Rationality and the Foundations of Positive Political Theory

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Abstract

In this paper, we discuss and debunk the four most common critiques of the rational choice research program (which we prefer to call Positive Political Theory) by explaining and advocating its foundations: the *rationality assumption*, *component analysis* (abstraction), *strategic behavior*, and *theory building*, in turn. We argue that the rationality assumption and component analysis, properly understood, can be seen to underlie all social science, despite the protestations of critics. We then discuss the two ways that PPT most clearly contributes to political science (i.e., what distinguishes it from other research programs), namely the introduction of strategic behavior (people do not just act; they *interact*) and PPT's more careful attention to the theory-building step within the scientific method. We explain the roles of theory-building and of empirical "testing," respectively, in scientific inquiry, and the criteria by which theories should and should not be judged.

1. Introduction

In the last two decades, a new version of Political Science, has allowed us to make many advances in our knowledge of politics. This branch of Political Science, called Positive Political Theory (PPT),[1] arose as a response to the anthropological, psychological, and sociological approaches that had dominated the field during the three decades following World War Two. PPT, like each of earlier approaches to the study of politics, entails the creation of *analogies*. Analogies about politics are taken from biology (as in "the body politics" and "the organic state") the study of the family, the study of mechanical systems (e.g., *), and the study of physics (e.g., *), and economics (e.g., political markets) among others. Theory-building of any sort requires abstraction from reality, and the details of how that abstraction is undertaken – i.e., the assumptions upon which the model of reality is constructed – defines the class of things for which the model will be useful, and the class of things to which it cannot speak. Each of the above approaches to the study of politics has arisen, prospered, and, sometimes, declined, as the number of questions for which the relevant set of analogies is helpful has been identified, exploited, and, sometimes, exhausted.

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1. Nomenclature is confusing in this branch of political science. The most common general name for all of the work to which we refer is "Rational Choice Theory," but we avoid this moniker due to the misleading use of the word "rational," as we will discuss in Section 2. Other common names are "the new institutionalism," and "economic theories of politics," but we find the first term vague, as it often refers to the statist literature (e.g. Evans et al. 1985), and the second misleading and inflammatory. We choose the term Positive Political Theory because it is the most descriptive and least susceptible to misinterpretation.

Not surprisingly, it is the tendency within PPT to be as explicit as possible about the assumptions made in building models that has made both those models and the entire PPT enterprise comparatively easy to attack. Earlier theoretical approaches left much of the analogy unexplained and unjustified, making challenges difficult. Thus, whereas prewar debates tended to focus on the normative implications of theories, most of the controversy concerning PPT centers on the assumptions underlying the approach.[2] To date, however, the criticism of the PPT research program has missed two key points. First is the point already alluded to – that all research programs or approaches are analogies and hence involve abstraction and simplification. Second, critics have paid insufficient attention to the role of analogies within the scientific method more generally. Every paper or book written in political science or any other field, from each and every research program within each field, invokes not one but several analogies as it moves from asking a question to generating hypotheses to testing those hypotheses empirically. In other words, the very nature of the *scientific method* is to string together in a logically coherent fashion, a series of analogies about how the world works, in an attempt to improve our understanding of human behavior.

Thus, blanket criticisms that PPT is too abstract or too sparse or too insensitive to the complexities of the real world miss the point of theory-building and do nothing to challenge the value of the research program. These "baby-with-the-bath-water" fusillades merely stifle the debate that we need to engage in, namely the struggle over which analogies at each stage of the scientific method we like, and for which questions. The purpose of this paper is to reintroduce

2. Because PPT arguments are deductively valid, if their premises are true, then their conclusions *must* follow. Hence, objections to PPT models *must* focus on assumptions.

the core assumptions of PPT, in order to begin this latter, more constructive debate.

All PPT models share three assumptions, each or which generates frequent criticisms:

1. ***Rationality*** -- individuals make reasoned decisions.
2. ***Component analysis*** -- only small parts of the system are important in predicting any type of human behavior.
3. ***Strategic behavior*** -- individuals take into account what other individuals are likely to do before making decisions.

PPT's supposed "lack of empirical content" also draws criticism. This means either that abstract PPT models seem devoid of any "real world" referents, or that PPT research is rarely tested and, when tested, rarely passes (Green and Shapiro 1994). In the following four sections of this paper, we discuss each of these critiques, in turn. Our aim is to clarify what is and what is not PPT, and, in so doing, to illuminate the place of theory and empirical analysis in social science.

2. The Rationality Assumption

Although it has been a topic of much debate and scholarship, there is little agreement or understanding of what the rationality assumption means. The most common critique of the rationality assumption is that it is too demanding -- that people often act irrationally. However, when critics contend that people are irrational, they usually are making normative judgments about people's goals. For example, a critic of the rationality assumption would argue that it is irrational for someone to light themselves on fire, but that people sometimes do so. To assert that people are rational, we do not need to have anything to say about the *reasonableness* of their goals; different people find pleasure in different things (Satz and Ferejohn 1994). In fact, people may be willing to incur some pain in order to gain some pleasure. Rather, we employ the

rationality assumption to say that people make *reasoned* decisions to reach their goals, irrespective of what their specific goals may be. We turn now to discuss the definition of rationality basic to all of its uses in the social sciences.

2.1 Rationality as Instrumental and Reasoned Decision Making

Our conception of rationality begins with animal physiology at a fundamental level and goes as follows.[3] All living creatures, from humans to inchworms to politicians, experience pleasure and pain. Assumptions about the subject's beliefs concerning the sources of pleasure and pain lie beyond the scope of the rationality assumption. *Rationality simply posits that all behaviors are directed toward the pursuit of pleasure and the avoidance of pain.* At times, this behavior seems reflexive, such as the instinct to pull one's hand away from a flame.[4] Other times, it becomes quite complicated, as when a voter undertakes a cost-benefit analysis over multiple issues to arrive at the decision to support some political party or candidate. At heart, in our non-normative view, the assumption of rationality *accepts* that everyone has a different conception of what is pleasurable and what is painful, but *asserts* that everyone makes decisions in the same way, as follows.

In order to increase pleasure and decrease pain, we base our behavior upon expectations about the consequences of our actions.[5] These expectations evolve from our beliefs about cause-

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3. Much of the discussion of rationality and beliefs here follows Lupia and McCubbins (1995).

4. Note, however, that newborns may not have yet learned this "reflex" (their first response to pain is to cry, not to attempt to distance themselves from the pain source), and that adults with sufficient discipline can learn to "overcome" this "reflex." Even reflexes are learned behaviors.

5. Note that increasing pleasure and decreasing pain does not imply maximizing utility. And, it

and-effect relationships in the environment, and these beliefs, in turn, have their genesis in our experiences. Changes in behavior, under this model, derive from changes in the environment. That is, we recognize that changes in the environment cause changes in pleasure and pain, and we adapt our behavior accordingly. Using the available information and cognitive capacity, and a belief structure that has been updated since birth, we make decisions that we believe will best help us to achieve pleasure and avoid pain. This is why, and with good reason, the rationality assumption is so often equated with the assumption that behavior is *instrumental*.

Rationality is reasoned choice. The reason many do not accept the rationality assumption is because we cannot *see* people making reasoned choices. So, when we watch people faced with a voting choice or a choice among products at the supermarket or a choice about whether to catch, avoid, or be hit by a thrown ball, we cannot see them reasoning out their decisions; we just see them reacting. Indeed, most decisions we make everyday seem to come about without any reasoning at all. We wake up in the morning every day and do the same things in the same way. We drive to work along the same streets and park in the same spot. Most of our day is made up of actions like these; they do not seem to require active cognition to accomplish.

There are, however, a number of things for which we actually do sit down and reason. In those rare instances where we actually see people thinking through their decision, we find that they do not gather much information. When we ask people to explain why they voted the way they did or why they purchased a certain car, they cannot even describe many of the facts that

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is compatible with, but not tantamount to, Simon's satisficing, which entails choosing the first satisfying solution based on some subjectively determined criteria (Simon 1955).

were important to their decision.[6] Churchland (1995: 22) notes:

> Humans are famously bad at describing their sensations -- of tastes, of aromas, of feelings -- but we are famously good at discriminating, enjoying, and suffering them. ...And yet, while we all participate in the richness of sensory life, we struggle to communicate to others all but its coarsest features. Our capacity for verbal description comes nowhere near our capacity for sensory discrimination.

Catching a baseball is a good example of how we make these reasoned decisions with so little information (Cipra 1995; McBeath, Shaffer, and Kaiser 1995). Every time we catch a baseball, we grow and reinforce neurons in our brains that allow us to react more quickly and accurately. When we first began to play catch, we were not very good at it, and we had to concentrate harder. Only through practice and trial and error did we learn. Catching is not a reflex; we have actually developed the neurons that allow us to judge the trajectory, angle of descent, and speed. Once we develop these neurons, we no longer have to do the calculations or adjustments. However, just because we "automatically" drive to work the same way and catch a baseball when it is thrown at us, it does not mean that we are not reasoning. Rather, we are simply relying on processes that we have developed through experience.

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6. Stigler (1961) points out that people spend more time when choosing durable goods as compared to other goods. For example, most car buyers do put some effort into their decision, comparing the price, color, safety rating, appearance, and optional features of different products. We are just pointing out that they also ignore a great deal of data that they could access if they chose to, e.g., about the details of the engine and the construction of the car. A large burden is put on the "feel" of the car, which consumers can distinguish better than they can articulate.

We developed physiological processes to deal with these problems because we have dealt with them many times. Since we have actively solved these problems in the past, we are able to just reapply what we have learned in the past to new episodes.[7] Along these lines, Lupia and McCubbins (1995) argue that knowledge is not simply an encyclopedia of facts; rather, it is related to performance. They explain that people do not need *all* the information available to them; rather, people need the ability to predict the consequences of their actions. Lupia and McCubbins call this ability "knowledge."

2.2 Rationality and the theories of human behavior

All theories of human behavior begin with the basic assumption of rationality, as just described. For example, cultural theories do not reject the rationality assumption. Instead, they *supplement* it by pointing to a set of beliefs that is commonly held by most members of a given society (see Wildavsky 1987). They do not deny that people act on those beliefs; they only deny that all people everywhere have the same set of beliefs. Given that beliefs are structured by experience and observation, we do not object at all to the cultural theorist's assertion that people in one society will share a common set of beliefs about how the world works that differs from the set of beliefs held by people in a different society.[8]

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7. Holland (1995: 31-32) explains, "...the agent must select patterns in the torrent of input it receives and then must convert those patterns into changes in its internal structure....the changes in structure, the model, must enable the agent to anticipate the consequences that follow when the pattern (or one like it) is encountered."

8. It is noteworthy, however, that people everywhere do share some beliefs in common. If they did not, communication would be impossible. For us to be able to translate even the gist of an

Similarly, although "prospect theorists" enjoy contrasting their approach with the "rational choice" approach (see Quattrone and Tversky 1988), they do not in fact deny that people are rational. They simply argue that when we make choices in our interests, the framing of those choices impacts the decisions we make. This too is compatible with our conception of rationality because people, irrespective of framing, seek pleasure and avoid pain. The same is true for Skinnerian behaviorists whose stimulus-response assumptions look identical to the set of assumptions about human physiology that we stated as the definition of rationality.

Economists typically begin with the assumption that people can rank their wants or needs and that they will, to the extent possible given their knowledge and capabilities, pursue efficiently the higher-ranked wants in their personal orderings. Our model of rationality is more primitive than this model. Ours suggests the primary ingredients, pleasure and pain, from which an ordering of wants can be constructed. The usual assumptions in economics *supplement* a more basic definition of rationality, and do not themselves *constitute* the definition of rationality.

Further, public choice economists commonly assume that people are wealth maximizers (Buchanan and Tullock 1962). Again, the assumption of wealth-maximization as a goal is offered *in addition to* the rationality assumption, not as part of the definition of rationality. The public choice research program, which begins by assuming people are rational, assumes further that greater wealth imparts greater pleasure and that tradeoffs between acquiring wealth and doing other pleasurable things are resolved in favor of acquiring wealth. It is this final additional assumption, not rationality as we define it, to which most objections are raised.

English sentence into Japanese, no matter how inaccurately we might deal with nuances, we must share some basic beliefs about the referents of language.

Positive Political Theorists themselves are in no way immune from the mistake of confounding rationality with other assumptions about human cognition. In addition to the assumption concerning the ability of individuals to create ordinal utility ranking mentioned above, some (e.g., Buchanan and Tullock 1962; Riker and Ordeshook 1973) go further, assuming "transitive preferences,"[9] "cardinal utility rankings" (that people can discern not just an ordering of preferences but actual amounts of utility that separate each member of that ordering), and "utility maximization" (that individuals always seek the top outcome in their ranking).

Each of these auxiliary analogies -- transitivity, cardinal orderings, and maximization -- is the focus of a great din of objection from critics of the PPT models that employ them. In this case, we agree with the critics; transitivity, cardinality, and maximization are often questionable premises. Again, however, we point out two things. First, none of these auxiliary analogies has anything to do with the rationality assumption or with the other two basic assumptions of PPT -- none is in any sense "fundamental" to the PPT research program. Second, judging the value of any of these individual auxiliary premises should be put off until we understand what a model that invokes one or more of them can teach us. Sometimes, the premises -- especially transitivity and maximization when the options are extremely limited in number -- seem more plausible. And always, the models that invoke these premises may help us to rethink "what is obviously important about a situation" in ways that eventually will lead to better theories about the question under consideration.[10]

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9. One is said to have transitive preferences if the following is true: if one prefers option A to option B and option B to option C then one necessarily prefers option A to option C.

10. An example recounted by Fiorina is illustrative. A Swedish meteorologist named Rossby

The rationality assumption does not require that people have good information, highly developed (or equally developed) calculation abilities, or consistency in choices. We have assumed nothing about their goals that precludes suicide, selfishness, or altruism. Further, it is consistent with the formulation that people make mistakes -- miscalculations leading to pain. We argue, then, that it is not the rationality assumption that many scholars find questionable. Rather, scholars are really taking issue with the specific *secondary* assumptions (or lack thereof) of PPT, behaviorism, prospect theory, cultural determinism, economics, or public choice.

We conclude, therefore, that all social scientists are rational choice theorists. Indeed, aside from neo-Calvinists who believe that everything humans do is pre-determined by God's Will, we all subscribe to the rationality assumption. We all begin with the same basic assumption of rationality, based upon human physiology. That means, of course, that ***the rationality assumption by itself buys us very little*** -- and it certainly provides no basis for

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sought to explain wobbles that he observed in the flow of air currents. As Fiorina (1995: 304-305) explains,

> Rossby placed a pan of water on a rotating turntable to simulate the Coriolis force produced by the earth's rotation and wrapped a heating element around the pan to produce hotter temperatures at the "equator" than at the "poles." Surprisingly, photos of aluminum flakes suspended in the water showed waves similar to those in the atmosphere. Rossby's two-variable explanation came to be the accepted one...Rossby's model incorporates several assumptions that are certainly empirically questionable...But by generating an analogue...the model changed what meteorologists viewed as the "obviously important features" of the phenomenon.

comparing alternative theories about human behavior. The divergence among various approaches to the studey of human behavior come after the common assumption of rationality – as scholars choose different analogies to deal with different research questions. What the rationality assumption does allow us to do, which would we could not do without it, is to talk in abstract terms about anonymous individual human beings or classes of human beings, without the need for *sui generis* descriptions of each individual actor's thoughts and beliefs.

3. Component Analysis

Another common criticism of PPT models is that they oversimplify the complex environment in which the behavior we study is taking place. We do not argue that our models are not abstractions -- they are by definition. Our brains are too small to understand the vast complexities of the universe in a single general model. Thus, we ***must*** begin with the presumption that we can bite off a piece of the world at a time, and that explanations about that piece of the world are not hopelessly biased away from the "true nature of things" because we failed to account for ***everything*** simultaneously. Were this not the case, the whole idea of building a science of human behavior would be a fool's errand.

For example, if we wish to study the behavioral responses of consumers and firms in the coffee market to a crop-killing freeze in Brazil, we can concentrate on the demand, supply, quantity, and price of coffee. While we know that there will be ramifying effects on the markets for tea (a coffee "substitute"), and for coffee machines and even perhaps coffee-house poets (both "complements"), we can learn a great deal by studying the coffee market in isolation.

Similarly, when we study the interaction of the president and legislators in the annual budget process, it is useful to ignore that other legislative activities are proceeding

simultaneously, that several of the legislators might be having family problems, and that air-conditioning systems in Washington are overworked due to a heat wave. This is not to say that these contextual complexities might not be important in and of themselves, just that it is safe and useful to consider them to be unimportant for understanding the relationship under examination.

A common abstraction used by Positive Political Theorists is spatial modeling, with which a theorist attempts to characterize a political decision problem graphically. Each participant in the decision-making process is deemed to have an "ideal point," a favorite policy, and is assumed to be interested solely in minimizing the Cartesian distance between her ideal policy and the eventual policy outcome. Spatial models range in complexity from the simple one-dimensional models of Hotelling (1929), Black (1958), and Downs (1957) to the two-dimensional models that characterize much of the social choice literature (McKelvey 1976; Kramer 1972; Plott 1967), to N-dimensional abstractions that cannot even be shown in graphical form and must be derived mathematically.

All spatial models make several simplifying assumptions, each of which can make them controversial. However, as with all models, the gains in clarity and precision hopefully outweigh the compromises in terms of abstraction from the "real world." The first of these assumptions is that we can capture the essential elements of preferences over policy options on some metric -- that in principle, policy packages can be plotted in N-space. Note that although this may appear akin to "putting numbers" on ideological positions or policy options, the spatial modeler only requires ordinal rankings (relative positioning), not cardinal values for these positions. Thus, the scaling is arbitrary, but the specification of the proper dimensions is assumed.[11]

11. As with Rossby's experiment (*Ibid.*), the hope is that the model captures the "most important

Another common spatial-modeling assumption is the notion that individuals' preferences are "single-peaked." This means not only that each actor can identify a favorite policy position, but also that greater deviation from this ideal point in any "direction" results in a monotonic loss of welfare for the actor. For (a one-dimensional) example, if one's ideal level of defense spending is $50 billion, then one is made worse off the further the actual budget deviates from that figure. This is true as the budget grows beyond $50 billion, and as it shrinks from $50 billion.[12] Naturally, as the number of dimensions increases (as the number of policy areas in the package increases) the modeler must deal more and more with tradeoffs and rates of exchange.

Social choice results that demonstrate the theoretical difficulties of equilibrium analysis in more than one dimension have prompted political scientists to rely heavily upon uni-dimensional models of decision making. We believe these models vary in the extent to which they reflect the reality of the decision-making process, and that these differences are important and worth careful consideration. But we also believe that even though they may force a multidimensional issue space down into a single dimension, they can provide valuable insights into the decision-making processes they purport to study.

Consider, for example, budgetary decision making by a legislature. We all know that the budget is by definition a multidimensional construct. But if decisions are made one item at a time, then one-dimensional models are useful. Shepsle (1979) points out that the committee

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features of the phenomenon under study.

12. Equal deviations in different directions need not result in equal decrements in welfare. So one might prefer a $45 billion budget to a $55 billion budget. But under no circumstances will one prefer $44 billion to $45 billion if one's ideal point is $50 billion.

structures of the U.S. Congress are designed precisely to ensure such item-by-item consideration. Alternatively, if players' preferences on one dimension do not affect their preferences on other dimensions, then one-issue-at-a-time abstractions are unproblematic.

We pause to point out that while positive political theorists frequently employ spatial models, they are not a necessary facet of PPT as an approach. Just like any other tool, the proof of their utility lies in the results -- how much more do these models help us to understand? How well do the predictions derived from these models comport with what we observe?

Note that neither spatial models in politics nor single-market models in economics seek to explain fully the variance in the dependent variable. The downward-sloping demand curve for coffee does not explain the price of coffee or the quantity of coffee purchased; it simply demonstrates the relationship between changes in one and changes in the other. A spatial model of a budgetary decision on defense spending cannot "explain" why the president and Congress agreed to spend exactly 300 billion dollars last year, but it can show the effects on spending of changing from a Republican president to a Democratic president, other things equal. Ultimately the direction in which we want to move is toward a better understanding of the dependent variable. But we get there by biting off small pieces and looking for simpler relationships first, controlling for mitigating factors (the component approach) and eventually attempting to build more general models, not by striving right away for a fully-contingent explanation of the dependent variable (a general equilibrium approach).

We must remember also that the assumption that a market operates in isolation, or that policies are decided one issue at a time, serves as a premise, not as a conclusion. Thus, the theorist asks the reader to suspend disbelief long enough to generate testable hypotheses, and check those hypotheses against observational or experimental evidence. If the hypotheses are not

borne out, then it might be the case that the abstraction from complexity is so severe as to bias the theory's conclusions. If, on the other hand, the theory helps us to predict real-world behavior, then we might feel satisfied that the assumptions are not too problematic.[13]

But we do point out that all who study social behavior, whether they use the scientific method (described in section 5) or not, must abstract from reality. What we should do, therefore, is strive to be as explicit as possible about the abstracting assumptions that we employ. Moreover, we should make a good case for why we think that the omitted variables are not so important to the behavior under examination that their exclusion will bias egregiously all our inferences about that behavior. Often, PPT models are attacked for oversimplifying because, while they explicitly state their assumptions, they do not devote enough attention to explaining why their abstractions should be accepted as unproblematic.

4. Strategic Behavior and Games

The first two fundamental premises that we have discussed, rationality and the efficacy of what we have called component analysis, characterize all political science (indeed all science). The primary distinguishing characteristic of PPT -- the assumption that separates it from the rest

13. Of course, such a feeling of complacency might be temporary. We might go on testing our model and eventually run across problems. That is the time to return to the premises in order to figure out what is amiss. Newtonian mechanics was thought for a long time to be truth. It was later shown to be incorrect, and was superseded by quantum mechanics. Interestingly, we still use Newtonian mechanics for everything from weapons development to bridge-building to automobile production. We understand that it is based upon flawed premises, but it still "works" for some purposes, so we do not throw it away.

of social scientific research (to say nothing of non-scientific social research)-- is the notion of strategic behavior, or gaming. PPT begins with the insight that people do not just act, they interact. Moreover, they do so with the knowledge that others' behavior is often contingent upon their own, and that the outcome (and their own welfare) is determined by the interaction of behaviors. This may imply, depending upon the circumstances, that behavior that looks suboptimal in the short run is designed to accomplish a person's objectives in the longer run. Or it might mean that a person will not choose "sincerely" because to do so will hurt their welfare, and it is the outcome and not the choice itself that motivates behavior.

Let us suppose that in a three-candidate race for a single seat, 2 voters prefer candidate C to candidate B to candidate A. These two voters believe that, given the preferences of all other voters, A will beat B by one vote, while C is doomed to come in third no matter how the two vote. In this case, it makes sense for the voters to vote not for C, their favorite, but for B. This will result in B winning, which they prefer to A winning, and which makes them better off. Without the recognition that other people's actions matter as well, we would conclude that our voters would choose C and grin and bear the results.

Similarly, in the prisoner's dilemma, it is each prisoner's realization that the other faces the same choice as himself that induces the defect-defect outcome and gains confessions for the district attorney (Rasmusen 1989:28-29). Were the choice to confess or stay quiet presented to a single suspect in a one-person crime, we suspect that a confession would not be forthcoming.

When we describe social interactions in terms of certain types of games such as the "prisoner's dilemma" or the "stag hunt," or "chicken," we do so in order to help us to predict something about the equilibrium behavior. This sometimes entails the quantification of "payoffs" and "discount factors," the use of mathematical equations, and the expense of a great

number of the reader's and the author's brain cells in order to arrive at a description of that equilibrium. Sometimes the result is that there are multiple equilibria, or that there is none.

Does this mean we believe that the actors in question quantify the payoffs and pause for a couple weeks to perform the math before deciding how to behave? Do people consciously decide to employ "mixed strategies?" Of course not. People face social interactions armed with a store of experience, and recognize what sort of game they are being asked to play. Indeed, the understanding is likely much more inchoate than that. They think, "I've dealt with something like this before, and I know I should do this and not that." People simplify, categorize, and use shortcuts to make decisions. In short, they develop or employ previously developed theories about the world. They may have arrived at the "equilibrium response" through trial and error, or indeed through evolutionary mutation (Smith 1982), but no matter how they decide, we can check to see whether they act as our complicated models predict they will. *The models are for us as observers, and we use them because we believe them to be good analogies to the expected behavior, given our assumptions.* Models rely on mathematical formalization to ensure internal consistency -- formalization does not determine equilibrium. Indeed, the models are necessary only because most analysts lack the intuition to predict behavior without them.

As to whether people know which game they are supposed to be playing and the rules of that game, we think that politics is actually considerably easier to figure out than most of what we do. The rules are written down in the constitution and in the laws governing the operation of elections, political parties, and interest groups. Indeed, a good deal of political activity involves the careful examination of what the rules allow, and the search for better and more subtle ways to exploit the loopholes to the advantage of an individual or group. Of course, a person certainly could be incorrect, and perceive one game when they are really in another. Any married person

knows that this sort of mistake -- when one spouse is "playing one game" and the other is "playing a different game" -- is not unheard of and can lead to bad outcomes, but we also expect that it will occur less frequently as time passes and experience accumulates.

Moreover, we know that people are capable of playing quite complicated games, games that are much more complex than the simple interactions we model in politics. Consider the millions of decisions entailed in driving a car in traffic,[14] and the extent to which one's driving decisions depend on expectations about the decisions of others (unless one owns a tank and has a callous disregard for both the law and the welfare of others). People drive cars and make these decisions properly not because they are able to make calculations at lightning speed and respond optimally, but because they recognize situations and behave as they have learned to behave.

Language is yet another example. Of the staggeringly huge variety of possible sound combinations that we could construct each time we open our mouths, we do not search through every one to find the optimal response to our interlocutor. Rather, we fall naturally into the category of speech appropriate to the occasion and "choose" from a much smaller set of options; even then, we are conscious of "choosing our words carefully" only under the most extreme

14. These include, but are not limited to, staying in designated lanes, accelerating and braking to avoid collisions from behind or in front, shifting gears, steering around obstacles, looking to change lanes or for others who are doing so, adjusting internal temperature and airflow, paying attention to the fuel level and other warning lights, playing with the radio, carrying on a conversation with a passenger, using windshield wipers, looking out for street signs, pedestrians, cross traffic or police cars, eating or drinking, dialing the car phone, reading maps or newspapers, choosing among alternative routes, and perhaps even applying makeup or shaving.

duress.[15] Note also that children are much less capable than adults at this screening. Young children do not even restrict their choices to recognizable words, let alone sentences appropriate to both the topic and social setting at hand. Older children still must be reminded to conjugate properly and when to be polite. Non-sequiturs decline as the experience of the speaker increases. But none of this means that linguists and cognitive scientists cannot theorize in general terms about language and verbal interaction. And these games, and many others, are vastly more complex than the voting situations or market entry decisions that are the grist for PPT models.

So what does the modeling of strategic behavior buy us? In contrast to most of the PPT and social-science assumptions and methods that we have defended throughout this paper, which purposely simplify, the strategic-behavior assumption actually buys us some verisimilitude. People do play games. Constitutions are written and laws are passed specifically to structure those games in one way as opposed to another. Models of social and political behavior that fail to recognize the strategic element of the context of that behavior are susceptible to bad predictions -- they may reach conclusions that are not supported by what we observe.

Obviously, when we model these games, we may be purchasing a little true-to-lifeness at the cost of further abstraction. But this abstraction is no different from the sorts of abstractions, already discussed throughout this paper, that social scientists make all the time. We deal with one game at a time, both because we believe that we can generate testable hypotheses this way and because we believe that people can concentrate on only one game at a time (Simon 1982;

15. Similarly, we have noticed that most people respond to sudden changes in driving conditions such as fog or heavy traffic or getting lost by first turning down the radio, presumably in order to "concentrate" their attention on the new problem and "drive more carefully."

Lupia and McCubbins 1995). We operationalize payoffs, strategy sets and preferences in a way that allows for the calculation of equilibrium behaviors. As with all abstraction, we must be careful about the match between theoretical concepts and real-world observation, but no more so in game theory than elsewhere. Finally, we may use high-powered math to get from premises to conclusions, but this does not imply that our actors are doing the same. Therefore, it does not imply that the conclusions are based upon hidden assumptions concerning cognitive capacity. The formalization is a method for testing the internal (deductive) validity of our models, and nothing more.

5. Theory-Building and Theoretical Progress

A fourth often cited criticism of PPT is that it is devoid of empirical content. For example, Green and Shapiro (1994), among many others, have the following criticism:

1. Rational choice scholars usually fail to formulate empirically testable hypotheses.
2. When they do formulate empirically testable hypotheses, these are rarely subjected to serious empirical scrutiny.
3. When rational choice hypotheses are tested, the tests are often so poorly devised as to be irrelevant to evaluating the models.
4. When some tests are properly conducted, the empirical findings tend to undermine rational choice theory or to lend support for propositions that are banal.

Responses to these allegations, in *Critical Review* and elsewhere, have pointed out that Green and Shapiro do not understand the scientific method or PPT's place therein. This is because they make the mistake of confusing the *PPT research program*, which must include empirical hypothesis testing at some point, with *individual PPT projects* within that research program,

which may or may not engage in empirical testing, depending on their focus.[16] Admittedly, most PPT projects to date have concentrated on the theory-building step in the scientific method. But the steps in the scientific method are separable from one another -- not every Positive Political Theory project has to engage in every step. Moreover, the failure of one PPT project does not invalidate the research program as a whole. We should not throw out the baby with the bath water. Indeed, some of the most spirited debates in present-day political science involve competing positive political theories that make divergent predictions.[17] To understand the role of PPT in political science, we will turn to a discussion of the scientific method generally and how PPT has fit in so far.

5.1. The Scientific Method: The Steps and the Chain of Analogies

Positive political theorists employ an approach that emphasizes the scientific method, which we see as consisting of five steps. ***First, a scientist observes something about the world and asks a question about how some aspect of that world works.*** For example, political scientists observe that some people vote and others do not, and ask why that is the case.

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16. See Lakatos (1970) for a discussion of research programs.

17. Perhaps the most fitting example, given that Green and Shapiro and others consistently single out the so-called Paradox of Voting (Riker and Ordeshook 1968) as a glorious PPT failure, is the comparison between outcome-contingent and act-contingent theories of voter turnout -- both part of the PPT research program -- offered by Cox, Rosenbluth, and Thies (1995). See also the debates between advocates of the so-called distributive (Shepsle and Weingast 1987; Weingast and Marshall 1988), partisan (Cox and McCubbins 1993), and "informational" (Gilligan and Krehbiel 1990) theories of Congressional organization.

Second, the scientist develops a theory to answer that question.[18] The theories that scientists develop are not designed to be complete representations of the world. Instead, they should be thought of as analogies, or models. Models abstract away from most of what is going on in the world in order to focus in on what are presumably the most important features of the phenomenon being studied. It follows, therefore, that an objection that "the world does not really look like the model" misses the point of what a theory is. Of course, premises ought to be plausible, and it is certainly acceptable to demand that a theorist replace premises that have been shown to be false or implausible with more plausible ones.

Unfortunately, the premises of most models involve things we cannot observe or measure. Political science theories about voter turnout, for example, necessarily begin with premises about what goes on inside prospective voters' heads. A political scientist might theorize that voting and other forms of political participation are a function of "political understanding" (Rosenstone and Hansen 1993: 13-14; Wolfinger and Rosenstone 1980). But "political understanding" cannot be measured very accurately or efficiently. So he might further reason that a person's level of understanding can be proxied by the extent of her education.

Once the scientist has developed a plausible theoretical answer to his or her question, ***the third step in the scientific method is to derive predictions from the theory.*** Following our example, a political scientist might predict that insofar as voting is driven by political

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18. Proposing theories is not unique to science; people build theories from earliest childhood. They observe the environment, understand pleasure and pain, and need theories to help predict how the world works in order to pursue pleasure and avoid pain. People learn to replace bad theories with better ones (better at prediction) as they gain experience in their lives.

understanding, which is a function of education, then better educated individuals will be more likely to vote than people with less education, other things equal.

The fourth step, then, is to design an experiment or a method of observation in order to test the prediction derived from the theory. Scientists rarely are able to test their theories directly, because theoretical concepts are typically difficult to measure or observe. Hence, this step requires the creation of a second analogy; the scientist tests an analogy to the theory. Again, our example is instructive. Let us suppose that our hypothetical political scientist would like to design an experiment or make observations so as to test the prediction that the likelihood of voting increases with education. But he cannot actually measure the *likelihood* of voting. The *best* he can do is observe actual cases of voting and non-voting. Similarly, education is not subject to direct measurement -- how can we actually gauge how educated a potential voter has become? Our political scientist may choose to "operationalize" education as "years of formal schooling completed." The number of years a person has spent in school is at least two logical steps removed from the true variable in the scientist's theory -- "political understanding" -- thus, the proposed measure is again only an analogy to the concept found in the theory. And recall that the theory itself is at best a plausible analogy to the real world phenomenon under study.

Let us imagine that our student of voter turnout has settled on estimating the relationship between years of formal schooling and actual voting behavior in order to "test" his theoretical prediction that the likelihood of voting is driven by the level of political understanding. He decides to create a survey instrument to measure respondents' voting behavior and years in school. He will then use some technique to search for correlation between turnout and schooling.

Note that this testing procedure involves the creation of (at least) four more analogies. First, the scientist must employ a model (implicit or explicit) of voter response in surveys. The

major premise here is that the measured responses are related in some way to the variables that the scientist wishes to measure. Among other things, this presumes that nothing important is lost in the respondent's understanding of the questions, or in the respondent's ability to create an answer for the scientist that reflects her understanding of the answer to the question.

Second, the scientist must design a survey instrument that employs the theory of respondent cognition, and allows him to measure the variables he hopes to measure. This time, the survey-design problem is how to avoid losing too much in writing the questions, and in interpreting the answers to those questions so that the voter is stimulated to tell the scientist what he wants to know, to do so accurately, and to do so in such a way that the scientist can understand it. These first two steps may seem trivial to the uninitiated, but a great deal of ink has been spilled to point out the pitfalls of survey research (see e.g., Zaller 1992).

The third analogy in the test involves the creation of a data-interpretation methodology. Our scientist might choose to use ordinary least squares (OLS) regression, which is a model with seven well-known premises, the so-called "Gauss-Markov assumptions." In this case, the third, methodological analogy is no longer in much dispute. But it is not a trivial piece of workmanship either, and its properties were not always so widely praised.[19] The most important

19. OLS is like Galileo's telescope. Lakatos tells us that "Galileo claimed that he could 'observe' mountains on the moon and spots on the sun and that these 'observations' refuted the time-honored theory that celestial bodies are faultless crystal balls. But his 'observations' were not 'observational' in the sense of being observed by the -- unaided -- senses: their reliability depended upon the reliability of his telescope --and of the optical theory of the telescope -- which was violently questioned by his contemporaries" (Lakatos 1970:98).

point for our purposes, however, is that OLS itself, even before it is applied to a particular set of data, is an analogy, a methodological model that abstracts from "the real data."[20]

The fourth analogy implied by the scientist's chosen testing procedure involves applying OLS to the data collected -- i.e., running the actual regressions. In our example, we can be fairly certain that this analogy is a bad one. The OLS model will create biases in our inferences about the data (and hence about our theory, and hence about the question of why some people vote and others do not) due to the bivariate nature of the dependent variable, the likelihood that omitted variables correlate with years in school, and so on. In this case, students of this last analogy (econometricians and statisticians) instruct us that we should substitute some other estimation technique, such as probit or logit (each with its own modeling assumptions), in place of OLS.

The fifth and final step in the scientific method is to rethink the initial question. What have we learned, and how might we push our research program in order to learn more? The important lesson to draw from this extended example is that the scientific method and theory-building is very complicated, involving several separate steps, any one or more of which could create problems for the social scientist. For good reason, each of the analogies (models) that we have identified is the subject a great deal of study. All political scientists work to create better measures of commonly used variables; survey researchers strive to understand and solve the various pitfalls of their technology; statisticians endeavor to develop better estimating techniques

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20. This is just like saying that the spherical object that Galileo saw in his telescope was not the real Moon, but his (magnified) mind's perception of the Moon. Once people accepted the analogy implied by the theory of optics behind the telescope, they were willing to abstract away from the differences between Galileo's picture of the Moon and the actual body, but not before.

and diagnostic tests that tell us which techniques to use. And so on. Indeed, a great deal of social scientific progress begins when scholars poke at one of the six (or more) analogies that we have discussed, find deficiencies, and remedy them in search of better answers (theories) to the questions that puzzle them.

One implication of this discussion is that failure to confirm an empirical expectation does not necessarily imply the rejection of the underlying theory. Indeed, in many cases, the poor analogy is not between the world and the theory, but rather between the theory and the empirical test (the second analogy) or within the testing procedure itself (the third, fourth, fifth, and sixth analogies in our example). The breakdown could be the result of any poor efforts to construct the analogies that we have identified. For example, a good theory might be rejected unnecessarily simply because a scientist used OLS when logit was more appropriate.

5.2 PPT's Place in the Scientific Method: Building Better Theories

Though it is often nice to see research that undertakes the task of developing several of the analogies within the scientific method (not only to build theories or derive predictions, but to design and perform tests for theories as well), we do not see this very often. Nor, as Green and Shapiro themselves imply, is this the standard for any of the sciences.

In studying environmental influences on human health, for example, the groundwork often is carried out by epidemiologists who correlate statistics on environmental factors with information on patterns of diseases in the human population. Epidemiologists find the correlations but do not necessarily propose theories to explain them.[21] Rather, it is left to

21. After World War Two, epidemiologists came up with the so-called "seven correlates of heart disease." They noticed a correlation between the likelihood of coronary disease and several

someone else, a physiologist or biologist, to propose a theory that explains the correlations found by the epidemiologists. Thus, the work of the epidemiologist is to identify patterns in nature that are to be explained; there are others whose role it is to explain these observed patterns.

Often, those scientists who develop the theories do not test them. Frequently, it is others who derive predictions from these theories that are then put to the test. The types of tests, whether they be clinical experiments or laboratory experiments, often are conducted by different sets of scientists at different places and at different times. The findings of these experiments, along with new epidemiological work, feed back to the improvement of existing theories (by making them more general or parsimonious) and to the development of new theories, new tests, and so on. It is unusual for any one scholar or set of scholars to undertake every part of the scientific method in a single project.

Having said that, how should we (and how does PPT) go about developing better theories of human behavior? We would like our theories to possess three characteristics: *deductive validity*, *parsimony,* and *generality*. By extension, we define theoretical progress generally as improvement in any one or more of these categories.

For a theory to be *deductively valid*, it is necessarily the case that if the theory's premises are true, the conclusions *must* follow. It is possible that a theory is "inductively cogent," meaning that if the premises are correct the conclusions *might* follow. An inductively cogent argument still makes an empirical claim about the world -- it rules out the possibility that the

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seemingly unrelated factors such as barrel-chestedness, and hair growth in the ear canal. They did not attempt to draw causal inferences (nor could anyone else ever do so) but they were accurate enough predictors of heart disease to be taught in medical schools for three decades.

conclusion *cannot* follow. The remaining task to generate a fully deductive theory is to specify the conditions under which the conclusion will or will not follow from the premises.[22]

Most scientific explanations are only inductively cogent. By asserting that an explanation linking premises to some observed (or predicted) conclusion is correct we do not necessarily rule out alternative explanations, based on different premises. To compare one explanation to another -- if each is cogent -- we must assess the relative efficiency and plausibility of their respective premises (Schwartz 1980:209-210).

We hasten to point out that theorizing by *induction* is perfectly acceptable within the scientific method. Indeed, in describing a process for theorizing about voter turnout, we began with an inductive step -- the observation of variations in turnout among individuals or across time or place led our hypothetical scientist to seek an explanation. We do not admit, however, that the scientific process can end there. Theories may be derived inductively, but their predictions must be derived deductively. They must spawn predictions unrelated to the original focus of observation that can then be subjected to testing in the ways we have described. To do otherwise is to theorize by "assuming the consequent," concluding that the first plausible explanation to come to mind is, *a fortiori*, the correct explanation.

Over the past 40 years, social scientists have come to pay much closer attention to checking the internal (deductive) validity of their arguments. Social choice theory and game theory, two of the scholarly roots of PPT, use mathematical formalization to add to the rigor and accuracy of social science by forcing scholars to recheck the internal validity of their theories. Sometimes this formalization has come at the expense of adding a great deal of sophisticated

22. A theory may also be invalid; i.e., if the premises are correct, the conclusions cannot follow.

mathematical symbology -- and, admittedly, sometimes the payoff in terms of empirical content has been difficult to detect. But *formalization, in and of itself, does not drive any interesting results*. Instead, the premises are the ultimate basis for any deductive argument.[23] To evaluate an argument, we assess the plausibility of the assumptions. We do this by checking whether the assumptions are sufficiently general and immune from counter-examples without being self-defeating and question-begging (Schwartz 1980).

Parsimony refers to the number and inherent plausibility of a theory's premises. Other things equal, we prefer to develop theories from as few assumptions as necessary.[24] We also prefer to begin from uncontroversial bases whenever possible. The fewer and less odious the suspensions of disbelief required by the theory's premises, the better, other things equal.[25]

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23. One purpose of formalization is to ensure that we have internally valid arguments. The second is often to "get the ball rolling." While informally stated premises can be fruitful, sometimes they are so vague that only their author believes anything that follows from them. When one formalizes such premises it may become possible for the first time actually to generate predictions from them. (We thank Gary Cox for discussions on this point.)

24. Schwartz (1980:215) refers to this as economy. He correctly points out that the proper measure is not the absolute number of assumptions but the ratio of explanatory power to complexity in our set of assumptions. Like fuel economy, we should be concerned with how much (explanatory) power we get out of what (in terms of complexity) we are paying to put in.

25. Thus, heliocentric astronomy is preferable to its geocentric predecessors because of its elimination of the need to believe in epicycles and epicycles of epicycles. Everything we observed about the orbit of celestial bodies fit the new theory with fewer assumptions than under

We prize *generality* for two reasons. First, more general theories are attractive because we have relatively small brains. The theory of gravity is superior to separate theories of falling rocks and floating leaves because it requires us to understand and remember much less. A general theory of voter turnout is more appealing than separate theories of voter turnout in each country under examination for each election. Second, and more importantly, we value generality because it allows us to make predictions about specific cases that we have not yet encountered or theorized about. In short, a general theory supplants a specific theory when it can predict everything that the specific theory could predict, and also explain additional phenomena outside the purview of the specific theory (Lakatos 1970). A general theory of voter turnout would allow us to predict turnout results in future elections in established democracies, and even in new democracies, whereas separate theories for separate elections would require us to begin anew, each time an election occurs somewhere, and to do so in each electoral district in which candidates compete and voters decide.

Positive political theorists, as the name suggests, are most interested in the theory-building part of the scientific endeavor. That they themselves do not always come up with the observations to be explained, or do not always continue on to test theoretical predictions is not a valid criticism. They just play one role in the study of political behavior.

6. Conclusions

Throughout this essay, we have avoided calling the research program we advocate by its most common moniker, "rational choice theory," opting for the less tendentious "positive political theory." This is because, in the first instance, we believe that most of the problems that

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the old theory, so we replaced the old theory with the new one.

critics have with the research program stem from a misconception of what we mean by "rational." Unfortunately, in English at least, the lexicon most commonly uses the term as a synonym for "reasonable," or "correct," or "sane," whereas we use it to mean much less than that. Rational means "reasoned," not "reasonable;" it implies that people emerge from the womb able to understand pain and pleasure, and spend their entire lives building and improving theories about how the world works in order to pursue things that they believe will lead to pleasure and avoid things that they believe will lead to pain. The term "choice," therefore, means exactly what it says: human beings are blessed with free will, and are able to select among alternatives. We argue therefore, that it is never accurate to call someone irrational, or to say that someone has "no choice." The latter is only correct once one has assumed something about an actor's goals; while the former is impossible for self-aware life forms. So, a second glance reveals the term "rational choice" to be accurate, if perhaps redundant. Nonetheless, we still reject it because, defined properly, it does not distinguish one branch of social science research from another. All who study human behavior subscribe to the rationality assumption -- all social scientists are rational choice theorists.

In this paper, therefore, we have dissected and defended the political science research program that we refer to as Positive Political Theory. We identified the three fundamental premises common to all PPT, namely rationality, "component analysis," and strategic behavior. Again, we pointed out that rationality and component analysis, properly conceived, can be found to be propping up all social scientific research programs, from cultural theories to prospect theory to Skinnerian behaviorism. Further, we discussed and debunked several misconceptions about what the PPT approach implies. We indicated how the three basic PPT assumptions can be separated conceptually from various sets of auxiliary assumptions that individual theorists

invoke, by showing what rationality does and does not imply, and by arguing that all theory building, indeed all explanation, requires the analyst to abstract enormously from "reality."

Finally, we looked at the place of theory-building in general, and of positive political theory in particular, in the development of a science of political behavior. Green and Shapiro and other critics of PPT claim (without evidence) that the predictions of rational choice theory do not hold up well to empirical scrutiny. This claim is, of course, too broad to evaluate because many positive political theories perform quite well (e.g., imposing costs on voting reduces turnout) while others perform poorly. And of course there often exist two or more PPT models that compete with each other to explain political phenomena. Our job should be to evaluate each effort independently, not as though they were a part of a seamless whole.

We enumerated the steps involved in the scientific method, and pointed out that theory building is only the first step toward answering the questions that scientists ask about the world. Of necessity, theories are models, abstractions from the vast complexity of the real world, and "testing" our theories further requires the invocation of a series of methodological models, any one of which may contain flaws that frustrate the search for knowledge. Externally, the relevance of the theory to the real world is defined not by its performance in empirical "tests," because they are after all just analogies themselves, but by the plausibility of the theory's own premises. Contrary to the perception of many, the "empirical content" of a theory is established at the outset, when the theory is proposed as an answer to a particular question about the world. This is the point when it is proper to ask, "is this really a response to the question being posed?" Internally, we require that our theories be deductively valid, and we strive to make them as general and parsimonious as possible, with the recognition that there are likely to be tradeoffs between these criteria.

We argue that the PPT approach has great value as a political research program. In particular, PPT models often supersede rival explanations of political behavior because they pay much closer attention to the internal imperatives of theory building. In stating more explicitly their premises, PPT models allow would-be critics to isolate more easily their contributions and drawbacks as proposed answers to political questions. We see models that are easy to pick apart and examine piece by piece as superior to models that are too opaque to evaluate.

That said, we recognize that PPT is just one research program among many in political science, and in social science more generally. Rival research programs should not be seen as mutually exclusive, but as complementary. While we hasten to reiterate a central point of our paper, that the fundamental premises of PPT do not differ from those of competing approaches to nearly the extent that is usually claimed, we do allow that at some point PPT models branch off from other approaches. We have argued that the basic assumption that sets PPT apart from other political science models is the notion that people behave strategically, that they consider others' actions before acting themselves, and we have defended this assumption as a fruitful one. Other approaches branch off by invoking different packages of supplementary assumptions. In all cases, each supplementary assumption should be judged on its own merits, by comparing what it buys us in terms of predictive power with what it costs us in terms of abstraction.

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Null space

Philosophers and social scientists have long theorized about human behavior in political situations. Many of these theories invoke models of individual behavior in order to justify their political prescriptions. Models of individual behavior have been used to justify everything from the divine rights of kings to theories of mass democracy.

In the last half-century, political science has shifted from using positive theories to substantiate normative arguments to developing positive theories to understand human behavior. While we can always derive normative conclusions from theories that social scientists develop, the goal of most modern social science is to take the first step -- to develop the positive theories of human behavior. There has been almost a complete paradigm shift from studying behavior as justification for normative arguments to studying human behavior to understand ourselves (or as a starting place for normative arguments).