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Social science as an ethical practice

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The fact–value distinction and the conception of science as a practice put a premium on the ethics of the scholarly community. The principles of ethics, not those of logic, govern this process of knowledge production. Values not only enable facts, but sustain our identities as scholars and enable the practice of science. In the absence of ethical commitments, we would become indistinct from polemicists and spin doctors. Ethics are instantiated and sustained through practices. As graduate students, we learn professional ethics the same way we learn other values and habits: by emulating those we respect. It is imperative that we practice and communicate tolerance and pluralism to our graduate students and younger colleagues, not merely the professional skills we associate with the production and evaluation of research.
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Introduction

In his thoughtful and persuasive contribution to this issue, Fritz Kratochwil (2007a) rightly rejects the possibility of a foundationalist epistemology. Neither the 'order of being' nor categories of the mind can provide a universally valid Archimedean point from which to assess truth claims. The truth is not a property of the 'world' out there waiting to be discovered, but is mediated by the concepts we bring to its study. These concepts are inescapably 'fuzzy' because they are built on and derived from still other concepts. There is an infinitude of questions, moreover, and no objective way of determining which are the important ones to ask. And, if a final straw is needed, it is that most of the answers to the questions we ask are neither 'yes' nor 'no' but something in-between. What are we to do in these circumstances?

Two improper responses, Kratochwil argues, are to deny the problem or offer indefensible warrants for knowledge. A striking example of the latter is King, Keohane and Verba's, *Designing Social Inquiry* (1994), perhaps the most widely used text in graduate methods seminars in the United States (US). It espouses a foundationalist epistemology based on the arguments of Karl

Popper, arguments that have been rejected by philosophers and Popper (1972: 78–81) himself. Truth claims must be tested and defended. We must keep the notion of a 'court', but one in which Kant already understood, scientists themselves are the claimants and judges. Kratochwil further asserts that we must become pragmatists in the sense of continuing our research in spite of uncertainties and unknowns and in the absence of logically defensible warrants. The rules governing research are best described as shared practices that evolve in response to the understandings of practitioners, emergence of newer methods and cumulative 'wisdom' in the form of new and more sophisticated questions. A scientific community, as Popper (1972: chapter 3) came to realize, is a set of persons who share certain techniques such as measurement procedures, methodological commitments and presumptions of what constitutes 'good practice' in a given field.

I have no problems with this approach to social science. Based on this understanding, Steven Bernstein, Janice Gross Stein, Steven Weber and I have argued for the reformulation of social science as a set of case-based diagnostic tools (Bernstein *et al.* 2007). Kratochwil (2007b) has contributed a thoughtful chapter to the same volume in which he elaborates on some of the themes only briefly touched on in his contribution here. In neither this essay nor his chapter does Kratochwil elaborate on what science as a practice entails, beyond noting that all assertions must be backed by evidence and there must be some agreed-upon rules for weighing evidence. As he fully recognizes, neither criteria — nor any other condition of science as a practice — are unproblematic. Criteria that depend on shared understandings are unavoidable but readily open to the kinds of distortion, exploitation and tautology that can make a mockery of science. In the pages that follow, I want to explore some of these problems in the context of the practice of political science in North America.

There is a widespread acceptance among philosophers and scientists that science is a set of shared practices within a professionally trained community (Kuhn 1970; Rouse 1987). The competent speaker, not the grammarian, is the model scientist, and each discipline, like speakers of a language, is the arbiter of its own practice (Guzzini 2001). All insights and practices, no matter how well established, are to be considered provisional and almost certain to be superseded. Debates are expected to scrutinize tests and warrants as much as research designs and data. Consensus, not demonstration, determines which theories and propositions have standing. In his last decades, Popper (1972: 78–81) himself spoke of relative working truths — 'situational certainty' was the term he coined — and emphasized the critical role of debate and radical dissent among scientists.

In the physical and biological sciences, and in psychology, science as a practice works reasonably well. One reason for its success is a general consensus in these disciplines about the goals of science, appropriate research methods and rules for gathering and evaluating evidence. In psychology, where I have a quondam departmental affiliation, there is a sharp division between clinical and other fields, but universal regard for carefully constructed and implemented experimental studies. Disputes are frequent about the relative merit of particular experimental strategies, statistical tests used to evaluate data and the kind of inferences data will support. The allocation of resources among different branches of the field is naturally also contested. One only infrequently hears discussions or reads articles (and never in peer-reviewed scholarly journals) about whether psychology, or its sub-fields, are sciences. In political science, by contrast, these conflicts rage, and articles about them appear in top professional journals. We differ from psychology and the hard sciences in at least two important ways. In psychology, there is not only an overall consensus about what qualifies as a scientific method, but a general willingness to let different fields work out their own more particular understandings. The newest field in psychology — neuroscience — has been successfully accommodated. Tensions exist, to be sure, but one finds few practitioners of neuroscience scoffing at other subfields and their research methods.

In political science, there is no consensus about fundamental issues in the discipline or within most subfields. Epistemological and methodological conflicts are often more acute within fields than between them, as researchers in the same field must compete for space in some of the same journals, jobs for their students and new positions or hires at their respective institutions. Intellectual controversies are the driving mechanism of scientific progress, but can destroy the practice of science when they get out of hand. There is often a fine line between progress and degeneration, and that line is more difficult to discern when there is no consensus about fundamental epistemological and methodological questions as in the case of political science.

The obvious solution to the problem of dissensus is to allow space for each intellectual community to pursue its project and develop its own 'court' for evaluating work within its tradition. To its credit, the *American Political Science Review* under the editorship of Lee Segelman has made considerable progress towards this goal. The *Review* now encourages work from all traditions and sends manuscripts out to review primarily to people who do the same kind of work. Some other prominent journals are less tolerant and less professional in their review procedures (Hellman and Müller, 2003). Almost everyone I know — regardless of their methodological orientation — has a horror story to tell about a major journal. Having served on the editorial boards of many major journals in the field, I tend to believe these tales. At a board meeting of the *American Political Science Review* in the 1990s, I was stunned to hear a prominent comparative politics scholar condemn non-rational choice approaches as being in 'the ash can of history', and not deserving of space in the *Review*. Intolerance led to a proliferation of journals, many with particular intellectual profiles. Neoliberals gravitate to *International Organization*, realists to *International Security*, quantitative researchers to the *International Studies Quarterly*, constructivists and critical theorists to *Millennium*.

Letting a 100 journals bloom is possible in a robust economy but has its drawbacks when it is based on approach instead of subject. Intellectual fragmentation will almost certainly increase because people from one tradition will be less likely to read the work of researchers from other traditions. It encourages representatives of traditions with entrenched institutional positions to play the journal ranking game. At many US universities, chairs and deans assess publications less by their individual quality than by the alleged quality of the journals in which they are published. They rely on rankings that privilege, of course, the dominant discourses in the discipline. British and European journals like *Millennium*, *International Relations*, *International Politics*, *Review of International Studies* and the *European Journal of International Relations* are ranked lower than their American counterparts, benefiting certain epistemological and methodological orientations over others in hiring, tenure and promotion decisions.

Consensus, or at least a 'live-and-let live' approach has been made difficult by waves of hegemonic projects in the post-war era. The first of these was the so-called behavioural revolution, whose leaders sought to advance their influence within the profession by wrapping themselves in the mantle of science and depicting their adversaries as fur-skin clad holdovers from the cave era. Once in authority, they purged many major departments of faculty in public administration, constitutional law and political theory, and reshaped the intellectual profile of the remaining fields. At more than one Middle Western 'Big Ten' university where quantitative researchers have achieved institutional primacy, they have consistently excluded and occasionally purged researchers from other traditions. A former dean at one of these universities announced that there was no room for postmodernists — in which category he included constructivists — at his institution. At certain coastal schools, rational choice scholars have behaved in a similar manner. Now it appears to be the turn of the modellers. Students in a leading Ivy League department are told in their introductory methods course that modelling is the future of the discipline and that they will find it difficult to get jobs if they opt for other approaches.

Prominent and influential professors — the kind of people Kurt Lewin (1947) called 'gate keepers' — have a special responsibility for demonstrating leadership and tolerance. Many live up to this responsibility but it takes only a few who do not to subvert, or at least severely stress, the kinds of practices that are essential to an open scholarly community. One of the biggest names in the field of international relations routinely writes unsolicited letters opposing the tenure of faculty who work in different traditions from his. In one political science department in which I taught, the chair received and circulated a letter from the professor in question, an intemperate attack on a constructivist under consideration for tenure, noting that it was unsolicited. I suspected that the chair sent the letter around convinced that in our department it would backfire — as indeed it did — and help guarantee a positive vote from non-constructivists. I can readily imagine that some other, less distinguished and self-confident departments might be influenced by such a letter.

Epistemological and methodological disputes open space for another kind of abuse: the passing off of polemics as scholarship. A recent example is John Mearsheimer and Stephen Walt's 'The Israel Lobby' (2006), published in the *London Review of Books* after it was rejected by the *Atlantic Monthly*, which had originally commissioned it. The authors contend that the Israel lobby has an enormous and baneful influence in the US and was influential in bringing about the Anglo-American invasion of Iraq. Their thesis is controversial, and controversy is beneficial in a democracy, but their argument is tendentious because their scholarship is so shoddy. They are wrong about key facts — among them their assertion that Palestinian Israelis do not have the same political rights as Jewish Israelis — use evidence extremely selectively, take self-serving statements of partisan political figures at face value when it suits their needs and make some key inferences without offering evidence of any kind. It is doubtful that such an article would have been published in a prominent venue if its authors were not well-known international relations scholars with Chicago and Harvard affiliations. When self-restraint gives way to self-promotion at the price of scholarship, the public is misled and the profession as a whole suffers. It need not be this way. Over the last 70 years, the practice of foreign policy has been significantly influenced by the writings of Edward Hallett Carr (historian), Thomas Schelling (economist) and Hans Morgenthau (political scientist), none of whom debased contemporary understandings of scholarship in their respective fields to better put their points of view before the public.

Both kinds of unprofessional behaviour encourage us to reflect on the relationship between the so-called facts and values. Because facts cannot be separated from values, social understanding is inherently subjective. Research agendas, theories and methods are conditioned by culture, beliefs and life experiences. So too is receptivity to research findings. Recognition of this truth has led some scholars to interpret social science as a political process and cloak for individual and group claims to privilege. Back in the 1960s, Hans Morgenthau (1966: 71–72) warned that the government disposes of a wide range of professional rewards that help to determine the status of professors. As social science was a reflection of the power structure, it was not surprising that its findings most often justified that structure and buttressed its legitimacy (cf. Smith 2003). 'Truth itself', he warned, 'becomes relative to social interests and emotions' (Morgenthau 1966: 140–44). Morgenthau was not persuaded of the efficacy of the barriers the scientific method erects against theories and propositions that cannot be falsified or are demonstrably false.

This problem is a real one and often the fault of scientists themselves. The 19th century biological and anthropological studies of cranial capacity 'proved' the superiority of the Caucasian 'race'. Some contemporary researchers are still trying to do this with data from intelligence tests (Herrnstein and Murray 1994). Well-founded scientific claims also encounter resistance from the wider community. The theory of evolution continues to provoke widespread opposition from fundamentalist Christians with the tacit support of the Bush Administration. Claims by medical researchers that smoking is harmful and, more recently, by environmental scientists that the waste products of industrial society threaten an irreversible transformation of the environment, have encountered predictable opposition from industries with profits at stake. The tobacco companies and some major polluters support scientists who dispute these claims.

Kratochwil makes it clear that there is no such thing as a 'scientific method'. Researchers and philosophers of science argue over what constitutes adequate specification and testing, the extent to which it is possible and, more fundamentally, about the nature and goals of science. Attempts to provide definitive answers to these questions, as Karl Popper recognized, inevitably fail and risk substituting dogma for the on-going questioning, inquiry and debate that constitute the core commitment of science. These controversies render scientific truth uncertain, but working scientists, invoking the techniques and skills they have learned, for the most part have little difficulty in distinguishing good from bad science. Those cases where there is no consensus are the critical ones. Disagreement among respected practitioners suggests that there is ambiguity or inadequacy in the rules we use to evaluate scholarship, or how they are being applied in a particular case. If the former, the controversy ought to welcomed as an opportunity to debate, rethink and further hone our procedures and practices. If the latter, it can provide an opportunity to foreground our commitment to appropriate norms. Both kinds of events, if handled professionally, can help socialize graduate students and junior faculty and increase the chances that future dissensus will be addressed with similar openness and introspection.

The scientific method in many ways resembles the Bill of Rights of the American Constitution. Its meaning is also interpreted through practice. And, like the scientific method, it has not always been interpreted or applied fairly. The Bill of Rights has sometimes failed to protect political, religious and the so-called racial minorities from the ravages of prejudice. In 1898, *Plessy v. Ferguson* paved the way for the principle of separate and equal education for African-Americans, which endured until *Brown v. Board of Education* in 1954.

De facto segregated education continues to this day in some locales. *Brown v. Board of Education* reflected the changing attitudes to African-Americans, the constitution and education more generally. Another impetus was extensive social science research that demonstrated that separate education was inherently unequal. Despite continuous controversy about the meaning of the constitution and periodic failures to apply its principles in practice, there is an overwhelming consensus that the Bill of Rights — and even more importantly, the American public's commitment to tolerance — remains the most important guarantee of individual freedoms. The scientific method is an imperfect but essential bulwark against many of the same kinds of passions. Like the constitution, it ultimately depends on the ethical standards and commitments of the community it serves.

Kratochwil suggests that there is an important distinction to be made between the questions we ask and how we find answers to them. What distinguishes us from ideologues is our commitment to finding and evaluating answers by means of a scientific method — a set of evolving procedures that we agree is appropriate for defining, collecting and evaluating relevant evidence. Social scientific research agendas are shaped by political beliefs, life experiences and desires for professional recognition. There is nothing wrong with these motives. Good social science should be motivated by deep personal involvement in the burning issues of the day. Research can clarify these issues, put new issues on the agenda, and propose and evaluate the consequences of different responses. It can also influence the way people conceive of themselves, frame problems and relate to the social order.

As Max Weber (1994) recognized, the fact–value distinction and science conceived of as practice put a premium on the ethics of the scholarly community. The principles of ethics, not those of logic, govern this process of knowledge production. According to Kratochwil (2007b), 'It brings to the fore the silent presuppositions and invites us to critically reflect upon them, establishing the importance of practical reason and judgment. In a way, these considerations also provide the strongest possible rationale for pluralism, not as the second best but as the most promising strategy for producing warranted knowledge'. Values not only enable facts, but sustain our identities as scholars and enable the practice of science. In the absence of ethical commitments, we would become indistinct from polemicists and spin doctors. Ethics are in turn instantiated through practices. As graduate students, we learn professional ethics in the same way we learn other values and habits: by emulating those we respect. If we see that tolerance and pluralism routinely violated by our mentors, we may calculate that it is in our professional interest to follow suit, if only because these values seemingly involve real professional costs.

In 1934, Hans Morgenthau published his third and final pre-war book, *La réalité des normes*. It addressed the problem of sanctions and its argument was deeply influenced by, but highly critical of, Hans Kelsen's abstract approach to international law. Morgenthau submitted it as his *Habilitationschrift* at the University of Geneva, where it was rejected by the first examination board. A second board accepted the manuscript primarily because the ever magnanimous Kelsen made such a strong statement on Morgenthau's behalf (Frei 2001: 45–9). If Hans Kelsen could act in this way under the fast spreading penumbra of the Nazi regime, those of us who live in supportive political and economic environments should be ashamed to act any less honestly.

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