

# What Rationality Assumption? Or, How “Positive Political Theory” Rests on a Mistake

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June 2008

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“I believe that discussion or application of game theory is utterly meaningless without a proper interpretation. This task cannot be left entirely to philosophers of science, for it constitutes the very essence of the theory.” ~ Ariel Rubinstein (1991)

“In analyzing games the theory *does not* assume rational behavior; rather it attempts to determine what “rational” can mean when an individual is confronted with the problem of optimal behavior in games and equivalent situations.” ~ Oskar Morgenstern (1968)

## Introduction: My Ambivalence

In this paper I concentrate on the reception of game theoretic methods in political science. In particular I take issue with the notion that such methods afford a plausible basis for what commonly is called “positive political theory” (PPT). I believe that the standard rationale offered for this enterprise is hopelessly confused, that the confusion stems from the influence of insupportable philosophical views, and that the confusion has generated baleful consequences in the discipline.

I should start by revealing my reason for writing the paper: ambivalence. I find game

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<sup>1</sup> Paper prepared for the “Workshop: Dialogue and Innovation in Contemporary Political Science,” co-hosted by *Political Studies* and The Centre for Research Methods in the Social Sciences, Oxford University, June 2008. I thank Susan Orr for her skeptical comments on this version. I presented earlier versions as a discussion paper at a conference on “Epistemologies of Rational Choice,” Department of Politics, NYU, December 2004 and as part of the Blalock Lectures on Advanced Topics in Social Research. ICPSR Summer Program - University of Michigan, August 2005.

theoretic models interesting and useful in something like the modest way David Kreps suggested some time ago.

As for ... the insights contributed by game theory, I contend that the major successes have come primarily from formalizing common sense intuition in ways that allow analysts to see how such intuitions can be applied in fresh contexts and permit analysts to explore intuition in and extend it to slightly more complex formulations of situations. . . . [G]ame theory has succeeded when it begins from a common sense observation and takes a few small steps further along” (Kreps 1990, 87-8).

Although I would not follow Kreps in conflating common sense and “intuition” - a move itself laden with suspect positivist philosophical baggage - I do think there exists a widely shared, common sense understanding that many people are instrumentally rational much of the time in most domains of social life.<sup>2</sup> When anyone premises her instrumental actions on what she expects some relevant others might do, she confronts a situation of strategic interdependence. Game theoretic models, as Morgenstern suggests in the second of my epigrams, help us understand what it means to be rational in a variety of more or less complex strategic settings.<sup>3</sup> As such, they are tools useful for the “analysis of the concepts used in social reasoning” (Rubinstein 1991, 909). Like economic models more generally, game theoretic models, in other words, arguably are most useful for conceptual rather than for primarily empirical purposes.<sup>4</sup>

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<sup>2</sup> As Harsanyi (1986 , 84) suggests our common sense concept of rationality suggests simply that “human behavior is mostly goal-directed, often in a fairly consistent manner, in many important classes of social situations.”

<sup>3</sup> This view, while not commonly endorsed, is not wholly idiosyncratic either. Hence, after discussing the assumption that actors in game theoretic models have complete, transitive preferences, the author of one primer suggests: “These assumptions, though, are preliminary and do not define what we mean by rational action. With them alone we cannot yet say what actions or strategies a person should choose in an interdependent-choice environment. Thus, because game theory’s ultimate purpose is to formulate general rules of action in such choice situations, *game theory does not assume that people are rational; rather, it attempts to define what we might mean by this concept.* Put differently, game theory seeks to identify the decisions that people might be reasonably expected to make in interactive decision-making situations, and these decisions correspond to what we mean by rational action” (Ordeshook, 1992, 61 stress added).

<sup>4</sup> This is a possibility typically neglected in putatively methodological discussions: “... few writers on economic methodology recognize that the activities of formulating economic models and investigating their implications are a sort of conceptual exploration. Instead, most

By contrast, I find the prevailing case for game theoretic methods among political scientists - and this includes many of my friends and colleagues - arrogant almost beyond belief. This arrogance is not uniform.<sup>5</sup> Nor is it always, or even usually, personal. Instead it is a peculiar intellectual arrogance that claims a monopoly on “rigor” and “clarity” and “precision” in ways that often are simply mystifying to me. Advocates of PPT rarely, if ever, provide much evidence that they base such proclamations on careful and charitable examination of even the exemplars of competing modes of social inquiry. My aim in this paper, however, is not to recommend some alternative approach to thinking about politics. Instead I aim simply to deflate common justifications of PPT in hopes of clearing the way for a more modest and hence more plausible defense of game theory and its uses in the discipline.

It will be helpful at the outset to state the obvious. Many political scientists have embraced game theoretic analysis. Over the course of two plus decades, advocates of PPT have written a number of systematic textbooks on the subject (Ordeshook 1986; Morrow 1994, Austen-Smith & Banks 2005; McCarty and Meirowitz 2007). In this respect, game theory, arguably is unique. So, when enthusiasts, for example, announce: “Investing in formal theory is investing in our discipline’s basic intellectual infrastructure” (Cameron and Morton 2002, 804) it is fair enough to imagine that they not only have great faith in formal theory in some generic sense, but game theoretic approaches in particular. The problem is that game *theory* is not one.<sup>6</sup> It is, as some practitioners sometimes admit, merely a “methodology,” a set of mathematical techniques for modeling strategic interaction (Kreps 1990, 1; Austen-Smith and Banks 1998,

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mistakenly regard these activities as offering empirical hypotheses and assess them in terms of some philosophical model of confirmation or falsification” (Hausman 1989, 115).

<sup>5</sup> It is hard, for example, to take umbrage at this pronouncement: “Positive political theory is concerned with understanding political phenomena through the use of analytical models which, it is hoped, lend insight into why outcomes look the way they do and not some other way” (Austen-Smith and Banks 1998, 259). Even so, while this statement is modest enough, it also remains agnostic about a set of important issues about the uses of models.

<sup>6</sup> Game theory is, on my view, simply one “family of models” among others collected together under a name “appropriated for dramatic effect” (Schelling 1978, 87-102). And, of course, one might argue that a theory is nothing but a collection of models (Clarke and Primo 2007). While I am skeptical of that claim, I set it aside for present purposes.

261; Cox 1999, 158; 2004, 177-83). And while this methodology is, I think, extremely useful, it is not in itself a “theory” of politics or of anything else.

Having stated the obvious, however, the important question remains: what are game theoretic methods useful *for*? I already have intimated my answer. It is just here, however, that proponents of PPT step forward and confidently advance a very different interpretation of game theoretic models and how they fit into the larger enterprise of social scientific inquiry. On this account game theoretic methods are useful because, starting from a set of explicitly stated assumptions, in particular the assumption that the agents who populate our models are rational, they allow us to derive predictions about “real world” behavior that then, in principle at least, can be tested empirically. This interpretation animates most critical discussion of game theoretic methods among political scientists.<sup>7</sup> Even quite vociferous critics of positive political theory accept it.<sup>8</sup> Unfortunately, at best, this interpretation is contestable. It proceeds from the unstated assumption that game theory properly is understood as an empirical enterprise in the first place. In so doing it ignores the qualms of esteemed practitioners who think it dubious that “the object of game theory is to predict behavior ... or indeed, that it is capable of such a function” (Rubinstein 1991, 909). It also has baleful consequences for the discipline, simultaneously reflecting and reinforcing the unfortunate propensity of political scientists to treat all important problems of social science as empirical problems.<sup>9</sup>

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<sup>7</sup> It informs recent notorious critics of the enterprise (Green and Shapiro 1994). It also informs nearly all replies to those critics from advocates of PPT (e.g., Chong 1995; Diermeier 1995; Lohmann 1995; Cox 1999). Likewise, it inspires the current enthusiasm for the empirical testing of formal models generally (Morton 1999; Cameron and Morton 2002) and for the well-funded initiative on the “empirical implications of theoretical models” in particular (Granato and Scioli 2004; Aldrich and Alt 2003). For a partial, not entirely unsatisfactory dissent from both the critics of game theory in political science and the standard pattern of defense see Johnson (1996).

<sup>8</sup> Hence, even Gerry Mackie (2003, 27) who goes to great - indeed, I think quite excessive and misguided - lengths to establish that central claims of positive political theory are not supported empirically, concedes: “I cannot imagine doing without non-cooperative game theory, which in the right hands yields rich insights into social life, along with testable, and supported, predictions.”

<sup>9</sup> On this tendency see Johnson (2002; 2003; 2006).

At bottom, then, my skepticism about efforts to subsume game theoretic methods into PPT has less to do with the technical apparatus on which it relies, than on the interpretation of that apparatus advocates of PPT advance and of the uses to which they claim to put it. The thrust of my argument will be that to be useful game theoretic models presuppose interpretation in several ways that render them continuous with, rather than distinct from competing modes of political inquiry. I thus agree with Rubinstein when he insists in the first of my epigrams that the technical apparatus of game theory requires interpretation where, as he subsequently explains, an interpretation just “is a mapping which links a formal theory with everyday language” (Rubinstein 1991, 909). The problem is not simply that advocates of PPT devote very little attention to such matters. Worse, when they do worry about matters of interpretation they labor under the influence of an unpersuasive, vaguely positivist understanding of the task (e.g, Riker 1990; Krehbiel 1998, 7-8, 20). So I also want to distance the recognition that formal models stand in need of interpretation from indefensible positivist understandings of what it means to interpret a particular model or the enterprise of modeling more generally. None of this is especially contentious in disciplines outside of political science. Indeed, my argument relies on the views of some respected philosophers and economists. And while I do not offer a fully developed alternative to the PPT interpretation of game theory, I do hope to point in the direction of one.

## **The Rhetoric of Arrogance**

Advocates of PPT, if you take them at their word, seem to be a remarkably arrogant bunch.<sup>10</sup> They, for instance, regularly announce that their work is characterized by a level of “rigor and precision” (Morrow 1994, 6; Gates and Humes 1998, 5-6) that, tacitly at least, they find lacking in other research traditions. Sometimes this tacit comparison breaks through in refreshingly frank ways, as when we are told that research that incorporates game theoretic

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<sup>10</sup> Curiously, advocates of PPT also seem especially anxious that they and their work are misunderstood or, worse, neglected. This anxiety has become a defining feature of several generations of formal modelers who, in their texts seek to surmount various perceived barriers to a proper, wider appreciation of PPT (e.g., Ordeshook 1986, ix-xiv; 1992, 1; Gates and Hume 1997, ix,5; Johnson 1998, ix).

modeling “seeks to satisfy a rigid definition of ‘theory,’ and not some ambiguous criteria of good journalism and insightful comment” that, presumably, characterizes most of what passes for social science in competing research traditions (Ordeshook 1986, ix).<sup>11</sup>

I find this arrogance increasingly irksome and have assumed that it is voiced as much to stiffen resolve among the faithful as to persuade unbelievers. Since it typically appears either in the introduction to texts or in their resounding conclusions, I usually can pass it over quickly enough. Eventually, though, one wonders just what advocates of PPT have in mind when they arrogate clarity and rigor and precision to themselves while thereby at least tacitly withholding such virtues from others. The answer seems to consist in two claims. First we have what I call the *explicitness thesis*. It holds that:

[i] Formal models incorporate precise, explicitly stated assumptions.

Thus, we are told that game theory demands a rigor that “forces the modeler to decide precisely what the assumptions of the argument are” and so to avoid the poorly specified or unstated assumptions to which verbal argument is susceptible (Morrow, 1994, 6). Others likewise insist that “[f]irst and foremost, formal analysis requires that the modeler make assumptions explicit” whereas merely verbal arguments “often possess hidden or blurred assumptions” (Gates and Humes 1997, 6).

The explicitness thesis grounds a further thesis which I will call the *deduction thesis*. It holds that:

[ii] Formal models show unambiguously how, starting from explicitly

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<sup>11</sup> This contrast sometimes comes packaged as a thumbnail teleology in which PPT emerges as the pinnacle of political inquiry. Thus, in their undergraduate primer for PPT Shepsle and Bonchek (1997, 5-8) relate an inspiring tale of how post-WWII political science freed itself from the limitations of “story telling” and “normative hand wringing” in order to produce “scholarly tomes” inspired by “reformist sentiments” and packed with “detailed contemporary description and political history.” Then, based on more systematic description and quantification, the discipline was ready for theory! Compare Morton (1999, 13-22) and Cameron and Morton (2002, 797).

stated assumptions, the analyst deduces particular predictions about the “real world”.

Hence, proponents of PPT insist that “Arguments structured by formal logic and mathematical analysis are explicit and unambiguous. Such models demand that the links between assumptions and analysis are clear. ... there is no room for whitewashing the details. Formal modeling requires a precision that rewards the investigator with clear insight, consistency of argument, and explicit reasoning” (Gates and Humes 1997, 6). Likewise, “formal models allow us to see exactly why the conclusions of a model follow from its assumptions” (Morrow 1994, 6).

In the remainder of the paper I focus on these two theses. Although they are entangled in practice I will separate them for purposes of argument and address them roughly in order.<sup>12</sup>

## **Questioning the Explicitness Thesis**

Simply stating one’s assumptions, of course, hardly is a virtue in and of itself. This becomes apparent upon reading work that incorporates formal models. There one regularly encounters a significant gap between the mere statement of an assumption and any effort to actually justify it.<sup>13</sup> In other words, there frequently is a marked disparity between the formulation of a model and the interpretation offered for it.

Consider a convenient and ironic illustration of this pattern. In her treatment of the

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<sup>12</sup> The entanglement is fairly clear in the passages I cite in the text. Consider another couple of examples: “A nonformal model becomes a formal model when a researcher expresses the real world situation in abstract and symbolic terms in a set of explicitly stated assumptions. ... Formal models have explicitly stated assumptions about reality which are used to derive predictions about reality. Formal models are deductive, because conclusions proceed from assumptions” (Morton 1999 36-7). Likewise: “Formal theories ... tend to be explicit, they are cumulative in many respects, and often they generate plausible and testable propositions about observable political phenomena” (Krehbiel, 1998, 18). Compare Cameron and Morton (2002, 793-4).

<sup>13</sup> I want to forestall an objection here. I am not worried that assumptions are not “true” or that modelers make no effort to establish whether they are or are not. In other words, I am sidestepping the realism-instrumentalism dichotomy into which most critical discussion of modeling rapidly falls (e.g., MacDonald 2003). I merely note that practitioners of PPT often skip the important interpretive step of motivating this or that assumption in the banal sense of explaining why they are making it.

exigencies of systematically linking formal (mostly rational choice) models to empirical research, Morton endorses the general view that formalization contributes to rigor by insuring the precision and clarity of basic assumptions. So too, she tacitly assumes that merely verbal, “non-formal” statements are somehow methodologically suspect (Morton 1999, 39-45).<sup>14</sup> In making her more general argument she relies on several running examples. For present purposes her discussion of one paper - Banks and Kiewiet (1989) - is especially revealing.<sup>15</sup> Morton is concerned, among other things, to show how formal models can illuminate substantive problems - in this instance perplexing patterns of candidate competition in U.S. Congressional elections - in ways that research not informed by formal theory cannot. In particular, she hopes to show how, because Banks and Kiewiet develop a model that proceeds from “explicitly stated assumptions,” they not only improve on prior, largely inductive research, but also establish the “agreed-upon premises for all subsequent arguments” (Morton 1999, 36-7). This is a particular example of her more general claim that formal models are essential to progress in the discipline.

The curious thing is that, having chosen Banks and Kiewiet to exemplify the virtues of formal rational choice models, Morton herself tacitly but quite thoroughly undermines our confidence in the explicitness thesis (Morton 1999, 36-8). She first lists “some of the

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<sup>14</sup> “One source of puzzlement among formal modelers is the belief by some in political science that non-formal models make less restrictive assumptions than formal model and that, because of this, non-formal models have advantages over formal models in empirical study. Imprecision is argued to have an advantage over precision because ambiguity is assumed to be more general and flexible than exactness” (Morton 1999, 41). For another version of this complaint, stated even *less* charitably, see Cameron and Morton (2002, 794).

Although I have never met one, I will take Morton’s word for it that there are political scientists who affirm, either explicitly or as an “unstated assumption,” the value of imprecision and ambiguity. Observe, though, that the authors whom she invokes to illustrate her point at no point sing the praises of ‘imprecision and ambiguity.’ In fact, I suspect that most critics of PPT simply challenge whether formal theory somehow has the sort of monopoly on clarity, precision, and rigor that its advocates regularly claim. Their point, I suspect, is not to endorse imprecision but to ask what it means to be precise. See, for instance, Norton (2004).

<sup>15</sup> On several occasions one of my colleagues has protested that this is a paper that the authors themselves would not consider their best work. As a matter of both biographical fact and independent assessment that may well be so. But it is strictly speaking beside the point here. I focus on this paper because Morton presents it as an exemplar of how formal models help illuminate political reality. It therefore allows me to assume the burden of argument.

assumptions” that Banks and Kiewiet make:

- that there are three possible candidates (an incumbent and two challengers);
- that the candidates are motivated by the prospects of electoral success rather than policy preferences;
- that the incumbent (credibly) aspires to only one more term in office and, if she wins it, will then retire;
- that the challengers face a dynamic strategic problem - at  $t_1$  each must decide whether to enter a primary to select the candidate to run against the incumbent and at  $t_2$  each must decide whether to enter a primary race for the open seat the incumbent will have vacated;
- that the challengers can run in only one general election and that if either wins the primary at  $t_1$  and then defeats the incumbent, the other will not challenge at  $t_2$ .

This indeed is only a partial list of the assumptions in the Banks/Kiewiet model. Morton concedes that several of them are unrealistic, that they may be “either unverified or false,” and that Banks and Kiewiet might well have deployed a different model based on alternative assumptions. None of that, from my perspective, is either surprising or terribly problematic.

The real trouble is that, despite the fact that she selected this paper as an exemplar of how PPT trades on explicitly stated assumptions, Morton herself does not seem entirely clear on why Banks and Kiewiet proceed as they do. After listing the assumptions in their model, Morton states “I *suspect* that Banks and Kiewiet assume three candidates in order to ...” and “I also *surmise* that Banks and Kiewiet assume that candidates maximize the probability of winning in order to ...”. If clarity and precision in PPT reflect the rigorous explicit formulation and defense of assumptions, why is Morton left supposing and speculating? And if even an accomplished and sympathetic reader such as Morton remains unclear about what motivates the very authors whose essay she herself uses to illustrate the virtues of formalization, why ought we to expect that others, less adept at, or less charitably disposed toward, formal modeling will find the enterprise as clear and precise and rigorous as advocates of PPT proclaim?

This predicament reflects ill-founded expectations. The modern positivists from whom advocates of PPT derive their brand name were preoccupied with formalizing the scientific enterprise and conceived of formal constructs as “uninterpreted.” Positivism is unpersuasive generally and with respect to social sciences in particular. There is no need to rehearse the

reasons here.<sup>16</sup> Nor is there any reason to chastise advocates of PPT for failing to faithfully implement the positivist program. It is by no means clear that they could do so if they wanted. The important thing to see is that advocates of PPT still presume that the move from “nonformal” to “formal” representations magically banishes (or at least radically mitigates) the ambiguity and imprecision the purportedly characterizes merely verbal formulations. This should be apparent from the discussion thus far. The point nevertheless bears emphasis.

Morton draws the distinction between “non-formal” and “formal” models in the following way:

*Non-formal model* - a set of verbal statements about the real world. These statements involve idealization, identification and approximation, but are given in terms of real observables rather than symbols or abstracts. The statements may be presented in a diagram or a graph. Sometimes these statements are directly tested as hypotheses about the real world.

*Formal model* - a set of precise abstract assumptions or axioms about the real world presented in symbolic terms that are solved to derive predictions about the real world (Morton 1999, 61).<sup>17</sup>

Morton actually discusses this basic distinction at considerable length (Morton 1999, 33-49). She makes it clear that, like most proponents of PPT, she believes that formalization not only confers a sort of clarity, precision and rigor unavailable to verbal formulations in particular instances, but that it is necessary to scientific progress more generally. It also is clear that this distinction and the virtues she attributes to formalization faintly echo the aspirations of modern positivism. Yet the echo is quite weak. In practice, advocates of PPT depart significantly from positivist impulses just insofar as the formal apparatus they deploy hardly is uninterpreted in anything like the positivist sense.

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<sup>16</sup> See e.g., Walsh 1987; Hausman 1992, 70-82, 283-5, 297-8. See also Johnson (2006).

<sup>17</sup> One wonders what rhetorical effect Morton hopes to sustain by drawing this distinction as she does. Rather than simply differentiating verbal from mathematical or formal, she assimilates the former to the “non-formal” and thereby tacitly depicts it as derivative or subsidiary, as “non.” I set this matter aside here. It nevertheless is useful to compare Morton’s discussion of models and their uses with another view. Thomas Schelling, for instance, suggests that a model consists in “a precise and economical statement of a set of relationships that are sufficient to produce the phenomenon in question” and that a model is a “tool” that affords “help in communicating” (Schelling 1978, 87,90).

Proponents of PPT typically have little to say about the task of interpreting formal models. In practice they reduce this task to the evaluation of results or, sometimes, of assumptions, where evaluation specifically means *empirical* evaluation (Gates and Humes 1996, 12; Krehbiel 1998, 21; Morton 1999, 102). And they treat this as a fairly unproblematic, if quite important, process that involves supplying verbal “embellishment” that facilitates translation of the predictions a model into hypotheses that might be empirically tested (Shepsle & Bonchek 1997, 11). I return to the unexamined assumption that grounds this preoccupation - namely, that game theoretic models actually are in any plausible sense directly empirical - in the next section. For present purposes I focus instead on how the need for interpretation enters into PPT in ways that many advocates of the enterprise neglect. This, in turn, allows us to question whether they can draw a sharp distinction between “formal” and “non-formal” models in anything like the categorical way Morton purports to do.

The problem is that “formal” models necessarily come pre-interpreted. This is obvious when we consider the various toy games discussed in elementary expositions of game theory - “chicken,” “battle of the sexes,” “prisoner’s dilemma,” and so forth - each of which is animated by a stock narrative (Norton 2004, 124).<sup>18</sup> Advocates of PPT no doubt will dismiss observations regarding such off-the-shelf examples as of dubious merit. So consider again the Banks/Kiewiet model. There we encounter a whole raft of political concepts - “candidate,” “running for office” (as opposed to, say, *standing* for a seat), “general election” and “primary,” “open seat,” “incumbent” and “challenger” (including both “strong” and “weak” types of the latter), “winning,” “deterrence,” and so forth - all stated verbally. These concepts shed none of their pedestrian meanings or everyday “imprecision” in the model. Yet, crucially, they provide the very terms within which, unavoidably, Banks and Kiewiet are able to stipulate the components of their model. Here, in other words, as in the process of constructing models generally, everyday language sustains the “substantive interpretations” we ascribe to the abstract parts of the model (Ordeshook 1986, 9). Absent such interpretations the model remains quite literally meaningless.

Advocates of PPT may not be overly troubled by this observation. But this is an instance

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<sup>18</sup> This is not a *shortcoming*. As Schelling notes in the same passage I cited in the last note, a model is useful for facilitating communication “especially if the model has a name.”

of a much broader point that ultimately deflates their exaggerated claims to precision, clarity, and rigor. For it simply is not enough to specify abstract sets of actors, their goals and preferences, the actions available to them, and possible outcomes as components of a model. As Rubinstein suggests, in constructing and solving a game theoretic model we necessarily are concerned with, and solely with, what the players deem “relevant.” A model thus must aim not just to represent the “physical rules of the game” but to capture “the relevant factors of the situation as perceived by the players” (Rubinstein 1991, 917).<sup>19</sup> In other words, the concept of rationality remains ill defined outside of particular contexts of action as seen by the agents who populate our models.<sup>20</sup> In constructing a model of some situation, then, we cannot *assume* rationality largely because we do not know what it might mean in the abstract, outside, that is, of some situation. The task of modeling strategic interaction, therefore, is more complex than even Morgenstern, for instance, lets on. This is so just insofar as what counts as the “situation” cannot be specified independently of the understandings that we can plausibly attribute to the agents who populate our models. The implications of this general point emerge when we turn to the deduction thesis.

Before proceeding, however, it will be useful to enter an important caveat. Advocates of PPT proceed as though articulating the rationality assumption requires nothing more of them than being able to specify a preference ordering or utility function for the agents who populate their models (e.g., Riker 1990, 172; Austen-Smith and Banks 1998, 263-4; Doron and Sened 2001, 20-22; McCarty and Meirowitz 2007). Such formalizations, however, merely a modeling device. They afford a convenient way of representing the beliefs and desires that constitute reasons for intentional agents. In other words the formal apparatus used to specify a preference ordering or utility function provides a technology for modeling unobservable features of agents.

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<sup>19</sup> “The single most important decision in modeling is the design of the game. The game states what choices we believe the actors see in the situation, what they understand about their choices, what consequences they believe follow from their decisions, and how they evaluate those consequences” (Morrow 1994, 57). Compare, surprisingly enough, Riker (1990, 172-3). I take it that this, at least in part, is what Myerson (1991, 4) means when he insists that the players who populate game theoretic models are not just rational but “intelligent” as well. This, of course, raises the issue, that I set aside here, of how to understand “see” or “perceive.”

<sup>20</sup> There are various ways to support this claim. See Schelling (1960), Nozick (1993), and Sen (2002).

And while they do incorporate familiar criteria of consistency (such as completeness and transitivity) that capture the common sense conception of rationality I mentioned at the outset, they say little if anything about what counts as consistency in any particular setting. The tools laid out in the opening chapters of standard game theoretic texts provide us with a language for talking about what rationality might mean and no more. They simply allow us to ask “what ‘rational’ can mean.”

## Questioning the Deduction Thesis

Advocates of PPT refer regularly to *the* rationality assumption as though it were well defined in some abstract sense. Consider, for instance, the following, wholly unsystematic sampling of remarks.

“Game theory ... requires the assumption of rationality ...” (Morrow 1994, 7).

“Much of political game theory is predicated on the idea that people pursue goals subject to constraints imposed by physical resources and the expected behavior of other actors. The assumption of rationality is often controversial. Indeed one of the most lively debates in the social sciences is the role of rationality and intentionality as a predictor of behavior” (McCarty and Meirowitz, 2007, 6).

“Recall the two basic assumptions of game theory: rationality (utility maximization) and common knowledge” (Gates and Humes 1997, 12n)

“The rationality assumption has been used most extensively and has seen its fullest flowering in economics. But there is nothing distinctly economic about rational behavior, as we shall see” (Shepsle and Bonchek, 1997, 15).

“Perhaps the most serious intellectual threat to the rational choice approach comes from empirical findings that challenge the rationality assumption” (Lohmann 1994, 128).

“Of course, this challenge to rational choice theories only makes sense if individual theoretical statements, such as the rational actor assumption, can be tested in a meaningful sense” (Diermeier 1994, 62).

“Rational choice theory is based on two central assumptions.: methodological individualism and purposeful action. [. . .] Purposeful action requires further clarification because it lies at the heart of the rationality assumption on which this entire research program is founded” (Doron and Sened, 2001, 20-21).

On the canonical PPT interpretation, when we construct game theoretic models we start from an assumption of rationality that, in combination with various subsidiary assumptions, allows us to deduce predictions that we, in turn, treat as hypotheses about the “real world.”<sup>21</sup> The latter can be (and, at least in principle, are) tested empirically.

Here proponents of PPT proceed from a crucial, unexamined, and indefensible assumption. They proceed as though empirical performance is the primary, perhaps the sole, criterion for assessing social science. Philosophers of science, of course, have for some time rightly argued that this basic assumption is mistaken. They remind us, quite plausibly, that assessments of sciences and how they advance are “multidimensional” in the sense that when making any such assessment we must concern ourselves not simply with empirical performance but also, for instance, with the resolution of conceptual problems and the development and refinement of instruments (Hacking 1983; Kitcher 1993, 90-126; Laudan 1977; 1981; 1984). Advocates of PPT who, like practicing social scientists generally, tend to be philosophically disinclined, might complain that proponents of this revisionist view disagree among themselves. So what? We need neither adjudicate those disagreements nor remedy all of the difficulties with any particular philosophical position to appreciate the basic point that what political scientists commonly and naively call “the accumulation of empirical knowledge” is a woefully inadequate basis on which to describe or assess social scientific progress (Johnson 2002; 2003).

Proponents of PPT - and their critics - proceed undeterred. It is fair to say that the entire discussion of rational choice models in political science has been framed as an epistemological debate between realism and instrumentalism (MacDonald 2003).<sup>22</sup> As a result, that discussion focuses almost exclusively on matters of empirical performance with realists preoccupied with the empirical adequacy of assumptions and instrumentalists with the empirical adequacy of

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<sup>21</sup> Using game theoretic technology does not entail a commitment to the canonical rational choice approach. See, for instance, Camerer (2003) and Rubinstein (1998).

<sup>22</sup> For examples of how these positions inform debate see Elster (1986), Satz and Ferejohn (1994) and Hausman (1995). Clarke and Primo (2007) offer a promising recent effort to set this unhelpful dichotomy aside.

predictions.<sup>23</sup> This dichotomous framework truncates debate by encouraging the discipline to neglect of views of science and how we ought to assess it that cut across the realist-instrumentalist divide. Pragmatists, for instance, are realist about theoretical entities and instrumentalists insofar as they see theories as tools for solving both empirical and conceptual problems. (Laudan 1981). Pragmatism thus provides a framework for interpreting the modeling enterprise less as the deduction of claims for empirical testing than as a sort of conceptual analysis (Hausman 1992, 78-82).<sup>24</sup> Rather than present an abstract theoretical argument for this possibility I instead will illustrate it.

Consider the Banks/Kiewiet model yet again. This model does not - Morton's claims notwithstanding - assume rationality and deduce predictions from that assumption.<sup>25</sup> Instead, Banks and Kiewiet ask what it can mean to be rational in a quite specific and seemingly not terribly conducive strategic circumstance. They provide an answer and, counter-intuitively, establish the sufficient conditions under which what appears to be a perplexing (because seemingly irrational) pattern of observed political behavior actually can be shown to be rational in just that sense. Banks and Kiewiet *rationalize* the political behavior in question.<sup>26</sup> In so doing

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<sup>23</sup> Prompting Morton, for instance, to address both sorts of concern in her discussion of the fundamentals of empirical evaluation (Morton 1999, 101-277).

<sup>24</sup> Morton acknowledges that not all formal models are empirical. Some, like Arrow's theorem, are normative. Others like the Rubinstein bargaining model are exercises in "pure theory" and so explicitly not designed to empirically evaluated. See Morton (1999, 79-80, 59-61). At least two disquieting questions arise here. First, are the normative concepts (embodied in his axioms) that Arrow explores part of the "real world" or not? Second, if pure theory is not aimed at empirical inquiry of what use is it? I suspect that the answers to such questions push us in the direction of interpreting all models - applied as well as pure, explanatory as well as normative - as conceptual exercises.

<sup>25</sup> Morton can be forgiven for this misinterpretation given that Banks and Kiewiet themselves claim that their model is susceptible to "empirical examination" precisely because it enables them to "make predictions about candidate behavior" (1989, 105).

<sup>26</sup> "What is the relation between a reason and an action when the reason explains the action by giving the agent's reason for doing what he did? We may call such explanations *rationalizations*, and say the reason *rationalizes* the action" (Davidson 1980, 4). For current purposes, this notion of intentional explanation trades on the underlying view that canonical game theoretic models invoke desires (preferences) and beliefs (expectations) in an unavoidable

they extend the domain over which our common sense conception of strategic rationality plausibly offers explanatory leverage. The conclusion of their modeling exercise, in other words, is not a prediction. Instead, their results warrant greater confidence in our ability to invoke particular causal mechanism when we offer explanations.

Advocates of PPT may find this re-interpretation of the Banks/Kiewiet model perplexing. It may therefore be helpful to sketch how I read the paper. Banks and Kiewiet start from a set of empirical observations that suggest what they consider a “puzzling” pattern of electoral competition (Banks and Kiewiet 1989, 1000). In races for seats in the U.S. Congress, incumbent legislators tend to encounter relatively weak challengers even in circumstances where stronger challengers are available.<sup>27</sup> Two features of this pattern are important. First, strong challengers are more likely to wait until an incumbent legislator retires from office and run for an “open” seat rather than challenge the incumbent directly. Second, weak challengers are more likely to run against the sitting legislator despite the quite significant and fairly well recognized electoral advantages that incumbency bestows. While the first of these features might seem unproblematic, the second is difficult to fathom. Prior research had ascribed this propensity of weak challengers to mount seemingly quixotic campaigns to a variety of irrational or non-rational factors such as partisan duty, expressive motivations, wishful thinking, and so forth.

Banks and Kiewiet find such explanations wanting. They are right to do so. It is only possible to attribute irrational or extra-rational behavior to some group of agents against the background of some notion of what would constitute rational behavior on their part. At bottom, Banks and Kiewiet are trying to establish that background. They ask what it would mean for challengers - whether weak and strong - to act rationally in the particular context of candidate entry in U.S. Congressional elections. They answer that it would be rational for a challenger of either sort to act in such a way as to maximize his probability of electoral success and they build a relatively simple game theoretic model that establishes sufficient conditions under which the

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way (Elster 1986; Cox 2004). This is perhaps a bit strong. Game theorists can depart from that underlying view at a cost - namely, giving up on the Nash program (Hausman 2000)..

<sup>27</sup> Banks and Kiewiet differentiate “weak” and “strong” candidates in terms of the past political experience, in particular whether the potential candidate has held prior elected office.

apparently perplexing pattern observed in empirical studies constitutes a Nash equilibrium.<sup>28</sup> As Banks and Kiewiet put it, they “show ... that under certain circumstances weak challengers making accurate, unbiased probability estimates will choose to run against incumbents for the same reason strong challengers decide to wait for an open seat - to maximize the probability of their being elected to Congress” (Banks and Kiewiet 1989, 1002).

The reasoning runs roughly as follows. Although the probability of defeating an incumbent legislator in a general election is very low, for a weak challenger it is higher than the probability of defeating any one of several possible strong competitors in a primary election that would decide her party’s nominee for a general election. By contrast, for a strong challenger the probability of winning a primary, even against other strong competitors, and of then beating a non-incumbent from the other party, is greater than that of defeating an incumbent legislator, assuming that the latter is not unusually vulnerable for some idiosyncratic reason (e.g., scandal). In short, what Banks and Kiewiet do is establish the *relevance* of competition in the primary as opposed to the general election for the decision-making of both weak and strong challengers. They specify the context within which potential challengers, weak and strong, reach a decision regarding whether or not to mount a campaign. The mathematics of their argument derives whatever cogency we attribute to it from this initial interpretive shift not from the trivial “assumption” that the actors who populate their models are utility-maximizing.

Three things are important here. First, the Banks/Kiewiet model is not idiosyncratic. It exemplifies the general pattern spelled out in texts on the use of game theory in political science. According to that template, when constructing a model, we specify a set of actors, a set of available strategies, and the conditions under which strategy combinations will generate

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<sup>28</sup> These conditions - specified for different actors in the model both in terms of the probability that he might prevail in some election and the utility he would derive from various electoral outcomes - emerge as *results* of Banks and Kiewiet’s model. They are not *assumptions* of the model. Rather they are meant to capture the circumstances under which the seemingly perplexing entry decisions of actors might constitute Nash behavior. Likewise, they are not *predictions* of the model. They do not represent a characterization of the anticipated behavior of actors in the model. Instead, they specify the consequences for different actors of adopting one or another strategy. Presumably each actor’s reasons for acting will consist primarily of an assessment of these consequences. These distinctions become important when we turn to the “empirical examination” Banks and Kiewiet conduct of their model.

equilibrium outcomes.<sup>29</sup> Second, what Banks and Kiewiet are doing is specifying the basic mechanism used in making a rational choice explanation. They are telling us, in Morgenstern's phrase, "what 'rational' can mean" in a particular situation and, importantly, identifying the conditions under which their conception holds. This is a conceptual task, that while essential to rational choice *explanation*, is not directly *empirical*.<sup>30</sup> Finally, the model that Banks and Kiewiet construct and the use to which they put it departs significantly from the standard interpretation advocates of PPT offer for their own enterprise. What they offer is "an explanation for the paradoxical pattern of congressional election competition" (Banks and Kiewiet 1989, 1008) that, because it trades on a well specified mechanism, allows them to expand somewhat the domain within which we might plausibly expect, provided specifiable conditions obtain, that mechanism to operate.

## One Obvious Objection

Those wedded to the standard PPT view of game theory might well find my reading of Banks and Kiewiet perplexing. It is unlikely that they will deny that the Banks-Kiewiet model of candidate entry is meant to be explanatory. That would require them, for example, to ignore the aim Banks and Kiewiet explicitly announce in their title. They might well object, though, that the model of candidate entry presented in the paper is not just *explanatory* - in the sense of addressing the conceptual problem of specifying causal mechanism animating an account of a perplexing pattern of behavior - but somehow directly *empirical* too. In other words, the orthodox might insist that Banks and Kiewiet do more than "merely" specify how an unobservable mechanism (i.e., the beliefs and desires that constitute reasons for potential challengers in Congressional elections to act in one or another way) operates under certain conditions in a manner that previous accounts lead us not to anticipate. They might insist instead that Banks and Kiewiet derive a prediction about how rational candidates will behave and then

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<sup>29</sup> For an especially useful presentation of this schema see Ordeshook (1986, 2-10).

<sup>30</sup> Here I assume without argument that rationalizations are explanations and that they are causal. This is in keeping with the claim that game theory contributes to causal explanation (Ordeshook 1986, xiii). On the importance of specifying mechanisms for the task of explanation see Johnson (2006).

test it. After all, the final third of the original paper is devoted to an “empirical examination of the model” (Banks and Kiewiet 1989, 1008-13).

Morton indeed asserts not only that “Banks and Kiewiet restate their results in the form of empirical predictions,” but that they explicitly “devised” their model to “to evaluate predictions or assumptions” (Morton, 1999, 56, 57). She seems clearly to interpret their paper in terms of the standard schema of ‘build a formal model, derive hypotheses or predictions from it, and test them empirically.’ It therefore is important to ask whether this schema captures what Banks and Kiewiet actually are up to when they examine their model empirically. When we attend to that question it is plain that they are not engaged in the enterprise Morton believes them to be.

Banks and Kiewiet rightly indicate that their “model gives only the conditions under which it makes sense for weak candidates to challenge incumbents while the strong do not. Other conditions will dictate other strategies” (Banks and Kiewiet 1989, 1008). After acknowledging the difficult measurement issues involved in specifying what counts as “strong” and “weak,” they turn to their first “empirical” task. Importantly, this involves the evaluation of neither assumptions made in the construction of, nor predictions derived from, their model. Banks and Kiewiet simply ask whether the conditions necessary for the results generated by their model to hold actually obtain. That is, they ask whether, given the perplexing pattern of behavior that initially prompted their inquiry, “weak candidates typically do face circumstances similar to” those they capture in their model (Banks and Kiewiet 1989, 1008). It is those conditions, those “circumstances” that sustain the simple probability estimates that animate the decisions of potential challengers.

Banks and Kiewiet, in other words, are interested not in establishing whether their model is true or false, but in persuading us that it might be useful in thinking about the problem that troubles them in the first place. This is a crucial task. Banks and Kiewiet must establish whether and to what extent the conditions actually obtain under which the seemingly “irrational” entry decisions of challengers can be characterized as Nash behavior. Otherwise, the explanatory mechanism they propose as an alternative to those that animate various accounts in the then extant literature would remain strictly speaking irrelevant. In the event, they are indeed persuasive on this point. While this clearly is crucial to their enterprise, however, it just as clearly is not a “test” of their model, its assumptions or predications.

In the next component of their “empirical examination,” Banks and Kiewiet aim to determine whether strong and weak candidates “in fact” behave the way the actors who populate their model behave (Banks and Kiewiet 1989 1009-10). To this end, however, they do not derive and test a “prediction” from their model. Instead they essentially establish with a new set of data the pattern of behavior - weak challengers entering against incumbents while strong challengers wait to run until the relevant seat is open - that puzzled students of political campaigns in the first place. This does not fall into any of the “four categories” of prediction that Morton delineates because it is not a prediction in any obvious sense at all.<sup>31</sup>

Finally, Banks and Kiewiet (1989 1012-13) invoke the underlying mechanism they identify with their model as a basis for exploring other features of their data set. In particular they suggest that the ‘rational choice’ mechanism they specify might afford a way of accounting for particular characteristics of ‘freshman’ classes in several Congresses. Maybe so. But in the first place, they do not offer much more than speculation on this matter. In the second place, this is not a prediction derived from their formal model. So, if this is an “empirical implication” of the Banks/Kiewiet model, it would be helpful to have some account of the sense in which that is the case. On the PPT interpretation we lack such an account and, arguably, the theoretical resources from which we might construct one.

In short, what Banks and Kiewiet refer to as the “empirical examination” of their model in no way conforms to the orthodox schema of “formulate a model - derive predictions from it - test predictions” that is central to the interpretation of game theory that advocates of PPT advance. Contrary to their own claim, Banks and Kiewiet hardly offer a “critical test” of “predictions” derived from their model. Indeed, they do not actually offer a test of the model at all. While this represents a failure, on my view, of the orthodox PPT interpretation of game theory, it does not represent a failure of game theoretic methods more generally. For, as I noted at the outset, there is no particular reason to *assume*, as advocates of PPT do, that game theoretic models are subject to empirical test in the first place. That is an assumption that requires some

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<sup>31</sup> Morton (1999, 102) identifies the following four sorts of predictions that might be derived from a formal model: “Point or equilibrium predictions; multi- or disequilibrium predictions; comparative static predictions; process or dynamic path predictions.” The differences between these are irrelevant for present purposes.

defense. And those who advocate the PPT interpretation of game theoretic methods simply do not provide such a defense.

## Conclusion

Rubinstein is right. A “proper interpretation” is central to the use and assessment of game theory. I have argued that among political scientists the standard PPT interpretation of game theoretic models is unpersuasive. The nature of my argument hopefully is clear. I am not offering an empirical survey and analysis of the ways political scientists have incorporated game theoretic methods into their work. I have not canvassed a sample of studies that incorporate models. Instead, I have criticized the most common - I would say orthodox - interpretation for how we should understand the use of game theoretic models in the discipline. I have focused on Morton’s arguments because she offers the most sustained recent case for embracing that interpretation. I have suggested that neither the explicitness thesis nor the deduction thesis - both of which are central to the PPT interpretation - apply to the example that Morton herself advances as paradigmatic of the virtues of formal theory generally and game theoretic modeling in particular.

I hardly anticipate that advocates of PPT will find my argument persuasive by my argument. They may want to distance themselves from the PPT interpretation of game theory. Alternatively, they might simply want to distance themselves from the particular way that Morton articulates and illustrates that interpretation. In either case, the task is to offer an alternative interpretation not just of this or that component of this or that model (as crucial as that task is), but interpretation of game theoretic modeling as an intellectual enterprise.<sup>32</sup> Fortunately, in recent years a number of younger researchers have undertaken that task.<sup>33</sup> Unfortunately, the general response among the orthodox has been to dismiss their concerns. Such dismissiveness, however, ignores Rubinstein’s challenge. The mathematical apparatus of game theory stands in need of interpretation. Otherwise, game theoretic methods rest on shifting sands.

I, of course, have not offered a full-blown interpretation of game theory and the way political scientists use it. I was clear about that at the outset. Insofar as they operate in the way I

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<sup>32</sup> For examples of this sort of enterprise see Kreps (1990) and Binmore (1990).

<sup>33</sup> See, MacDonald (2003), Lovett (2005), and Clarke and Primo (2007).

propose, game theoretic models allow us to make conceptual progress in a fairly simple and unproblematic way. They provide us with tools for specifying how rationality operates as an explanatory mechanism and for identifying the conditions under which we can expect it to operate reliably.<sup>34</sup> The methodological lesson here is that we might move in the direction of generality without embracing the idea that we can specify covering laws. Advocates of PPT tend to assume that we can and ought to pursue the latter (Riker 1990). Yet the quest for generalizations need not follow that pattern (Little 1993). Nor need a preoccupation with causal mechanisms induce skepticism about generalization due to the difficulty of specifying the conditions under which they operate in expected ways. Game theoretic models are useful in part because they afford an example of how we might avoid such skepticism.

That said, we - and this includes advocates of PPT - cannot “assume” rationality because in many situations we do not know what rationality might consist in. Nozick (1993, 133-5) rightly points out that the commonsense understanding of instrumental rationality occupies the intersection of nearly all more expansive conceptions of rationality. He also points out that even though this means when we discuss rationality we must have in mind *at least* the common sense understanding we thereby have made scant progress. This is because our more particular conceptions of rationality, and thereby that of the agents who populate our models, and our broader understandings of the world, the possibilities it contains, and of ourselves are interdependent. Game theory is useful for helping us explore that interdependence. And that is useful enough to set aside the arrogant claims bandied about by advocates of positive political theory.

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<sup>34</sup> Here an analogy might help. Case studies are useful, in part, because they exploit relatively “thick” descriptions of events in order to specify explanatory mechanisms and explore how they operate. On this point see Johnson (2006). Likewise, formal models use what we might call “thin descriptions” to do the same thing. This analogy is not terribly contentious; it is how I understand Thomas Schelling’s brief for using models (Schelling 1978, 87-91).

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