The Complex Politics of Relevance in Geography

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As a discipline, geographers have debated what it means to make research relevant. In this article, we argue that the issue of what makes research relevant cannot be separated from the questions of *why* research should be relevant, *how* research becomes relevant, the *goals* of research, and *for whom* it is intended to be relevant. In this sense, the determination of relevance is a social and political process. We make this point through an evaluation of various writings on relevance, editorials that have appeared in the *Newsletter* of the Association of American Geographers, and through interviews with researchers. We argue that relevance can be intended, but that commitment to relevant research requires a long-term view and an appreciation for the indirect pathways of relevance. *Key Words: relevance, knowledge construction, social studies of science, ethics.*

What makes research relevant? How do we know relevant research when we see it? While these are empirical and practical questions, they are also clearly—and perhaps primarily—*political* questions. The issue of what makes research relevant cannot be easily separated from the question of for whom it is (or is meant to be) relevant, and the issue of how relevance is recognized cannot be separated from the person making the evaluation. While researchers may intend that their research be relevant in certain ways, that it may be picked up and used for particular ends, they do not by themselves determine whether their research will be relevant to some particular cause or constituency, some policy or problem, or instead consigned to irrelevancy. Rather, later users of research—other scholars, students, lay people, politicians and bureaucrats, and workers in near and distant fields—decide what is and is not valuable to their own projects, needs, and questions. And what is available to be used is a function, inter alia, of structures and institutions in the academy, in publishing and the media, and in funding organizations. The determination of relevance, in other words, is a social as well as a political process.

While calls for geographers to strive for greater relevance in their research are common, it is far less common for these calls to be framed within explicit discussions of either the politics of relevance or of the social practices that condition relevance. The purpose of this article, therefore, is to focus explicitly on the politics of relevance. Our basic position is that research is not born relevant; it is made relevant in practice and for particular ends. Any consideration of the sociology and politics of relevance, therefore, requires consideration of *why* research should be relevant, *how* research becomes relevant, the *goals* of research (including political goals), and the *intended audiences and beneficiaries* of research. Each of these is a vexed issue; each is a locus of considerable debate as researchers with differing intellectual, social, and political agendas contend over what is right and what is best. Yet, in important ways, attention to these issues is muted in much of the published work on relevance. This may happen because writers take the political nature of relevance as axiomatic and therefore not necessary to discuss. It may also be that authors are not concerned with *which* politics or goals are mobilized; they just want more engagement. It may also be the case that authors assume a particular kind of relevance, but do not articulate it.

As we shall argue, many narratives about relevance circulate through the discipline; they can be found in the presidential addresses of the AAG, in editorials, published articles, and conversations with colleagues and in classes. We attempt to bring these ideas into dialogue and, in so doing, hope to democratize discussions through inclusion of a broad range of definitions, perspectives, and political goals. Our conclusion may seem paradoxical: we argue that relevance may best be thought of as an attitude and a set of practices intended to produce research that will be useful in some way—large or small, in public or private venues, or even in changing ways of thinking or conceptualizing issues—but that may not be manifest in any particular policy or outcome. From this perspective, relevance is not easily measured, and it may not be directly observable. While

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this perspective may be at odds with the performance- or productivity-based outcomes that increasingly dominate evaluations of research, we argue that our approach recognizes the ineluctably political nature of relevance and the diverse goals that we, as a community of scholars, promote. Our approach also highlights the role of academics as educators of students, of policy makers, of the public, but it is a particular view of education that involves listening, critical analysis, and capacity building, as much as recommending particular actions.

Debates over relevance are at a critical point, as funding agencies want to see the societal merit or benefit of research (e.g., the National Science Foundation), as donors want to see tangible outcomes from their contributions, and as public universities face criticism for being removed from the public they are intended to serve. But unless we confront different ideas about relevance and the politics embedded within them, these debates are likely to be unproductive and to be largely irrelevant to academic practice. Our goal in this article is to democratize the debate about relevance in ways that bring voices and concerns not often aired in formal settings into dialogue with voices from the institutions. In so doing, we hope to provide a foundation for continuing, broadly based discussions of relevance in the discipline.

The article is organized in four sections. In the first, we provide a short overview of debates over relevance within the discipline, focusing on the ways in which ideas about relevance have often been framed within different approaches to research. We argue that many of the recent debates about relevance have pitted the "-isms" against each other, for instance, implying that pragmatism may be more likely to yield relevant research than Marxism, feminism, or poststructuralism. These debates sometimes seem to equate the method or theory of research with the politics of research, but they ultimately provide little leverage in understanding different ideas or definitions of relevance and the politics underlying them. In the second section of the article, we present our theoretical and methodological approach to analyzing relevance. The third section presents a framework for understanding narratives of relevance based on an analysis of the published debate over relevance in the 1990s and on interviews with researchers. While there are many points of convergence between the published discussions about relevance and our interviews, there are also some important tensions. In the final section, we focus on these convergences and tensions in the hope of providing a foundation for continuing, broadly based discussions of relevance in the discipline.

Relevance and the "–isms" in Geography

Discussions about relevance in geography are not new, as they have been a part of the modern discipline since its founding in the second half of the nineteenth century. More recently, intense debates over the relevance of the discipline and specific research streams within it erupted in English-language geography at the end of the 1960s, as numerous scholars sought to tie the discipline more closely to the politics of the period and to find solutions for the numerous social, political, and environmental problems these politics exposed.[1] For many, the highly quantitative, positivist-oriented research of the previous decade (as codified, e.g., in Harvey 1969) was inadequate to address the social problems that seemed so deep and pressing, despite the best intentions of practitioners (Yeates 2001). The putatively "value-free" orientation of the research masked, many thought, a complicity with the social forces at the root of inequality, racism, and the alienation that marked modern life (see, e.g., Blaut 1969; Morrill 1969; Peet 1969; Stea 1969; Prince 1971; Zelinsky 1975; Mitchell and Draper 1982; Mercer 1984). This sense of complicity turned many geographers in two, not necessarily unrelated, directions: toward more humanist modes of knowing rooted in moral philosophy and toward radical theories and politics rooted in anarchism, Marxism, and other critical movements. One of the most interesting aspects of the relevance debates of the late 1960s and early 1970s was how charges of irrelevancy were directed at a mode of knowing that was positivist and statistical. The movement from attempts to establish a more scientific footing for geography, to the questioning of the values implicit in this science, to the humanist and radical movements of the 1970s led to what Sayer and Storper (1997) have called a "normative turn" in geography (see also Kobayashi and Proctor 2003).

Concern with the normative—with what *should be*—implies concern about intentions. For many geographers who were not much taken with either the humanist or radical movements, this concern implied not abandoning the positivist, quantitative, skills-based geography, but, instead, putting it more directly in the service of society. So, at the same moment that humanist and radical theories were being imported into geography, efforts were also undertaken to better facilitate direct involvement by geographers in the solving of social problems (see, e.g., Berry 1972; Harvey 1972, 1974; White 1972; Trewartha 1973). A critically important aspect of this desire to be more closely involved in social problems has been a focus on the development of technical knowledges and skills that could be put in the service of public policy or that

could be pragmatically useful to business (cf. Abler 1993). Often labeled "applied geography" (because it is geography as applied to specific practical problems), this pragmatic orientation to making research relevant has been charged (even by its proponents, e.g., Kenzer 1989, 1992) with being atheoretical or ad hoc rather than systematic; it is said, therefore, to run the risk of complicity in projects that undermine its own normative values. But the point here is that applied geography focused on specific policy and business needs has developed out of a desire among geographers to be relevant. But because the *means* of relevancy of applied geography differ from those of radical and humanist geography, applied geography promoted a notion of relevance that is in some ways sharply at odds with other ways of conceptualizing relevance.

Humanist and radical scholars were also deeply concerned about immediate social problems, but they sought to understand the root causes that gave rise to them—the political, economic, and environmental contexts within which they arose, and the deep implications that they had for people's lives. For both sets of scholars, values were absolutely critical (Harvey 1973; Buttimer 1974; see Kobayashi and Mackenzie 1989). This meant asking "big questions" about geopolitics, ecological transformation, economic restructuring, human welfare, war and peace. For radicals like David Harvey (1984, 10), such big questions implied developing historical-materialist "people's geographies" "that open[ed] the way to the creation of new forms of society in which common people have the power to create their own geography and history in the image of liberty and mutual respect of opposed interests." For humanists like Anne Buttimer (1976) or Yi-Fu Tuan (1976), "big questions" entailed exploring the nature of differing people's "life-worlds" (see also Seamon 1979).

By the early 1980s, Janice Monk and Susan Hanson (1982) were calling on geographers to "not exclud[e] half of the human in human geography," as feminists showed that any analysis was relevant only to the degree that it took difference to heart; they argued that both social theory and social policy must recognize that differences in identity matter seriously and incorporate this understanding into research practices. Similarly, the later rise of geographies of sexuality reinforced the point that identity was a crucial, ineluctable part of geography (e.g., Bell and Valentine 1995). Antiracist scholars have made a similar claim with regard to race and ethnicity (e.g., Kobayashi 1994).

The rise of various forms of radicalism, humanism, and feminism in geography, and their mutual interaction, led to a period of intense development of social theory in the discipline. In a generation, geographers recapitulated a century or more of social theoretical argument and development that had largely passed the discipline by. Much of this debate has been difficult, and the language in which it has been conducted necessarily reflected that. And herein lies a central irony of human geography's turn to social theory: while rapid theoretical development has perhaps drawn geography closer to the heart of main debates in the social sciences and humanities, it may have also drawn it further from the social movements, political formations, policy makers, and lay people many of us hope to reach (see Kitchen and Hubbard 1999; Castree 2000; but see Pain 2003 for a more positive assessment). And so the debate about how to make research relevant continues, even within groups of geographers adhering to a common approach to research.

Approaching relevance through the lens of paradigms or "the -isms" has led to a sense that some theoretical and methodological approaches—say, pragmatism as compared to postmodernism—may be more conducive to relevant research than others. Such a conclusion, however, diverts attention from the nature of relevance itself and is often associated with an outlook that puts relevance in opposition to theory. As Bruce Mitchell and Diane Draper (1982, 2) argue, the desire for greater relevance discourages geographers from "confining themselves primarily to theoretical problems" and instead encourages them to commit themselves "to resolving societal problems." In turn, this leads "more geographers to become involved as professional consultants, offering their services to clients whether in the public or private sector." The authors assert, however, that significant ethical and values questions arise when scholars concern themselves with "practical significance, advocacy and consulting" (p. 3), and their book is dedicated to exploring these conflicts. They argue that the desire to resolve societal problems only compounds the riddle of what constitutes relevance, since resolving societal problems requires both practical interventions—as advocates and consultants—and the reformulation of the problems themselves—the more traditional scholarly role. We need, then, a way to talk about relevance that avoids the dualism between theory and practice and that eschews the temptation to imply that some research traditions are less amenable to relevance than others.

Theoretical and Methodological Approach

We take it as axiomatic that scholarly research is a social enterprise (Bloor 1976; Law 1986; Pickering 1992; Demeritt 2000). A key argument from the sociology of

scientific knowledge and from feminist studies of science is that the boundary between what is studied and the social context within which it is studied is not fixed (Collins 1982; Shapin and Schaffer 1985; Harding 1991; Livingstone 1992; Shapin 1992). Rather, the delineation of a research area, the development of theory, and the findings produced interact with the social context in which the research is conducted. Scholarly practices themselves are deeply implicated in larger social processes and forces that are critically important to the production of knowledge (Harding 1991; Katz 1992; Kobayashi 1994; Demeritt 2000). How people do their scholarly work, why they do it, and how others do and do not join in their practices (by picking up and transforming their ideas, by drawing on them in their own research practices) shape the knowledge we produce, if not the actual physical or social processes. In this sense, the production of knowledge is reflexive (Woolgar 1988; Ashmore 1989; Katz 1994; Kobayashi 1994; Nast 1994; Latour 1996). But, equally, so is the *consumption* of knowledge (Hannah and Strohmayer 2001).

In these terms, the issue of relevance may be defined—as it often is—as one of understanding the ways that people do and do not use scholarly knowledge.[2] The important point here, however, is that how people do and do not use research is complex, wrapped up in myriad practices and social forces; it is not just an effect of the research itself (e.g., good ideas will win out over bad) or how it was presented (e.g., good writing will attract adherents). To put it another way, what makes research relevant is shaped by the social context in which research is presented, interpreted, and used.

Yet at the same time, research is a response to—and is produced out of—diverse social pressures. In the most general sense, these social pressures might be understood as issues facing a society (e.g., White 1972), but the pressures also include the norms or values of scholarly communities, the teaching needs of departments, the reward structure of the academy, the priorities of external grant agencies and corporate sponsors, the press of political events, and the ongoing restructuring of the publishing business (Castree and Sparke 2000). The pressure from each kind of audience may generate different and often competing definitions of relevance, and each influences how research is done and what research means. The myriad structures of influence that shape the traffic in knowledge are not easily swept aside. Research cannot thus become "more relevant" through sheer force of will—especially if we are not crystal clear about for *whom* it will be relevant. Thus, relevance involves exceedingly complex questions of *whose* relevance and *whose* geography.

To evaluate how relevance is understood in geography and to explore the social and political practices that make research relevant, we have adopted a two-pronged approach. First, we analyzed published calls for and discussions of relevance. These included disciplinary statements such as presidential columns in the AAG *Newsletter* and the prominent National Research Council (NRC) (1997) report, *Rediscovering Geography: New Relevance for Science and Society*, as well as key books and journal special issues that explore the topic in some, often critical, depth (e.g., Mitchell and Draper 1982; Pacione1999a; Castree and Sparke 2000). These published statements may represent only a portion of a broader discourse of relevance, however, so it is important to bring in a different set of voices—voices without, perhaps, the legitimacy or credentials of the first set.

The second element of our approach, then, involves interviews with people who are not part of the public discourse but who, nevertheless, have something to offer the discussion by virtue of their participation in the discipline. As part of a larger project exploring the sociology of knowledge with regard to a single research area, we asked twenty-five geographers engaged in public space research in the U.S. to reflect on issues of relevance, its practice, and its politics.[3] We then attempt to create a dialogue around different conceptualizations of relevance that emerge from the literature and from the interviews. In so doing, we take a grounded approach in order to recast the terms of the relevance debate in geography.

Some concerns related to the interviews, however, need to be addressed. First, the use of interviews such as these to assess issues related to relevance may be open to the criticism that it allows self-serving statements or is not objective. While such may be the case, that possibility is balanced against the fact that our interviews with scholars allowed us to explore aspects of relevance that might be hidden in the publication process and in the sorts of normative statements that constitute the public discourses. For example, the review process may lead authors to shape their arguments in particular ways that may or may not be entirely reflective of the author's intent (Johnston 1991). Similarly, some have argued that the reward structures within the academy often work against advertising the fact that scholars are involved in outreach activities (e.g., Sabin 2002), and interviews allowed us to explore whether respondents presented their research in settings or in formats intended for nonacademic audiences. And, finally, through the interviews, it became clear that it was important to explore the importance of *teaching* for the sociology and politics of relevance, which, as we will show more fully below,

turns out to be a critical, and underdiscussed, foundation of relevance.

A second, perhaps more serious, concern is that in the context of the current article—which purports, by its title, to examine the politics and sociology of relevance for *all* of geography—interviews with a group of researchers selected as part of a wider project on a specific research area (public space) might seem idiosyncratic. That limitation noted, there is considerable diversity of theoretical, methodological, and empirical approaches brought to studies of public space, which means that research in this area is emblematic of the eclecticism of geography in important ways. Furthermore, by interviewing a more focused sample of researchers, we were able to ask about relevance *in relation to* a specific research object. That is, we were able to ask a diversity of scholars about how different kinds of research on the same topic did or did not (or could or could not) become relevant. Finally, because public space researchers examine a topic that is also the focus of intense political, policy, and citizen debate, they often have had to think carefully about the social value of their research; in this, they are no different than other geographers. We want to be clear, however, that while our interviews are with public space researchers, we do not focus on public space.

Our purpose in combining an analysis of published statements, on the one hand, with interviews with members of a research community, on the other, is not at all to suggest that these two discourses are always oppositional. Indeed, as will be seen, we have found a much more interesting and complex dynamic at work. Quite often, the views of disciplinary leaders, as they made normative statements about the need for more relevant research, converged with the views of researchers engaged in specific projects. At other times, striking dissonances arose. At still other times, the lines of reasoning seemed to move in entirely separate, even unrelated, directions. The two sets of material we analyze, in other words, work together like counterpoint in a musical composition. By developing two separate sources of information, we have been able to compose a more varied and complex view of the sociology and politics of relevance than a focus on only one of the sources would have allowed.

Aspects of Relevance

From our reading of the literature and the interviews, several ways of thinking about relevance—about aspects of relevance—emerged. Three of these are neatly described by Michael Dear in a 1999 article in the *Scottish Geographical Magazine*: relevance as pertinence, as commitment, and as application. We use his framework as an entrée to the issue because it matches the themes that emerged from the published articles and interviews. But our analysis also suggests two further aspects of relevance circulating in the debate: relevance as intellectual centrality and relevance through teaching. Each aspect of relevance is hotly contested, as authors and interviewees describe what relevance means to them and, perhaps, how these ideas should be taken up by the discipline.

One of the most significant distinctions between these elements of relevance is in where relevance is located: in the issue examined, in the act of research being used, or in the mobilization of core values. As we shall argue, these are not simply different conceptualizations of relevance; they reflect particular political and social positions in the debate over what constitutes relevance and for whom research should be relevant. In the following pages, we examine the ways in which authors and interviewees talked about relevance and the issues that confront researchers as they attempt to make their work more relevant. We make no effort to provide a final, conclusive statement about what relevance "really" is, but instead highlight the point and counterpoint in the dialogue, the debate and dilemmas in the narratives about relevance.

Relevance as Pertinence

Dear (1999, 144) defines "pertinence" (or "timeliness") as when some issue "has significance (however defined) for a particular time and place; by extension, exactly the same issue could be irrelevant at another time and place." Making research pertinent, then, involves the identification of important issues and making interventions in them—making interventions that are "appropriate, suitable in nature or character," as standard dictionaries define pertinence. But it also means ensuring that such interventions are *timely*. In the president's columns of the AAG *Newsletter*, it is almost taken for granted that geographers can and should be pertinent in this dual sense. The integrative nature of the discipline (e.g., Cutter 2000) in combination with powerful analytical tools provided through geographic information technologies (e.g., Golledge 1999a) mean that geographers are well poised to react quickly and appropriately to developing events.

In published statements on relevance, pertinence is often associated with the "big issues" of the day. Gilbert White's (1972) call for more relevant research began with an inventory of crises: the widening gap between

rich and poor countries, the carrying capacity of places and the world as a whole, environmental degradation, racial tensions, social fragmentation, the decay of cities, and urban sprawl. Similarly, the NRC's *Rediscovering Geography* (1997) report identifies looming problems that geographers can address including economic and environmental sustainability, ethnic conflict, health care, and climate change (see also Brown 1997). Over the past several years AAG presidents have reiterated many of the same issues—and added hunger and war to them. These are critical issues, they argue, that must be addressed for a livable and fair world; the implication (and sometimes the direct statement) is that geographers should address them if our work is to be relevant.

The researchers with whom we spoke also see pertinence as an important element of relevance; nineteen of twenty-five researchers mentioned this issue in one form or another. But in addition to looming social and global issues, our respondents often discussed pertinence in terms of the issues confronting people in their everyday lives and in terms of helping people to understand these problems, if not solve them.[4] For these scholars, pertinence was expressed as a desire to use research on contemporary social issues as a way of helping people think about their world—a fairly different sensibility than using such research to directly solve problems. Meghan Cope, for example, spoke in her interview of the desire to conduct research that touches people's lives, arguing that such research would help people better understand the society in which they live. To do so, Cope and several other respondents (e.g., John Paul Jones, III, Sallie Marston, Edward Soja) discussed the importance of theory in showing how daily experiences are connected to broad structural issues. Research is pertinent when it helps people draw those connections to make the "everyday" a "big" issue. Significantly, it is not just researchers who work from a critical social theory perspective who talked about pertinence in this way. For example, Richard Frankaviglia said,

> I think what makes research relevant is just that it has potential applicability. In my particular research, if it helps them plumb their deeper collective unconsciousness, great. If they can understand that they're part of a broader process that's going on, it is relevant in that regard.

But pertinence is tricky. Many of the people with whom we spoke mentioned the difficulty of identifying issues that would allow people to draw connections between broad social structures and daily life. Good, clear writing with lots of graphics or images was seen as important in this regard, but so was the question of the *kinds* of issues that research addresses. Larry Ford suggested that focusing on "consensus issues" was a means to assure relevance. He seemed to define consensus issues as those that came from people using a space rather than from a predetermined theoretical question. Identifying those issues sometimes may involve a bit of luck or a special knack for finding compelling issues, as Roman Cybriwsky noted. In a similar vein, Jennifer Wolch believed this often involves conceptualization of an issue that is

> just beneath the level of the existing public agenda, or needs to be brought onto the public agenda. And it is framed in a way that allows people to understand how the results might actually affect day-to-day practice.

The sense of relevance as pertinence expressed by Cybriwsky and Wolch is in some ways different than that espoused in the published discourse. For Cybriwsky and Wolch, pertinence consists in bringing *new* issues to the table. In the published discourse, pertinence is more often defined in terms of responding to already existing issues and debates, making geographers' voices heard in agendas that already exist, a position that is shared to some extent by Ford. But there is also a difference between the sort of "consensus issue" pertinence advocated by Ford and that indicated in presidential columns and publications such as *Rediscovering Geography*. For the former, consensus issues arise from the street, as it were, and are matters of social concern, big or small. For the latter, specific "big" problems—the mechanics of global warming or the rise of financial networks that fund terrorism, for example—deserve geographers' attention.

But all agree that *timeliness* is central to pertinence, as Dear's (1999, 144) definition made clear. And a persistent fear is that geographers are either too slow in responding to the rise of important issues, or that they somehow fail to identify them. But timeliness is also difficult. Thinking about policy work, Mitchell and Draper (1982) argue that the time horizon for policy makers is often relatively short and emphasizes immediate answers to pressing problems; such a horizon focuses on short-term analyses and solutions in which theory and a longer perspective may seem irrelevant, at least when relevance is defined in terms of pertinence. Yet they argue that understanding the processes that give rise to pressing problems often requires a longer-term view and an articulated theoretical perspective. Balancing the two sets of needs—immediate responses to immediate problems and longer-term analyses of broad social processes—is difficult, but necessary, if

research is to be pertinent. This balance may require different kinds of analyses, not all of which may be represented in policy documents, but each of which is critical to understanding and evaluating the effectiveness of potential responses.

The preceding discussion suggests two important conclusions about relevance as pertinence. First, "pertinence" is itself a slippery term; it can mean a variety of things, ranging from intervening so as to change intellectual and political debates, to contributing to those debates, to contesting them. Those calling for greater pertinence of geographical research need to be cognizant of this. Second, and more importantly, while pertinence can be intended, it cannot really be predicted. Attempts to do so rely on a model that both inflates scholars' power to intervene in societal movements and underestimates the complex pathways by which disciplinary interests, concerns, and modes of analysis develop. Pertinence is determined post facto by the users of research. There is an important moral to be taken from this: calls for "easy" pertinence can run the danger of narrowing the scholarly enterprise and thus limiting the range of work that may later have the possibility of being made pertinent. What we can hope for is that researchers look for venues through which their work might become pertinent or that they might attempt to bring their work to venues in which it might become pertinent; in short, that they bring an attitude to their work that is open to making their work pertinent.

Relevance as Commitment

Agenda setting, contributing to consensus issues, and contributing directly to the solution of some specific problem all require the making of *commitments* on the part of scholars, if for no other reason than finite time requires committing to one course of scholarship rather than another. But such commitments are usually not only commitments of convenience or even of scholarly sensibility; they also entail making a *political* commitment or a commitment to some sort of action (Pain 2003). Relevance as commitment speaks to the motivations for and goals of a research program. Dear (1999, 145) relates this way of thinking about relevance to the promotion of liberal and leftist political and research agendas in the 1970s that were focused on the reduction of inequality and the promotion of social justice. In those cases, the motivations for research reflected a commitment to working for a more just world. Much of the work in the pragmatist tradition—which is more wary of the overtly political agenda of social justice research—also originates a strong sense of commitment. Gilbert White and Bob Kates, for example, are motivated by a profound desire to improve chances for human survival and, beyond that, for improved lives (White 1972; Kates 1994; see also Smith 1984). For some researchers, the source of commitment is a moral framework rooted in spiritual beliefs (Pacione 1999b); for others, the moral framework is rooted in politics. As such, commitment is laden with normative values, even if those values do not map easily onto a political spectrum of right and left. Commitment of this kind is usually commitment to some sort of "project" that, in turn, can be decisive in how research is conducted. Susan Hanson (1999), for example, argues that commitment to feminist values leads directly to feminist research practices.

Such commitment is not exclusive to feminists or to researchers who directly refer to their politics in their work. Rather, as Mitchell and Draper (1982) argue, all research ultimately rests on values, whether articulated or not. And, in fact, the values and commitments that provide the foundation for research in geography often are not articulated, even when people are provided a forum for addressing the discipline on the topic of how it should be developing. The AAG presidency, for example, provides prominent scholars with a "bully pulpit," and many presidents use their *Newsletter* column to promote their vision of what geography should contribute to society. While one gets a general sense of commitment in some columns, it is not always easy to identify the nature of the normative visions—or values—that shape commitment, to identify the object of commitment (commitment, after all, requires that there be some thing or idea to which one is committed), or to trace the influence of commitment on research practices.

There are several reasons why this might be the case. First, not everyone feels comfortable issuing calls to the barricades, perhaps preferring other means to rally commitment. Second, relevance as commitment involves normative judgments as to whether an agenda is important, worthy, and/or scientific. Ideas of an agenda or a normative vision of what society should be like are something that many in the academy seem wary of addressing individually, never mind for the discipline as a whole. Presidents of the discipline may feel it is inappropriate or ineffective to imprint their own values and judgments by setting out an agenda or project, so speaking from a bully pulpit to a discipline can be a difficult position within which to operate, raising intractable questions. How can a geographer act on her/his own sense of commitment when also speaking for a diverse discipline? And what role should politics and moral values play in research initiatives within a discipline and in organized efforts to act upon them? Because

it is rooted, however ambiguously, in politics, goals, and agendas, commitment complicates the evaluation of research, even as a commitment to, for example, some form "objectivity," may limit the degrees of freedom in any research project.

By contrast, the researchers we interviewed frequently situate their research within the context of specific political commitments and values. The following exchange with Robert Tata is typical:

> LAS: Why do you think [public space] is an important topic?
> RT: Well, it kind of gets back to some basic philosophy of my own. Mostly, I would consider myself a conservative, but on the other hand, I believe in government support of education and legal systems and also the health system. I always have felt that government should provide at least a minimum standard of living for every citizen, and there are certain groups within society that, for whatever reason, can't keep up and get the benefits the entire society has to offer. It's government's responsibility to provide those things if we're going to have a just society.

Commitment here is to a moral and political philosophy of social justice, and research is directed both toward conforming to that commitment and toward helping to realize the values that lie at its root. These values can be deep-seated, and they can take a variety of forms, including a political project, a set of values, or a particular outcome.

We assume that the views about commitment held by the various participants in published debates are likewise complex. But what is curious is how little this sense of the complexity of commitment comes through in published narratives. Relevance as commitment requires a commitment *to* some object, some set of values, some political outcome—or some combination of these. Any call for relevance must confront this aspect of relevance, if for no other reason than because a commitment *to* is a necessary precondition for even beginning to debate relevance as pertinence.

Relevance as Application

Relevance as application is probably the most common way of conceptualizing relevance. The preponderance of *Rediscovering Geography* (NRC 1997), for example, is directed toward this aspect. For several AAG presidents, the importance of relevance as application follows from a concern for pertinence. And ten of the twenty-five researchers we interviewed said that, by definition, relevant research had to result in application or action of some kind. One researcher, who requested anonymity, made this argument even as he critiqued it:

> Relevant research has to result in some kind of action. It has to affect behavior in some way. Otherwise it remains intellectual, which isn't bad. I don't know if anything I've done is relevant, and I am not sure that relevance is highly important to me. But if I am going to define it, that is the way I would define it. I do think it probably is normative because it sets certain conventions about behavior.

As in the published discourse, some of our interviewees saw direct connections between relevance as pertinence and relevance as application. For Larry Ford, orienting his research questions around people's use of space (which gives it pertinence) means that his research can be of immediate use for architects and planners—as long as he communicates the information clearly. The same is true for David Seamon, who argued that

> the first criterion for relevant research is practical usability. In the case of my work, helping to facilitate actual place-making through design.

Seamon's argument is significant not just for the linkage he makes between pertinence and application, but also because he works from a phenomenological perspective. Phenomenology is one of those approaches—at least as it developed in geography—that has been indicted as being either too individualistic or too philosophically abstract to incorporate directly into policy or planning. Moreover, phenomenology is in some ways hostile to the sort of technical rationality that planning discourse often requires (see, e.g., Relph 1970, 1981), and that is often taken as the sine qua non of applicable research. Seamon, however, makes the connection between phenomenological approaches and design applications easily.

Even so, relevance as application is not something that flows organically out of research; if it were, all research would have a fairly direct application. Indeed, addressing a perceived paucity of applicable research, AAG presidents and others have called for changes in the ways that we, as geographers, ask questions, train students, and market ourselves. Will Graf may well have been the most persistent of recent presidents in his calls for applicable research, and he devoted a considerable number of his presidential columns to explaining how this might be achieved. Among other things, Graf argues that applicable research may require that research questions be modified to reflect concerns beyond specializations within the discipline:

> We need to ask the right questions, questions that will generate answers useful to us as geographers, but also useful to other natural and social scientists, humanists, decision makers, and managers. . ..We need to generate questions susceptible to geographic inquiry that will produce answers needed by others outside the narrow confines of our discipline.
>
> —(Graf 1998a, 2)

Graf recognizes the tension between "what we want to do and what is needed," but he argues that it is a waste of intellectual capital to invest in research that cannot be directly linked to the needs of those who can use some element of the research; research should have a direct instrumentality. Graf also warns that it will take effort on the part of geographers to ensure that the investment of intellectual capital is made. He argues that geographers must approach policy makers, rather than wait for policy makers to approach us. Furthermore, geographers need to be sure to have something to offer that policy makers can use (Graf 1998b). In a similar fashion, Golledge (2000b) argues that we must ask whether the research we produce and the students we train have relevance for the needs of business.

In thinking about the limitations of geography's potential for application, many articles and respondents pointed to the theoretical turn in the discipline. The sharpest concerns have been that high theory can mask fuzzy thinking or sloppy research, but other comments suggest that the business of theorizing distracts from the business of solving problems; policy makers, it is argued, do not concern themselves with theory. In his interview, however, Michael Dear argued that downplaying theory does not mean abandoning it. Rather, he spoke of the need to bracket a theoretical framework when addressing different audiences:

> On a day-by-day basis, if I went out and spoke to a bunch of developers on the west side about how important it is for them to understand the looming hybridity of spaces around them, they would not listen to me. If I went out to the East LA barrio and tried to explain how their postmodern hybrid concoctions are really interesting to me, they would just smile and go away, and that would be the end of that. I think, however, that my whole approach to community issues is determined by my theoretical apparatus. . ..The theoretical way I see things colors exactly the way I am going to do my practice. . ..They seem reasonably seamless to me in that the way I am going to pursue my research is very much bound up by the way I see the theory that I use to form the way I see things. . ..But on a day-by-day basis, I do effectively bracket certain aspects of that understanding so that it doesn't get in the way of effective communication.

Dear's point is that theoretical work is necessary to the development of research that can be applied in particular social settings or to particular problems, but that the means of communication—the modes of address—will necessarily be different, depending on the needs of different audiences.

Changing the mode of address, however, is not easy, given the structure of the academy. Several untenured faculty (most of whom requested confidentiality) commented that the tenure system seems to prioritize theoretical development and publication in flagship journals over research that is conveyed to a general audience of nonacademics or a specific audience of policy makers, activists, or others who can use the research. One person, for example, put it this way:

> Every publication has an audience, especially the mainstream press. There are thinking people, working class and educated people. . .other people who haven't got college degrees, and who are out there with shitty jobs. But they do read things. They do read newspapers and they read magazines. A lot depends on how you calibrate your argument for your audience. . ..To get tenure, you need to publish in the right places. You have to publish in the *Annals*. The *Annals* is a decent journal, but the discipline has a real story to tell, and it might be good to get that out there, as well. I publish stuff in [popular progressive magazines], none of which means a damn in academic thinking. I don't have tenure, and it's not going to help my case.

This scholar's concern was that publishing in journals such as the *Annals* would not have the impact that publishing in mass-market magazines would have, but that by writing in these more popular outlets, he would be deemed less of a scholar. Other researchers noted that they also write differently for audiences of policy makers or when making public presentations, but commented that few in the university seemed to recognize their work in these venues; this was frequently mentioned by those who work with community groups or in arenas that are less visible than those of the policy world at the national level (see also Gober 1998). Developing applicable research, that is, may in fact be militated against by the very structure of rewards—including current tenure standards—that govern our jobs. Comments that these activities can be an additional activity or an add-on miss the point of exactly how much time and effort it takes to engage in meaningful work with community organizations, for example. As with questions of commitment, this is a point that needs specific discussion and debate within geography and should be confronted head-on when calls for greater relevance in geographic research are made.

Relevance as Centrality

Accompanying many of the calls for contributions to public policy are statements about the way this will enhance the *centrality* of geographical knowledge to society, to scientific discourse, and to scholarship more broadly. Relevance can be defined, therefore, not only as pertinence, commitment, and applicability (as with Dear 1999), but also as centrality. There are several aspects to relevance as centrality, including being central to specific inter- and multidisciplinary projects, being central to key scholarly developments and debates, and being central to how lay people conceive their worlds. These different aspects of centrality are not often clearly distinguished. They should be, however, since each may require tailoring geographical research in a different way.

AAG presidents have been keen to promote geography within the broader academic community—making it central both to multidisciplinary projects and to critical scholarly debates. Seven of our interviewees also specifically mentioned the importance of making geography central in this manner. The implicit argument is that the more geography is insinuated in scholarly and public discourse, the more relevant it is. In a *Newsletter* column, for example, Susan Cutter (2001a, 3) asked,

> [W]hat are we doing (individually and collectively) to reach colleagues in cognate fields or influence public policies? How can we impress the intellectual and policy communities with our theoretical, analytical, and methodological advancements if we only talk to one another in highly specialized journals, abandoning the broader audience for some of our work?. . . We have intellectual capital that has considerable worth, but we're just not marketing it as well as we could or should.

The strong implication is that isolation breeds irrelevance.

Embedded in the arguments about relevance as centrality are several issues. The first is that geography is unusual among the disciplines in that it is integrative and spans the range of academic disciplines. At a time in which complex problems in the physical and human realms require reaching across intellectual boundaries, geography should be well positioned to make a contribution, or to be relevant, in addressing complex problems. The reality that geography is not seen as more central is variously attributed to the fact that we are a small discipline, occupy a generally weak position within universities, and the fact that geographers have not been significant players in large, multidisciplinary research efforts (Mather 1992; Graf 1998a; Golledge 1999b; Abler 2000; Martin 2001).

Our interviewees frequently made arguments about centrality. But they often discussed centrality in terms different from—indeed, perhaps in opposition to—the public discourse. For example, many of the researchers we interviewed worked in multi- or interdisciplinary research or teaching centers (e.g., Michael Dear, Richard Frankaviglia, David Seamon, Edward Soja, Neil Smith, Jennifer Wolch, Terence Young). While published statements most often discuss the benefits to other scholarly and societal pursuits that geography's centrality could provide, these geographers in interdisciplinary settings instead noted that working with people from a variety of perspectives helped to sharpen *their own* arguments, as the taken-for-granted could no longer be taken for granted. That is, centrality is valuable because it improves *geography*, as much as vice versa. Furthermore, teaching in interdisciplinary programs means that geographers are teaching people who are, or might later become, practitioners, such as planners or policy makers. Teaching in such a setting forces researchers to draw connections between fields fairly directly and in terms that would be understandable to people outside geography or to those who operate from different theoretical perspectives.

Yet there are costs that may be associated with interdisciplinarity that may, in fact, reduce geography's seeming centrality. One of these arises because geographers often mask their disciplinary training in these initiatives as they fill roles as planners, environmental scientists, social studies teachers, and so forth (e.g., Monk 2001); true interdisciplinarity requires that scholars set aside the particularities of their discipline and act as scientists or scholars in common, searching for discovery, enlightenment, relevance. Presumably, the value of interdisciplinary work is that perspectives emerge from collaborations that are separate from what an individual might bring, and so it is difficult to tease out what the value of a specifically geographic approach might be. Bob Kates (1993, 2) suggested that some people might be wary of complete integration, pointing to "many thoughtful geographers who see in such activities the weakening of the discipline and the diffusion of intellectual resources." He argued, however, that this is both an unnecessary worry and one that would not make the discipline useful in addressing pressing issues and concluded, "We should strengthen our place in our institutions, not behind crumbling wall of uniqueness, but with a fine web of intellectually symbiotic relationships."

Yet there is a tension here, as those who promote centrality also often promote a "big G" geography—one in which the disciplinary identity is foremost. Cutter (2001b), for example, advocated changing the name of the AAG to the Association for the *Advancement* of

Geography as a means of promoting the centrality of geography. Such arguments, however, seem to shift the discourse of relevance in a subtle, but important, way. Relevance seems to be defined as the relevance of the discipline, rather than a contribution to the solution of problems. There is an instrumentality in this shift that appears unseemly to some of our respondents. David Seamon, for example, worried that the discipline has too often touted its centrality through the promotion of GIS technologies at the expense of the more holistic approach that has been the discipline's strength. Speaking specifically about the ways in which geography has been incorporated in landscape architecture, he argued that "unfortunately, landscape architects mostly use geography right now because of its technical stuff. I mean, it pays for the discipline, but it is really instrumentalizing the discipline."

Moreover, bemoaning the lack of centrality of geography often ignores the ways in which geography has, in fact, been central to innumerable scholarly endeavors and debates. Work by geographers as diverse as David Harvey, David Lowenthal, Donald Meinig, Larry Ford, Will Graf, Julie-Kathy Gibson-Graham, Gilbert White, Yi-Fu Tuan, James Blaut, and Gillian Hart (to name but a very, very few) is not just central to a number of wide intellectual movements and debates, but has actually established the direction of the movements and the terms of the debates. Any number of other cases could be adduced.

Centrality here is not an issue of promotion but of influence. And influence comes largely from the depth, breadth, and insight of scholarship, not because geography's most influential scholars have dedicated themselves to solving either narrowly technical issues or specific problems. As some of our respondents noted, it is quite conceivable that pushing for greater instrumentality in geography might work directly against its intellectual centrality since it will divert resources from the sort of basic, often long-term, theoretically and empirically deep research that also leads to intellectual influence. They argued that if geography is to be a relevant discipline—relevant in the sense of being central to intellectual projects—then this sort of intellectual influence needs to be recognized and the means to support it need to be developed. The point here is that different kinds of centrality require different kinds of engagement with academic, political, and social worlds.

Relevance for Teaching

The final sense of relevance—relevance for teaching—emerged from our interviews, rather than from published statements about relevance. Many of our respondents argued that using research in training students was an important, although often overlooked, pathway by which research becomes relevant. Indeed, one of the most striking aspects of the published statements is how little effort has been made to link relevance to the broad, multifaceted goals that college teaching necessarily entails. To be sure, the issue of relevance in teaching has been raised, but usually in only a very specific way. Relevance in teaching is linked to a larger argument about centrality, in which good teaching plays a major role; it is often then narrowed to a discussion of instrumental relevance—the purveying of specific skills and the linking of teaching needs to business or government desires (e.g., Graf 1999; Golledge 2000a). Teaching is important for geographers' centrality, according to many AAG presidents, because making geography a more central part of the curriculum at the kindergarten through twelfth grade and collegiate levels—and improving its quality—will help solidify the discipline's position within educational institutions; it will help assure our disciplinary survival. Moreover, better teaching will presumably enhance the geographic literacy of government and business leaders, as well as the general population. Some people argue this achievement will only be possible if we stress more technique-oriented teaching (Graf 1999).

The presidents' columns often seemed to advocate teaching as a form of advertising for the discipline, whereas interviewees addressed ways in which faculty help students draw connections in ways that enable action. For example, Sallie Marston, like many other geographers, uses current geographical research in her classroom teaching and individualized mentoring. She argues that research becomes relevant by helping students develop a framework they can use to understand their experiences. She was not alone: twenty-one of the twenty-five respondents used research on public space in their teaching; at least three of the respondents included service-learning components in their courses; nineteen of the respondents identified students with whom they had worked who are now engaged in some nonacademic practice related to public space (e.g., as planners, developers, or activists); nine of these nineteen respondents were not themselves involved in applications of their research through consulting, advising, or activism. Thus, while our respondents may not have made their research directly applicable through their own actions, it may have become relevant in specific projects through their students.

While overlooked in disciplinary statements about teaching, our respondents strongly believed that teach-

ing was an important means by which research is made relevant. Students were specifically mentioned as the primary audience for six of the respondents who wanted their research to be relevant. Researchers noted that it was the combined missions of teaching and research that called them to academic positions and that it was through students that their work would have the greatest effect. Significantly, researchers did not assume that their work would take on broader relevance through the paid employment of students upon their graduation. Instead, several people spoke of the hope that they could give students tools and a framework for understanding issues that the students could take to voluntary work or activism—arenas that were not prominent in the calls for relevance in published articles and newsletters.

Many of our respondents were also clear that politics is important when considering teaching as an avenue toward relevance. Allan Pred, for example, argued,

> to me, relevance begins in the classroom. One deals with hundreds of students every year, and you're dealing with them at a period that in many respects and instances is formative in how they subsequently come to view the world. Many of these students are going to wind up in positions of some kind of responsibility and influence. And so, teaching students is highly relevant. And in my own mind, teaching is a subversive activity, or should be a subversive activity. Subversive in the sense that one at least forces them . . . into dealing with their own take-for-granteds. . ..When the issue of political relevance is addressed to me, I always say it begins with teaching.

Not everyone will agree with this assessment, but Pred's point connects back to the issues of pertinence and commitment in a way that forces a broader consideration of how, why, and for whom research becomes relevant. It requires a consideration of more than ideas, or of training in specific skills, but also of knowledge, including knowledge for whom and for what ends.

Yet there are worries that theoretical work in geography may undermine geography's relevance in the classroom and its ability to induce the kind of transformative experience Pred talks about. The following exchange with David Seamon, who teaches in an architecture program, is indicative:

> DS: Most of [my] students will end up in an architectural or landscape architectural firm.
> DM: And can you see the kinds of thing that you've talked about in class or meetings with students or so forth affecting the way they go about their practice?
> DS: Well, it's very difficult because in architecture as in geography, [with] the poststructural rage right now, you know it's very fashionable for young architects to believe right now that they are artists and their major aim is to shock people in their design. So, I'm having a fairly hard time here right now because our younger faculty have no interest in what I talk about, and some of them are openly hostile.

As Seamon and Pred make clear, no less than relevance as commitment, or relevance as application, relevance for teaching entails a politics: a politics of ideas and a struggle for respect within suites of competing discourses and political positions. How we use our own and others' research in the classroom, how we reflect on it with our students, and how we help them to understand are all important aspects of the sociology of relevance. Relevance—or at least some important aspect of it—resides exactly in *learning*.

The Complexity of Relevance in Geography

There is a broad commitment to making geography and the work of geographers useful in addressing problems facing the world. This commitment is clear in the statements from AAG presidents, in published commentaries and reports, and in the personal statements of scholars. But, too often, we do not appreciate either the multifaceted nature of relevance or that the complexity of relevance makes it a deeply political issue. Our goal in this article has been to draw out this multifaceted nature and to reflect on the political issues that it entails. We have tried to indicate that relevance is always a social issue, rather than a purely personal one. As experience in the classroom indicates, what constitutes relevance is a function of users, of those who read or hear our research, learn from it, and use it. Understanding how different audiences—students, activists, policy makers, lay publics— read, learn from, and use our research is an issue that requires a much clearer sense of what we mean when we are talking about relevance. In this sense, relevance can be intended—can be an attitude, perhaps—but not a neatly defined product.

The implication of the preceding pages is that recent debates over relevance in geography have generally adopted a too narrow view of where relevance is located or enacted. There is a strong focus on policy and business and with the promotion of either an instrumental relevance as application or a kind of relevance as centrality that may be little more than disciplinary advertisement. Thinking seriously about relevance for business and government is of course important; these agents have enhanced power to effect change, to redistribute re-

sources, to shape behavior and rules, to launch wars and remake geopolitical maps, to push toward either environmental sustainability or environmental destruction, and to produce the spaces within which we must all live. If one can change the way that actors in government and business think and act, the relevance of geography to society will be obvious. And even though many of us worry that linking our work too closely to business or government may only reinforce the status quo, several of the scholars with whom we spoke were explicit in their desire to bring progressive, even radical, politics to their work with policy makers and business groups. Relevance as commitment drives such scholars to think hard about the best ways to effect the social changes they hope for. Simultaneously, thinking about relevance as commitment encouraged many of those with whom we spoke to identify with grassroots activists and to attempt to shape social policy and public debate through community or other activist groups and through their work with students. Many of them wanted to give activists and citizens the tools to speak for themselves so that their voices would be heard directly. Indeed, the importance of bringing citizens and policy makers into dialogue was mentioned—unprompted—as a goal by fourteen out of twenty-five interviewees. Through attempts to enable and foster dialogue, the work of geographers could be quite important, yet relevance here might well be indirect and perhaps untraceable.

But does "fostering dialogue" really constitute relevance? Or does research only become relevant when it is directly instrumental to some practice? The published literature and some of our respondents support the idea that it is ultimately through practice that research becomes relevant. Yet there is disagreement as to what constitutes the practices that make research relevant. By thinking of relevance as multifaceted, as being defined by shifting combinations of pertinence, applicability, commitment, centrality, and teaching and by understanding that assessments of relevance always entail normative stances, we can see that there should be disagreement over appropriate practices among those who intend their work to be relevant.

Those aspects of the research-reception process that we can at least partially control—language, theoretical orientation, research foci—exist within a set of structural opportunities and constraints, including structures central to academic work, such as tenure, processes through which merit raises are awarded, shifting demands on work-time, and so forth. Whatever these constraints may be, it seems clear that geographers do work to bring their research to wider audiences than those composed of other scholars and students. Seventeen of our respondents reported that they have taken their research to nonacademic audiences. They engaged in discussions with policy makers and sat on policy-making boards, gave interviews and wrote articles for the popular media, were engaged in paid and unpaid consulting, and participated in political activism. There was a strong desire to speak directly to the public, and occasional success. This desire was voiced particularly as a desire to reach an educated lay public who might have progressive tendencies or who could be swayed in that direction. The evidence from our interviews, all this is to say, indicates that many calls for greater relevance are not just too narrowly drawn. Geographers already are engaged in the hard work of trying to make their research relevant—whatever the sociological constraints on their own intentions may be—and already devote considerable time and energy to the issue. They hope to be relevant.

Involved as they are in activities to make their research and knowledge relevant to wider audiences, most of our respondents felt, at best, only marginally successful in their goal of influencing public discussion and debate; many believe their efforts have failed. Several people, for example, had hoped that the books that they wrote would be read by an educated lay audience, but were frustrated that they had not been successful in reaching this audience. But contra the arguments in the published discussion of relevance, theory and language are only some of the difficulties they faced. Finding an audience and having a platform is another problem. Compared to European countries, for example, a robust intellectual periodical press—a press that takes the debates of scholars seriously—really does not exist in the U.S. The outlets for intellectual debate—the number of pages in national newspapers and magazines devoted to intellectual debate, for example—are exceptionally few. Being a "public intellectual" in the U.S. is not an easy proposition. As Will Graf has argued in his presidential columns, geographers need to be creative, proactive, and persistent in trying to reach an audience. Yet, as our interviews with untenured scholars indicated, the reward structures of the academy and the limited opportunities afforded by the media, often mitigate against expending the considerable energy such creative persistence requires.

In fact, almost all the people we interviewed mentioned that institutional barriers exist to making research relevant—an issue rarely mentioned in the disciplinary discourse. These barriers include lack of credit or recognition afforded to policy, consultative, or advising work, the prioritization of publishing in scholarly rather than more popular journals, and messages that scholars

should shy away from political issues. For pre-tenure researchers, the messages to stay away from efforts to take research beyond the academy were very strong, but they also affected more senior scholars. Indeed, promotion and tenure committees were frequently—if jokingly—mentioned by researchers as their primary audience for their research. This joke masked further issues, too: the relative undervaluing of teaching in tenure and promotion decisions at many universities often divert scholars' energies from exactly the place where they can (and, many would argue, should) make the biggest difference—the classroom. So, the pressures to prioritize academic publication are keenly felt across the academic ranks, and simply calling for more relevant work does nothing to address those pressures or to change the ways in which research is evaluated.

But equally important to those kinds of structures is the way in which ideas are received. At some level, reaching a broader audience—or even tailoring a project into a fundable research project—might mean having to change the ways in which issues are framed or couched, and this, in turn, might raise conflicts with the value of commitment. Commitment is unavoidably a part of relevance, not a by-product of it or something that can be somehow set aside for the time being. Making an idea understandable to a different community than the one within which it was developed might mean changing the idea in ways that are incompatible with the moral and political values that are the basis for relevance. The boundary between research and its context is a hazy and shifting one, but it, nonetheless, it is determinant. It is a struggle for ideas, but researchers are not guaranteed of winning that struggle or even necessarily guaranteed of being able to control the conditions under which ideas are produced.

And it is here that the complex pathways by which research may become relevant can be understood. Most of our researchers were committed to using their research to achieve a particular vision of society. But in the face of attitudes within society that did not always recognize the importance of the issues that animated the researchers, some sought to make their research pertinent through teaching functions of various sorts—through their courses, public speaking, working directly with students, or working with other activists. Note the phasing here: researchers *sought to make their research pertinent*. Pertinence is not only a function of fitting research to preexisting agendas but also can be a function of direct political intervention. As Jennifer Wolch suggested, a primary aim of many researchers is to reach below the surface and bring to light issues and ways of thinking that might change how people understand problems or evaluate what is important. While researchers might ultimately hope that their research will be applicable, their commitment to a particular vision of relevance for a just society requires a longer-term view and a greater appreciation for the indirect pathways of relevance than most of the debates about the issue allow.

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Notes

1. What follows is a summary of developments in human geography. We leave it to others to trace the development of physical geography and its changing relevance to other sciences and society at large. What is obvious, though, is that something akin to the revolution in theory that has transformed human geography and placed it at the center of so many critical social-theoretical debates, has been paralleled by an impressive growth of physical geographic theory and techniques that have likewise reasserted the centrality of physical geography to environmental sciences.
2. One can argue that the mainstream of debate in SSK, especially as represented by the so-called Edinburgh School, has had little to say about "relevance as such." That may, to some extent, be true, but what is at stake is how *we* use the ideas from SSK and related discourses in order to develop a frame for interpreting discourses of practices of relevance in our own field. Indeed, the whole point of the development of the field of social studies of science has been to create a means to turn a critical eye on the social practices of knowledge production. In geography, concerns about, debates over, and practices of relevance have been a central part of our social practices. And while Edinburgh School sociologists of science may not have interested themselves in "relevance as such," they, and their interlocutors like Latour (1987, 1996, 1999), have been deeply concerned not only with how scientific (and other) knowledge is produced, but also how it is used.
3. As noted, these interviews were conducted as part of a larger study of public space in which the production of knowledge (in its diversity) was a central concern. We set out to interview all academic geographers based in the U.S. who have published public space research on U.S. topics in the period 1945–1998. Thirty-three scholars were identified as fulfilling these criteria. One person refused to be interviewed, six were unavailable for interviews during the period we conducted the research, and one had died. Respondents were given the option of maintaining confidentiality. Six people maintained their right to confidentiality; no names are associated with quotations from these individuals. While our own research is

related to public space, we did not interview each other: our views on the topic will be apparent enough in this article.

4. Susan Hanson was one of the few AAG presidents to talk about this issue in her columns. Her columns were also notable for the references she made to her family. These references suggest an additional sense of relevance in her presidency—that our personal lives are relevant to the ways in which we operate as a discipline.

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