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**The Flight From Reality:
A response to Hochschild, Mackie, and Laitin**

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I will not comment on Jennifer Hochschild's illuminating discussion of the different ways in which various kinds of submissions to *Perspectives on Politics* fail to be problem-driven, and on the wise editorial feedback she gave to the authors. Her editorship of the first three volumes has set a high bar for those who will follow. She has done a major a service not only for the political science profession, but also for those outside it who want to learn what political science can add to their understanding of politics.

Turning to Hochschild's second set of remarks about perspective-driven research, I agree with the substance of her point. However, Occam's razor counsels against creating a new category here. What Hochschild has in mind goes not so much to the generality or otherwise of the claim as to its arresting character: describing something in a new way that pulls you up short and makes you think about the phenomenon differently. This I deal with in *Flight* under the rubric of "problematizing redescriptions." Examples I give are recharacterizing the Westphalian system of sovereign national states as *global Apartheid*, or recharacterizing bipartisan agreement as *collusion in restraint of democracy*.¹

Such alternative descriptions invoke a theory that is not usually associated with the phenomenon in question. I agree with Hochschild that this kind of redescription can be illuminating even when it overreaches. This is because it highlights an unexamined assumption in the prevailing way of seeing things. Most people take it for granted that respecting national sovereignty, within some limits, is morally benign and that bipartisan

¹ Ian Shapiro, *The Flight From Reality in the Human Sciences* (Princeton University Press, 2005), pp. 199-203.

agreement is a good thing in democratic politics. The redescriptions at least make us wonder. But to go beyond being arrestingly thought-provoking, there has to be more: a two step process, I argue, by which one shows first that an important feature of what needs to be accounted for is missed by the prevailing characterization, and a demonstration that the proposed recharacterization speaks to the inadequacies of the prior account. One mark of success will be persuading skeptics and adherents of the old view, as distinct from who were already partisans of the proposed new view. This, I argue in *Flight*, is where so much that travels under the rational choice banner does so poorly.² An example of success—though not a rational choice one—that I discuss is the reexamination of industrial policy in capitalist democracies through the lens of liberal corporatism rather than pluralism. This enabled researchers to understand a great deal more of how industrial policy comes to take the form it does, and the different roles played by business, organized labor, and government in that process, than the competing Marxist and pluralist visions that had prevailed hitherto.³

Gerry Mackie's comment leaves me unpersuaded of the advantages of method-driven research as he describes himself as having engaged in it. Granted, scouring the social world for Schelling-conventions would be dishonest if one pretended to find them when they were absent. Mackie is to be commended for not doing this. Nonetheless, it is far from obvious that Schelling-conventions are the best place to start when trying to understand a particular phenomenon such as inheritance practices. It would depend on what one wanted to know about them, but I would start with the existing theories of how they came to be established in a particular setting and then ask what—if anything—those accounts failed to explain. If that was something I wanted to know, then I would cast about for alternatives and see how well they did in accounting for the phenomenon in question.

As for Mackie's discussion of foot-binding and female genital cutting, I fail to see why one has to crank up a bunch of game theory to get to the hypothesis that if someone does something that seems harmful to themselves or someone we have reasons for

² Hence my criticism of Bawn on ideologies as devices for maintaining group solidarity and Hardin on constitutions as coordinating devices (*Flight*, pp. 185-6) that David Laitin finds perplexing.

³ *Flight*, pp. 199-202.

thinking that they care about then there may be some greater harm they are trying to avoid. Nor does one need it to suppose that, if the greater harm were removed, then the people in question would no longer engage in the lesser harm. Indeed, one need not even know what a coordination game is to consider this a plausible possibility. Mackie's suggestion that Dahl failed to consider possibilities of this sort in *Who Governs* "because he was blinded by behaviorist methodology" scarcely meets the objection. Nothing in *Who Governs* implies that people do not accept lesser harms to avoid greater ones, and nothing in Mackie's research involves looking at anything other than behavior.

Mackie's comment seems to me to be unnecessarily self-flagellating in that his major book, *Democracy Defended*, is manifestly a problem-driven enterprise.⁴ In it he takes on the literature that was motivated by the theoretical possibility of voting cycles first observed by Condorcet and formalized by Kenneth Arrow, and he asks whether such cycles actually occur in practice. His exhaustive investigation of virtually every alleged instance of a cycle in the literature of the past several decades reveals them to have been misidentified, supporting his conclusion that the supposed threat to democracy is chimerical. Is there a real problem here?, Mackie asked, and answered: no. We are all in his debt for having debunked this spurious though influential literature.

I have considerable admiration for David Laitin's empirical work in political science, but his comment on *Flight* is disappointingly obtuse. A good title for it would be "Shooting the Messenger with Non-Sequiturs," inasmuch as he tries to direct a series of barbs at me that for the most part fail to engage with the argument of the book. When he does engage, he gets it badly wrong.

The first non-sequitur is Laitin's lament that although I complain of a flight from reality in the human sciences, "the text is virtually empty of any supported claims about that reality." *Flight* is a collection of methodological essays, not a summary of my substantive work. As he acknowledges in a garbled footnote, my substantive work is published elsewhere.⁵ Moreover, only by engaging in the most tortured imaginable reading of chapter 1 could anyone take Wendt and I to be saying there that philosophers

⁴ Gerry Mackie, *Democracy Defended* (Cambridge University Press, 2003).

⁵ The footnote is garbled inasmuch as my recent work on South Africa is not about injustice and my recent work on injustice is not about South Africa.

“need not be worried about saying true things about the world.” We defend philosophical realism as a desirable basis for social research on the grounds that it does not bias the enterprise in ways that the going alternatives do, but we also argue that Roy Bhaskar and others are mistaken in believing that a commitment to philosophical realism by itself entails the truth of any particular theory about the world. That, we argue, is a task for social research, not armchair reflection.

By the same token, my criticism of Laitin’s work in *Flight* is not directed at his substantive work on Africa, language and political identities, or civil war, but rather at his proposal to do for political science teaching what Samuelson did for economics teaching. This proposal does indeed rest on a denial of reality, but not for the reasons Laitin reports me as having given. His claim is that political scientists should teach a universal introductory political science course, the syllabus for which Laitin has already designed. I argued in chapter 6 of *Flight* that the type of standardization he seeks has been a bad development in economics, divorcing undergraduate teaching for the controversies at the research frontiers of the discipline and alienating serious scholars from undergraduate teaching. The practice in political science, where the plurality of introductory courses reflects the reality that scholars disagree on what the basic problems of politics are, is, I argued, superior. It is curious that this sometime student of hegemony misses the irony of his proposing that current teachers and future generations of students all be disciplined by his view of these matters.

Laitin’s attempt to sidestep this by saying that he gives “primacy to political theory in addressing consequential real-world concerns that have motivated participants in the tradition for two millennia” scarcely meets the objection. There are many ways of reading the classic theorists in the tradition. As I noted in my original discussion, the basic challenges about democracy that Laitin extracts from the tradition reflect a view of democracy in which solving Arrow’s problem is important. On my view, by contrast, preference-aggregation is comparatively unimportant; things like fostering political competition and opposition matter more. And I noted that many political scientists who teach about democracy would disagree with us and with one another more than Laitin and I do. In this context, suggesting that there is an accepted view of the matter that can be extracted from the tradition “involves kidding ourselves or kidding our students. I guess

the latter is worse, but the former has little to commend it either.” Kidding the outside world in order to extract funds from granting agencies, which he comes close to advocating as well, hardly seems much better.⁶

My preference is that we continue teaching undergraduates in ways that reflect the disagreements at the research frontiers of the discipline, betting for the long haul on the competition of ideas instead of captive audiences for all the reasons that John Stuart Mill maintained that we should. In the short run, this will at least yield courses that engage teachers and students more than serving up chapter three of some textbook in week three of every semester in every department in the country. Fortunately, it seems unlikely to me that the diverse reality of the political science discipline will bend to Laitin’s will any time soon. But I find it unfortunate that he believes that it should. Contra his suggestion that I am opposed to changing the political science curriculum, I have no desire to stop him from teaching it as he sees fit and trying to convince people that he is right. My own introductory course (based on a rather different account of the Western tradition of political theory than Laitin’s) seeks to do just that. When I turned the course into a book, rather than propose that it become the universal standard I noted in the preface that I would be satisfied “if instructors find it a helpful teaching tool, yet feel the need to argue with it as they teach it.”⁷ That seems to me the appropriate aspiration.

A second non-sequitur concerns Laitin’s charge, piggybacking on Ferejohn and Satz’s critique of *Pathologies of Rational Choice Theory*, that advocating problem-driven research allegedly ignores the fact that there is no pre-theoretical way of characterizing problems. I will not repeat our response to Ferejohn and Satz here, which can be found in chapter 2 of *Flight*. The matter is taken up in greater depth in (and indeed is the central topic of) chapter 5, though you would never know this from reading Laitin’s comment. In that chapter I note that every political phenomenon admits of multiple true descriptions, each of which invites a different type of explanation—reflecting the inevitability of theory-ladenness. Much of what is contentious in political science comes down to disagreements over which description is most apt, with partisans of different reductionist enterprises—Marxist, rational-choice, feminist, functionalist, and others—proposing their

⁶ *Flight*, p. 207-8.

⁷ Ian Shapiro, *The Moral Foundations of Politics* (Yale University Press, 2003), p. xii.

preferred cut as the right one.⁸ No architectonic venture of this kind has much of a track record of success in political science; indeed this is a major reason why no single conception of what the discipline is or should be has won the day. Against all such ventures I argue for an anti-reductionist view of political explanation, one that jettisons the assumption that a particular cut will be the right one for the varied kinds of things political scientists study. My approach places the burden of justification for adopting a particular cut for a particular phenomenon on the researcher rather than on the skeptic. Shouldering this burden includes showing why the proposed cut illuminates more than the going alternatives—something that is seldom attempted in practice.

A third non-sequitur concerns Laitin's allegation that my criterion for praising work is that it be authored by Yale colleagues. Tempting as it is to call this a low blow, that would suggest that it actually gets off the ground. No matter that I criticize Robert Dahl's behavioral account of power and defend John Gaventa's realist account as superior. As for his claim that I "hold back" my "full fire" with respect to my discussion of my colleagues Alan Gerber and Donald Green, Laitin both misreports what I say about their field experiments and gets the import of my discussion of them exactly backwards. I never claim that "Gerber and Green choose their problem (whether it is more efficient to phone or visit people if you seek their vote) based on its tractability with the experimental method that they are promoting." In fact I have no idea how they chose this particular problem. What I *do* say, which Laitin ignores, is that *if* the research agenda for political science starts to be driven by what can be studied by means of field experiments, this will exclude the vast amount of what draws people to political science in the first place. This is the partial list I give: "the effects of regime type on the economy, and vice versa; the determinants of peace, war, and revolution; the causes and consequences of the trend toward creating independent central banks; the causes and consequences of the growth in transnational political and economic institutions; the relative merits of alternative policies for achieving racial integration, such as mandatory bussing, magnet schools, and voluntary desegregation plans; the importance of constitutional courts in protecting civil liberties, property rights, and limiting the power of legislatures; the effects of other institutional arrangements, such as parliamentarism v. presidentialism, unicameralism v.

⁸ See especially *Flight*, pp. 187-8, 199-203.

bicameralism, federalism v. centralism on such things as the distribution of income and wealth, the effectiveness of macroeconomic policies, and the types of social policies that are enacted; the dynamics of political negotiations to institute democracy. I could go on, but you get the point.”⁹ Apparently Laitin didn’t.

Perhaps Laitin thinks I pull my punches because I note that there is a class of phenomena that do lend themselves to field experiments, to wit, “the study of behavioral variation in settings where the institutional context is relatively fixed and where the stakes are comparatively low, so that the kinds of interventions required do not violate accepted ethical criteria for experimentation on human subjects.” But part of my antireductionist message is that people should abandon the search for the one-method-fits-all holy grail of political science. The phenomena that interest political scientists vary too much for this to make sense. We should avoid the shell game where the successes of one method are compared to the failures of another which is then judged to be ready for the scrap heap. With methods, I argue, as with people: “If you focus only on their limitations you will always be disappointed.”¹⁰ Here it might be worth reiterating that Green and I never declared rational choice explanations to be worthless; only that their indiscriminate application in political science had not yielded identifiable advances in knowledge. Indeed we suggested that rational choice explanations might be expected to do well when the following conditions hold: (i) stakes are high and players are self-conscious optimizers; (ii) preferences are well-ordered and relatively stable; (iii) there is a clear range of options and little scope for strategic innovation; (iv) the strategic complexity of the situation is not overwhelming for the actors whose strategic capabilities must not differ significantly from one another; and (v) actors have the capacity to learn from feedback in the environment and adapt.¹¹

Even within an appropriately specified domain, we should never lose sight of the fact that all the methods political scientists employ run into enormous difficulties when matched with the complexities of actual politics. Most quantitative studies are plagued by the bad data that scholars have to work with. Case studies might be unrepresentative and

⁹ *Flight*, p. 197.

¹⁰ *Flight*, pp. 198-99.

¹¹ *Flight*, p. 94.

more often than not are selected either because of the idiosyncratic interests of the researcher or on the dependent variable—or both. Public opinion surveys are weak guides to what people believe for a host of well-known reasons. Interviews involve their own problems of potential bias and manipulation. Rational choice applications often run afoul one or more of the strictures mentioned above. Archival work limits the researcher to what happens to have been preserved and is available. Given this reality, often the best that can be done is to surround the problem with multiple methods rather than opt for one. Some of the best work in political science does exactly that. Eric Schickler's *Disjointed Pluralism* and Dan Carpenter's *The Forging of Bureaucratic Autonomy* are recent examples that come to mind.¹²

As for Laitin's claim that on my account of problem-driven research "any problem will do," I go out of my way to warn those who might be seduced by the promise of field experiments (they confer the advantages of experimental controls without being subject to the questions about external validity that typically plague lab experiments in psychology) that part of what spawned the model-mania of the 1990s was the disaffection with the trivial, if tractable, questions that consumed a good deal of 1960s behaviorism in the study of American politics. As a result, I argue, the mainstream of political science that they came to define "seemed to others to be both utterly devoid of theoretical ambition as well as detached from consequential questions of politics; frankly boring. To paraphrase Kant, theoretical ambition without empirical research may well be vacuous, but empirical research without theoretical ambition will be blind."¹³ Making a convincing case for the importance of the problem under study is integral to problem-driven political science on my account. It trumps methodological tractability. When the problems that are recognized as important are difficult to study with the available methods, the task of political theorists is to keep them on the agenda "and challenge the

¹² Eric Schickler, *Disjointed Pluralism: Institutional Innovation and Development in the U.S. Congress* (Princeton University Press, 2001), Daniel Carpenter, *The Forging of Bureaucratic Autonomy: Reputations, Networks, and Policy Innovation in Executive Agencies, 1862-1928* (Princeton, 2001). Laitin might complain that Schickler was once associated with the Yale department.

¹³ *Flight*, p. 198.

ingenuity of who are sufficiently open-minded to devise creative ways of grappling with them.”¹⁴ Hopefully, Laitin agrees.

¹⁴ *Flight*, p. 203.