

Theory Talks

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THEORY TALK #31

BRUCE BUENO DE MESQUITA ON GAME THEORY, PREDICTION AND FEAR OF LOGICS IN IR

Theory Talks

is an interactive forum for discussion of debates in International Relations with an emphasis of the underlying theoretical issues. By frequently inviting cutting-edge specialists in the field to elucidate their work and to explain current developments both in IR theory and real-world politics, *Theory Talks* aims to offer both scholars and students a comprehensive view of the field and its most important protagonists.

Citation: Schouten, P. (2009) 'Theory Talk #31: Bruce Bueno de Mesquita on Game Theory, Predictions and Fear of Logics in IR', *Theory Talks*, <http://www.theory-talks.org/2009/06/theory-talk-31.html> (27-06-2009)

BRUCE BUENO DE MESQUITA ON GAME THEORY, PREDICTION AND FEAR OF LOGICS IN IR

While many scholars interviewed here debate how we can make sense of international politics in a changing world, for Bruce Bueno de Mesquita, international politics can be explained by underlying logics based on rational choice. Dubbed ‘the New Nostradamus’, Bueno de Mesquita uses mathematical models to not only explain international politics but also predict outcomes – not only to a scholarly audience, but also to high-placed decision makers in the government of the US and to the CIA. In this comprehensive Talk, Bueno de Mesquita, amongst others, explains how prediction in IR is done; refutes the resentment against using rational choice in predicting outcomes in IR; and shows how formal logics demonstrates that neo-realism is simply false.

What is, according to you, the biggest challenge / principal debate in current IR? What is your position or answer to this challenge / in this debate?

I assume the question refers to the study of IR and not to the biggest challenges in international relations per se. Within the academic study of IR I think there are several important challenges. For me, the purpose of studying IR is to understand how international affairs work in the world and why they work that way. As we develop better understanding we also develop better prospects of being able to improve outcomes, especially with regard to avoiding violence and finding ways to settle disputes peacefully.

Among the debates that impinge on understanding I think two are central. One relates to the extent to which our focus might be better placed on individual decision makers and their interests rather than assuming that their interests and the state’s interests are the same. Later in your questions, for instance, you conflate the two, assuming that the welfare of the state is what decision makers are concerned about. This conflation of state and individual interests is, in my view, a fundamental impediment to advancing our understanding of IR. States, as I see it, do not have interests or preferences or beliefs, people do. We may speak of the “national interest” as some aggregation of what most people want or what many people want, or what a few powerful people want, but each of these meanings can produce entirely different expectations about what is in the “national interest.” Indeed, we know that adding up people’s preferences to some aggregate view of the national interest (perhaps short of the survival of sovereignty) has the problem that x can be preferred by a supermajority to y and y can be preferred by a supermajority to z and z can be preferred by a supermajority to x – and so we cannot say that policy x advances the national interest more than policy y or policy z. (This cycling of aggregated preferences is, of course, the Condorcet result from the late 18th century. Its refinement by Kenneth Arrow,

leading to his impossibility theorem, was a major factor behind his receipt of the Nobel Prize in Economics).

The second dimension of debate that I believe impinges on our understanding of international relations is that there is insufficient emphasis on the careful application of logic and evidence – rather than personal opinion or personal values – in the way graduate students are trained and in the way debate is carried on in the field. I am always surprised, for instance, to discover that some individuals offer individual case examples as evidence for a hypothesis when a correlation of 0 between two variables implies that about half of the time when x increases so does y, and half the time when x increases y decreases, and likewise, when x decreases half the time y increases and half the time it decreases so if x and y have no relationship to each other we can find cases that seemingly support any hypothesis (or refute it). Likewise I am surprised when people offer a single case as a presumptive refutation of a probabilistic hypothesis even though probabilities inherently imply some distribution of outcomes that can only be assessed against a large number of observations.

In short, too much time is spent debating methods on grounds of personal predilections or based on expert testimonials instead of reflection on the logic and evidence behind assessments.

How did you arrive at where you currently are in IR (people who inspired you, books, events, how did you conceive your ideas)?

I started out as an area specialist (South Asia – my dissertation was on strategies of opposition parties in coalition governments at the state level in India, later published as my first book. I did field work, studied Urdu for 5 years, and did modern Indian history as one of my dissertation examination fields) but was exposed in my first year of graduate school to the then nascent formal modeling approach. I was greatly influenced in a graduate course taught by Donald Stokes. I read and prepared an oral presentation on William Riker's Theory of Political Coalitions for that course and discovered that the strategic principle in that book was incorrectly derived. This was my first exposure to formal modeling and the first time that I saw how rigorous logic (a formal model in this case) could be used to conclude that a claim was false, not as a matter of opinion but as a matter of straightforward logic. This, plus my undergraduate exposure to basic statistics gave me a way to look at arguments in terms of systematic evidence rather than selected cases and in terms of logic rather than personal judgment. This work also fit well with my interest in studying coalition governments in India so I was able to combine my area specialist interests with my then new interest in quantitative and formal analysis.

I was also profoundly influenced by Kenneth Organski's Stages of Political Development in which he looked at how different coalitions of interests shaped political and economic development. What he called the syncratic model had a particularly deep influence on my thinking about coalition strategies and was an important factor in my decision to go to graduate school at the University of Michigan to study comparative politics (I did not do IR as a field).

Finally, the chair of my Ph.D. committee – Richard L. Park – had a deep influence on my thinking and my approach to teaching. He demonstrated a tolerance for a perspective different from his own that I found inspiring. Dick Park was one of the founders of modern South Asian studies. He found my rational choice and quantitative approach to Indian politics rather different from his own thinking but he encouraged me, supported me, and nurtured the confidence that allowed me to go forward despite resistance from many other leading lights in the South Asia research community at the time. One of my most satisfying academic experiences is having had the opportunity to co-author a book with him (India's Political system, 2nd edition) just before his untimely death.

Later, thanks to David Rohde when he and I were assistant professors at Michigan State University, I had the opportunity to meet William Riker and to move to Rochester where I learned to improve my skills as a political scientist.

What would a student need to become a specialist in IR or understand the world in a global way?

Graduate programs seem to vary greatly in the extent to which they emphasize learning the literature and learning tools for doing research. I think that students who want to study IR should, for starters, think carefully about which type of graduate program they want to be in. I believe that much of the received wisdom about international relations does not stand up to careful logical or empirical scrutiny. A successful student – whatever they conclude about ongoing debates – ought, I think, to have the disposition to be willing to challenge received wisdom and come to a reasoned conclusion about what has merits and what does not. I believe training in the tools of analysis facilitate such reasoning better than does a heavy focus on the literature. Of course, it is essential that students know what the debates are in a field and what the evidence is for alternative perspectives and so they must know the essential literature but they must also know how to evaluate the evidence. That means they need to master some diverse mix of the tools of analysis: research design, archival and historical research, statistical analysis, and mathematical, logical reasoning. Learning tools on one's own is more difficult than keeping up with the literature so, in my opinion, graduate training should emphasize the acquisition of analytic tools.

What are, according to you, the constant factors underlying international politics?

As I see it, 1. People are self-interested; 2. Leaders want to come to and maintain themselves in power; 3. International politics and domestic politics can create challenges to political survival that run counter to each other, forcing leaders to try to find a way to balance domestic and international threats; 4. This balancing between satisfying the demands of domestic supporters and the demands of foreign rivals is the fundamental problem in international relations and necessitates continuous inventiveness to find solutions.

Rational choice and game theory hinge heavily on logics. Can you tell me what logics is about in the rational choice approach?

I have a quite primitive conception of logics, which goes something like this: if someone says ‘if A, then B’, I want to see how that follows from the assumptions and axioms. Natural language has its ambiguities, and formal logics cannot solve but make explicit and more comprehensible many arguments made in natural language. A friend of mine is a linguist and a mathematician, and he gives the example of the expression: ‘I saw a man with a telescope’. Now this is a completely ambiguous statement: does it mean I was looking through a telescope and saw a man? Does it mean I saw a man who was carrying a telescope? If I wrote this down as a mathematical expression, I would have defined the terms much more precisely. The syntactical logic of the utterance would then be unambiguous. Don’t misunderstand me: mathematics also has its ambiguities, with answers to some questions simply looking like ‘not zero’, but it has a lot less of those issues than natural language. And that leads to one of the main advantages of rational choice: however one may disagree with it, at least it makes clear and explicit the assumptions it makes in its hypotheses by formalizing them as much as possible, which is not something one can say about many of the other approaches out there. One of the main misunderstandings about formal logics is that we interpret utterances. But we don’t try to capture what someone out there meant; we try to make explicit what the theorist meant. If you make what the theorist had in mind explicit, you can see if it holds in reality. Then you don’t end up with conversations in which somebody says ‘well, that’s not what I meant’.

In the classical social science divide between understanding and explaining, where would you put yourself and why?

My motivation in studying international affairs is first and foremost to understand why things happen the way they do. My personal interest in understanding how the world of international politics works is driven by the expectation that a thorough understanding will lead not only to explanation but also to the ability to predict and even modify future outcomes. I believe that the scientific method is the most reliable way to create convergence between understanding, explanation, and (often probabilistic) prediction.

For many, we now live in what is called a ‘risk society’, where risk assessment and controlling the future and outcomes has supplanted material threat as a number one preoccupation for advanced societies. Your company Decision Insights portrays itself as follows: ‘a unique information company that possesses the most accurate decision-making and problem-solving system available in the world today. A system that has successfully analyzed thousands of sensitive issues for government and business

obtaining a verifiable accuracy rate exceeding 90% plus.’ Can you tell us something about how this system works?

First, Decision Insights (DII) is not my company and I am not a member of its board of directors. I am one of the larger shareholders in it but without influence over its policies, postings, or activities. Although I was a founder of a company called Policon in the early 1980s, Policon was taken over by Decision Insights. Since 2003 my involvement with DII has just been to pay 20% of my company’s gross revenue to DII to use the forecasting model I developed many years ago. DII owns the commercial (but not academic) rights to that forecasting model. My company is Mesquita & Roundell, LLC (M&R) so I will answer the question from M&R’s perspective.

The forecasting and policy engineering model I developed assumes that stakeholders on any policy issue care about two things: the outcome on the issue and the extent to which they are seen as instrumental in putting an agreement together (or blocking one). The model specifies a rather simple game and solves the game, in the process estimating how much each stakeholder values the policy outcome relative to being seen as instrumental in shaping the outcome. It also estimates how each player perceives its relationship with each other player, what proposals players make to each other regarding resolution of the issue (including no proposal at all) on a round by round basis. The model estimates how player positions change and also updates player estimates of the willingness of others to take risks. It does quite a bit more as well. This model depends on expert inputs based on an intensive interview process that elicits who the stakeholders are who will try to influence an outcome, what outcome they currently argue for, how much persuasive clout they could bring to bear, and how salient the issue is to them compared to other issues on their plate. Experts are not asked how they think the issue will be resolved and the model frequently disagrees with the conventional wisdom on what is likely to happen.

Interested readers should read my 2002 book *Predicting Politics* (Ohio State University Press) or my 1994 book (co-edited with Frans Stokman) *European Community Decision Making* (Yale University Press) or my 1997 article in *International Interactions* for explanations of how the model works. They should also read Stanley Feder’s 2002 article in the *Annual Review of Political Science* for an evaluation of what the model can and cannot do based on the experiences of someone who used it more than a thousand times at the CIA.

Recently I developed a new forecasting and policy analysis model, which not only estimates changes in player positions over time, but also changes in their salience, flexibility, and power to shape the outcome over time. I am in the process of constructing user-friendly software for this model and will post it on the web for free access at least for several months in association with my forthcoming book, *The Predictioneer’s Game* (Random House, 2009). Depending on how it gets used (or abused) I will decide on whether to continue to make it freely available for academic use or restrict access to it.

Considering that, according to the CIA, this model is right in 90% of the cases that experts who provide input are wrong, doesn't this make loads of IR scholarship redundant?

I do not quite understand what is meant here by “make loads of IR scholarship redundant.” The accuracy of a model such as my forecasting model provides a tool for IR scholars to use that may help us understand, explain, and predict events. It also provides policy analysts and decision makers with assessments that have transparent logic so they can argue with its conclusions, generally based on their own data. I do not see any of this as redundant – it is what most scholarship I believe aspires to do. Of course, understanding and explanation tend to be subjective whereas predictive accuracy is readily measured and so provides a challenge for other approaches. The CIA assessment is that this particular model is accurate about 90 percent of the time. My new model so far seems to do better. The CIA assessment of my old model also says that it “hits the bulls eye” more than twice as often as the experts who provided the data inputs and that when it and the experts disagree about the anticipated outcome, the model is almost always right rather than the experts. That is, the model appears to provide a more reliable way of evaluating the experts' information than they have themselves. Still, without the expert inputs, the model is just a bundle of equations. It represents, then, a natural synergy between area specialist knowledge and decision making/game theoretic analysis. If we could only have area expertise or a model it clearly would be better just to have the area expertise but there is no reason for us to be limited to one or the other. The combination of the two provides more reliable assessments, at least according to the CIA's evaluation across a very large sample. It is worth noting that the CIA's assessment is readily evaluated by academics because I have also had many articles and books published with a large number of predictions about events that had not happened at the time of publication or acceptance of the manuscript. Thus, anyone can check out the accuracy of those predictions.

My question about redundancy is rather: isn't a lot of the post-work unnecessary when the stuff you and other rational choice people work with is so good in explaining and predicting? What's the value of alternative modes of explanation, if any?

The focus in everything I read is strictly on first the logic and secondly the evidence. So first of all, I look if the conclusion – whatever the explanation or the prediction intended – follows necessarily from the argument. If it does, it is potentially an explanation or prediction of things, and then as a second step I look at evidence in the world to see whether the assumptions that lie behind this logical argument actually explain behavior.

Let me give you a concrete example. I find that the logic of the constructivist argument (the first step) is entirely plausible. It is internally consistent, it could be the way things are. However, making the next step, I find little systematic evidence that confirms expectations of constructivism in IR: I don't see people inculcate norms, and that their values and behavior changes significantly as a consequence of that. I don't find the individual case studies that constructivists tend to use informative as evidence because if you have a zero correlation between

two variables then you can find cases where X goes up, Y goes up, when X goes down Y goes up, when X goes up Y goes down, X goes down Y goes down. So picking singular cases doesn't tell you what pattern of behavior is: the predictions in the constructivist argument are about general patterns or norms of behavior and thus are only appropriately evaluated on the basis of a large number of cases. And in fact, up until now, there are several sound empirical studies that in fact point in the opposite explanatory direction. That does not, however, close the discussion because there are simply not enough studies yet. The same goes for other explanatory frames: I was an Indianist earlier on, and I worked on system-level explanations incorporating polarity and so forth, but I simply couldn't find the evidence to match that explanation out there, and in fact even the internal consistence of the underlying logic was far-fetched.

Stephen Walt has made the comment about people such as myself that we are overly concerned about logical consistency and, while he is of course entitled to his view, I hold that an argument that is not internally consistent can allow you to say anything you feel like, and therefore is just an argument from personal taste or predilection and whatever you say might turn out to be empirically consistent. Now that doesn't make for an argument for me: one needs then the additional step of looking backwards from this empirical evidence to construct what are in fact the logics that would lead to that outcome. I see consistency as fundamental to understanding how the world works.

Game theory or rational choice approaches actors as rational + self-interested. But world leaders have frequently acted against their interests: Saddam by committing 'political suicide', as Robert Jervis puts it; the Bush administration by a whole series of actions that undermined the US position in the world; and, let's face it, America has lived quite unsustainable for the last decades, both in economic and ecological terms. That raises questions on access to correct knowledge of self-interest, which, already according to Tolstoy, one can only access in hindsight, that is, looking back. Does this raise a problem for game theory?

I could not disagree more with the portrayal of examples in this question as evidence of irrational behavior. Before delving into a detailed answer – which follows – let me recall the CIA's assessment of a 90 percent accuracy rate for my simple forecasting model. Neither those CIA analyses nor my perhaps hundreds of published forecasts involve hindsight so the evidence tells us that the claim in the question regarding hindsight is not consistent with evidence. Appeals to authority or revered figures like Tolstoy (or Jervis, for that matter) are not substitutes for logic and evidence. They essentially invoke the pre-Pascal, Jesuitical understanding of what is probably true as the preponderance of opinion by clerics rather than as the preponderance of evidence.

One of the most common errors made by anti-rational choice analysts is to engage in post hoc, ergo propter hoc false reasoning. Let's take the two examples offered in the question. Did Saddam Hussein act against his self-interest? Well, it certainly turned out badly for him but could he have known that ex ante when he had to make choices? I contend that the answer is no and I explain why.

As it happens, I use the example of Saddam Hussein's failure to illustrate several basic principles in my textbook, *Principles of International Politics*. First, we know now that things turned out badly for Saddam Hussein but neither we nor he could have known that before the fact when he had to make choices. Indeed, based on what he could know (such as the prior history of the United States government in dealing with him) he chose actions that were rational; that is consistent with what appear to have been his interests in survival. In the first Gulf War (1991), despite his army having been completely routed (and Colin Powell arguing to the Congress before that war that the United States would suffer perhaps tens of thousands of casualties and deaths), the US did not march on Baghdad and overthrow him or his government. Indeed, the Bush 41 administration did not even compel an unconditional surrender. Based on that experience, Saddam would have had solid reason to doubt the US government's resolve to remove him from power. Second, we know now that he did not have WMD, but many thought he did before the 2003 invasion. It is quite possible that he thought he had WMD, we do not know. What we do know, is that some arguments against the 2003 war (which I opposed for other reasons at the time – namely that I saw no clear and present danger that would justify a pre-emptive attack by the US government) revolved around concern that there would be massive American casualties because Hussein was likely to use his WMD capacity (he had, after all, used nerve gas against the Iraqi Kurdish population and in 2003 American soldiers were deployed with anti-chemical weapons gear, apparently indicating that this was seen as a credible threat). Thus, by interfering with international inspections he was able to increase the belief at the time that he had WMD (see my textbook, third edition, for an explanation of the Bayesian updating calculations) and this could have deterred an American invasion. Thus, his actions seem to have been his best hope of political survival given that exile was not a good option (Bush was against it and, as the Pinochet experience surely taught Saddam Hussein, just because one is promised a secure exile does not mean that the promise will be kept – such promises lack credible commitment or enforcement). Once he knew that Bush 43 was serious and not bluffing it was too late for Saddam Hussein to extricate himself or to alter his earlier policies which had, after all, worked well for him for nearly a quarter of a century. (George Downs and David Rocke's resurrection hypothesis, published in the *AJPS* back in late 80s or early 90s, provides a good account of this sort of choice pattern. And we should remember that sometimes these extreme risks pay off – the Tet offensive was a military failure for the North Vietnamese but it was a tremendous political coup for them).

The Bush 43 case you point to is an example of confusing/conflating the interests of the American people (however those might be known) and George W. Bush's interests. Your evidence for Bush acting against his self-interest is that he undermined the interests of the United States around the world. But, let's look at Bush as a political leader and not treat his interests and U.S. interests as one and the same.

First, we might take note that George W. Bush was re-elected by a strong margin in 2004. Despite the indeterminate outcome of his first presidential race and the revelations of Abu Ghraib and torture before the 2004 election, he won re-election, a major objective for most democratic politicians. What is more, while with hindsight we believe that the policy of torture (which I strongly opposed from the first revelations), harmed American interests, it is worth noting that John Kerry did not speak out against the policy during the 2004 campaign,

presumably out of fear of being labeled as soft on terrorism. So, while the actions may indeed have harmed America's standing internationally, it seems evident that Bush saw the policy as beneficial to him (and probably believed and still believes it was the right thing to do) and Kerry, apparently, did not see opposing it as beneficial.

Second, even after an incredibly unpopular presidency and a disastrous economy, Bush's Republican Party managed to win 48 percent of the popular vote for the presidency in 2008. Pursuit of a third presidential term for a party is difficult even when the presidency has been rather successful and popular (e.g., Eisenhower was quite popular in 1960 and the presidential race was extremely close, but still Nixon lost; despite Clinton's popularity, Al Gore, though in an extremely tight race, did not become president). So it must be said that given the record of Bush's presidency, the 2008 race was not a blow-out. If we assume, as I do, that politics is about political survival (of individuals, their parties and their policies), then the assumption that Bush acted irrationally seems odd at best. His core constituents remained happy enough with what he did that they made the 2008 race perhaps surprisingly close.

Finally, the example of Americans (and virtually the entire world) living unsustainably is not evidence of irrationality. It is evidence that people discount the future rather heavily compared to their short-term welfare (remember Axelrod's shadow of the future?). Everyone does things that they find beneficial in the short-run even when they know that it is harmful in the long run. People smoke, they eat too much, they save too little, sleep too little, etc., all of which just indicates that they do not share your implied willingness to sacrifice now for benefits later. There is nothing irrational about that; it is just indicative of the fact that different people tolerate risks differently, discount the future differently, and evaluate information differently. This is a good example where personal values – which I happen to share – are being substituted for logic and evidence in coming to conclusions about what motivates people.

Joseph Nye is another colleague of yours who has also done policy work. One of the main conclusions he draws is that policy makers, by operating under stress and with imperfect information, are very little helped by our analyses. What's your take on that?

Let's be a bit more precise about what Joe Nye claims. He does not claim that policy makers are little benefited by the sort of analysis he does. Rather he contends that they are little benefited by the sort of analysis that I do – that is, mathematically grounded, game theory analysis. (See his recent piece in the Washington Post).

I do not know whether policy makers benefit from what I will refer to as conventional political analysis. They surely listen to the individual wisdom and personal insights offered to them by some scholars though whether that is beneficial or leads to better policy outcomes is hard to say. (They certainly listened to such academic IR scholars as Henry Kissinger, Zbigniew Brzezinski, and Condoleezza Rice).

What I know from personal experience is that there is a push in the intelligence community to bring more technical forms of analysis, including statistical, decision-theoretic, social network, and game theoretic analysis, to bear on intelligence assessments. In fact, I am on a National Academy of Sciences committee charged to examine this question at the request of the intelligence community, presumably because they think such analysis will be improved by more scientifically grounded methods than those currently most in use.

One of the arguments that Joe Nye makes against mathematical analysis of the sort I do, is that policy makers do not understand it and therefore do not pay attention to it. This is an odd argument. It implies that people who use game theory either do not or cannot speak their own language clearly enough to communicate ideas and results without resort to technical jargon. As someone who has been consulting in the intelligence and national security world for 27 years I must disagree. I have briefed people at quite high levels of government in the United States and Great Britain over the years as well as high level corporate executives. I have not encountered any particular difficulty communicating to them ideas and results without resort to math or technical jargon. The results I report on, to be sure, were derived from game theory analysis, and questions they posed to me were answered based on such analysis, but always in plain, straightforward English. I do not report on my personal opinions and am not purporting to offer wisdom. Without grounding my reports in the analysis I do, I might have come to completely different conclusions and, as noted by the CIA's evaluations, my personal, "expert" judgment was likely to be less accurate and less reliable than my model-based assessments.

That is not to say that I or anyone else in the international relations research community has a particularly large impact on policy. I think that by and large we do not and I think the reason for that is a combination of things. As a community we have not produced enough reliable results along with evidence for those results so that we should expect decision makers to pay close attention. And not enough of us are willing to put our reputations on the line by offering explanations or insights in print into events before they happen so that policy makers and academics can evaluate who gets things right – by whatever means – and who does not. That is one reason why I have emphasized publishing analyses about important policy questions before the outcome is known. That way, people can decide for themselves whether my approach is helpful based on the evidence rather than just their personal preferences.

Since you do, as you have indicated, quite some consulting work for government, I'd like to ask: how influential is game theory and rational choice in US foreign policy formation?

Joseph Nye and Mearsheimer, for instance, have said that nobody in government pays attention to this sort of work, the fact is that they do. First of all, I did extensive research on highly important national security matters for the US at the specific and direct request of William Casey when he was director of the Central Intelligence Agency, at the specific request of Undersecretaries of State and Defense, and briefed them on the results. Likewise, in the UK I have done briefed Cabinet Secretaries and members on important policy matters in the Tony Blair administration. To give you a recent example, I did a report that was crucial in changing the

Intelligence estimates with regards to Iran's nuclear program. This report took off of the table for the Bush administration large part of the possibility of attacking Iran. Secondly, while there is a huge diversity in commitment to idealism and world views in the people working with national security, I'm at the head of the National Academy of Sciences at the moment on request by the director of Intelligence for the purpose of trying to get broader use of quantitative and mathematical methods by analysts in the intelligence community, because people at the senior level apparently have come to the conclusion that some important policy failures were the result of not using these methods, but that these failures were rather the result of applying more casual approaches to national security and international politics more general. Since we are working with people at a high level of government, we will have a substantial impact: why would the government ask for such a committee if they didn't take the work seriously? Thirdly, the CIA has incorporated and is regularly using, for almost 25 years, a model called Factions, which is my original Static Forecasting Model. And they reported that it has had a substantial impact on decisions made by the President of the United States in crucial foreign policy matters.

Is the European Union, giving up sovereignty for not much of a gain in political leverage at the supranational level, a rational project?

The members of the European Union – and their voters -- certainly made important economic and environmental gains as a result of the Union. I doubt that the Union was agreed to by its members out of a strong desire to increase their supranational political leverage. Rationality, remember is an assumption that individuals do what they believe is in their best interest, taking constraints into account. It is not an assumption about what specific interests (supranational political leverage) they have or want. I am certainly not an expert on the European Union so your question would be better pursued with someone who is (Frans Stokman at Groningen, or Robert Thomson at Trinity in Dublin, for example). I would be wary of pointing to a single outcome and then assuming that this was the purpose behind formation of the Union. Of course, a model could be constructed around the assumption that the objective of the members was supranational political gains and, assuming those gains were not had, we could then conclude that either incomplete information led to an ex post incorrect choice (this is the fundamental insight in a nut shell into the ex ante attraction of cartels and their ex post failures) or the specific choice model was falsified.

You've run a prediction on Iran in April at Ted Talks. Now things start to happen there. Was your model close, or did it miss out on anything?

The model so far has done rather well. I predicted that the Iranian presidential race would be close but that Ahmadinejad would win. We don't actually know how close it was – we do know that the reported results are inconsistent with polling and with a reasonable statistical projection from the previous election. The modeling results also predicted that Ahmadinejad and Khamenei's power was going to enter a period of significant decline even though Khamenei

would remain a major political force probably until he retires. Clearly the events since the election indicate that Ahmadinejad and Khamenei have faced an unprecedented political challenge. We have to go back to the 1979 revolution to see something comparable so I think the evidence supports the prediction that they are entering a period of declining political power (which should not be confused with saying they will be ousted any time soon). Having gone back into my output to look at other details, I was intrigued to see (as a non-expert on Iran) that the model predicted a sharp increase in the political power of students and dissidents starting now and continuing for several months, although with fits and starts (I can send the graph if you like – it is just an excel plot of results produced on November 1, 2008 as I prepared my TED talk).

On the nuclear front, I am predicting that by around 2010 or early 2011 (the model is not as precise as I would like about timing; it is better at sequencing) there will be an agreement that limits Iran to producing small, research-quantities of weapons-grade fuel. I have not modeled the inspection regime that would be required to support such an agreement. It is worth noting that President Obama has acknowledged publicly that Iran has the right (under the NPT) to produce civilian nuclear energy (and, implicitly, to enrich uranium for this purpose), something apparently denied by the Bush administration. So Obama has moved the discussion forward toward the outcome predicted in my TED talk so as I see it things are moving as predicted both on the political influence front and on the nuclear front.

What's the biggest challenge to rational choice/game theory approaches so far?

The biggest challenges are a shortage of students and professors attracted to political science with the mathematical skills and exposure to the scientific method so that they recognize the actual limitations as opposed to the imagined limitations put forth by people who do not have a good understanding of the wide range of rational choice theories. By way of illustration, it is common for IR scholars to put prospect theory forward as an alternative to rational choice yet prospect theory is a rational choice theory. It is an alternative to expected utility maximization, but it (like minimax regret, quantal response, satisficing and other approaches) just makes different assumptions about how people convert their values and beliefs into action intended to improve their welfare.

If one wants to understand the world from a game-theoretical way, who should we absolutely read and why? What's the most elegant piece on game theory you know?

I assume this question is asking about the "IR" world and not everything in the world. I think the people right now who are doing the most to advance our understanding of important IR phenomena from a game theory perspective are, in no particular order, James Fearon, Alastair Smith, Robert Powell, James Morrow, Branislav Slantchev, Eric Gartzke, Ethan Bueno de Mesquita, Shanker Satyanath – I had better stop but there are others equally important to read so I inevitably will leave out important, cutting edge scholars for which I apologize.

I cannot list one work as the most elegant and I am confident that elegance is not the criterion I would use to recommend works for others to read. Let me suggest some works for people to read that can help provide insight into IR from a game theoretic perspective. I find Robert Powell's *In the Shadow of Power* deeply insightful in clarifying some of the important limitations of realist and neorealist thinking. Jim Fearon's papers on rationalist explanations of war and on audience costs provide a strong foundation for understanding fundamental features of interactions in international affairs. Ethan Bueno de Mesquita's papers on terrorism provide a strong understanding of the difficulties inherent in negotiating with terrorists, explanations for many significant empirical regularities associated with terrorism, and insights into the structure and factionalization of terrorist organizations. Alastair Smith and Jim Morrow (along with Randolph Siverson and me) try to provide some perspective on IR and its relations to domestic politics in *The Logic of Political Survival* and related papers. I would hope that IR students and researchers would know at least most of these works.

Last question. Why has there been so much resentment against using rational choice in IR?

Ignorance and fear. I don't view scholarship as a popularity contest, and some scholarship can be very demanding. As with any method, I find there are good critiques of game theory as a way of addressing a certain kind of problems, but the good critiques are almost never made by people who are not rational choice people, because people who don't do rational choice simply don't know enough. They haven't done enough homework to actually know what rational choice theory is about. If you go through the citations of people who make these harsh critiques of rational choice, shockingly, you see they are simply unaware of the literature. What they're citing is people who don't do rational choice and their critique is therefore invoking a straw man. So the argument that, for instance, rational choice has not added anything to our understanding, is a patently false statement: if you actually read the rational choice literature in IR, you know it's false.

One should go even further and acknowledge that a substantial body of rational choice literature has addressed some of the most prominent theories of IR in a very logically careful way, and has shown that the central arguments don't follow. Now that is done by people who didn't start out being hostile to the arguments analyzed, rather the other way around. For example, the work of Robert Powell, a student and friend of Kenneth Waltz, has formalized significant parts of the neo-realist argument and shown that essential elements don't logically lead to the conclusions that neo-realists think they lead to, and that they don't play a role in generating outcomes in actual international politics. Powell has shown, for instance, that anarchy is not unique to IR – one should rather conceptualize this as non-cooperative games (read a summary of the argument here). He has also demonstrated very carefully why the debate between relative gains and absolute gains is incoherent logically.

In a 1989 book, *The Balance of Power* and in subsequent articles, Emerson Niou, Peter Ordeshook, and Gregory Rose took the neo-realist argument, formalized it, and they deduced

four theorems from the neo-realist, so-called ‘Waltz-assumptions’. They make a distinction between essential and inessential states. An essential state is a state that can turn a losing coalition or block of nations into a blocking or a winning coalition. An inessential state is a state whose addition to an alliance or coalition can’t change any outcomes in terms of winning, losing or blocking. The four theorems that follow from a careful picking apart of the neo-realist argument are: (1) essential states never become inessential; (2) essential states never cease to exist; (3) inessential states never become essential; (4) inessential states do cease to exist. Empirically, however, we can say that each of those four logically derived results from the neo-realist argument is false. Austria-Hungary and the Soviet Union in their days were essential states, and they no longer exist. In the 16th century, the Netherlands was certainly an essential state, and it is not today; the US in the 1820s was not an essential state, and it certainly is today, and so forth. We can amass cases that contradict the theorems that follow logically from Waltz’s theory. And this is just a part of a large body of literature that has contributed significantly by formalizing prominent IR theories and by showing that – in the case of neo-realism – these central beliefs are wrong. And yet, this rational choice based literature is either ignored or dismissed by the people that rational choice has shown are doing research the central tenants of which are simply false.

Now apart from these negative results, rational choice and game theory specifically has also produced some important positive results and insights into politics. If you look, for instance, at Jim Fearon’s work on rationalist explanations of war, you see that the reason of why wars occur, is not about ethnic, economic or other differences, but it’s rather about three elements, of which you have to have any one to get a war: (1) uncertainty; (2) a dispute over something indivisible; or (3) a commitment problem. Now this is a very significant contribution, because you can now predict the probability if any one dispute will become violent. Furthermore, as a policy maker, you can then zoom in on these three and eliminate them. Now if theory is about engaging with empirics, about testing explanatory value, then this is not a bad track record. Morgenthau in the preface to the third edition of *Politics amongst Nations* indicates he was urged to respond to critics of the logics of his theory, but he writes: ‘I will not stoop’. But this is not stooping, this is how science progresses! If your theory doesn’t hold to empirical scrutiny, what is it worth? In my view, realist theory and balance of power theory is affected in its core by the empirical and logical challenges posed. They have been sufficiently refuted, they are false theories, and we should move on. But they don’t move on.

This is not to say that it is in all cases ‘rational choice versus IR theory X’, because I hold, for instance, that there is potential for a marriage between rational choice and constructivism. They don’t exclude but rather complement each other. Constructivism is all about how preferences arise and rational choice takes them as given to predict outcomes, so this is potentially a great combination.

Bruce Bueno de Mesquita is a senior fellow at the Hoover Institution and the Silver Professor of Politics at New York University. His most recent books include *The Logic of Political Survival*, with Alastair Smith, Randolph M. Siverson, and James D. Morrow (MIT Press, 2003, winner of the 2004 Best Book Award from the Conflict Processes

Section of the American Political Science Association), *Predicting Politics* (Ohio State University Press, 2002), and *Principles of International Politics*, 3rd edition (Congressional Quarterly Press, 2006), as well as the forthcoming *Strategy of Campaigning*, with Kiron Skinner, Sirhey Kudelia, and Condoleezza Rice (University of Michigan Press). He is also the author (with George Downs) of "The Rise of Sustainable Autocracy" in *Foreign Affairs* (September 2005) and numerous other policy pieces in major newspapers and magazines concerned with means to promote nation building and the impediments to success. Additionally, he has authored more than one hundred articles and fourteen books on politics, as well as one published novel, *The Trial of Ebenezer Scrooge* (Ohio State University Press, 2001).

Related links

* Faculty profile at NYU

* Read Bueno de Mesquita's Game Theory, Political Economy, and the Evolving Study of War (*American Political Science Review*, 2006) here (pdf)

* Read Bueno de Mesquita's Foreign Policy Analysis and Rational Choice Models here (pdf)

* Read Bueno de Mesquita's Development and Democracy (*Foreign Policy*, 2005) here (pdf)