

Whither Geography?

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I argue that scientific disciplines are esteemed, supported, and patronized largely to the degree to which they are perceived as providing a “return” on invested societal resources. This “return” takes the form of scholarly products that help answer deep human questions or otherwise materially benefit members of the society whose resources they are. Such a view implies that disciplines exist in a “market” in which members compete for these limited resources by delivering products seen as valuable. In such a market, disciplinary relevance and survival are ultimately tied to decisions individual scholars practicing within the disciplines make about which research they pursue, the greater the perceived “return” the better for the long-term health of the discipline. **Key Words:** geographic discipline, scholarly products.

Introduction and Overview

What kinds of inquiries do we associate with scientific disciplines that generate sustained societal support? Why do some disciplines command public esteem while others remain marginal? And, in light of answers we might give to these questions, how can requests for the diversion of resources to geography (or any other discipline) be justified against competing claims of other disciplines within higher education when the fiscal environment is tight?

What I am after here is some understanding of why we have gone to such great lengths through the history of Western science to patronize and support the particular scientific fields we have. Further, the question is not why have humans partitioned the world into the current scheme of disciplines, as opposed to some other scheme, but instead, what is common to the most successful disciplines that generates such support and allegiance at both the individual scientist’s and the aggregate (societal) levels? And if we can answer these questions, do any legitimate normative implications follow?

These questions arise partly in response to the thoughtful discussion in a recent issue of the *Annals of the Association of American Geographers* regarding the “contested identities” that are evident in geographic scholarship, which seems to have been underlain by wider questions about the nature of the geographic discipline (Turner 2002). Observers have noted the unusual effort

geographers have historically expended trying to explain, describe, or justify the nature of their field, and that article continues the tradition. Perhaps the release of *Rediscovering Geography* in 1997 (NRC) opened the door to renewed dialogue about this issue, which appears unlikely to be put to rest by either of these two stimulating efforts.

My objective in this essay is *not* to offer yet another reading of what geography is or ought to be, although I find much with which I agree in Turner’s analysis. Instead, I wish to share some thoughts on what I take to be the more general purpose of science as this purpose relates to the geographic discipline. Briefly, this article is a linear argument in which I will maintain that inquiry in general, whether conducted by individuals or collectively and systematically as a scientific discipline, is justified by the return on the resources invested in much the same way that other rational human pursuits are justified. This emphasis we place on “return” arises from the limited nature of our resources and our (presumably evolved) human need to see our efforts translated into some kind of perceived self-benefiting result. If so, then scientific disciplines in general will engender allegiance, support, and sympathy largely to the degree to which they appear to deliver products that materially better our lives or otherwise answer significant questions. After briefly considering how well I think contemporary geography has done in this regard, I

conclude that the research agenda of geographers needs to be better connected with human need than has historically been the case.

In general, declarations about what we think *should* occur are more convincing when they are supported by reasoned argument, and thus the purpose of making this particular argument is to bolster the normative implications I draw at the end of the article. Making this argument will first involve a general sketch of some characteristics of human inquiry overall, which is then followed by more specific observations pertinent to scientific disciplines, including the field of geography. Finally, my remarks apply to the discipline regardless of whether we regard it as a nomothetic (law-formulating) science or as a more idiographic undertaking.

Details of the Argument

The Nature of Human Inquiry

Why do humans engage in inquiry? Even an approximate answer to that question would probably include physical and/or intellectual need as its progenitor. Whether our needs are to avoid harm, lessen disease, alleviate pain, or reduce hunger or to seek more comfort, convenience, and security, the central question is how to do so. Sometimes the need is intellectual, as we are discomfited by what we think we know and are prompted to seek new understandings (Peirce 1966). Whatever the precise stimulus (and the details are not relevant to the discussion that follows), in general it appears to be a needs-driven, goal-directed, problem-solving behavior geared toward bettering the condition of the inquirer while entailing the expenditure of resources (time, material, and expertise). By this view, then, whether conducted by solitary individuals for private reasons or as a systematized, formalized effort in association with scientific disciplines, the goals of inquiry revolve around mitigating negative or enhancing positive dimensions of the human condition or grow simply out of curiosity about a deeply nagging question.

What is relevant about the resources it consumes is that their quantity is limited. I do not have infinite time, material, and expertise to conduct every inquiry I would like, and so

choices among all the possibilities need to be made. But how? As consequentialist thinkers deciding what to do, we customarily choose among such options according to their expected outcomes as related to our need. As a result, although such choices could be made randomly, in a rational approach (see Nozick 2001, 116) we are more likely to make them according to an outcome-based scale. Hence, with the value of the expected payoff as a guide, a rational individual would seem unlikely to continue pursuing a line of ongoing inquiry where no—or no progress toward—answers obtain, or where obtaining answers offers no reward and alternative priorities are beckoning. Thus, early on, the matter of a “payoff” or “return” for the effort emerges as an incentive. Although the calculations used in assessing the value of such payoffs are often complex, limited resources and the dictates of survival nonetheless necessitate that we make hard choices about prioritizing our inquiries according to a metric that balances variable costs and variable (expected) benefits. As a result, we are more or less “forced” by incentives to invest our scarce resources carefully, and we will choose to do some things—including some kinds of inquiries—but not others, in an exercise of classic economics.

At the core of this issue is the locus of authority for assessment of the costs and benefits involved. For the sake of this discussion, the important point about *individual* inquiry—that is, inquiry conducted in association with private goals—is that both the resources being expended and the outcomes obtained pertain to the individual. Therefore, to whatever extent ownership confers a right, the *individual* inquirer whose resources they are reserves the right to judge whether or not the inquiry has been successful at meeting his/her needs and also whether the outlay of resources was justified by the payoff. Thus, not only are the *resources* internal, but so are the *criteria* of success.

Inquiry in Aggregate: Scientific Disciplines

Aggregate, systematic, organized inquiry we associate with scientific disciplines (as typically represented by scholarship generated by members of disciplines within academe) is similar to individual inquiry in many ways. That is, the research functions still involve mitigation of negative conditions, enhancement of welfare, or investigation of profound human questions for

their own worth. And again, because of resource limitations, research agendas will have to be prioritized according to expected benefits. In one important respect, however, such inquiry differs from that conducted by individuals for their own benefit: because most disciplines are not self-supporting, the resources supporting such inquiry are usually situated *external* to the discipline, and this difference leads to other notable contrasts.

Such disciplines depend on external (public and private) granting agencies for research funds, and they attract human resources (students) from without as well, in some respects interacting with their environment in an almost organismic manner, taking in resources and generating (scholarly) output. At the same time, such an external locus of resources means that the standards for disciplinary success and the actual assessment of disciplines in this regard likewise reside outside the discipline (again, to whatever extent ownership of the resource confers this authority). So, whereas individuals judge whether their allocation of their own resources to particular (individual) projects was justified by the gains, the discipline does not reserve that right. Instead, it resides in the hands of the parties from which the resources flow.

Given their limits, standard issues of prioritization again emerge (unless resource allocation is random). But according to what scheme? By what justification do members of a discipline make claims to resources originating elsewhere—or, turning the question around, by what criteria would the distribution of such resources among disciplines be determined? At bottom, it seems to me that such justifications must necessarily turn around the value of the payoff that such scholarship has produced for the supporting entity. Any other (non-outcome-based) strategy would be inconsistent with the widespread human practice of judging courses of action according to expected return.

If so, then the payoffs most desired would be those that most address human need—that is, they mitigate suffering or other adversity (of humans or things important to us), enhance welfare, or are perceived to have answered the most significant questions that we face. Some disciplines related directly to human health, such as epidemiology, provide such obvious benefits in reducing disease, improving health,

or otherwise advancing the quality of human life that their value is unquestioned. Other applied disciplines, such as engineering, provide technological applications that enable us to overcome hindrances to welfare. Inquiry (scholarship) in more theoretical disciplines such as physics, while having no direct application to our human condition, solves underlying problems that pave the way for more applied disciplines such as engineering to proceed. Still others, such as cosmology or archaeology, have little prospect for applied benefit but nonetheless grapple with profound questions about who we are and what the future might hold.

It is, however, empirically demonstrable—and probably so obvious as to be unnecessary to demonstrate—that resources flowing into the various scientific disciplines are unequal. The question is, why? Why do some disciplines seem to command our respect and approval and therefore capture a disproportionate share of societal resources, while others struggle with their image and public support and esteem?

If it is rational for an individual to allocate scarce personal resources to inquiry according to the value of the expected outcome, why would it not be equally rational for the society to allocate its scarce resources in the same way? A rational system for societal resource allocation would identify those enterprises (disciplines and their associated scholarship) that are most likely to offer the greatest return on investment. A science's claim on (future) resources would then be roughly proportional to the degree to which it was perceived to have returned value on the investment of past resources. Just as I would be irrational to continue to invest my own resources in inquiries that had not yielded some benefit before (unless I had reasons to expect that the future would be different or that my resources were infinite), so would a society be irrational to invest in such a disciplinary enterprise.

What emerges here is a picture of different areas of scientific inquiry all making competing claims on limited societal resources, as if they are in a kind of market. Through the activities of its practitioners, each area vies with others for its share based on the value of its past scholarship, and allocation of future resources flows accordingly. Because resource allocation decisions are in the hands of decision-

makers *outside* of the disciplines themselves, what matters is not their self-assessment, but rather the perception of their success (reputation and track record) as viewed from without—that is, from the perspective of the entities (granting agencies, for example) actually providing the resources. The basic question is one of the degree to which the inquiry associated with discipline is seen as having bettered the lives of others (through mitigation of negative conditions, enhancement of positive ones, or advancement of knowledge regarding profound human questions) who are providing the support. Thus, resource allocation is a prediction—an extrapolation of sorts—about the likelihood of future performance based on an assessment of the recent past. Because not all areas of inquiry are seen in an equally favorable light in this regard, not all disciplines receive commensurate shares of societal resources.

In summary, a discipline can be viewed as something like an organism, with its “health” measured in “stocks” (current status in terms of human and material resources) and “flows” (inputs—human and material, and outputs—scholarship), with the level of societal input determining its long-term health. Sources of inputs and their gatekeepers are external to a discipline; future inputs are proportional to the perceived payback on past investments. Therefore, a discipline’s claim on societal resources is commensurate with the perceived contribution of its scholarship to society’s welfare, and disciplinary vigor generally tracks the nature and success (as inquiry goes) of that scholarship. It is not an accidental outcome. Finally, an obscure or marginal discipline is unlikely to have a value disproportionate to its status, because, in such a disciplinary “market,” the principal measure of value *is* external support.

By this view, over the long haul, the worthwhileness of a discipline is seen as roughly proportional to the good it serves, and society subscribes to it just to that degree. High-yielding disciplines provide useful products and, as a result, survive and thrive, while those failing to provide such products or to generate applied outcomes seen as bettering their constituencies or providing other useful output are eventually consigned to obscurity, atrophy or becoming absorbed by their more vibrant disciplinary neighbors. Like architects, rather than artists, disciplines need “clients”

who will support their services by sponsoring their scholarship.

Normative Implications of a Disciplinary “Market” For Geography

If, over the long run, (perceived disciplinary) return on investment dictates (societal) resources allocated, how does the discipline of geography look through such a market view of science? As a geographer, I want to think well of my field and believe it to be an essential, high-yielding enterprise. Unfortunately, this image may not square with some recent trends in enrollment in our courses, numbers of majors in our departments, percentage of our scholarly articles never cited (Abler 1993), and other measures of disciplinary esteem and demand (particularly when the impact of interest in GIS is removed). Aside from whatever significance we might (or might not) attribute to these numbers as indicators of the health of the discipline, however, the focus here is on two alternative questions: no matter how well (or not well) we may regard the discipline’s health, how can we geographers do better, and why should we bother? If my argument is correct, at issue is our disciplinary return—particularly from our scholarship—on societal investment.

As individual scholars pursuing our own research agendas, how might we justify the research we undertake? We could do so *absolutely* if it connects directly with human need (its products materially improve the lives of others or have useful policy implications, and this usefulness is itself a reason for doing it), or *relatively* if the results are then useful to others working on related research questions. We might think of scholarship justified relatively as analogous to pieces missing from a jigsaw puzzle. If so, however, the value of those pieces depends on the value of the completed puzzle. That is, scholarship’s value “piggybacks” onto the value of that other work (is derived from *its* value). Where does the value of *that* research obtain? Why that *particular* jigsaw puzzle? Such relative justifications create a logical regress by relying solely on the work of others; at some point, *we have to ask why this research has independent value*. Relative justifications of our own work that appeal to similar scholarship of others working in our own narrow research specialty are empty

if, at some point, that scholarship does not have the potential to address a human need, which serves as the *ultimate* justification for all scientific research. Against a relative justification of research, basic questions about “who benefits from this work?” are unanswerable except with reference to the participating scientists’ careers.

For a scientific discipline such as geography (as with any science), the practitioners of which want to avoid the difficulties of relative justification, this line of reasoning means that our own research needs either to have an applied value or connect with work that does, or to address a basic and vital question of interest to a wide segment of the population (much in the way that cosmology might). As we saw earlier, the nature of the research we undertake poses no justification problem in a world of infinite resources, but, for most of us, limited resources impose constraints and provide us incentive to restrict our choices to questions promising the greatest payoff. Nor does it matter whether the field in which we labor is widely regarded as a science or as a “softer” discipline. What matters is *only* that the resources expended by the discipline originate external to it, which forces it to interact with parties outside itself and generates accountability when they impose expectations on how those resources will be used.

So, how well is geography currently doing in this regard? The release in 1997 of the National Research Council’s *Rediscovering Geography* provides what we can only assume is a thorough inventory of recent scholarship within the discipline that purportedly establishes how well it “connects with the broad concerns of society and science” (NRC 1997, 9). This ambitious effort includes citations to over 300 publications by geographers to help establish its point. Yet the Association of American Geographers listed over 7,000 members at the time this work was prepared (NRC 1997, 12). Either the 300-plus citations are a selective subset of work *typically* done by geographers that routinely “connects with broad concerns of society and science,” or they are cited because they are the exception. If they are typical, one then has to wonder why the field’s reputation for such work is so disconnected from reality that *Rediscovering Geography* was judged to be needed in the first place. It is difficult to imagine members of a robust and oversubscribed discipline ethically

arguing for the allocation of scarce resources for such an inventory when such work is already understood to be the norm. If these citations are not typical, one then might wonder what kind of scholarship the other approximately 6,700 members of the AAG are generating.

If such work is indeed the exception (hence its inclusion in this volume as more of an example of what the field *could* do), drawing our research agendas more from societal need (Pielke 1997) or, alternatively, pursuing research questions that speak more to the core of our common human curiosity could have broad positive consequences. If, as I have argued, the value of our scholarship is established by users and ultimately because of its (actual or potential) contribution to human welfare, then geography is only as valuable as its products are valued, and generating more scholarship that addresses genuine human need will surely elevate the esteem in which we are held by others beyond wherever we think that level of esteem now lies. That is, such a research agenda will, either directly or indirectly, lead not only to the production of scholarly products more applied to societal problems but also, in so doing, to elevated disciplinary esteem: while *we* may think our individual research agendas are important, what really matters is how that work is seen from outside the discipline. To use the trite but apt expression, we will do well as a discipline by doing good.

Such a prescription, however, avoids a previous question: that is, why *should* the well-being of a discipline such as geography be a matter of concern? We geographers, perhaps more than members of most other disciplines, seem to have a “bunker mentality” that leaves us sensitive to turf issues and prone to decry inroads into our traditional areas of scholarship or to justify courses of action in terms of preserving or defending our discipline. Unfortunately, such a mentality is unproductive and self-undermining because it is based on an unstated and questionable premise: namely, that a discipline is “good” or “valuable” just because we think it is. No discipline is automatically worth society’s resources just because we think so or happen to have found satisfying careers laboring within its confines. If, as I have argued, whether a discipline is valued depends entirely on its (perceived) contribution to our needs, then that value is set, not by its

members, but by the market and is indicated by societal patronage and support. Meanwhile, allocating resources to spruce up its image without altering its fundamental commitment to address human need is misguided and, in my view, doomed to failure. In the history of science, disciplines come and go. Some, such as human medicine and related fields, continue to occupy center stage, while others struggle in the dim margins. In the disciplinary marketplace, what geography contributes is a result of choices geographers make, and obscurity will result if we choose scholarship that, in the broadest sense, is low-impact—that is, fails to satisfy our deep curiosity, solve practical problems, and/or inform public policy (Abler 1993).

Critics of this argument will rightly point out that many pressures and histories account for the direction taken by scholarship within a discipline. Faculty careerism, university politics, academic fads, and scientific reductionism number among the many factors that sometimes disconnect the research agenda from human need. While they may *explain* such a disconnection, these factors fail to *justify* it unless serving them as ends promotes greater good than would obtain from a more socially connected agenda. Further, some research agendas are more valuable than others at meeting (what many would agree is) a worthy secondary goal of research: namely, fostering the health of the discipline, which is met by achieving the primary goal of addressing societal need. That is, if we believe that disciplinary health is a good to be pursued derivatively, then a research agenda disconnected from societal need is inconsistent with the secondary goal of enhancing the welfare of the discipline.

Just *why* is the welfare of the geographic discipline important? If a discipline holds valuable insights that we agree need to be transmitted in a systematic and orderly manner to the public, this transmission is probably best accomplished in our schools. Where will the teachers engaged in that transmission be trained, and by whom? Most likely, such training will take place in our colleges and universities and will be conducted by faculty who are affiliated with centers (academic departments or programs) where such knowledge reposes and is developed. How is the cost of these centers to be justified within academe when resources are limited or shrinking? While budgetary justification may turn around

claims about the inherent value of transmitting such knowledge, in the context of ever-greater emphasis on extramural funding and external ranking based on research output that now characterizes much of academe (at least in the United States), such a claim seems unlikely to carry the day. If not, then departmental survival appears certain to turn ever more strongly on faculty research output, particularly high-visibility funded scholarship. If we think that geographic knowledge and skills are important for the public to have, there seems to be no better way for us to assure that centers of such knowledge remain viable than to look after the value of our scholarship, because (at least by this view) it ends up underwriting the repository function of departments in modern academe.

Final Thoughts

Serious problems loom on our horizon. These currently include the growing HIV/AIDS epidemic ravaging sub-Saharan Africa (and later, perhaps, Asia), worldwide environmental degradation, the social effects of economic and cultural globalization, world hunger, and what some see as the rise of religious fundamentalism around the world, to list just a few. Where will geography be as we attempt to understand and address these and other pressing problems? I think that, as a discipline, we *will* have more to say when, as individual scholars, we endeavor to arrange our own research agendas around such questions, rather than justifying our research mainly in terms of other specialized scholarship within the discipline that may or may not connect even remotely with societal need.

As to whether we geographers *should* be concerned with geography's future, I think that beyond our own narrow career interests, such a question is meaningless. The disciplinary market will set our value for (and independent of) us. If, as individuals, we create valuable products, then our discipline will be valued in the aggregate. If not, I see no point in propping up a discipline with low market value when, at least in this context, market value is the only value.

Undoubtedly, the surge in interest in GIS has brought geography new attention over the last few years. Whether this interest will be sustained is a question the answer to which

requires prescience I lack. Given its usefulness, however, the diffusion of GIS applications and research into other sciences is already occurring and seems likely to accelerate. In what discipline will the requisite technical research and development likely be seated, and is such research adequate to comprise the core of a scientific discipline such as geography? If B. L. Turner (2002, 56) is correct in observing that geography has been the only science to define itself by its methods rather than by its content, and if, by implication, such a position is one of weakness, then substituting GIS technical research for more traditional methods-based emphases in geography would seem an unhelpful step.

In addition, sometimes, in these kinds of discussions, we geographers seem to confuse means with ends. That is, we focus on such issues as recruiting and numbers of majors in our college programs, enrollment in our courses, and numbers of offerings of high-school geography as if they are synonymous with disciplinary value, and treat them as worthy goals in and of themselves. Granted, emphasis on short-term fiscal "accountability" in higher education often turns discussions with administrators toward these kinds of measures rather than more substantive issues. But we would do well to remember that disciplines seen as generating vital, useful scholarly products generally have a healthy public image and seldom need to be concerned about such matters in the first place. If not, then developing strategies to enhance enrollment and majors without addressing the nature of the scholarship geographers do puts the proverbial cart before the equally proverbial horse and raises means/ends questions.

Meanwhile, whether a renewed emphasis specifically on the human/environment tradition in geography with which Turner (2002) is concerned would lead to more valuable problem-

solving scholarship and whether, if geography were to stake out this research territory as uniquely its own, the research agenda of its many subfields would then become clearer if they were subsumed under it (or any other one) are both unclear. What *is* clear to me is that geography's long-term future as a substantive (as opposed to methodological) discipline turns largely on the ability of geographers to connect their research agendas with our needs as humans. If the human/environment theme—or any other strand—is to be the focus that will enable us to do that, then, I would say, let us get to work on it. ■

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