REVIEW ESSAYS

RETURN TO POLITICS

Perestroika and Postparadigmatic Political Science

RETURN TO REASON by Stephen Toulmin. Berkeley: University of California Press, 2002. 243 + x pp.

MAKING SOCIAL SCIENCE MATTER: WHY SOCIAL INQUIRY FAILS AND HOW IT CAN SUCCEED AGAIN by Bent Flyvbjerg. New York: Cambridge University Press, 2001. 204 + x pp.

UNTHINKING SOCIAL SCIENCE: THE LIMITS OF NINETEENTH-CENTURY PARADIGMS, 2d ed., with a new preface by Immanuel Wallerstein. Philadelphia: Temple University Press, 2001. 286 + xii pp.

PARADIGMS EXPLAINED: RETHINKING THOMAS KUHN'S PHILOSOPHY OF SCIENCE by Erich von Dietze. Westport, CT: Praeger, 2001. 183 + x pp.

BEYOND THE IVORY TOWER: INTERNATIONAL RELATIONS THEORY AND THE ISSUE OF POLICY RELEVANCE by Joseph Lepgold and Miroslav Nincic. New York: Columbia University Press, 2001. 228 + xii pp.

SCHOOLS OF THOUGHT: TWENTY-FIVE YEARS OF INTERPRETIVE SOCIAL SCIENCE by Joan W. Scott and Debra Keates. Princeton, NJ: Princeton University Press, 2001. 403 + x pp.

Political science is receiving increased critical scrutiny as a discipline these days, and much of that scrutiny is coming from within its own ranks. A growing number of political scientists have signed on to a movement to challenge the dominance of positivistic research, particularly research that assumes political behavior can be predicted according to theories of rationality and that such predictions underwrite cumulative explanations that constitute the growth of political knowledge. The movement to question such thinking is most dramatically represented in the network of scholars that has

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developed in response to the eponymous Mr. Perestroika letter that raised this challenge in poignant terms when it first circulated over the Internet back in October of 2000.

A loose collection of political scientists, from graduate students to senior scholars, Perestroikans do not always themselves agree on which features of the dominant approach they want to critique—some focus on the overly abstract nature of much of the research done today, some on the lack of nuance in decontextualized, large sample empirical studies, others on the inhumaneness of thinking about social relations in causal terms, and still others on the ways in which contemporary social science all too often fails to produce the kind of knowledge that can meaningfully inform social life. As a group, the Perestroika movement, however, has championed methodological pluralism, charging that exclusionary practices have made graduate education less hospitable to historical and field research, qualitative case studies, interpretive and critical analysis, and a variety of context-sensitive approaches to the study of politics. The major journals of the field, Perestroikans argue, have become preoccupied with publishing research that conforms to overly restrictive scientistic assumptions about what constitutes contributions to knowledge of politics. Perestroika is a healthy development for political science and all other social sciences as well, opening for reconsideration these very questionable assumptions about what constitutes political knowledge in particular and social knowledge in general.

From the vantage point of many Perestroikans, the dominant paradigm in the field operates according to the following hierarchy of assumptions: (1) political science exists to help promote understanding of the truth about politics; (2) political science research contributes to this quest by adding to the accumulation of an expanding base of objective knowledge about politics; (3) the growth of this knowledge base is contingent upon the building of theory that offers explanations of politics; (4) the building of theory is dependent on the development of universal generalizations regarding the behavior of political actors; (5) the development of a growing body of generalizations occurs by testing falsifiable, causal hypotheses that demonstrate their success in making predictions; (6) the accumulation of a growing body of predictions about political behavior comes from the study of variables in samples involving large numbers of cases; and (7) this growing body of objective, causal knowledge can be put in service of society, particularly by influencing public policy makers and the stewards of the state.

This paradigm excludes much valuable research. For instance, it assumes that the study of a single case is "unscientific," provides no basis for generalizing, does not build theory, cannot contribute to the growth of political knowledge, and, as a result, is not even to be considered for publication in the leading journals and is to be discouraged as a legitimate doctoral dissertation project.[1] While there have always been dissenters to the drift toward "large-n," quantitative research in service of objective, decontextualized and universally generalizable truth about politics, there is a good case to be made that the dissenters have increasingly been marginalized as the center of gravity of the discipline has drifted more and more towards reflecting these core assumptions about knowledge of politics.

Perestroika in political science has at a minimum provided an opportunity to halt this drift by questioning these assumptions and posing alternatives. At its best, the Perestroikan impulse creates the possibility to question the idea that political science research exists as a unitary enterprise dedicated to the accumulation of an expanding knowledge base of universal, decontextualized generalizations about politics. In its place, Perestroika would put a more pluralistic emphasis on allowing for the blossoming of more contextual, contingent, and multiple political truths that involve a greater tie between theory and practice and a greater connection between thought and action in specific settings. Perestroika lays open the possibility that political science could actually be a very different sort of discipline, one less obsessed with proving it is a "science" and one more connected to providing delimited, contextualized, even local knowledges that might serve people within specific settings.

Such a political science would therefore have very different standards as to what counts as meaningful political knowledge. It would, for instance, be less interested in studying such things as "development" or "modernization" in the abstract as objects of inquiry on their own, as when economics becomes the study of "the market" as opposed to the examination of the variety of markets. Instead of focusing solely on "development" or "modernization" per se, political science would be more about studying change in particular countries or using concepts like development or modernization in contextually sensitive ways to compare change in different countries.

This alternative political science would also be less preoccupied with perfecting method or pursuing research strictly for knowledge's own sake. As Rogers Smith has underscored, "knowledge does not have a sake"; all knowledge is tied to serving particular values.[2] Therefore, this new political science would not be one that is dedicated to replacing one method with another. Instead, such a discipline, if that word is still appropriate, would encourage scholars to draw on a wide variety of methods from a diversity of theoretical perspectives, combining theory and empirical work in different and creative ways, all in dialogue with political actors in specific contexts. Problem-driven research would replace method-driven research.

My own version of Perestroika would build on this problem-driven, contextually-sensitive approach to enable people on the bottom working in dialogue with social researchers to challenge power. My Perestroikan-inspired political science would be open to allowing ongoing political struggle to serve as the context for deciding what methods will be used in what ways to address which problems. This new dialogic political science would not find its standards for credible scholarship in arcane vocabularies and insular methods that are removed from local contexts and seem objective but are not without their own agendas. Instead, my political science would find its standards of knowledge in asking whether scholarship can demonstrate its contributions to enriching political discourse in contextualized settings.

Such a new political science, however, would at the same time recognize the risks associated with connecting to ongoing politics. It would guard against losing its critical capacity for the sake of achieving relevance. It would retain its critical capacity while in dialogue with ongoing political struggle, providing therefore a powerful "critical connectedness"—what Charles Lemert has called "global methods."[3] It would however be less interested than the old political science in serving the state with objective knowledge. It would forego the dream of scientific grandeur that aims to produce socially useful, decontextualized, objective knowledge, independent of politics.

A political science that forgoes the dream of a science of politics in order to dedicate itself to enhancing the critical capacity of people to practice a politics is, for me, an exciting prospect. A political science that does this to enhance the capacity to challenge power from below is all the more exciting. I would argue that the new political science would not just be more politically efficacious but also more intelligent, offering more robust forms of knowledge about politics.

Important philosophical justification for this Perestroikan-inspired alternative to political science can be found in Stephen Toulmin's magisterial book *Return to Reason*. Tolumin's book builds on his life's work in the philosophy of science, ordinary language philosophy, rhetoric, and the analysis of practical arts. It is written with an erudition rarely seen. Its sweeping panorama places the problem of scientism in the social sciences in a historically rich context. His primary argument is that since Descartes, and especially since Kant, Western philosophical thought has been increasingly enchanted with the dream of realizing universal rationality as the highest form of knowledge and the basis for truth. Yet, Toulmin stresses that it was only relatively recently with the twentieth century that this dream came to be ascendant as the hegemonic ideal for organizing knowledge practices in the academy in general and the social sciences in particular. The dream of universal rationality as the gold standard for objective knowledge of truth became ascendant with the rise of modern science and the growing influence of the argument that science, as best represented by particular natural sciences, was the best route toward achieving universal rationality, objective knowledge, and truth with a capital *T*. In its wake, the modern university was built, and then increasingly compartmentalized into the multiversity, with growing numbers of specialized disciplines, each increasingly preoccupied with perfecting its own methodological prowess as to how to best arrive at truth.

Toulmin's main argument is that this derangement was a long time coming involving arduous efforts as part of a campaign that achieved hegemonic status relatively recently only in the twentieth century. For Toulmin, before then, much of the history of modern Western philosophy can be understood in terms of striking a balance between universal rationality and contextual reason. The campaigners had to confront time and again the problem that what is universally rational may not be reasonable in particular situations. For centuries, the dream of universal rationality was counterbalanced with the practice of everyday reason. Humans experienced their lives and made sense of them between these poles. Yet, the rise of modern science increased the emphasis on the production of objective knowledge in the most abstract and generalizable terms possible. Theory was everything and practice was subordinated to it. Theory-driven modern science's preferred discourse was mathematics that, since Descartes, was the ideal idiom for expressing in abstract and generalizable terms the objective knowledge of universal rationality. Sciences began to be ranked by the degree to which they could produce universal rationality as expressed in mathematical terms. Physics envy spread. Then again, in the twentieth century, science in general became ascendant as the best way to produce such knowledge. The fact that "science as use" was conflated with "science as truth" helped greatly in vaulting science to the forefront as the supposed superior road to truth as dramatic developments in technology were increasingly showcased as proof positive that science not only could do things but also knew the truth of what it was doing.[4]

The idea that there is a distinctive scientific method that all sciences shared began to gain greater currency, and all other forms of knowledge production came to be seen as inferior to the degree that they failed to conform to the dictates of the scientific method. Physics envy morphed into science envy with the social sciences increasingly miming what was seen as the methods of the natural sciences in order to lay their own claim to scientific legitimacy. At this point, the precarious balance between emphasizing abstract rationality and everyday reason was now seriously upset, and universal rationality in service of abstract generalizable knowledge stated in the mathematical terms was seen as the only real form of truth worth taking seriously. The wisdom of everyday reason was increasingly relegated to folklore or to applied fields and it itself started to become a popular area of study, not so much for the truths it afforded but as an object of inquiry that could be used as data to test various hypotheses about which types of people in what cultures tended to think in what ways and why. The science of wisdom, as it were, whether studied in anthropology or philosophy, was a sure sign that rationality had triumphed over reason.

Toulmin effectively illuminates the rise of universal rationality first in philosophy from Descartes on, then in the sciences, but also in the social sciences and applied fields as well. He highlights how a consistent bias in favor of abstract knowledge of universal rationality continued to work its way across disparate realms of knowledge production. Toulmin is not a social scientist and in the past he has written about almost everything but. Yet, *Return to Reason* demonstrates a real feel for how the social sciences rose in the shadow of the preoccupation with the abstract knowledge of universal rationality and how that played out in selected fields. This is a wide-ranging book, written in a very inviting conversational style, from an Olympian vantage point; however, this is no mere dilettante rumination on the misguided project John Dewey called the "quest for certainty."

My favorite example in the entire book is Lancelot Brown, the famous nineteenth-century landscaper, who was also popularly known as "Capability" Brown because the designs for his quintessentially British gardens developed out of the available landscape, rather than, as with the French style, imposing an idealized image of a garden on the landscape and forcing it to conform to that ideal. Toulmin uses Capability Brown to demonstrate how British empiricism in contrast to French idealism very pragmatically offers a way to "play it as it lays" and work with what is available within any particular context rather than trying to impose abstract, universal ideals on situations. In Toulmin's hands, Capability Brown effectively illustrates the value in a return to reason as a counterbalance to the excessive emphasis on abstract knowledge of universal rationality.

Toulmin is most convincing when he notes that for the social sciences, the scientistic preoccupation with universal rationality was a particularly troubling turn. His primary case in point is the popular one—economics. He calls it the "physics that never was." Toulmin effectively shows that the history of the development of economics as a discipline involved the progressive elimination of historical and social considerations, increasingly decontextualizing its subject matter in ever more abstract and mathematical terms to produce its own universal rationality of market-related behavior. The application of abstract economic models to problems of public policy increasingly became the vogue. Theory dictated to practice in often-ruthless terms, particularly when First World lending institutions prescribed "structural adjustment" or "shock therapy" policies that required nation-states to retrofit their economies to conform to the models' requirements. The central problem here for Toulmin, as for so many others, is that these sorts of applications all too often mistook contextually specific understandings of predictable market behavior as universally applicable, abstracted them from those contexts, and imposed them in social settings, cultures, and political systems where they make very little sense at all, and did so all too often at great cost to the well-being of the people who were supposed to be helped by such "development" schemes. Toulmin counters these disasters of "top-down" theory-driven economic practice with the example of Muhammad Yunus, who works from the "bottom up" with his Grameen Bank that provides small loans in over 50,000 Bangladeshi villages so that local people can develop "appropriate" enterprises fitted to their communities, values, and local practices. Yunus, a professional economist, is quoted by Toulmin as saying, "If Economics [as it stands] were a social science, economists would have discovered what a powerful socio-economic weapon credit is. . . . If we can re-design economics as a genuine social science, we will be firmly on our way to creating a poverty-free world" (p. 65). Toulmin ends his tale of the disenchantment of economics by saying, "This message does not, of course, affect Economics alone: similar traditions in the other human sciences have led to similar misunderstandings and errors of practical judgment" (p. 66).

For Toulmin, the antidote to the twentieth-century hegemony of universal rationalism is respect for everyday reason, as practiced in contextualized settings, in ways that can not be legislated by theory from the top down and are open to living with the uncertainty that such situated knowledges must accept as the ineliminable contingency of what Toulmin calls the "clinical arts." The social sciences are, for Toulmin, more akin to "applied sciences," but "applied" mischaracterizes the situation, suggesting that theory is applied in practice—an idea most significantly popularized by the reports Abraham Flexner wrote on professional medical education in 1913 and on social work in 1915. Instead, drawing on the work of Donald Schon and others, Toulmin wants us to learn that social theory is better seen growing out of practice, as an intensification of those meditative moments in social practice. Toulmin sees the need for social sciences, operating ever more beyond disciplinary boundaries, to be more about teaching practical wisdom, *phronesis*, as Aristotle termed it, as something that grows out of an intimate familiarity with the contingencies and uncertainties of various forms of social practice embedded in complex social settings. We need therefore to revise the standards for acceptable research methodologies, reincorporating context-sensitive research, such as case studies, not to dictate what is to be done but more to inventory infinitely unique cases from which social actors can learn to appreciate the complexities of social relations and practice various social crafts all the more effectively. Social science would be more like bioethics than like moral philosophy, basing itself on the insight that Toulmin provides when he notes that bioethics owes very little to moral philosophy, which, as theory, is incapable of specifying from the top down most bioethical decisions, that instead grow from the bottom up, in unlegislated form, varying with contexts, negotiating ambiguity, living with uncertainty, and still doing the necessary work of determining life and death every day. Case study research for bioethicists and many others, often conducted in dialogical and collaborative relations with the people being studied, can help enable social actors to use knowledge to address their problems. Such participatory action research would for Toulmin be more fitting of a real social science that better understood its relationship to its contingent, contextual and ever so thoroughly social subjects. For Toulmin, the return to reason will then best be evidenced in the social sciences when wisdom of this sort is taught not as an object for scientific scrutiny, as evidence of cultural variation, but as the very goal of knowledge production itself.

In his introduction, Toulmin cites one book as a sign that some social scientists are tapping into the themes he emphasizes. The book is *Making Social Science Matter: Why Social Inquiry Fails and How It Can Succeed Again* by Bent Flyvbjerg. It too is a remarkable book that adds fuel to the idea that perhaps Perestroikans are part of broader academic currents. Flyvbjerg's book takes us one step further down the road that Toulmin has laid out for us and it does it eloquently with its own impassioned argument that not only demonstrates what is wrong with the social sciences today but provides a detailed list of examples of how a phronetic social science is already possible and already happening here and there among the detritus of contemporary social science.

Flyvbjerg's book is such a breath of fresh air; he creatively uses Aristotle, Nietzsche, Foucault, Bourdieu, and others to make many of the same points as Toulmin, but in his own distinctive way. He fuses an Aristotelian concern for *phronesis* with a Marxist concern for *praxis*, adding in a Foucauldian critique of Habermas's preoccupation with consensus to demonstrate that a phronetic social science that can offer a praxis worth pursuing is one that would work within any contextualized setting to challenge power, especially as it is articulated in discourse. Flyvbjerg's phronetic social science would be open to using a plurality of research methods to help people challenge power more effectively.

Flyvbjerg begins where Toulmin left us, in the present with social science hopelessly lost seeking to emulate the natural sciences with a quest for theory-driven abstract knowledge of universal rationality. Flyvbjerg adds a compelling critique that demonstrates convincingly that there is no symmetry between natural and social science in that natural science's interpretive problems are compounded by what Anthony Giddens called the "double hermeneutic" of the social sciences. By virtue of its distinctively human subject matter, the social scientists inevitably are people who offer interpretations of other people's interpretations. And the people being studied always have the potential to include the social scientists' interpretations in theirs, creating an ever-changing subject matter and requiring a dialogic relationship between the people doing the studying and the people being studied. For Flyvbjerg, this situation unavoidably means that there can be no theory for social science in the sense that social science needs to forego the dream that it can create time-tested theories of a static social reality.

As a result, argues Flyvbjerg, the social sciences should not seek to emulate the natural sciences. In such a comparison, the social sciences will always fare very poorly, being seen as inferiors incapable of producing knowledge based on tested theories that can evince prediction of the worlds they study. Instead, Flyvbjerg feels that the social sciences are better equipped to produce a different kind of knowledge—*phronesis*, practical wisdom—that grows out of intimate familiarity with practice in contextualized settings. Local knowledges, even tacit knowledges, that cannot be taught a priori, grown from the bottom up, emerging out of practice, foregoing the hubris of seeking claims to a decontextualized universal rationality stated in abstract terms of false precision. Add a sense of praxis, seeking the ability to push for change, leaven it with an appreciation of the ineliminable presence of power, and this phronetic social science can help people in ongoing political struggle question the relationships of knowledge and power and thereby work to change things in ways they might find more agreeable and even satisfying. Such a phronetic social science can contribute to what I have called "radical incrementalism" or the idea that praxis involves promoting change for the least advantaged by exploiting the possibilities in current political arrangements.[5]

Yet, what is most exciting is that Flyvbjerg not only goes beyond critique to offer a positive program; he demonstrates it in detail, pointing to a rich variety of contemporary work from that of Bourdieu, to Robert Bellah, to his own work. Flyvbjerg's research spanned fifteen years and focused on a major redevelopment project initiated by the Danish city of Aalborg, where Flyvbjerg continues to teach urban planning. His research on the project evolved over time, quickly becoming more phronetic as he came to appreciate how social science could make real contributions to the ongoing dialogue over the city's redevelopment efforts once his research was retrofitted to the specific context in which the issues of development were being debated. At first, Flyvbjerg was put off that decision makers rejected the relevance of studies about education elsewhere and he came to be concerned with power. Without saying so, he evidently took to heart the idea that he had to work harder to produce research that, even while it challenged power, demonstrated its sensitivity to the Aalborg context. In the process, power relations got challenged in a very public way, the framing of the development agenda got successfully revised to include more grassroots concerns, an ongoing dialogue with participants in the redevelopment process got richly elaborated, and social science research that gave up an interest in proving grand theories became critical to a very robust discourse on urban planning. As a result, the Aalborg planning project gained increased visibility as a successful project that went out of its way to democratize its decision making in part by allowing social science research to help keep it honest, open, and collaborative.

Phronetic social science such as this would be very different than the social science that predominates today. For Immanuel Wallerstein, that would be a good thing, at least for the most part. A second edition of his 1991 work *Unthinking Social Science: The Limits of Nineteenth-Century Paradigms* was released in 2001. Revised to include a new preface, these essays demonstrate the consistency in Wallerstein's thinking even as he continues to add new concerns, as he did in his 1999 collection, *The End of the World As We Know It: Social Science for the 21st Century.* For over thirty years, Wallerstein has championed his World-Systems Analysis not just as a critique of global capitalism's 500-year climb to ascendancy but also, as the titles of his more recent books indicate, as a critique of the epistemological assumptions that undergird that system, especially in terms of the implications for social science. His preoccupation with social science's role in the reproduction of the World System is not some idle theoretical point. In 1996, as chair of the Gulbenkian Commission on the Restructuring of the Social Sciences, he issued, with the nine other international committee members, a report, *Open the Social Sciences*, which has since been published in twenty different languages. For Wallerstein, when the contemporary social sciences began to be formed in the nineteenth century, they were organized to produce specialized knowledges that would be consistent with the World System's need for a universal rationality that would rationalize its dominance across the globe, over a diversity of cultures and wherever capitalism sought hegemony. Wallerstein places the quest for universal rationality by the social sciences on a very dramatic world-historical stage and in a most critical light.

World-Systems Analysis provides, therefore, not just a way of understanding how the capitalist core metropole subjugates the periphery for resource extraction and expansion of commodity exchange relationships. World-Systems Analysis, more profoundly, is for Wallerstein a way of critiquing the knowledge assumptions that inform the World System. World-Systems Analysis is first and foremost a way of unlearning or "unthinking" the social sciences that serve the World System. Such an unthinking involves breaking down disciplinary boundaries, rejoining normative philosophy and empirical research, understanding the production of knowledge in terms of its relationship to the structural imperatives of the World System, and finally, and perhaps most importantly, appreciating that the World System is increasingly in crisis and that its grip on knowledge production is loosening, thereby opening opportunities in our time to contribute to its demise by developing the World-Systems critique.

Wallerstein has much to offer the campaign to revitalize social science and move it in politically protean directions. I am a bit hesitant, however, to endorse what Wallerstein calls in a moment of modesty the World System "hypothesis." As historically rich and politically trenchant a critique as is Wallerstein's, it still risks merely replacing one paradigm, as he calls it, with another. I have two problems with the Wallerstein call for a paradigm shift. First, his World-Systems paradigm tries but fails to resolve its own contradictions regarding the relationship of structure to agency, with structure overdetermining agency and agency largely becoming the pantomime of structural insistences. Second, I am increasingly convinced that social science is ideally better seen as postparadigmatic rather than as organized by one paradigm or another.

For me, the idea of paradigm has no relevance to social science except as its own form of mimicry. Paradigmatic research is what natural scientists do. Social science for the reasons provided in this essay ideally should not be seen as amenable to being organized paradigmatically in any strict sense of the term. The strict sense of the term is of course subject to intense debate starting with its author Thomas Kuhn. Erich von Dietze makes this clear in *Paradigms Explained: Rethinking Thomas Kuhn's Philosophy of Science*.

Dietze provides a very clearly written, extremely systematic and comprehensive recitation of Kuhn's theory of paradigm, the critics' complaints, Kuhn's responses, and finally Dietze's own assessment that suggests that Kuhn's admittedly brilliant and important work failed to shore up paradigm as a sustainable concept for understanding the framing and structuring of knowledge production in the natural sciences. Dietze concludes by suggesting a "coherence theory of evidence" as a replacement for the concept of paradigm.

Paradigm, Dietze notes, started in Kuhn's *The Structure of Scientific Revolutions* and served as the lynchpin for his theory that in any one field "normal" science was periodically punctuated by "revolutionary" science that induced a conceptual transformation of the subject matter and initiated new ways of studying it. Dietze notes that from the beginning Kuhn struggled to respond to critics by relying in particular on two additional concepts—exemplar and disciplinary matrix. An exemplar is an exemplary example, usually in the form of an innovative experiment or analytical treatment, that by its very success implied a particular way to understand and study the subject in question. To the extent that they are contingent upon exemplars, Kuhn's paradigms are to a great degree therefore implicit in the very act of "learning by doing" in a contextually sensitive fashion, making them in their own way forms of phronetic reasoning, learned and elaborated through situated practice.[6] The disciplinary matrix is the social, institutional, and organizational side of the process where cohorts of scientists were introduced to the paradigm and encouraged to practice normal science according to how they were socialized by the disciplinary matrix. It therefore is as if paradigms had both material and symbolic dimensions. Through learning to practice exemplars, graduate students became normal scientists. Natural science was its own form of *phronesis*, if only so as to practice natural scientific reasoning in the context of actually doing it.

Once a new exemplar arises that is seen as providing a preferred understanding of the subject matter in ways that the prevailing paradigm cannot, scientists have to learn the new rules for study implied by the new exemplar. Translation into the old system of study would not work because the paradigms were, in Kuhn's mind, to an ineliminable degree, by definition, incommensurable. Each paradigm's evidence is of a nature that it always has to be evaluated by its own standards, in its own context, making it impossible to use evidence to decide if one paradigm was better than another. For Kuhn, knowledge does not grow cumulatively with one paradigm building on another. We should never say that we now know more or better only, that with a paradigm change we know differently. What was most radical then about Kuhn's notion of paradigm is that it unmasks the necessary fiction that the twentieth century metastory of science teaches us about the growth of objective knowledge. This Kuhnian claim led critics to charge him with relativism on the grounds that Kuhn seemed to be implying that one paradigm might be as true or right as another. Kuhn spent much of the rest of his life responding to critics with clarifications that more often than not moved him away from the relativistic implications of his work.

Dietze is of the opinion that Kuhn's responses did not salvage the concept of paradigm successfully. Dietze missteps here by going beyond Kuhn in accepting the charge of relativism as posing a legitimate problem in need of solution. He therefore concludes by trying to make the best of what he sees as an untenable situation by offering an alternative—the coherence theory of evidence. This position builds on the Kuhnian insight that empirical adequacy alone is not sufficient for theory adjudication. Underdetermined by evidence, theories need more than facts to receive affirmation within a field of study. There are "superempirical" considerations, including in particular "coherence virtues" appropriate to all knowledge claims regardless of their theoretical premises. Beyond evidence, a knowledge claim must fit logically with related claims of a theory in ways that are consistent with the generic standards of how interrelated knowledge claims ought to be seen as fitting logically together. Dietze defends this perspective as one that salvages Kuhn's theory of paradigm from the charges of relativism. Dietze concedes, however, that the superempirical and transcontextual criteria for coherence among interrelated knowledge claims are themselves constraining conditions that need to be examined for their potential to exclude certain understandings as illogical when they are in fact just logical according to a different standard.

Dietze provocatively notes at the end of his book that Kuhn's relativism is really a product of a failure to break more fully with logical positivism. Kuhn took too seriously logical positivism's press clippings and ended up showing that its supposed objective facts were more context dependent, value laden and theory laden than it was prepared to admit. Kuhn was right about that, but for Dietze, Kuhn makes his case in too aggressive a fashion, sliding into a relativism that he himself was reluctant to endorse.

I leave it to others to decide whether paradigm or coherence theory helps more to understand that science is not objective, does not produce cumulative knowledge, and does not lead to universal rationality or absolute truth. I would suggest however that Dietze prematurely accepts the charges of relativism as pointing toward a legitimate problem. Dietz tries to save Kuhn by standing him on his head and making the issue of relativism the problem it is not. As Richard Rorty has reminded us, when someone calls you a relativist, the better responses include saying thank you for highlighting your well-founded commitment to challenging the illegitimacy of the master narrative of science.[7]

Yet, Dietze joins others in appropriately leaving to the side whether paradigm has relevance to understanding social sciences. Given the subject matter, there ideally should be no normal science in any one of the social sciences. Regardless of the fact that both natural and social science are forms of learning in context that produce value-laden facts, social life, as opposed to the objects of natural scientific inquiry, involves multiple interpretive lenses offering a cacophony of competing perspectives emanating from its origins in conscious, thinking human beings. Under these conditions, no one form of disciplined study of social life should be organized paradigmatically to exclude the consideration of multiple perspectives.

Ironically, the objectivists in social science themselves most often resist the application of Kuhn's idea of paradigm to their fields since it implies that their scientific work was value laden. I agree with them about resisting its application to social science but for the different reason that multiple perspectives are inherent in the subject matter. It is a sad irony then that even though the objectivists resist paradigm, methodological hegemony by objectivists is the reality today in social sciences such as political science and economics. This is a doleful reminder that paradigms involve the very human power struggles of a disciplinary matrix as much as they do the practices of inquiry demonstrated in exemplars. Paradigms can be imposed socially even where they are most inappropriate intellectually.

Yet, it is one thing to issue a postparadigmatic call for a phronetic social science and it is another to emphasize the idea that social theories should serve as the foundation for practice. The latter is exactly what Joseph Lepgold and Miroslav Nincic do in *Beyond the Ivory Tower: International Relations Theory and the Issue of Policy Relevance*. Lepgold and Nincic know the field of international relations theory inside and out. Their book is well written and their thesis is provocative. They argue that academic scholarship on international relations has become increasingly technocratic and disconnected from the practice of international politics. The authors succinctly overview the field's developments in this regard. Eventually they point to several strands of contemporary scholarship on international relations, which they suggest indicate ways in which academics studying international relations can make their work more relevant to, and even serve as a foundation for, international relations practice.

While I find the critique of the field's increased technicism to be persuasive and consistent with the arguments made by Toulmin and others, their examples of work that is connected to international politics are less so. The authors provide two examples of international relations theory that have usefully connected to international politics—"interdemocratic peace" research on how democracies are more peaceful with one another than other political systems, and international institutions research on how institutionalized settings create conditions and incentives for cooperation and conflict. The analysis provided on global peace never really demonstrates effectively how such theorizing provides, any more than any other political ideology, the explicit basis for practicing international relations politics by nation-states or nongovernmental actors. And international institutions research too does not come across as the firm foundation for practice that the authors suggest, though with qualifications, it could be. Both examples are from within the hegemonic camp of objectivist political science and both share the hegemonic camp's insensitivity to context. Yet, more important, the very argument that international relations theory should overcome its alienation from practice by reestablishing itself as a foundation for practice strikes me as misguided. The solution to overcoming the alienation of international relations scholarship from international politics is not to reinscribe epistemic privilege and the idea that theory is fundamental and should serve as a blueprint for practice, or even more concretely for specific public policies. This foundationalism simply recreates the problem in the first place, encouraging scholars to privilege theory as some legislating and authorizing activity that it is not. Such foundationalism reflects a lingering commitment to universal rationality and fails to appreciate the contextualism that Toulmin and Flyvbjerg emphasize as central to understanding and contributing to social and political life.

A better approach would be a phronetic social science that sees theory as growing out of the practices in specific contexts while still working to achieve critical distance on prevailing understandings of those political practices. This would be the beginning of research that could better help ordinary people—nongovernmental actors—challenge power internationally.

Phronetic social science already exists; it is just not organized or recognized as such, existing here and there where scholars come to it on their own. It also has multiple sources of intellectual sustenance. One prominent institution that has at times successfully promoted social research that makes a phronetic approach plausible is the School of Social Science in the Institute for Advanced Study at Princeton, which from its inception in 1973 sought to encourage innovation in the social sciences by promoting interdisciplinary work along interpretive and critical lines. Independent of any university, including Princeton, the School has survived handsomely, celebrating its twenty-fifth anniversary symposium in 1997 with the publication of *Schools of Thought: Twenty-Five Years of Interpretive Social Science*, edited by Joan Scott and Debra Keates. This collection includes essays by twenty prominent philosophers, theorists, and social scientists, each responding to the symposium's theme to address changes in their field of study in the last twenty-five years. As Clifford Geertz, the first appointed professor at the School, notes in his introduction, the School was always meant to be a place that was open to innovation and resisted promoting any one program or paradigm. It did, however, seek to challenge the orthodoxy of scientistic social science and it stressed interdisciplinary work that contributed to what came to be called "interpretive social science."[8]

The essays in this volume are a rich set of diverse pieces. Some, like David Apter's piece on empirical theory in political science, tack more to a historical line overviewing major developments in a field. Others, like William Sewell's piece on the rise and decline of social history, tell interesting stories about significant events that were part of those developments. Still others, like Joan Scott's piece on the changing understanding of "history," illustrate how a particular form of interpretive social science makes an important difference in the practice of social inquiry in specific areas. And still others, like Anna Tsing's piece on globalization, examine a substantive issue and its implications for a discipline like anthropology. Together these essays comprise a rewarding collection, suggesting the significance of what several of them refer to as the "interpretive turn," the subsequent linguistic turn, and all the other related turns that followed, once the hegemony of scientistic social science had begun to be seriously challenged.

Yet, this collection rarely points to an instance of what I am calling here with Flyvbjerg phronetic social science. The interpretive turn helps provide resources for developing such work; it does not by itself constitute that work. Will the road ahead take more turns? That depends to a large extent on the plays of power, in the academy, the government, the think tanks, and anywhere else knowledge and power are being "disciplined."

NOTES

1. The Perestroika listserv is replete with examples of dissertation advisers and journal editors who as a rule will not consider case studies. The archives of the listserv can be accessed by e-mailing perestroika_glasnost_warmhome@yahoo.com.

2. See Rogers M. Smith, "Should We Make Political Science More of a Science or More about Politics?" *PS* 35 (2002): 199-201.

3. Charles Lemert, *Social Things* (Lanham, MD: Rowman & Littlefield, 2001), 176-206.

4. "Science as use" versus "science as truth" is from Jacqueline Stevens, "Symbolic Matter: DNA and Other Linguistic Stuff," *Social Text* 20 (spring 2002): 105-36.

5. See Sanford F. Schram, *Praxis for the Poor: Piven and Cloward and the Future of Social Science in Social Welfare* (New York: New York University Press, 2002).

6. Etymologically, *paradigm* is from the Greek *paradeiknunai*: literally "to show beside," from *para*, "alongside," and *deiknunai*, "to show," implying learning by imitating an example.

7. Richard Rorty, "Thomas Kuhn, Rocks, and the Laws of Physics," *Common Knowledge* 6 (1997): 6-16; John G. Gunnell, "Relativism: The Return of the Repressed," *Political Theory* 21 (1993): 563-84.

8. It is telling that in order to mount its challenge to orthodoxy, the School of Social Science at the Institute for Advanced Study had to declare its independence from the supposedly independent ivory tower of the university, further complicating the issue of what it takes for social science to challenge power.

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