

Bringing Political Science to Bear on Tropical Conservation

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Abstract. With the renewed international emphasis on governance for sustainable development, political scientists wishing to have an impact on global environmental problems will find significant opportunities to collaborate with practitioners. These collaborations can be very rewarding, but academics should enter them with their eyes open to the associated challenges. Bridging the worlds of research and practice entails tensions between what is true according to the research, and what is possible given the constraints of specific organizations; between the problems that practitioners need solved and the puzzles that researchers find appealing; and, importantly, between the divergent cultures that shape the expectations and incentives of academics and their counterparts.

Key words: conservation, governance, political science, research impact

Abbreviations: NGO – Nongovernmental organization; OTS – Organization for Tropical Studies; UNDP – United Nations Development Program; WDR – World Development Report

1. Introduction

It has been said that there are two types of political scientists working on environmental questions: those interested in the environment principally insofar as it provides a fruitful context for exploring the larger questions of political life, and those who, however committed they are to theory-building, are motivated to a significant degree by a normative commitment to improving environmental outcomes. Though the characterization is undoubtedly oversimplified when expressed in its ‘either/or’ form, these two aims fairly represent the axes of a two-dimensional space in which environmental politics researchers settle, and often resettle, over the course of their careers. The significance of this distinction lies in the fact that many norm-inspired scholars – and I count myself among them – would like to see their research influence the world. Although I had a few years of applied experience prior to graduate school, I had not until very recently made a serious effort to use my research to influence practice. In 2001, however, a unique opportunity arose when I moved from Duke University to the Johns Hopkins School of Advanced International Studies in

Washington D.C. to spend two years conducting interviews for a new book project. Having discovered that traditional funding opportunities were limited, I supported my work by serving as a research consultant – first with The World Bank, as a consultant for the *World Development Report 2003*, and subsequently as policy director at the RARE Center for Tropical Conservation, an international NGO whose president read my book *Environmental Leadership in Developing Countries* (2001) and asked for my help in designing a new multinational program in conservation governance.

On the basis of these experiences I learned two overarching lessons. First, there is significant unmet demand for political science research on global environmental problems. Scholars seeking to have an impact on the world of practice will find no lack of opportunities to collaborate with practitioner organizations. Second, however, I discovered that bridging the worlds of research and practice presents much larger constraints than I had previously imagined, even when dealing with enthusiastic and well-funded collaborators. The tenor of my remarks is not intended to be pessimistic so much as sobering. Meaningful collaboration between environmental scholars and practitioners is possible, can result in smarter practices and more sophisticated theories, and can be very personally rewarding. But academics should enter these collaborations with their eyes open to the challenges, and should shape their expectations and strategies accordingly. The trajectory from novel ideas to improved practices is an iterative series of negotiations between what is true according to the research, and what is possible given the constraints of specific organizations; between the problems that practitioners need solved and the puzzles that researchers find appealing; and, importantly, between the divergent cultures that shape the expectations and incentives of academics and their counterparts. The purpose of this essay is not to provide generalized conclusions about the impact of research on practice, but to share information from a limited set of experiences that may inform the decisions of others. Rather than repeatedly qualify my conclusions with reminders of this fact, I leave to the discerning reader the question of the applicability of these points to their issue areas and organizational milieus.

2. The Growing Demand for Political Analysis

John Kingdon (1984) has pointed out that policy agendas change when pre-existing answers, long in development, are appended to urgent new questions. So it is, by analogy, that timing plays an important part in the current demand by international environmental practitioners for political science research. ‘Governance’ has become an issue of great concern in international policy circles, and there is today significant unmet demand for expertise on governance-related topics long within the purview of political science. These topics include institutional change, policy processes, institutional design, democratization, decentralization, civil society, state–society relations, and organizational performance, to name a few. The origins of this trend are

diverse – ranging from demands for greater governance accountability in overseas aid allocations, to the transnational campaign against corruption, private sector concerns about the impact of weak national institutions on trade and investment, and the challenges of institution-building in the wake of democratization and decentralization. An often-cited watershed in this trend was the *World Development Report 1997*, in which the World Bank, reversing its earlier emphasis, acknowledged that effective government and institutions are a prerequisite for economic growth and social prosperity. The trend has found a sympathetic hearing in international environmental circles, as is evident in efforts ranging from UNDP's Capacity 21 initiative to the emphasis on governance at the 2002 Johannesburg Earth Summit. Within the nonprofit sector, numerous international environmental NGOs have recently launched or revitalized programs with titles featuring 'policy,' 'institutions' and 'governance.'

While the renewed interest in governance arose from factors other than political science research, this has opened a space for political science (and, by extension, political scientists) to influence the ways in which this cross-cutting trend translates into specific policies and programs. Importantly, this trend has coincided with the significant growth over the past decade of political science research on the global environment. In Kingdon's terms, there exists both a new set of questions and a substantial supply of potential answers. Supply and demand do not, however, constitute a market; there is a need to create channels to facilitate the exchange of goods, and this requires entrepreneurial efforts on the part of reflective practitioners and practice-oriented scholars. Some lessons from my own improvised efforts in this arena are described below.

3. Courting Relevance

My collaboration with the World Bank began with an introductory meeting with the principal authors of the *World Development Report 2003* (WDR 2003), a respected group of economists from diverse parts of the organization who were converging on the idea of using the Report to say something meaningful about the role of equity and institutions in promoting sustainable development. As the World Bank's capstone research products, WDRs are taken seriously in the upper echelons of the organization and receive wide play in international development circles. World Bank decisions, in turn, are taken seriously by the international conservation community because the Bank has sponsored many ecologically disastrous projects in the tropics and, of equal importance, is the world's largest source of funding for biodiversity conservation. A decade earlier, the influential *WDR 1992* helped to recast the global environmental agenda in terms of priorities facing developing countries, such as inadequate sanitation and indoor air pollution from biomass fuels. The *WDR 1992* marked a turning point in my own thinking on these issues and I welcomed the opportunity to participate in the decadal sequel to the Report.

In the course of this initial meeting it quickly became apparent that political science does indeed have something to say about many of the core questions of concern to the Bank. Just as some political scientists would be surprised to learn of the substantial economics literature on social equality, these economists were pleased to learn of the burgeoning literatures on, for example, the origin and effectiveness of international environmental regimes, comparative metrics of civil society, institutional resilience and change, and social learning. Over the course of the year I produced a report on the role of civil society in environmental governance in developing countries (Steinberg 2002) and served as a resource for various topics covered by the Report. The content of the WDR was significantly influenced by these consultations and the core argument of *Environmental Leadership* was included in recommendations forwarded by the World Bank to the Johannesburg Summit. While there is a considerable distance between this sort of 'impact' and changes in practices on the ground, the experience left the impression that there are many additional opportunities for political scientists to influence international environmental practices. It also demonstrated that while there is a large potential demand for political science, to realize this potential we must first educate practitioners about the types of questions political scientists can address.

My collaboration with the RARE Center for Tropical Conservation differed markedly from my World Bank experience. RARE is a small, fast-growing NGO that develops replicable tools for use by grassroots conservation leaders throughout the tropics, in areas such as environmental education and ecotourism. The organization is led by Brett Jenks, a dynamic social entrepreneur who had for some time been interested in expanding into policy issues but was uncertain whether and how to proceed. He found my book's emphasis on domestic policy leadership consistent with his organization's approach to local empowerment for conservation, and approached me with a proposal to help create a new policy program. Most of the observations that follow are based on this latter collaboration, as this brought me closer to the experience, and associated challenges, of bringing research to bear on environmental practices.

4. Challenges

In an ideal world, the scholar hoping to have a positive impact through rigorous and relevant research would encounter the following scenario: A leading practitioner would somehow discover your work, take the time to read it, and decide to create a program to implement your ideas. There would be a staff in place that did not already have a full set of commitments and whose skills, resources, and location would match the requirements of your recommendations. Uncommitted funds would be available to implement your ideas. Your role would be limited to occasionally providing feedback and advice, on a schedule consistent with your academic responsibilities. Any additional research needed by the organization would be

interesting to you personally, consistent with your research trajectory, and publishable in top journals.

The reality, however, looks somewhat different. In practice there are a series of filters that a scholar's ideas must pass through in order to have a significant impact on an organization's agenda and activities.¹ A few of the more important ones are as follows.

4.1 MISSION

When a leading practitioner enters into discussions with a university professor over a potential collaboration, there is only one question in the practitioner's mind: "How is this idea going to enable my organization (unit, group) to meet its core mission more effectively?" Academicians, accustomed to creative latitude as the precondition for both their intellectual productivity and job satisfaction, may think it reasonable to expect the practitioner to meet halfway between the goals of the organization and the interests of the researcher. Academics may be unpleasantly surprised to find the other party unyielding. Only in unusual circumstances would a manager consider changing the organization's mission in response to outside ideas. The best managers will not budge beyond the boundary of mission, preferring to forego the prestige of an academic collaboration and hobble along in the right direction, if necessary, rather than devote resources to a rigorous distraction. In statistical terms, they will choose error over bias.

This presents a dilemma for the scholar seeking to have a social impact, who undoubtedly has a (research) mission of his or her own. There is an inherent tension between a research question's theoretical importance – which increases in proportion to its generalizability – and its relevance for improving practice, which derives from its ability to help solve specific problems in a particular place and time. In this respect, the professional incentives of academia – notably the rewards for publishing in top-ranked, theory-oriented journals – are in direct conflict with the priorities of practitioners. This tradeoff echoes the tension between policy research and political science more generally, described by Sabatier (1991). One way to resolve this tension is through a division of labor in which some researchers focus on the production of more 'upstream' ideas of broad applicability, while others who work in more applied settings translate these ideas into the world of practice. For those of us who do not wish to place ourselves entirely in one or the other role, however, this solution is unsatisfactory. An alternative is to simply accept the fact that some research projects will be more amenable to impact, while others are more oriented toward publication. It can be discouraging to realize, however, that many applied projects must be 'written off' in terms of their publication value. Social impact is often inversely proportional to research productivity as traditionally defined. This tradeoff is especially difficult for junior faculty who need to establish a track record for tenure, suggesting that it is harder for scholars at the beginning of their careers to engage real-world issues.

4.2 SALABILITY AND NICHE

To have an impact, a research idea must not only mesh with the mission of the organization implementing it. In the competitive world of nonprofit organizations vying for the attention of donors, the idea must also allow the organization to distinguish itself from the pack. The product or service that a public-interest organization produces – be it community empowerment strategies, habitat conservation techniques, or staff training curricula – must be readily distinguishable from those of the competition.² The scholar's ideas must match the self-constructed niche of the organization in question and must not overlap with that of another organization.

A case in point came when RARE asked me to recommend strategies to improve conservation policy outcomes in the tropics. I recommended the most effective approach I have encountered in the past decade of research, in which an organization in a developing country invites high-ranking leaders from diverse political parties to ecological retreats, combining educational seminars and field trips with unstructured time for discussion and networking. Pioneered by the Organization for Tropical Studies (OTS), this approach has been used in several tropical countries and is often cited as a turning point in building high-level political support for conservation in a manner that is catalytic yet mindful of national sovereignty. Unfortunately, because this approach is already used by OTS, I discovered that RARE would be foolhardy to build its policy program around this technique – even though OTS is unlikely to expand its policy training program beyond a handful of countries. For organizations in competitive environments, the recommended research idea must not only be accurate and powerful – it must translate into a unique product.

This is but one illustration of a broader point regarding the salability of research ideas, which must satisfy the numerous – and at times shortsighted – criteria of those paying for them. There is today a strong current in the donor world to judge programs based on their ability to produce measurable, predictable, short-term results. Continuing with the OTS example described above, this technique produces a very high likelihood that some significant conservation policy initiative will result within a few years of the educational retreat. One cannot, however, specify the particular policy result in advance, because the popularity of the approach stems from the latitude granted powerful decision-makers who translate these general ecological concerns into specific policy proposals. (Indeed, if an international organization could anticipate the specific policy changes in advance, the approach would raise troubling questions regarding national sovereignty.) A donor who strongly supported my work at RARE explained that he simply could not raise money for a high-likelihood, high-impact initiative whose precise outcome and time frame could not be specified in advance. The final program design (described later) was a better match for RARE, but the experience served as a reminder of the multiple institutional filters through which research ideas must pass in order to influence practice.

4.3 RESOURCES

To translate an idea into a program, someone on the front lines must do the day-to-day work – designing the curriculum, lobbying the legislature, working with the tour operators, or rallying the community. Accordingly, an organization is more likely to adopt ideas consistent with the skill base of its existing staff. In the case of RARE, most field staff were experienced local community organizers with little exposure or aptitude in the policy arenas of their countries. Moreover, RARE maintains a strong organizational presence in some regions but not others; though I might prefer to select priority countries and regions based on extant policy conditions, I came to appreciate that RARE was more likely to be effective by expanding into policy-related activities in those sites where its staff already enjoyed local legitimacy, rather than allocating staff to new regions that might look better on paper.

Because researchers in these collaborations are typically introducing a novel idea or approach, there may be few resources to implement this idea beyond minimal start-up funds, particularly when working in the non-profit sector. This may be perfectly acceptable if the plan is to lay the conceptual foundation and strategy for a broader fundraising effort. But this places the near-term success of the program and any associated pilot projects in the hands of those who do control the resources: the mid-level managers who coordinate and prioritize the activities of field staff, and fundraising personnel who must prioritize among the needs of diverse programs within an organization. The sooner one identifies the key resource allocation pathways within a given organizational environment, the better one will be able to identify whose ‘buy in’ is essential for successful implementation.

Ultimately, the shape of RARE’s nascent policy program bore little resemblance to my initial proposal. Building on RARE’s existing comparative advantage in grassroots environmental education, we launched a ‘civic conservation’ initiative that uses RARE’s well-honed awareness-raising techniques to mobilize grassroots constituencies to improve policy implementation. Additionally, in light of RARE’s resources and legitimacy working at local levels, we developed a proposal on capacity-building for municipal conservation. The ultimate form of the program was something that neither RARE’s staff nor I could have invented on our own, pointing to the importance of extensive discussion and incubation of ideas as scholars and practitioners come to understand the relevance of questions they were not previously prepared to answer.

4.4 CULTURE

The process of forging a working relationship across the academic–practitioner divide is in many respects a cross-cultural experience. Initially it can be quite disorienting as the parties grope in the dark to discover one another’s motivations, capabilities, and constraints. For many practitioners, it is a mystery why academics would pursue research questions lacking immediate application. The environmental

practitioner values people who produce tangible results on the ground – laws passed, projects completed, revenues increased, education campaigns implemented, environmental threats averted. In academia, the highest professional status is accorded those who have the greatest influence on the thinking of other academics. In contrast, the public interest practitioner's greatest reward is the satisfaction that comes with accomplishing, or at least doggedly pursuing, a positive impact on the world through collaborations with others sharing this outlook. The academic's most cherished asset is autonomy and the intellectual freedom to explore and express new ideas. The practitioner weighs the merits of a theory by the degree to which it corresponds with or otherwise illuminates his or her practical experiences – what the practitioner knows (or feels) to be true. The academic measures the significance of those experiences by comparing them to the big picture – what is known from numerous analyses in diverse settings. Cultural differences manifest themselves on more mundane levels as well. The academic keeps irregular hours and is judged by the quality of the product of his or her labor. In many practitioner organizations, one's commitment is signaled and judged by the number of late nights put in at the office. Academics are not accustomed to reporting to a supervisor on a regular basis nor are supervisors accustomed to granting a newcomer broad leeway on this front.

This cultural divide explains why it was in many ways easier for me to fit into the organizational culture of the World Bank despite the fact that my commitment to conservation more closely matched the normative orientation of RARE. The World Bank employs large numbers of Ph.D.s, many of whom alternate between academic appointments and applied positions, making it much easier for staff to understand my professional background and mindset. Trust was more of an issue at RARE, where in the early stages of the collaboration more than a few staff (including those who eventually became close collaborators and friends) were uncertain of my intentions and wary of my claims to expertise.

In my experience, the impact of a scholar will increase to the extent that he or she spends a significant amount of time working on site with the staff. To understand the culture of a place, and the associated constraints and opportunities affecting the feasibility of research ideas, one must spend time on site to learn what people care about and how they construe effectiveness. One must observe patterns of deference among staff and listen for background developments – recent organizational shakeups, controversies, rivalries, or trends in the external environment – that shape operations but might not be mentioned during the occasional check-in by phone that characterizes long-distance collaborations.

As the scholar and practitioner struggle to make sense of each other's worlds, they need key informants who serve as cultural translators. The scholar needs someone who can provide insight into important and often subtle aspects of organizational culture. Conversely, the scholar not only introduces the practitioner to his or her own research, but is the unappointed delegate of an entire field of inquiry – perhaps even an entire discipline, as was the case when I sat with my World Bank colleagues answering questions such as “What does the political science literature have to say

about institutions?" There is a danger in this. On several occasions I observed Ph.D.s, either full-time staff or hired consultants, who represent themselves to a practitioner organization as the resident social science expert on a given topic, when in fact they were not widely published or known in that field. (In some instances they were not even trained in the relevant discipline!) Certainly forthright communication of the limits of one's own expertise is an important part of constructive engagement. Additionally, practitioners would do well to balance the views of more than one scholar and to check the credentials of research advisers through consultation with others in their stated field. Conversely, when seeking information about an organization, academics should diversify their key informants. Many staff are eager to volunteer assessments of 'how things really work around here,' and those assessments will differ significantly from one person to the next. Repeated discussions at different levels and in different divisions of an organization are an essential part of the task of familiarizing oneself with a new and unfamiliar action arena.

The scholar enters the culture of a practitioner organization as both a newcomer and an outsider. I found that the limitations on effectiveness that this dual condition might impose at RARE were mitigated to a significant extent by the actions of the organization's president, who sent clear and consistent signals to the staff that the new policy program was a high priority. This provided me with a measure of legitimacy and clout far exceeding the relative resources of the new program. Additionally, by creating the new position of policy program director and treating the program as an experiment under his direct supervision, he created a semi-autonomous, protected environment that made it easier to experiment with new ideas.

5. Prospects for Collaboration

Collaboration across the research–practice divide can take many forms, including some that are less intensive than the experiences described here. By academic standards, the extent of these collaborations was unusual, as I spent hundreds of hours working with RARE and the World Bank from 2001 to 2003. My hope is that the resulting reflections, which could only have come from such close collaboration, can inform the efforts of other academics attempting to bridge the gulf between knowledge and practice – whether serving in an informal advisory capacity, on expert review panels, as short-term consultants, or full-time resident researchers on sabbatical. In this essay I chose to focus on some of the sobering constraints facing researchers who wish to have an impact on global environmental practices. But let me end by reiterating the potential for constructive engagement. Political science now has a great deal to say about many pressing global environmental problems, both because of the growth of literature on global environmental politics and the recent shift in international practitioner circles toward questions of governance and institutions. Given that so many researchers care deeply about the social and

environmental outcomes they study, it is well worth the effort to get involved. An essential step in this process is to create demand for our research products by helping practitioners to become familiar with the field. The answer to the question “Is anyone listening?” will turn in large part on whether, and to whom, we are talking.

Notes

1. My remarks focus on one mechanism of influence, conforming to my experience, in which the researcher’s impact on the world is mediated by a formal organization and therefore a function of influencing the practices of that organization.
2. The intensity of competition within the professional arm of the environmental movement was captured in a joke over lunch told by the director of international programs at a large NGO. “What’s the difference between cannibals and environmentalists? Cannibals don’t eat their own.”

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