Introduction. My criterion for intellectual success is deliberately broad: that creative intelligence be applied such that abundant insights result. That criterion is well satisfied by Ian Shapiro’s *Flight from Reality in the Human Sciences*, a collection of his previously published essays on how to do political science. Given my history, it’s inevitable I’ll discuss the rational-choice question. Before doing so, I want to recognize the essays I won’t discuss at length here. The essay on Richard Posner’s praxis is an instructive destruction of popinjay jurisprudence. Many a student would learn how to avoid seductive error by working it through. The essay on gross concepts in political arguments is an innovative contribution. It diagnoses the professional prejudices and incentives that lead much of political theory into debates between polarized ‘isms. Too much of this is the consequence of a flight from empirical controversy, says Shapiro. I recall a philosophy seminar on two different formulations of international justice. I objected that the two conceptions, if implemented, would not differ in practical action until after about fifty if not a hundred years of development. “We don’t discuss facts here,” I was told. Like Shapiro, I think it’s better for political theory to be connected to empirical political science, although I wouldn’t say that about any individual political theorist. We need theorists of all varieties: counter, original, spare, and strange.

I’m reluctant to participate in methodological struggle for several reasons. I spent a full day in graduate school going through *APSR*s from the 50s and 60s. The most interesting articles were by people who made intriguing observations about the world, despite the observational meat being served between moldy slices of behavioralism or functionalism. The least interesting articles were methodological disputes. As for today, I find it difficult to adjudicate among methods in the abstract. Run that method around the track a few times, and let’s see what it’s got. The best philosophy of social science might be done by example, or that’s how I console myself for failing to have developed views on the philosophy of social science. Finally, I haven’t practiced in the profession long enough to confidently evaluate its wide range of efforts. Having established my lack of qualifications, here are my thoughts.
Rational Choice Theory. I do think that *Pathologies of Rational Choice Theory* was a well-deserved polemic.\(^1\) Attending the APSA convention as a graduate student, a friend tipped me that there might be fireworks at a big panel, an attack on rational choice that I hadn’t heard of before. It was a great show, but I was sitting near two besuited gentlemen who leered, winced, grimaced, and whispered bitterly at every word uttered by Green and Shapiro. Hang on, I thought, this is science, not religion. Theories, hypotheses, facts are frequently wrong, and all must be constantly subject to relentless criticism. It’s not religion, it’s not even football, there’s no place either for cheering your team just because it’s your team. Theories and methods should be like clothing which you change as appropriate to the occasion, not like skin which burns with pain as it’s peeled away. As I’ve said elsewhere, I agreed with many of Green and Shapiro’s evaluations – for several years the *APSR* was so irrelevant to politics that I rarely unbundled it from its plastic wrapping – but not so much with their conclusions about rational choice.

I am a methodological pluralist, but a critical one: not anything goes. I doubt, for example, whether Freud has anything to tell us about politics or about anything else. I am a rational-choice theorist, but in the spirit of Schelling, Davidson, Elster, not in the spirit of Tullock or Riker. I think the theory is misnamed, confusing both proponents and opponents. It would be better named *purposive actor* theory, in my view. Anyway, I first learned rational choice theory from John Orbell and John Dryzek. Orbell is a social-dilemma experimentalist and from him I learned that the game-theoretic prediction fails if you assume that subjects are motivated only by the material rewards offered in the experiment. Since the model is logically valid, the failure implies that many people are motivated by more than dollar payoffs (and many more interesting findings came out of his research program). From Dryzek I learned the hypothesis that it is not rational choice but the assumption of self-interest which leads to the falsified predictions known as the paradox of voting and the paradox of participation; and I was introduced to the communicative reason of Habermas. From Jon Elster and Diego Gambetta I learned that rational choice can be applied with startling subtlety and insight.

Rational choice, according to Gary Cox, is “any argument that takes actors and their goals as primitives, then proceeds to explain how the given actors’ pursuits of their posited goals leads to systematic

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tendencies in observed behavior.”\(^2\) What was exciting to me about rational choice is that it displaced all the failed behind-the-back theories of social causation and put human agency in the center of our considerations. Humans are not the puppets of subconscious demons nor of systemic function nor of age, income, and education variables. For me the doctrine is not the bonehead assertion of egoism as exclusive motivator, nor the careerist generation of mildly modified mathematical models of zero substantive interest. I deplore the latter developments as much as does the passionate perestroikan.

It is not a theory of everything. Patterns in constraints are exogenous to the theory. The explanatory force of W.H. McNeill’s accounts of disease and history – for example that an imperial metropolis unintentionally fragments and subjugates its rural frontier by the inadvertent export of urban disease – has to do with patterns of constraints rather than with the desires and beliefs of the actors.\(^3\) Or consider Jared Diamond’s theory of emergence of civilization varying according to the availability of domesticable species and ease of east-west diffusion.\(^4\) Or the effect of military technology on the conduct of war. Or whether an individual is a bonder or a bridger in a network of social relations.

Rational choice makes a rebuttable presumption that beliefs are consistent with one another and similar because they connect to the world and that desires are consistent with one another and similar because of our similarity as humans. It could be that humans are not mostly purposive, it could be that the folk psychology of belief and desire is somehow illusory, and it could be that there are no tendencies to consistency and similarity. These may be necessary conditions of human agency, but if one denies human agency, or denies the explanatory power of human agency in human affairs, as do some exotic continental thinkers, I suppose they could be done without. The Davidsonian idea is that they must be true much of the time in order for us to make sense of the exceptions. There are plenty of exceptions: beliefs contradict one another, as do desires, people hold to false beliefs and to heteronomous desires, and people make mistakes. Other than the consistency and similarity presumptions, the patterns of beliefs and desires in a population are exogenous to rational choice, and often require independent explanations, as do errors in beliefs and in desires.


Thus, I am not a universalist in Green and Shapiro’s terminology: I accept both that rational choice applies in some circumstances and not in others and that it sometimes explains in conjunction with other independent variables. Following Cox, one might say that nevertheless rational choice is more universal than the flotsam and jetsam of concepts left over from the shipwrecks of behaviorism, behavioralism, functionalism, Marxism, Freudianism, and other grand vessels. Could the present methods of rational choice be superseded 20 years from now? Sure, maybe cognitive psychology, neuroscience, and agent-based modelling will be fruitful, or probably methods we have yet to imagine.

Rational choice as I understand it is an interpretive strategy, and game theory especially offers good explanations of interdependent actions, much of the stuff of politics. Shapiro’s Chapter 1, about realism and acquiescence to power, puts rational choice into the mistaken logicist camp. Shapiro and Wendt characterize rational choice as Friedman’s instrumentalism sundered from the constraint of accurate prediction. That may be an apt description of a line of work in political science, one that occupied too much of the center of our profession for too long a time. If true, however, it describes not rational choice but an erroneous version of rational choice. Rational choice theory is not necessarily instrumentalist, and it is not necessarily opposed to accurate description and prediction! According to Shapiro and Wendt, for logicists (including the anti-evidential and instrumentalist rational choice theorist), acquiescence to power is epiphenomenal, a logical artifact of assumptions themselves not subject to evidentiary considerations. But I say there could easily be a pro-evidential and realist rational choice theory as well, which relies on evidence to infer the (real) desires and beliefs of the relevant actors over actual and counterfactual states of the world.

Shapiro and Wendt justly fault the approaches of behavioralism and noncausal interpretivism to the topic of acquiescence to power. Each is misled by the phenomenal, and thus each fails to detect the cold shadow of power. Gaventa, understood by Shapiro and Wendt as the proper realist alternative to logicism, behavioralism, and interpretivism, resolves the puzzle of quiescence.5 But he does so by calling our attention to counterfactuals, as does good game theory. The question is: why do miners acquiesce to their subordination? Such acquiescence is a puzzle in the first place because people don’t like being subordinated, so stipulate that. Rational choice suggests that they acquiesce to

subordination in order to avoid a worse alternative. With that interpretive understanding, one then looks for evidence of what that counterfactual alternative is, what people believe would happen if they defied power. We can ask them what they believe, we can investigate historically what happened to their relatives and friends who did challenge power, we can observe what happens when the believed costs of defiance go low, and from all that we can infer that they would defy power if they believed that they could get away with it. If, in addition to acquiescing due to a belief about what would happen if one did not, acquiescence is due to the shaping of false beliefs and heteronomous desires in the subordinated by the subordinators, then additional psychological and sociological variables are required for explanation. That is because rational-choice proper has no wide theory of preference formation and change.

**Problem-Driven or Method-Driven?** A major theme in *The Flight from Reality* is the folly of method-driven political science and the wisdom of problem-driven political science. I shall dispute this contrast. The faults that Shapiro identifies in method-driven work, I say, are the faults of intellectual self-deception, or, most charitably, error. Problem-driven work is subject just as much to intellectual self-deception, I shall suggest. I do not mean to lecture from a high horse, I consider myself just as likely to err in this regard as others, until constrained by the criticism of peers.

In fact, my most successful application was method-driven. I did just what Shapiro said we shouldn’t do. I was fascinated by Schelling’s account of conventions as coordination games. I had a hammer in search of a nail. Hume, who had some thoughts about justice as a convention also suggested that inheritance practices are conventional in the same sense. I read everything I could about convention and about variation in inheritance practices. My paper concluded that very little about inheritance practices was conventional in Schelling’s sense. The striking regularities in practice, said the best literature on the topic, were mostly due to the local political economy: primarily the local agricultural pattern, secondarily legal requirements. Inheritance is not conventional, because choice of practice is independent or quite weakly interdependent between families. I noted that in contrast marriage choices are strongly interdependent between families, and that in future work one might look for Schelling conventions in that arena. I do not think Shapiro could fault my method-driven exercise, which yielded a useful if tiny conclusion, that inheritance practices are at best only weakly conventional. Where I
would have gone wrong, and where he could object, is if in a self-deceptive fashion I tortured the facts about inheritance to make them fit my model. Notice how an honest method-driven investigation sharpens scope conditions of the method, and sends inquiry in the proper direction. Suppose a scholar trains up on social network analysis. Would it be wrong for her to look for problems where the structure of relations is suspected as a key causal factor? Would she add to knowledge if she discovered that, contrary to expectation, the structure of relations had little effect in some area, and explained why?

A year later David Laitin suggested as a paper topic that someone could do a game-theoretic investigation of the painful, dangerous, and puzzling practice of female genital cutting. Initial research showed that the practice is associated with marriageability, that it is nearly universal in intramarrying groups, that its features strongly parallel the features of footbinding in China, and that footbinding had ended suddenly rather than gradually in intramarrying groups. Eureka! The preliminary indicators said that FGC is probably a Schelling convention. Then followed an interplay of theorization and factual investigation that settled into a reflective equilibrium. Wide rational choice theory, supposedly vacuous, provided the key. Why would a family impose one of these painful and dangerous treatments on their daughter? The answer must be: in order to avoid a worse alternative. What is the worse alternative? Unmarriageability of the girl. How could one help end the practice? Only by solving the marriageability problem. How did the Chinese end footbinding? By recruiting the members of an intramarrying group into a declaration of coordinated abandonment, such that daughters retained their marriageability. Would the same method work in Africa? I predicted that it would. That prediction was fulfilled by unique mass abandonments in Senegal guided by the theory, through 700,000 people in 1500 villages, and planning is afoot to implement Africa-wide. The exercise mostly satisfies Shapiro and Wendt’s criteria: it retrodicts empirical regularities, conforms with most of existing understanding but adds considerably more, and allows us to intervene successfully in the relevant environment. Abandonment interventions based on other theories of the FGC practice have uniformly failed.

The account in full is probably what Green and Shapiro would call a partial rational-choice theory, but by far its largest part is, to quote Cox

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6 I do not align myself with Laitin in his quarrel with Shapiro, nor do I agree with his more dogmatic views about how best to conduct political science.

again, “an argument that takes actors and their goals as primitives, then proceeds to explain how the given actors’ pursuit of their posited goals leads to systematic tendencies in observed behavior.” The agency-based approach is also a happy way to promote abandonment of the practice. The account seeks to understand the people involved from their own point of view. In practice, the intervention does not tell people what to do, rather, it discloses that there is an effective way out (and most people decide to go that way). Alternative theories, apparently more humanistic than supposedly cold mechanical game theory, mistakenly understand the people involved as dopes buffeted about by mysterious id or almighty culture or systemic powers, and speak not of abandonment but eradication of the practice (as if it were a mosquito). Eradication consistently fails and only abandonment has succeeded.

At the panel, Ian Shapiro responded that my exercise fails the grandmother test, his refuge whenever a rational-choice application is successful. I suspect he is not well-informed about my work in this area, however. And, I suggest, Shapiro’s grandmother test is ill-defined, more rhetoric than argument. Of course, any observed action is to avoid the worse alternative, he replies, but that’s trivial. Well, the causal power of the counterfactual was not obvious to Dahl in *Who Governs*?8 Dahl failed to notice that people acquiesce to the implicit threat of power because he was blinded by behavioralist methodology. It took years of controversy, conceptual analysis by Lukes and others, and empirical investigation by Gaventa and others, to sort it out. The counterfactual shadow of power on acquiesence is obvious in retrospect, after being called to our attention by creative observers. Further, it’s obvious today (or is it?) that a light object and a heavy object will fall at the same speed, but it wasn’t obvious before Galileo. Most of mathematics is tautologous, but is nevertheless enlightening because humans lack logical omniscience. Competing hypotheses about FGC and footbinding are almost all behind-the-back theories, and none of them counsel us to consider first the beliefs and desires of the people involved, nor to look for the worse alternative avoided. The purposive-actor approach to FGC and footbinding may be “obvious” in its presumptions, but is not obvious in its results. It was a revelation in theory, and led to a revolution in practice. Maybe its results will become obvious in another generation, but they were not at all obvious to me as I worked them out, and weren’t obvious to anyone before that.

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In developing the convention account I did inference to the best explanation. I charted a matrix with theories on the vertical axis and observations on the horizontal axis, marking an X where theory accounted for an observation. Some of the theories were the convention account, the patriarchal sado-ritual syndrome, the mindful body account, functionalism, modernization, ostentation, and the ethnic marker account. Some of the observations were universality, persistence, link to marriageability, practice by those opposed, and the sudden end of footbinding. The relatively simple convention hypothesis accounted for more of the observations than did alternative theories.

The problem-driven, inductivist sado-ritual-syndrome theory sought to explain FGC, footbinding, and sati, and taking the three practices together accounted for more observations than did the convention account, even though it neglected to explain why a universal patriarchy caused such extreme practices in three different locations but did not elsewhere. It encompassed many observations because whenever it met an anomalous observation the ramshackle inductivist theory just added another proviso! If footbinding is caused by universal patriarchy, why did footbinding end suddenly when patriarchy did not? Because malicious males also practice “patriarchal reversal,” their sadistic pleasure includes both forcing a disfigurement on women and their further humiliation in abrupt reversal of the practice. How about another problem-driven, inductivist theory? Germaine Greer, neglecting to consider the quite different desires and beliefs of the human agents involved, and the different constraints they encounter, lumps together FGC, footbinding, plastic surgery, obsessive self-mutilation, tattooing and piercing, surgery on infants born to ambiguous gender, transgender surgery among adults, adult cosmetic surgery on the genitals, and unnecessary obstetric and gynecological surgeries. The problem to be investigated is why women get cut, and the theory offered is that women secretly desire to be mutilated. If these examples are representative, then problem-driven theory obscures rather than enlightens.

I presented the rudiments of the convention account at one of our best universities, and a senior figure was visibly appalled that I would use game theory to help explain these practices. Instead, this person said, we should turn to something like Freud. There are already Freudian “explanations”: Freud hypothesized that FGC assists the transition from clitoral orgasm to vaginal orgasm, and that footbinding allays male castration anxiety. The Freudian hypotheses were unique in my abductive inference exercise for explaining none of the observed features of the practices. But, hey, they’re not rational choice!
The problem here is not with problem-driven investigation, not with method-driven investigation, the problem is intellectual self-deception or error. My method-driven investigation could have erred, but did not. The problem-driven investigations could have avoided error, but did not.

**Conclusion.** Moreover, I want to suggest, theory, method and problem are inseparable (I must add that this implies no relativism about the facts). One’s investigations might be more theory-driven at one point, more method-driven at the next, and more problem-driven later. Consider Alan Turing. This mathematician turned to philosophy after his best friend died. He wanted to understand how the mind was embodied in matter, and this led him to quantum mechanics, and then to mathematical logic. He encountered the logical problem of undecidability and provided a solution (which founded computer science). From his point of view he was problem-driven. From the point of view of any nonlogician he was method-driven. (The Turing case is instructive for those national science foundations which require that research be relevant to national goals of peace and prosperity. “Dear Mr. Turing: We are grateful to have received your ambitious proposal to pursue a solution to Hilbert’s *Entscheidungsproblem*. I am afraid that the committee has determined that such logical speculations are of no conceivable use in war or commerce. Naturally, we wish you the best of luck with your career.”) Turing turned his method-driven discoveries to cracking of the German cipher in World War II, and to the development of the computer thereafter, until his cruel early death following official persecution for being an open homosexual.

I worry that the call for problem-driven work could degenerate into a demand for relevance. It’s probably best for many political scientists, and especially for beginning ones, to pursue a concrete problem. But it would not be best to eliminate those who choose to march down unbeaten and apparently irrelevant paths. Turing is one example. Hubert Dreyfus might be another. Why should a (philosophy) department keep around a cantankerous Heidegerrian? Dreyfus’s resolute phenomenology called the great emperor, the supposed science of artificial intelligence, naked, in his *What Computers Can’t Do*. Dreyfus accurately predicted that artificial intelligence implemented as the manipulation of internal symbols by internal rules would fail, and stimulated intellectual revolution in cognitive science. Dreyfus is controversial and I’m sure

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he’s wrong about many things. However, his “obvious” but discerning observations about the acquisition and exercise of human skill led to the identification of a degenerating research program, no mean feat.

Speaking of naked emperors, a third approach begins to emerge in Shapiro’s Chapter 5 on what to do about what’s wrong in political science, one of critical reappraisal (one I followed in the debunkery of my Democracy Defended). He says that political theorists should serve as roving ombudspersons for truth and right, holding the discipline’s feet to the fire (I hope that members of other subfields take part, and that some political theorists are allowed to pursue their peculiar fancies). Science is not religion, it’s not football, it is criticism. We always need challenging factual and ethical redescriptions of the world. The best theories inevitably neglect some facts and elide some injustices, not just neglect them but hide them away.

Not only critical reappraisal, but, I would add, discerning observation is a worthy goal of political science – so many observations are obvious in retrospect but go completely unnoticed until disclosed by the creative observer, a Dahl, a Fenno, a Gaventa, or a Goffman. Physics is more unified in theory and method than political science. Physicist Richard Feynman was asked the secret of his success, and he answered that (even in physics) those beautiful models leave things out. He always hunted for observations unaccounted for, ignored, repressed in the standard version. He found them – there, under everybody’s noses – and then he explained them. My review of ancient APSRs taught me that discerning observations outlive the theories and methods which midwifed them.

Perhaps critical reappraisal and discerning observation better describe the right direction for political science than the choice of problem-driven over method-driven investigation. Successful applications in political science probably result more from intuition, talent, and luck, however, than from marching behind any methodological banner.

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