What Can We Know? How Do We Know?

Richard Ned Lebow

This book was conceived in the course of a long, wet afternoon in Columbus, Ohio. Inside, in a small, brightly lit auditorium, enthusiastic graduate students took turns presenting papers that were the product of a year-long seminar intended to help them develop dissertation proposals. Their words fell on the ears of their fellow students and six professors in international relations. Their presentations, although diverse in subject, were remarkably uniform in structure. They began by laying out a few propositions, went on to describe the data sets or cases that would be used to test these propositions and ended with a discussion of preliminary research findings. The professor who had taught the student participants exuded an avuncular aura throughout the proceedings, and my colleagues, who were encouraged to interrogate the students, largely queried them about their research design and choice of data. For the most part, the students provided competent answers to these questions.

Another colleague and I raised the tension in the room by asking each of the students in turn why they had been drawn to their subject matter. What puzzle or policy concern animated them? What light might their preliminary findings throw on that puzzle or problem? Their responses were largely unsatisfactory. Two students were flummoxed. One insisted he was “filling a gap in the literature.” Two more defended their choices in terms of the availability of data sets. Another noted that his subject was a “hot topic,” and that a dissertation on it would increase his chances of landing a good job. Only one student justified her research with reference to her sense of urgency about a real world problem: regional conflict.
When pushed, she nevertheless found it difficult to describe what implications her propositions might have for the trajectory of these conflicts or the efforts to ameliorate them. Another colleague, also dissatisfied, questioned the choice of two of the data sets, suggesting that they lumped together cases that had played out in quite different political-historical contexts. After the session, two of my colleagues, including the professor in charge of the seminar, told me I had been too hard on the students.

Two other colleagues were supportive, one of whom, from another field, had heard reports about what had transpired. The three of us agreed that our students, beginning in their introductory scope and methods class, were encouraged to privilege quantitative over qualitative research and choose dissertation topics based more on their feasibility than on their theoretical or substantive importance. They had a sophisticated understanding of research design—but only in so far as it pertained to the strictures of statistical inference. Despite—or perhaps, because—of three years of graduate training, they were correspondingly uninformed about the more general problems concerning evidence. Most gave the impression that it was just “out there” waiting for them to harvest, and failed to realize the extent to which it is an artifact of their theories. They were largely insensitive to context and the understandings of the actors, and how they might determine the meaning of whatever observations they as researchers made. All their proposals conveyed a narrow understanding of science as a form of inference whose ultimate goal is predictive theories. They were not particularly interested in causal mechanisms, let alone in other forms of political understanding such as the constitution of actors.

We agreed that epistemological and methodological narrowness, although pronounced at Ohio State University, was common enough in the discipline to arouse general concern. In our view, the use of King, Keohane, and Verba, *Designing Social Inquiry* (KKV), as a core reading in so many scope and methods courses could only make the situation worse. My colleague, whose reputation was based on “mainstream” quantitative research—a shorthand term I use to describe those who more or less accept the unity of the sciences—felt just as strongly as I did. He considered many of KKV’s recommendations for collecting and evaluating data quite sensible, but he rejected its epistemological foundations as seriously flawed, its characterization of science as ill-informed, relegation of qualitative research to second-class status as unacceptable, and its almost exclusive focus on the construction and analysis of data sets as regrettably narrow. Conversations with a few other dissatisfied colleagues at Ohio