I will return to the conceptualization of resources below, but let me plead guilty for Mayer and me—we began with an impoverished conception of resources, but I don't think we are going to get away with blaming Bill Gamson for our silence. Bill did notice our silence, in his "Introduction" to our 1987 volume of essays, calling it the "soft underbelly of the theory," but he takes no responsibility for it.

A Blindness to the Role of Culture in Social Movement Processes. Critics took our silence about culture as, what in fact was an unintended, lack of appreciation of its role. Both Mayer and I were steeped in the "social construction of social problems" tradition, and we took it for granted, which was important in explaining our silence. We thought of the social construction of grievances, loyalty and partisanship as backdrop to understanding the mobilization of solidary groups, and I still find work in that tradition quite useful. I discovered over the years that many sociologists, including many CBSM, colleagues were unaware of that tradition, and my impression is that much of the new cultural analysis of movements does not take it very seriously either. The giant cultural turn that has taken place throughout the discipline as well as among CBSM scholars has had the consequence of magnifying our earlier silence on how culture matters in social movement processes. Scholars who privilege the role of culture in making sense of social movement processes have been especially critical of our early statements. Of course, culture matters. The question to be asked is how do we both systematically and empirically study its effects, one that is only still in the process of being answered adequately. Some of the best work, in my opinion, has been done by RM scholars or their fellow travelers (e.g. that of Bill Gamson, who called

culture a “blind spot” in our approach, and that of David Snow and his colleagues). Maybe it is the constant barrage of criticism we have taken on the role of culture that is partially responsible for Mayer having been so strongly influenced by the cultural turn. He will have to speak for himself on his cultural turn, however.

An Underdeveloped Analysis of the Role of Political Opportunity. Starting with Chick Perrow’s early and trenchant critique (1979) of us through Herb Kitschelt’s more recent one (1991), a number of critics have pointed out that our early statements, for the most part, ignored the role of political processes. This was in contrast to the more central role they were accorded in the early statements of Tilly (1978), Oberschall (1973), and Gamson (1975). Of course, we took the thrust of the statements of our theoretical RM compatriots seriously, and our early statements made clear that we believed that a comprehensive theory of movement processes would include a strong element of political process. Their muscular, emergent, sketches of the political process approach were also taken for granted backdrop to our early statements. Anyone who knew Zald’s work on the political economy of organizations would have appreciated the centrality of political process background meta-assumptions for what we were about. (The Epilogue to the Vanderbilt Conference volume [Zald and McCarthy, 1979], for instance, claimed that RM approaches brought the study of social movements back into the mainstream of political sociology).

Political process approaches, however, have become the 800-pound gorilla of movement scholarship. They have provided the dominant image for thinking about movement emergence and trajectories in the last decade. And, its leading scholars have pioneered a research methodology, protest event analysis, that has become a major force in the empirical study of movement processes. I must admit that Mayer and I each have been complicit in allowing political process understanding to become dominant. I believe, nevertheless, that the ascendance of political process accounts of movement processes has deflected scholarly attention from several of Mayer’s and my most cherished lines of analysis. This is because political opportunity approaches are, in general, blind to organizational processes and privilege political over economic processes.

An Over-reliance upon Rational Choice Accounts of the Participation of Activists. Our early statements were admiring of Olson's casting of the problem of collective action participation, to be sure. But, in practice, our thinking, as reflected in our analyses, tended much more toward the cost than the benefit side. And, we (and here I think I can speak for Mayer) still remain persuaded that as the costs of participation (ranging between ordinary life constraints through severe state repression) social movement participation, in the aggregate, declines. This is an expectation, by the way, which receives strong empirical support across a whole range of kinds of political participation in the recent brilliant study by Sid Verba and his colleagues, Kay Schlozman and Henry Brady. (1995) This includes protest participation, where middle-class citizens are more than twice as likely to have taken part in a demonstration than are poor citizens (1995:191) But, I am probably mostly singing to the choir here.

There is, of course, a vigorous and ongoing debate around these issues, which we are still accused of having a hand in launching, and, about which we were far less orthodox than our critics have been willing to believe. This has become a lively scholarly arena, and the issues have come to be framed in ways that makes them far more complicated than we had imagined them to be. The meaning of social movement participation has been conceptualized in more sophisticated ways and measured with far more precision than when we wrote. In John Loflands judgement studies of movement participation have dominated movement research.

Conclusion. On the first three charges, then, I think we were guilty as charged. On the fourth, we were not as guilty as many of our critics charged—we were never orthodox rat choicers. God forbid! Nevertheless, I am sure costs count.

Where We Began, How and Why

I have led with some of the most important concerns of our critics. That we have had to defend ourselves for ignoring some of the most important alternative accounts of movement emergence, growth and form underscores the implicit, if not always realized, power of our more organizational resource mobilization approach.

Motivating Questions. Two of the key insights upon which we built were the important role of new organizational technologies in activating citizens and the important role of elite resources in subsidizing the use of such technologies by SMOs. Both of these insights were based upon observation of movement activity in the late 1960s and the early 1970s. Remember that we were working at a time when a consensus had begun to develop among sociologists, and a few political scientists, that our earlier understandings of pluralism were inadequate as a solution to problems of social, political and economic inequality. The main line of attack held that social infrastructures were unequally available to social groups, and, as a result, many deprived groups remained severely underrepresented in the informal system of interest groups. Many isolated individuals with common interests, such as consumers, were similarly underrepresented.

We aimed to understand how the social change playing field could be leveled through the application of technologies and the transfer of resources from advantaged to disadvantaged communities: Many of the SMOs spawned by Ralph Nader's efforts seemed to offer the possibility of leveling the playing field for some classes of the underrepresented, unorganized, common people's interests, and, of course, our image of the professional social movement organization (PSMO) was modeled on those prototypes. As well, many of the resource aggregation and mobilization technologies used by those groups, such as direct mail and grass roots lobbying, appeared to offer much broader possibilities as means for empowering disadvantaged groups. We were also struck by the flow of outside resources to many of the SMOs active during that period, which led us to the concept of "conscience constituents." We focused, especially, upon the role of institutional conscience constituents in aiding movements of the disadvantaged. Craig Jenkins, in his early synthesis of RM approaches concluded that our

“..entrepreneurial model appear[ed] most relevant for movements among deprived groups and broad disorganized collectivities.”(1983:531)

Key Assumptions. Our thinking was embedded in the prevailing meta-assumptions of organizational analysis of the 1970's. Mayer had just recently published his *Organizational Change: The Political Economy of the YMCA* (1970), that developed a political economy approach to understanding organizational transformation. And he and Robbie Garner (then Ash) had, a few years earlier, tried to bring current organizational approaches to bear upon the classic Weberian and Michelian takes on the problem in their, still fresh, paper on SMO transformation. Steeped in Herbert Simon's analyses of how bounded rationality constrains the means-ends calculations of organizational leaders, we were inclined to stress the dilemmas that confront SMO founders and leaders as they calculate how to mobilize adherents.

Olson's analysis was important in leading us to this focus. Regardless of whether one accepts the details of his public goods analysis, his insights about the difficulties of mobilizing members and the logic of group mobilization resonated with our assumptions about how organizations function. And, following the lead of our RM compatriots on social movement processes in general, we assumed that SMOs should not behave any differently than firms or YMCAs. Art Stinchcombe argued soon afterwards that little attention had yet been paid to how organizations, in his terms, mobilize partisan attachments. (19xx:xx) His was another way of stating this key question. I think, unfortunately, that among SMO analysts, his assessment still holds:

Method and Style of Argument. Our way of proceeding combined close observation of the nuts and bolts of movement activity with a quest for clear definitions of central elements and processes within the intersecting intellectual traditions of movement and organizational analysis. We were looking for tools to make sense out of what we saw, in Randy Collins phrasing, “seeking theory in the confusing details of thick descriptions of social life.” (1981:xx) I must admit that we tended to be cynical and debunking as we constructed our accounts. And sometimes we intended that our choice of language might offend the sanctimonious scholars in our midst—characterizing all of the SMOs in a social movement as an industry, for instance. Not that this was, or does it remain, a difficult task.

Prescient Empirical Claims. Our efforts to read the patterns in a variety of kinds of data allowed us to make a couple of strong empirical claims about emerging social trends that have turned out to have been prescient. The first was a judgement that traditional forms of social involvement appeared to be declining. At present there is a lively debate around the existence of such a trend, and Bob Putnam, who makes strong claims similar to those we made 25 years ago, has finally agreed that we were among the early spotters of it. The second was our strong claim about the professionalization of social change efforts, an unashamed theft of Pat Moynihan's "professionalization of reform." As Pam Oliver has said many times in public, "If they were right about anything, it was professionalization." Of course, it was these two trends that provided our original piece its plot line—the puzzle was how could social movement activity appear to be increasing vigorously at the same time that social participation was declining?

Underdeveloped Lines of Analysis: Where We Should Be Going

There are a number of lines of analysis, more or less explicit in our foundational statements, which I think should be pursued more vigorously than they have been thus far. I would view increased levels of attention to these problems as a sign of reinvigoration of our strand of RM. Without renewed signs of interest in these lines of thinking, I will have to judge certain elements of our strand of RM moribund. Mayer and I will pursue them, of course. And, we intend to live forever. The evidence, unfortunately however, has not always supported our most fondly crafted predictions, as I will recall in a moment.

How to Think About Transferring Resources Between Groups. We pointed to instances of the transfer of resources from advantaged to disadvantaged groups, suggested that such transfers were not uncommon, and argued that they are central to modern resource mobilization processes. In this we were different from our RM compatriots. Each of them focused primarily upon the mobilization of resources within solidary groups, and the how such groups use resources in attempting to exert influence. And, each of them worked with a far richer conception of resources than we did. Most attention was paid to

synthetic dimensions of resources that transcend resource category—such as amount, slackness/availability and jointness. And, any analysis of resources must attend to these dimensions.

But, in addition, thinking about resource transfers across solidary group boundaries leads directly to the need for a typology of kinds of resources, their fungibility, the symmetry of their fungibility, and the legitimacy of their transfer. Yet, SM analysts have devoted relatively little attention to the development of nuanced categories of kinds of resources, either appropriating the standard categories of economists, e.g. Tilly's "land, labor and capital." (1978:69), or focusing attention only upon money and labor, e.g. Oliver and Marwell (1992). A notable exception is the effort of Cress and Snow (1996) that created a typology of kinds of resources generated out of a thick description of the mobilization of resources by local homeless insurgent groups. And with minor exceptions (e.g. Oliver and Marwell, 199x), no systematic attention has been paid by SM analysts to questions of fungibility.

Let me list what seem to me to be the important theoretical questions that must be addressed before the process of transfer of resources across solidary groups boundaries can be investigated more systematically. This is, of course, a general problem and its solutions should be appropriate to the transfer of resources across any solidary groups boundaries, including those from the poor to the rich. And while I focus upon voluntary transfers, a complete specification of the process would also include involuntary transfers, such as those operating through state taxes and organized crime.

What are the Kinds of Resources? Since we began this adventure, there have been important developments in other parts of the discipline toward the conceptualization of a more sophisticated range of resource types. Beyond land labor and capital, we now have rich conceptualizations available and measurement armies working to specify the distribution of human capital, social capital and cultural capital across, and even more specific subtypes of each, e.g. "civic skills" as a form of human capital. (Verba, et al., 1995) And there are technological resources, which are independent of the others, those

most familiar to SM analysts being protest and organizational repertoires. It appears to me that there are enough giants shoulders to stand upon now in order to develop a typology of kinds of resources.¹

Which Groups Control Which Kinds of Resources? Resources may be controlled by individuals, networks, organizations, and states. We know quite a bit about how land, labor and capital are distributed across such social units, but we know less about how, for instance, social capital is distributed, and, especially, inequalities in its distribution. The control of some kinds of resources is inherently more exclusive, such as money and land, than others, such as resource aggregation technologies like direct mail techniques.

How Fungibility and Symmetrical are Kinds of Resources? We think of money as the most fungible of resources, although Zelizer has convinced many of us that even its fungibility is in many instances socially constrained. So money can be converted to labor, land, and maybe even under the right circumstances human, cultural and social capital. But labor may not, in the short run, be convertible into human capital, as when an SMO needs an accountant, but only high school volunteers are available for work.² On the other hand, congregation based IAF groups seem to be successful in turning the paid labor of organizers through a long process, employing a widely available social technology, into social capital. (Rogers, 1990)

I have offered a very brief sketch of a program for beginning to think more systematically about the transfer of resources between groups for social movement purposes. Approached in this way, the problem of transfers can bring the study of social movements more directly into dialogue with those who study social stratification. Many of us have long thought what we study has direct implications for stratification processes, but we have not made our case very well beyond the boundaries of our own small research community. Chuck Tilly's *Durable Inequality* is a major step in that direction, by the way. And, if you have not yet come across it, I recommend it to you very strongly.

¹ Christian Lahusen's (1996) analysis of the combination of financial resources and celebrity in the production of cause music concerts goes some way in specifying a typology of resources broader than land, labor and capital.

Origin, Timing and Consequences of Transfers. The one substantive arena where we do have some solid empirical work on the transfer of resources is through the money provided by foundations for social change efforts. Craig Jenkins has been tracking these patterns over the last five decades. (Jenkins and Halci, 1999) Debates about our earlier claims concerning the role of such support have led to a number of looks at the timing of such transfers. For many movements, those resource flows follow rather than lead, but not for all. We know less about state transfers, and most of that comes from conservative critics who aimed to “defund the left.” (e.g. Bennett and DiLorenzo, 1985)

The Study of Social Movement Organizations and SMO Fields. The role of organizations in social movement processes has been an abiding concern of scholars of political change and social movements at least since the classic statements of Michels and Weber. And while there has been a massive amount of social science scholarship focused upon organizations, very little of it lately has been done by social movement scholars. There are three questions implicit in our foundational statements that still cry out for more extensive and systematic attention. These are 1) organizational transformation, 2) the extent to and conditions under which organization building facilitates collective action and challengers interests, and 3) the manner in which social movement organizational field dynamics facilitate and shape insurgent challenges. Let me take these in order.

Organizational Transformation. Questions of organizational transformation were initially conceptualized by Weber and Michels as processes of bureaucratization and oligarchization. I am sad to say, however that the Weber/Michels legacy is almost defunct among movement researchers. To the extent these questions are being addressed at all lately, they are conceived rather as ones of professionalization and the institutionalization of social change.³ As I have mentioned, it is widely

² Oliver and Marwell (1992) offer a cogent discussion of the question of the question of fungibility of labor and money in the social movement context.

³ An exception is the work of Debra Minkoff, whose recent paper directly examines the transformation of organizational strategy (1999). In it she describes how national groups representing the interests of women and minority groups have increasingly adopted advocacy as their main strategy.

conceded now that SMOs, at least those with a focus outside of their local communities, are highly likely to be professionalized in two senses. First, SMOs are more and more likely to be what Mayer and I termed professional social movement organizations. And second, SMOs are more and more likely to be staffed by employees hired on the basis of their technical competence rather than their commitment to a cause regardless of whether or not the SMO attempts to recruit an active membership. Suzanne Staggenborg (1988) and Jack Walker and his colleagues (1991) have done some very useful work on this problem, most notably, but to my knowledge there is not an ongoing research conversation around the details of this central social process. Needless to say, I believe there should be one still.

A recent discussion has indirectly touched upon SMO transformation, however, that of whether or not social movement processes have become institutionalized. The focus of much of this discussion is on the routinization of collective action, the inclusion or marginalization of challengers dependent upon their acceptance of institutional arrangements for collective action, and the cooptation of challengers through participation in institutional arrangements. (Meyer and Tarrow, 1998) There seems no doubt that SMOs are central to the story of the institutionalization of protest (McCarthy and McPhail, 1998), but there does not appear to be much effort to systematically investigate their role. In my judgement the institutionalization of social protest in the U.S. is inextricably interwoven with processes of organizational transformation, and associated processes that shape the composition of organizational populations. The process of institutionalization cannot, therefore, be understood in isolation from them.

Do Organizations Matter? Even though we take it for granted that organizations matters, Frances Piven and Dick Cloward have continued to be a thorn in our sides. And, I think, we do benefit from their prodding. If organizations rarely matter, then the justification for studying SMOs is undercut. Consequently, I believe we must continue to think about how to ask and answer the questions they pose. I recall the interchange between Ernie Cortez and Dick Cloward at a recent ASA meeting around the question. Or, should I call it a shouting match? In any case, afterwards I found myself, in conversations with graduate students, struggling to defend my bedrock assumption that organizations can matter. Of course, the problem becomes interesting when we ask which kinds of organizations under what kinds of

social and political conditions. Bill Gamson blazed the trail in attempting to answer these questions in his *Strategy of Social Protest*, and in my judgement is still the best attempt to do so. We do not, however, have an equivalent effort to address them for the post-war period. Given that, in Chick Perrow's phrase, we have become a "society of organizations," that SMOs have also proliferated, and that we know that the dominant form of SMOs has changed, there are very good reasons for thinking that the conditions under which organizations matter may have changed. In my view, this question can only be adequately addressed through empirical mapping of the processes of the social movement sector.

SMO Field Dynamics. We went some way, in our 1977 *American Journal of Sociology* piece, toward specifying the shape and importance of an agenda focused upon what organizational researchers might now call SMO fields (DiMaggio and Powell, 1983; DiMaggio, 1991; Fligstein, 1990). We coined the phrases social movement Industry (SMI) and social movement sector (SMS) to capture what we saw as the most important boundaries of SMO fields, SMOs struggling for change around similar issues, and all of the SMOs in a state. There has been quite a bit of work done on organizational fields by scholars of organizations, but only a moderate amount of work on social movement fields or industries, and even less work done on what we called the social movement sector.⁴ I find this distressing since subsequent organization researcher has only made clearer how single organizations are shaped and constrained and channeled by the dynamics of organizational fields and national organizational sectors.

Last semester I taught a seminar in movements, and made a concerted effort to gather together work based upon systematic descriptions of samples or populations of SMOs within a movement or field as well as across the spectrum of SMOs drawn from all fields. The results were pathetic. Though not a lot, there is some good work on the organizational dynamics of social movement fields. Most of it, however, is restricted to single movements such as the environmental (e.g. Brulle, 1996; Brulle and Caniglia, 1999; Dalton, 1994), the peace (e.g. Edwards and Marullo, 1995), student anti-apartheid (Soule, 1997),

⁴ Our rationale for employing the economic terms was not only to enrage, but also because economists had thought seriously about how to clump firms into industries, and we thought the SMO field problem was a similar one.

nonprofit associations (Knoke, 1990) and the Women's' and ethnic/minority ones (e.g. Minkoff, 1995; 1999). But the coverage is spotty. If there is such work on the pro-life SMI, probably one of the most vigorous of the 1990s, I have not located it. And, as far as I have been able to determine, there does not exist an attempt to provide a census or a sample of SMOs in the U.S. since Gamson's effort. Jackie Smith's (1999) effort to describe the population of transnational SMOs registered with the U.N. is a rare exception. I am happy to report, however, that Deb Minkoff is in the advanced stages of planning a census of U. S. national SMOs.

Conclusion. I have made some strong claims about the recent vigor of various lines of research. Pursuing my preferred agendas would emphasize a very different set of movement processes than those emphasized by either POS or movement participation research pivoting on questions framed by debates about rational choice. We can return to a discussion of the validity of my characterization of research emphases in the field, but now let me proceed with an attempt to account for the lack of vigor of the SMO research agenda.

Why Aren't SM Scholars Going Where We Think They Should Be Going? Accounting for the Intellectual Directions of Research Communities

Of course, great ideas should lead research agendas. But ideas are only a part of the equation. What factors figure in a comprehensive account of which research agendas win and which ones lose. There are a number of factors that must enter into any account of research roads taken and not taken in social science scholarship. My list draws heavily upon the work of Nick Mullins (1980), Ed Shils (xxx) as well as that of Jonathon and Stephen Turner (19xx). The question I address is what explains why groups of scholars choose to work on one or another broad problem or paradigm. Any comprehensive account includes at least the role of each of the following factors: 1) compelling, coherent ideas, 2) a research community buttressed by 3) research sites, 4) a codified research method, 5) easily available sources of systematic evidence, 6) a steady supply research support, 7) a steady supply of young scholars, and 8) broader disciplinary and cultural trends. You will notice that my list of factors lean heavily toward

the availability of resources, both material and infrastructural, since I believe that the mobilization of scholarly groups should not look very different than the mobilization of groups in general.

Notes Toward a Comparative Look at the Problem. I had intended to set up a comparison between the two lines of analysis not taken that I have discussed with the two lines of analysis that, in my judgement, now dominate movement scholarship, POS and studies of individual participation. But my time is brief, so I will limit myself to a sketch the elements important to an account of one analytic road not taken. Let me try to develop a coherent, credible account from my list factors for the lack of attention to the study of SMOs and SMO fields.

Why the Dearth of Research on SMOs and SMO Fields? In some ways accounting for the vigor of this analytic road is the easier task, partly because I know the arena best, and partly because the broader institutional forces are more obvious. One of the most important factors, I think, in making sense of the study of organizations in recent American sociology is the business school dominance of organizational studies. Much of the best scholarly talent interested in organizations is drawn to business schools by the great financial incentives they can offer, and the rewards in those scholarly settings do not, typically, flow to those interested in insurgent organizations. The consequence of this dominance is that we know lots about firms and substantially less about non-profit organizations, including, especially, SMOs.

Compelling Ideas. Interest in organizational fields is hot in organizational analysis, and remains of great interest to movement analysts. One could easily make a case that the sophistication of the study of organizational fields is adequate to support a far more serious effort by movement scholars to address them. As well, a stronger case could be made for the importance of studying SMO fields and the SMO sector than I have made here.

Communities and Sites. Research communities and sites are very important in facilitating work around their intellectual foci. RM social movement analysis has been blessed with several research sites that, each, for a period of time brought together something like a critical mass of scholars. CRSO here in Ann Arbor was probably the most richly endowed in resources and scholars (anchored by Bill Gamson,

Chuck Tilly and Mayer) and the most long lasting. Stony Brook was like a shooting star where a coterie of graduate students coalesced very briefly around Chick Perrow, and his colleagues Michael Schwartz and Lou Coser. Andy McFarland labeled RM the “Vanderbilt School” because Mayer, Tony Oberschall and I overlapped there briefly. But, in fact, I was gone by the time we held the 1977 conference, and Mayer was on his way back home to Michigan. And later, Arizona brought together for a time an impressive, if eclectic, concentration of movement scholars. Each of these sites was very important in concentrating resources and intellectual energies for a while, and each of them spawned students who went on to have distinguished careers as movement scholars. But, none of the sites specialized primarily in organizational research. Probably Catholic University was the closest approximation of an SMO research site for the 1985-95 decade. Several of the students who worked with me over those years at Catholic University have focused heavily upon SMOs, including Jackie Smith, Bob Edwards, John Crist and Ron Pagnucco.⁵ Many of the younger SMO scholars are, essentially self-taught, having been trained in programs with rather little strength in movement analysis. (e.g. Minkoff, trained at Harvard, and Staggenborg, trained at Northwestern)

Codified Research Method. Protest event research, as I have mentioned, has become increasingly codified. The major advantage of a codified research method, such as survey research is the fact that it lowers the entry costs of new scholars (take that Chick!) into a research arena. Abraham Kaplan’s oft quoted passage about giving a small boy a hammer, captures the essence of the process. “Give a small boy a hammer and he will find something to pound. Give a social scientist a new method and (s)he will find something to study.” We are not even close to having a well-codified method for studying SMOs. Bill Gamson offered a template in his *Strategy of Social Protest*, but one that has not been emulated, as far as I know. And, a number of scholars have relied on the *Encyclopedia of Associations*, creating something like a standard method for sampling its entries and coding their contents. Jack Walker provided

⁵ Bob Edwards has mentioned to me that his analyses of recent citation patterns reaffirm how narrow the circle of research scholars is that make SMOs central to their work.

the closest thing to a template for studying SMOs in the modern period, but his sampling frame was too narrow. Deb Minkoff's project mirrors the strengths of Walker's efforts. I should emphasize that codified methods build in theoretical assumptions and foci. Methods dependent upon surveys of individuals implicitly disadvantage organizational accounts of social processes.

Available Data Bases. Probably, quality databases that are widely available only are developed after the codification of a research method. In any case, when a database is available, young boys (and girls) have something to hammer. The development of a vigorous research community can only be enhanced by the existence of a comprehensive, and, ideally, regularly updated, data base. Think of the way in which the GSS has nourished a number of research communities in our discipline. Even more obvious is the role of the PSID in creating a community of research scholars who study issues of the dynamics of poverty in the U.S.

For the study of SMOs, we have had either poorer quality existing data sets (such as the *Encyclopedia of Associations*) or gargantuan, unrepeatable, data collection efforts, such as the earlier ones by Gamson and Walker, and the more recent ones of Edwards and his colleagues, and of Smith. The triumph in movement analysis has been the widespread application of the method of protest event data collection and the, consequent, proliferation of protest event data sets. But such data is not very helpful in studying organizational populations or processes, and turns our attention to other theoretical problems.

Research Support. The ability of a research community to develop and codify a credible research method and to launch successful database creation projects depends upon the availability of funding sources sympathetic to their ideas. No doubt, these are reciprocal processes, in that to the extent that ideas matched with codified methods empower research communities to successfully solicit funding. To my knowledge there are only two sources of research funding that have provided much in the way of support to SMO research. These are NSF and the Aspen Nonprofit Sector Research Fund. NSF is difficult money in that it comes in small portions, it is highly competitive, and highly codified methodologies and associated data sets are advantaged in the funding process. This has meant that, while

some SMO research has been funded over the last decade or so, proposals to collect SMO data have not fared well as far as I have been able to determine.⁶

The other source of funding for SMO studies has been the Aspen Nonprofit Sector Research Fund. This funding agency is a creature of the elite of the non-profit organization world, and aims to raise up the role of non-profit organizations. Luckily, SMOs are non-profit organizations. Unfortunately, the funder has the wrong motivations for funding studies of SMOs. Nevertheless, a number of our younger scholars (e.g. Minkoff and Smith) have been successful in getting support for their work from this agency. I believe that this success is, partly, the result of the fact that there is not a very well organized non-profit organization research community. As a consequence, our halting efforts at codification shine.

Young Scholar Flows. Before WWII movement scholarship in the U.S. was dominated by academics that had little direct experience with movements. After the 1960s, movement scholarship has been dominated by academics with previous movement experience complimented by scholars currently active in movements both within and outside of the academy. Each new cohort of enthusiasts has come to movement scholarship with different movement experiences and different intellectual agendas. I have found that movement organizers outside of the academy are sympathetic to the SMO research agenda, but that new academic cohorts drawn to the study of social movements through the extent to which they embody lived movement identities have, typically, not been so sympathetic. My limited data suggests, as well, that most of them have rarely had experience organizing in the trenches.

Broader Disciplinary and Cultural Trends. There is no question in my mind that the cultural turn in the discipline has had the consequence of diverting attention from SMO analysis. This is not necessarily the case, as Bill Gamson has shown us. As well, Lis Clemens has pointed the way for SMO analysts who wish to bring culture into their analyses. Nevertheless, the turn to cultural analysis has, in

⁶ Of course, my experience is limited. I have been quite successful in gaining NSF funding, but I know a number of SMO scholars who have not been so successful with proposals I have judged excellent, and my term on the NSF Sociology Panel confirmed my impressions.

general, disadvantaged structural analysis. As well, the unrelenting trend toward individualism, and individualistic interpretations of social life, that John Meyer and his colleagues have chronicled, shapes the broader demand for interpretations of social processes. In such a milieu, it is maybe a miracle that scholars pursuing SMO research can succeed.

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