

REVIEW

The Flight from Reality in the Human Sciences, Ian Shapiro. Princeton, NJ: Princeton University Press, 2005, 235 pp., \$24.95 hardbound.

Scott W. Talkington

With the academy enthralled by radical hermeneutics and other interpretive “methods” in order to facilitate corporatist claims about multiculturalism, one might be forgiven for thinking that a drift toward rational choice, exemplified by the mathematically elaborate political science literature or by game-theoretical Public Choice economics, constitutes a step in the direction of “reality.” After all, these approaches build from individual preference, values, convictions, and attitudes. But in a new book about method in social science, Ian Shapiro, a chaired professor of political science at Yale, has a different cut. His take in *The Flight from Reality in the Human Sciences* is that both radical interpretivism and logical empiricism are part of a “flight from reality,” and that the correction lies in a problem-driven approach to research that chooses methods based on pragmatic real-world problem solving. It’s a reasonable idea and a worthy project. The problem is that if you need a book that puts some order into the mangrove swamp of the methods debate, as an aid to the typical social science researcher, this might not be what you’re looking for.

To give him his due, Shapiro manages to simplify the terrain somewhat by categorizing the primary contenders in this battle over method down to four: logicism and empiricism (the two fragments

descended from Humean philosophy), interpretivism, and realism. Nevertheless, the sense one has as a reader is often that one just walked into a room of poultry farmers during a heated debate over whether the egg, the rooster, or the chicken came first—and no one has you in mind as their audience. You’re just eavesdropping on an insider conversation between people with vested interests. Or to put it another way, it’s like that feeling you had as a first-day university freshman when you accidentally wandered into the class on post-modern art.

For one thing, the alternative that Shapiro extols, which he calls either “realism” or “scientific realism,” is never precisely or systematically defined. If you haven’t sauntered through the door with this understanding already in your head, you’re in trouble. In fact, if we envision method as the vehicle that’s supposed to take us from a state of relative ignorance about a topic to a state of relative comprehension, parts of the bus seem strewn over a broad swath of the desert floor, so that your task is first to search for pieces, distinguishing them from the rest of the environment, and then start the reassembly without a schematic. In other words, reading *Flight from Reality* resembles the investigation or reconstruction of an accident or a terrorism scene. And that’s especially ironic because the endeavor that could conceivably benefit the most from this sort of deep critique of method, terrorism, isn’t even mentioned. Not once. The single over-arching reality to which the debate between interpretivism and logical empiricism seems most urgent, and for which “realism” might well be the appropriate cure, is nowhere to be found.

In fact, one could take the view that the book is revealed at least as much by what it omits as what it includes. Beside leaving out a systematic description of the primary thesis or any allusion to the defining issue of our era, there are other omissions. For instance the book includes numerous references and discussions of Hempel's "covering law theory," but not a single reference to either Karl Popper, Alfred Schutz, or Max Weber, three figures of some significance to prior attempts at sorting out the confused territory between positivism and interpretivism. Perhaps Schutz is omitted, even though he originally coined the phrase "flight from reality," because his meaning isn't compatible with Shapiro's. Schutz built a case that methods can't be mixed, but more importantly he suggested that the "pull" of an objective is more important than the "push" of proximate cause. One might bear this advice in mind as one ponders the implications of a shift toward a problem-solving strategy. We do this in order to . . . ? Is the focus on the attraction of the objective, or avoidance of the pitfalls?

One might argue that these important contributors, as well as others, were omitted because they failed in their attempts at resolution, but it seems important to know how and why the failures occurred lest one make the same mistakes. And it's not as though none made significant contributions to the debate that helped us make sense of the social world, or solve real-world problems.

Again, to be fair, the author makes a number of specific conceptual contributions that seem very helpful. These include the idea of "gross concepts" which,

he argues, create false dichotomies in political argument that mislead researchers down blind paths. Another involves the idea that scientific laws are real entities rather than artifacts of a correspondence between theory and fact. The point of this latter contribution is that it rescues the idea of progress, though most would think that problem already settled. Finally, Dr. Shapiro borrows a useful concept from Robin Dawes that seems also entertaining: the notion of the "Grandmother Test." According to this standard, if a research discovery or finding turns out to be something your grandmother is already likely to know, then it doesn't count as a significant contribution to the field. But I'm guessing our grandmothers already know that scientific laws actually exist, as opposed to simply being in unverifiable conjunction with the real-world events they attempt to model or explain. Nonetheless, these contributions provide robust clues to what the author and his supporters have in mind by extolling "scientific realism." It just seems unfortunate that he never seems to get around to helping the reader with a thorough exegesis of the method. In fact, I'm not sure we shouldn't regard what Shapiro is recommending as an "anti-method" of some sort?

There also doesn't seem to be many clues in the book to where the author might rest on a grid mapping the chief ideological controversies that have troubled academia for the past 30 years, though that's not necessarily a deficiency. He does, however, devote an entire chapter, and a fair portion of two or three more (as well as a previous book), to a loaded concept he calls "the pa-

thologies of rational choice.” But there are no balancing references to some of the wilder theories often advocated with little logical justification by many poststructuralist feather-chasing sociology departments. Presumably the descendents of the Scottish Enlightenment are riddled with malignancies while the descendents of the Counter-Enlightenment are afflicted with mild colds.

But am I misled? Is it merely that interpretivist method doesn't provide enough purchase as we grope toward the brake lever? Or are my expectations really too ambitious for what ought to be regarded as a modest project? Perhaps the analogy to an accident or terrorism scene investigation, though apt, really represents a valid and wise approach to the problem? If so, the book seems more a beginning than an advanced contribution, in which case the title promises too much. But Dr. Shapiro contributes a small and distributed list of systematic recommendations for Scientific Realism, the remedy that must transcend, at least in part, the theory/method/technique muddle that constitutes our phantasmagoric getaway. That, at least, has the virtue of boldness. The recommendations include:

1. Social science research ought to be problem—rather than method— or theory-driven.
2. A way to accomplish this is to generate multiple descriptions of a social reality. This would help us resist the temptation to build or defend a theory, or hone a favorite method.
3. Some descriptions are more theory-laden than others.

But this seems a fairly sparse list for such a bold project. Is this all we need in order to proceed? In an attempt to expand upon and clarify what he means by the practical implementation of these recommendations, during a chapter on problems in political science, the author uses an example. He points out that different explanations for the marriage of a young woman exemplify the fact that there can be more than one “true description” of a social phenomenon. Her marriage can be seen variously as a reproduction of hierarchy, an expression of love, obedience, social ritual, economic self interest, or her selfish genes. But what isn't clear is why one couldn't simply approach this as a multivariate problem, employing some sort of counterfactual method to produce coefficients for all the contributions to outcome.

What is there about the problem that demands but one motivation for the act, and why do we have to assume bias in the choice between them? Besides, if one were looking for good examples of theory-bias they shouldn't be too hard to find given the extraordinary divisiveness produced by present extremes in ideological outlook. But even these could theoretically be reduced to multivariate problems, as even some rather ideologically-driven academics, like Cornel West, have observed. We disagree about the “coefficients” because of our uncertainty about the data and our understanding of the relationships. The cure for that might well involve a realistic research program, but it ought to address these uncertainties as a primary objective, shouldn't it?

Rather than establish an incisive set of systematic criteria, or defining condi-

tions, Shapiro addresses the issue of method or theory-driven research with concrete examples. We discover, for instance, that John Nash could never imagine a good example of an equilibrium condition from which no one has an incentive to defect. But here too, it's not clear why we can't simply conclude that equilibrium is both attractive *and* untenable. After all, it's not as though this combination of attributes is unknown to us. To take another example, we find that Charles Murray has trouble defending the theory that out-of-wedlock births are due to the perverse incentives created by Aid to Families with Dependent Children, when he's confronted with the data that these births have been increasing proportionately while benefits have been declining. But just how does one determine whether perverse economic incentives are a component of the conditions producing out-of-wedlock births without holding things equal by using some version of counterfactual method, such as multivariate analysis or a comparative method? Rather, what Shapiro asks us to conclude is that the argument is flawed not by the inadequacy of the theory so much as the perverse disjunction between closely-held theory and reality. Fair enough, but, once again, the theory that's singled-out for this sort of debunk is one that's closely related to convictions about anti-statism and classical liberalism, though apparently attacking theory-driven research in general. This may be a false impression of bias, but how is one to know?

So there may not actually be a way out

of the swamp after all. Nor are we even being offered a systematic process by which to rank the "theory-ladenness" of approaches to problem-solving, though we're assured by idiosyncratic examples that there are fine gradations of the taint, like Bill Murray's laundry in *Ghostbusters*. Indeed, I'm willing to be persuaded by that point of view, but I just don't want to see a step-wise descent from theory-driven through method-driven to technique-driven research under the guise of chasing a problem-driven practice. How does one keep one's eye on the ball, and remember to swing the bat?

The book is divided into five separate chapters that, although loosely related, appear to be distinct argumentative papers that are part of an ongoing dialog. Apart from the conceptual flaws noted, there's nothing wrong with this. But the segments haven't been pulled together into a very useful explication of the thesis that might benefit an outsider. And, possibly due to the *ad hoc* and idiosyncratic mode of the analysis, *Flight from Reality in the Human Sciences* threatens to be more nearly technique than problem driven. Or to be more precise, it's technique-pulled since that's what remains if we eschew theory and method. The irony is that the remedy seems in dire need of something that looks, for all the world, like a coherent theory or a systematic method, whatever one calls it.

Scott W. Talkington is director of research at the National Association of Scholars, Princeton, NJ 08542-3215; talkington@nas.org.