

Contingency, Politics, and the Nature of Inquiry:
Why non-events matter

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Abstract

Contingent events are probabilistic. Acknowledging that realized contingencies alter observed political outcomes, however, does little to advance the systematic study of politics. This paper suggests that political science should focus on understanding how foreseeable contingencies, rather than truly “exogenous” unforeseeable events, alter political behavior. The most useful tool for understanding the effects of these foreseeable contingencies on political interactions is the formal and informal analysis of strategic behavior in the face of uncertainty through the use of game theory. This paper identifies the limitations of using observed contingent events to understand the role of contingencies in politics. More broadly, it suggests that the proper analysis of anticipatory strategic behavior has implications for the allocation of research resources across many topics of interest to political science.

Contingent events are probabilistic. They manifest or do not manifest because of some uncertainty about the future that is unknown or unknowable to human participants. A simple toss of a coin, for example, will produce an observed outcome of “tails” about half of the time. The contingent outcome “tails” following a coin toss is, prior to the toss, a contingency. How should political science incorporate the fact that important political interactions are embedded in situations where outcomes are unknown prior to their occurrence?

The problem political scientists face in confronting contingency is particularly difficult for two reasons. First, rarely are we interested in contingent events for their own sake. Whether a political leader survives an assassination attempt is interesting, in most lines of political inquiry, because we believe that who leads matters and not because the continued persistence or death of that particular leader is itself of interest. We as political researchers are therefore interested in the downstream effects of potential and realized contingencies. In contrast, in gambling or operations research, one might care only about the contingent event itself (does the coin come up heads, or how frequently do we need to test a product to ensure that it is of high quality 99.95% of the time?). Political scientists must therefore incorporate contingency as a mediating factor in the political outcomes they study.

Second, political actors are strategic. As such, manifestations of contingencies as contingent outcomes are shaped by the choices of political actors and themselves shape the behavior of strategic actors in anticipation of potential contingent events. For example, political leaders in parliamentary systems may call elections now rather than in the future if the economy is doing well and they fear the contingent outcome brought about by the combination of a surprise future economic downturn and retrospective voting behavior. Thus, elections may be more likely to take place during good economic times than bad ones. Similarly, wise leaders

avoid starting wars with enemies much more powerful than themselves. While there is uncertainty about the contingent outcome of any military interaction, strategic actors nonetheless anticipate the likely contingent outcome (defeat) and plan accordingly. In contrast, those studying the behavior of coins or the quality of production can safely assume that their subjects do not change their behavior strategically. Human decisions, unfortunately, are substantially more complicated.

If one accepts the basic notion that contingencies matter, both because they shape subsequent outcomes and anticipatory behavior, what should political science do to grapple with and manage this uncertainty? My argument here is twofold. First, not all contingencies are the same. In particular, there are some events that are fundamentally unforeseeable *ex ante*. These events are rare, however, and it is my contention that they should not be the focus of our research. Rather, political science should make primary those foreseeable contingencies around which strategic political actors maneuver. Foreseeable events, like unforeseeable events, are fundamentally probabilistic. Nonetheless, they matter because political actors have beliefs about the probabilities that they will manifest, and these beliefs shape the strategic interactions among political actors.

Consequently, my second argument is that political science already has the capacity for incorporating this second form of contingency, knowable uncertainties, into analysis of strategic behavior. The way to account for this form of contingency, however, is not to study the observed probabilistic manifestations of contingent outcomes, but instead to study how strategic actors behave in light of those potential contingencies. The most useful tool for understanding the effects of these foreseeable contingencies on political interactions is the formal and informal analysis of strategic behavior in the face of uncertainty through the use of game theory.

The remainder of this paper is organized as follows. I first review different types of contingency and make the case for focusing on those contingent events about which strategic political actors may maneuver. Next, I demonstrate why and how the analysis of anticipatory strategic behavior should lie at the heart of the study of contingency in politics. Third, the paper identifies the limitations of using observed contingent events to understand the role of contingencies in politics and suggests that analysis of anticipatory strategic behavior has implications for the allocation of research resources across many topics of interest to political scientists. Finally, I conclude by addressing several potential counter-arguments to my position.

Forms of Contingency and Their Study

All contingencies are not the same. In particular, it is analytically useful to distinguish those contingencies about which we know something, anything, and those of which we are unaware.¹ A useful label for the latter category is unknowable and unnatural disasters—things that are so out of the ordinary that we do not even believe they could occur. (An extreme example would be the universe simply ceasing to exist. A more realistic example is of a hurricane striking in the time before weather forecasting and the maintenance of historical records.) Because these events are fundamentally external to our immediate cognitive environment, they cannot affect, *ex ante*, our strategic behavior.

Clearly these types of contingencies matter—an unforeseeable hurricane kills many people. But because these events are not even considered by strategic actors prior to their

¹ For a different categorization of contingencies, see Schendler (this volume). Contingencies of which we are initially unaware are catastrophes in Schendler's typology, although, as I suggest below, many catastrophes are foreseeable. I group Schendler's remaining categories of contingencies together as known contingencies.

occurrence, generalizing about the effects of unexpected hurricanes or other fundamentally external influences on human behavior is difficult and of limited value. Unforeseeable events are singular occurrences. In societies with written records, a hurricane is only an unforeseeable event once. Thereafter individuals could choose to alter their behavior in anticipation of a hard to predict surprise storm. Unforeseeable events are also generally rare and unique, so inferring from them the effect of other unique events is difficult. In short, the first flood is likely to have different effects than the first hurricane, etc.

Three objections might be raised to the argument that unforeseeable contingencies are best avoided by political science. One might claim, for instance, that if we are really interested in how humans behave in response to becoming aware of new contingencies, realized unforeseeable contingencies are interesting. The experience of the French Revolution, for example, has large effects on subsequent democratization efforts. But once the contingency of mob rule manifests the first time, it become foreseeable and is best approached in the manner described below. (Alternatively, one could go further and argue that strategic participants knew about the risk of mob rule, but simply revised upward their belief about its likelihood after observing it in the French case.)

Alternatively, one might state that there are many interesting circumstances in which some of the participants in a strategic interaction have knowledge of contingencies but others do not. In insider stock trading cases, for example, corporate insiders might know that a company is about to be purchased whereas ordinary stockholders do not. These are not really examples of unforeseeable events, however. Instead, they are cases in which there are ordinary informational asymmetries. These example abound, as when individuals who have better knowledge of their

own ideology and competence run for office before voters who know relatively little about the candidates.

Finally, one could argue that unforeseeable events are common and important. In designing public policy, for example, there are numerous instances of “unintended consequences” that have yielded dramatically different outcomes from those sought by policymakers. My argument, however, is that few of these cases are actually unforeseeable contingencies. Rather, strategic policymakers generally make decisions that yield outcomes that were considered, but were dismissed as unlikely to occur. Of course, it is interesting that policymakers make decisions that are, *ex post*, clearly badly informed. But, to presage the argument below, it is my claim that these sorts of strategic decisions are where political scientists should focus their attention rather than beginning with an analysis of the effects of these decisions. (Furthermore, as is explained below, if one does not consider the strategic sources of bad decisions, empirical analyses of the effects of these decisions are likely to be biased.)

So, if we instead focus our research efforts on foreseeable events, what are the implications for the study of politics? Prior to addressing this question directly, it is useful to note that even relatively rare events may influence strategic human behavior and thereby the outcome of realized contingencies. For example, serious earthquakes are rare in San Francisco, but the city has nonetheless chosen to impose stringent building codes. Earthquakes are even more rare in Chicago, but they still occur. In the event of a major earthquake in either city, however, individual buildings in Chicago are likely to fare less well because the city has not adopted stringent building codes to protect its citizens from the realized contingency of an earthquake. That is not because earthquakes are unknown, but because mandating earthquake-proof buildings is not seen as worth its relative cost (wisely or not). Similarly, six inches of snow

in Atlanta brings the city to its knees while Hartford shrugs because it has invested in the technology necessary to cope with the more-frequently anticipated contingency of snow. Preparing (or not) for earthquakes and snow are strategic decisions in anticipation of foreseeable contingencies.

Whether to prepare for natural disasters is representative of the numerous cases in which individuals make strategic choices in light of their beliefs about the likely effects of these choices on desired outcomes. This, I claim, is the realm where political science is likely to find the most success in building generalizable models of human interaction. Rather than asking what effect did the assassination of Yitzhak Rabin have on the Middle East, a far more useful endeavor is to focus on the more general question of what the problem of potential assassinations has on the behavior of government leaders.

How to Confront Contingency: The power of anticipatory action

In this line of reasoning, it is short step from being assassinated to losing an election. Both outcomes matter (and not only for the incumbent). How should political science internalize these contingencies? Here, I answer this question by way of an extended discussion of a particular area of political science research. One of the fundamental questions of politics is how the practice of selecting government officials alters their behavior. Taking this question more narrowly, one area of ongoing research focuses on debates about the appropriate method for selecting judges. If one is to make arguments about the (in)appropriateness of judicial elections, however, it is necessary to have a theory that links judicial behavior to systems of judicial selection. How should one do this?

One approach is to seek out what appear to be interesting “contingencies” surrounding judicial elections. In other words, one might look for historical events where the practice of

electing judges appears to matter. One might imagine that such an approach to this topic could lead one to discover the following (hypothetical) historical narrative:

Judge Smote ascended to the bench after beating the incumbent, Judge Light, in a bruising electoral contest that focused on Judge Light's earlier sentencing behavior. Judge Smote's campaign advertisements focused on a case in which Judge Light sentenced David Randall, who was convicted of indecent exposure, to a probationary sentence in lieu of time in prison. While on probation, Mr. Randall was arrested for exposing himself to a young child. In an indecent exposure case now before the court, Judge Smote assigned the defendant John Davis the maximum prison sentence allowed by law.

One could ask many different questions in light of this historical incident, but for my purposes I wish to focus on one potential question and how it reveals the limitations associated with using realized contingencies as explanations for politically relevant outcomes. Specifically, one could imagine political scientists interested in the effects of judicial elections raising the question of "what explains the harsh sentence given to the Mr. Davis?" Taking as given Mr. Davis' decision to commit the crime for which he was convicted and the process that led to his conviction, one explanation for Mr. Davis' sentence is the presence of Judge Smote rather than Judge Light on the bench. Analytically, it follows that this line of inquiry now requires us to explain why Judge Smote, and not Judge Light, was sentencing on that particular day.

Thus, we must explain the outcome of the electoral contest between Judges Light and Smote. Given the historical narrative above, we might attribute Judge Light's defeat to her lenient sentence for Mr. Randall's combined with his decision to commit a crime while on parole. We might then conclude that the harsh sentence given to Mr. Davis is the product of a pair of realized contingencies given the institution of judicial elections: Judge Light's lenient sentence and Mr. Randall's recidivism.

What are the potential limitations of this explanation that are internal to the logic of the explanation itself? (That is, putting aside any concerns about the generalizability of the

inferences that can be drawn from this single historical episode.) Foremost, we must establish that Judge Smoot and Judge Light would have sentenced Mr. Davis differently. At first glance, this is a trivial matter (assuming the two cases are, on the merits, actually alike). After all, Judge Light sentenced an earlier defendant convicted of the same crime to a lenient sentence. But would Judge Light, if she had never been challenged or had won reelection, been similarly lenient? Alternatively, absent her successful campaign against Judge Light, would Judge Smote have been so punitive? Consider in turn three revisions to the above story.

First, suppose that Judge Smote never challenged Judge Light in the first place. In facing the Davis case, Judge Light might nonetheless have sentenced more punitively because of her experience in the Randall case. When sentencing Mr. Randall, Judge Light was uncertain about the probability he would recidivate. The realized contingency of Randall's recidivism could have led her to revise upward her beliefs about the appropriate sentence necessary to prevent this recidivism.

Second, imagine that Judge Light had managed to hold off Judge Smote in a close election. Facing the Davis case with the experience of having nearly lost office on the basis of a decision that was perceived as too lenient, she might then have sentenced more punitively to inoculate herself in the next election against the criticism of being too liberal.

Third, what if Judge Smote had not obtained the bench as a result of beating Judge Light but had instead been appointed to the bench because of Judge Light's untimely accidental death? In this instance, Judge Smote would face the Davis case without the experience of knowing that the lenient sentencing of an individual convicted of the relatively minor offense of indecent exposure could become the flashpoint of a successful campaign against an incumbent judge. If so, she might have sentenced Mr. Davis as leniently as Judge Light did in the Randall case.

In the first alternative account for the observed sentence given to Mr. Davis, the institution of judicial elections is irrelevant. Rather, fundamental uncertainty about the nature of recidivism explains the different sentences in the Randall and Davis cases. In the latter two accounts recidivism is also important, but it is important solely because of the institution of judicial elections. More fundamentally, however, judicial elections explain the more punitive sentence for Mr. Davis, but this more punitive sentence will occur regardless of whether Judge Light is defeated by Judge Smote. Rather, it is the institution of judicial elections and the potential defeat of judges who assign lenient sentences that leads judges to sentence more punitively than they would otherwise.

The problem political analysts face in explaining the single historical case outlined above is that these four competing explanations are all potentially valid. Skilled researchers might bring secondary evidence to bear to establish that Judge Smote is indeed more punitive than Judge Light (even apart from the need to posture to gain office), but these efforts do little to address the core question for political scientists about the role of judicial elections in explaining judges' decisions. This is precisely because, by focusing on observed "contingencies," one can miss the enormous power of strategic behavior in anticipation of those contingencies and attribute to events, rather than strategic behavior, important political outcomes.

To illustrate this point more fully, return to the above simple question: How do judicial elections shape judicial behavior? The above narrative suggests that lenient sentencing coupled with recidivism can result in incumbent judges losing office. Very few incumbent judges lose office, however, despite persistent claims of judicial liberalism. Why not? One explanation is that judicial elections don't affect judicial behavior (perhaps voters are so uninformed that judges pay elections no heed). But another potential explanation is that judges are perfectly aware of the

possibility of having their sentencing revealed as inappropriately lenient and, ex ante, adjust their sentencing behavior in response (Huber and Gordon 2004). Thus, the realized contingency of lenient sentencing and recidivism may not manifest because judges do not sentence leniently when they anticipate politically costly recidivism.

At the very least then, an analyst searching only for instances of observed judicial leniency (a sentence is “too” lenient when recidivism occurs) would understate the effects of judicial elections on incumbent behavior. We might even wrongly interpret that elections have little effect on judicial behavior. Furthermore, if judges do account for the potential publicity costs of lenient sentencing in their initial decisions, those instances of judicial leniency that do manifest in recidivism are likely to be unrepresentative of the cases where judges feel most constrained by the institution of judicial elections (All judges might recognize that sex offenders are likely to recidivate, but not realize that one of the many drunk drivers they see in their courtrooms is going to get drunk and cause an accident involving a school bus).

Fortunately, political science is relatively well equipped to deal with these impediments to direct inference from observed outcomes. The primary tool is the use of models of strategic interaction under the rubric of non-cooperative game theory. While many formal game theoretic analyses are expressed mathematically, the logic of strategic action in the face of uncertainty translates broadly and can be expressed rather informally. The critical insight of this approach is that we can understand how contingency (uncertainty) affects behavior by causing actors to anticipate outcomes before they occur.

Before applying this approach to the question at hand, consider a very simple example from the world of academia: Why don't teachers in large lecture courses assign the same exam question about the most important topic covered in a course each time they teach the class? The

answer is simple: because students, who presumably have access to past tests, would learn exactly what they had to study to get an A and would study nothing more. Randomizing examination questions forces students to anticipate a range of examination topics. Presumably this serves the professor's interest in encouraging students to learn all of the material relevant for a given class, even at the cost of not getting to probe students' knowledge about the single most important topic covered. In other words, professors anticipate students' strategic studying and construct exams accordingly. Given that professors think some topics are more important than others and also have other things to do with their time, they may not write entirely new exams each semester, but the exams will incorporate enough new questions and variation in topics to keep students on their toes.

Applying the same logic to the question of judicial elections, consider the following highly simplified model of this important political interaction. There are three strategic "players": A voter, an incumbent judge, and a (potential) challenger to the incumbent judge. The voter, like most people, is busy and would prefer not to spend his time monitoring the minutia of judicial decision-making. The incumbent judge enjoys being a judge and is willing to give up some of her potential judicial sentencing authority to retain that esteemed office. Finally, the potential challenger would like to someday become a judge, but would prefer to experience the costs of a campaign only if she has a reasonably good chance of defeating the incumbent.

In an active campaign, the challenger can seek to publicize incidents of judicial "malfeasance" by the incumbent such as taking a bribe, presiding over a case in which the judge has an interest, or simply assigning a sentence the public doesn't like. Because it is easier for the public to understand the reasons that a convicted felon deserved a harsh sentence than the mitigating circumstances that warrant a more lenient one, however, challengers will have an

easier time communicating to the public cases of apparent excessive judicial leniency than cases of excessive judicial punitiveness. Busy voters will rely on claims by challengers (perhaps skeptically) to evaluate incumbent performance, in effect allowing the challenger to “audit” the incumbent’s performance on their behalf. In anticipation of this behavior, incumbent judges have an incentive to become more punitive in order to reduce the number of cases a challenger could use to advertise the incumbent’s apparent judicial leniency, thereby reducing the probability the potential challenger will actually enter the race in the first place.

The point then is that judicial elections can “matter” by leading incumbent judges to become more punitive *even though* there are few, if any, cases of judges losing office because of sentencing too leniently. From an incumbent judge’s perspective, the relevant contingency is whether or not a given sentencing decision will later be portrayed as excessively lenient. If challengers are highly effective at uncovering instances in which the public might perceive judicial leniency, judges must be concerned about leniency in all of their cases. Alternatively, if the claim of underpunishment is persuasive only when accompanied by realized recidivism, then judges must weigh the probability of recidivism against their own sentencing preferences. Ex ante, however, whether a convicted felon will recidivate is unknown (although judges might have a better sense of which convicts are most at risk). The judge, therefore, cannot strategize knowing if the contingency will manifest, but has to behave as if it might manifest. Thus, the possibility of a contingent event occurring alters the sentencing behavior of incumbent trial court judges in the presence of judicial elections.

Implications: Guidance for Research and Resources

So far, this essay has argued that manifestations of contingency may reveal relatively little about the important implications of contingencies for the strategic behavior of political actors. The

aforementioned case of judicial selection, however, may appear too narrow for some political scientists. Does the logic of strategic action in light of contingency travel to other areas of research, for instance debates about the role of events as causes or the process of democratization? The answer put forth here is that analysis of strategic action in light of uncertainty is not only appropriate to this diverse body of topics, but also essential if political science as a discipline is to allocate its scarce resources to understanding the political world.

How can analysis of strategic behavior help political science in identifying the appropriate allocation of scarce disciplinary resources? Assume that one is interested in the effect of political campaigns on voter turnout. One common approach to studying this topic is to use existing survey instruments and data collection projects to analyze the covariation between campaign contact and voter turnout. The problem, of course, is that campaigns do not target a random sample of potential voters. Rather, campaigns seek to mobilize voters who would otherwise not vote in the absence of their activity and tend to ignore those who would vote or not vote regardless of the campaign.

Estimating the general effect of campaigns on turnout from campaigns that are targeted at those most likely to be affected by the campaign is therefore nearly impossible. Instead, the knowledge that ordinary professional campaigns are targeted has spurred calls for field experiments to randomly campaign and then observe the effect on turnout. The results of incorporating the realization of strategic behavior by campaigns into empirical research on this topic are stunning. Using National Elections Survey data, Wielhouwer and Lockerbie (1994: 216) estimate the effect of campaign contact on turnout at 23.6%. In a more recent study, Hillygus (2005: Figure 1) estimates that campaign contact increases the probability of turnout by 40% among those who do not already intend to vote. In a randomized field experiment that

removes the bias introduced by targeted campaigning, however, Gerber and Green (2000: 658) find a much more modest effect of personal contact of about 7%. At best, given strategic behavior, “there is an illusion of learning from observational research” (Gerber, Green, and Kaplan 2002). At worst, however, political science as a discipline is investing in uninformative projects *at the expense of* potentially more revealing forms of inquiry. (For example, administering the National Election Survey, which provides the data used in the Weilhauer and Lockerbie article, has an annual cost of about 1 million dollars. The Gerber and Green experiment cost less than \$50,000.)

But what about “larger” questions, like the conditions that lead to the solidification and consolidation of democracy (Stokes, this volume)? Is strategic behavior really relevant for understanding the contingencies that manifest in successful democratic consolidations and those that result in returns to military rule or authoritarianism? Here again, it is my contention that focusing too much on the success or failure of observed efforts at democratization may overlook the strategic interactions that explain when and how autocratic leaders or exclusionary regimes choose to experiment with democratization in the first place.

Consider Chile, where the possibility of the resurgent democratic state turning on the remnants of the Pinochet regime led those actors to enact constitutional measures giving persistent powers to interests of the old regime. The constitutional provisions, in anticipation of a contingency that may manifest anyway (witness Pinochet’s potential extradition), have had profound effects on the practice of Chilean democracy (Londregan 2000). Or consider the fears of property expropriation that may lead minority regimes with immobile assets to be unwilling to experiment with democratization for fear of having those assets expropriated by the newly powerful majority (Boix and Garicano 2001). In short, a great deal of the “action” in

democratization may take place prior to the decision to experiment with democracy and may also determine the form that the experiment will take. By looking only at observed institutions and the relative success or failure of democratic regimes, however, we risk attributing to those institutions success and failure without realizing that the institutions themselves are fundamentally rooted in prior political conflict (Boix and Adsera 2004). In short, contingent events within new democracies matter, but what chance events are likely to occur (and whether they will matter) is shaped by the institutional choices that precede the trial of democratization.

One might accept my contention that institutions, rules, and practices are themselves the contingent outcome of larger political conflict, but still maintain that contingencies internal to these systems matter in determining political outcomes. That events—particularly wars and depressions—are causes of political outcomes is the fundamental contention in Mayhew (this volume). Clearly wars and depressions matter, the critical question, however, is why do they matter? The Japanese surprise attack on Pearl Harbor plunges the nation into World War II and gives the Roosevelt administration great latitude to pursue its interests in allying with European countries against the Axis. Critically, what makes the event such a powerful “cause” is that it is external to the behavior of the Roosevelt administration. Americans do not blame Roosevelt or the Democrats for Pearl Harbor, and if anything, the Democrats subsequently succeed in making political hay out of Republican insistence that isolationism would protect American interest over the long run from “Europe’s war.”

In contrast, consider the events of late August 1998. At that time, the Clinton administration ordered a military assault on alleged terrorist facilities in Sudan and Afghanistan. One key target of the attack was Osama bin Laden, the head of a terrorist organization alleged to be acting to threaten the United States. At the time, President Clinton stated the attacks were

necessary to “counter an immediate threat.” Skeptics were quick to suggest that Al Qaeda was not a real threat to the United States and that the motivation for the attack was to distract the nation from the growing domestic crisis Clinton faced because of his relationship with a former White House intern, Monica Lewinsky. (The attack took place three days after Clinton first addressed the nation regarding this relationship.) How then did the public react to this critical event? Was the president justified in going after bin Laden, or was he an invented enemy who posed little threat to the United States? At the time, a CNN survey found that more than one third of Americans thought Clinton ordered the attacks partly to divert attention from the Monica Lewinsky sex scandal (CNN 1998).

This episode helps understand how we should approach the problem of contingency in studying politics for two reasons. First, strategic actors must consider the sources of seemingly external events. In this case, Americans disagreed about whether Clinton had obtained new information that led him to stage an attack abroad (and which might warrant rallying to the flag to support his leadership in a time of crisis) or was trying to pretend as if he had obtained such information but was instead acting simply for political preservation. Those who opposed Clinton had a strong interest in convincing the public that the attack was unjustified. On the other hand, supporters of Clinton sought to do otherwise. This point may seem obvious, but it is important because it suggests that in strategic situations actors will consider carefully the motives of those who claim to have access to “private” information about “exogenous” contingent events that explains their actions.

Second, the manifestation of the attack provides further insight into the nature of contingency as a source of non-events. Clinton ordered the attack despite the realization that some skeptics would doubt his motives. Imagine if he had not, despite the clear evidence (at least

in the post-9/11 world) that bin Laden posed a real threat. One could easily have seen him arriving at this conclusion precisely because the public would doubt his motives. (Clinton did not take more extreme action, such as invading Afghanistan or substantially increasing pressure on Pakistan to end its alliance with the Taliban, perhaps because of the broad skepticism that would have surrounded such an aggressive move during the Lewinsky scandal.) That is, Clinton's range of feasible political activity was limited because of the public's uncertainty about the merits of claims of private knowledge. This suggests that political inquiry must also consider how the fact that strategic actors evaluate claims of externally driven contingent events skeptically shapes *inaction*. Overall, political actors might not take desirable action precisely because of ex post negative evaluation of their actions, even though outside political actors would, if completely informed, endorse the move.² (This is precisely the logic underpinning Canes-Wrone, Herron, and Shotts' [2001] model of pandering in presidential leadership.) It is not surprising therefore that a great deal of real political conflict concerns debates about which events are unforeseen contingencies and which are altogether unforeseeable. Political actors have an incentive to take credit for unforeseeable good contingencies by portraying them as products of good leadership. They also have an incentive to prevent bad foreseeable contingencies, or to distance themselves from these unfortunate events by claiming they were unforeseeable and beyond their control. (Voters, for their part, sometimes seem able to distinguish who is "at fault" in different contexts.

² One could push this line of reasoning even further by arguing that terrorists might have been emboldened to take risky actions precisely because they knew Clinton would have a more difficult time garnering public support to stop their advances during a time of domestic political strife. A purely rational strategic public would have to incorporate this possibility into its evaluation of the likelihood that external events justified the use of force abroad.

For example, Ebeid and Rodden [2006] show that gubernatorial election outcomes are less tied to local economic condition in states vulnerable to international market forces [e.g. commodity price swings] than in more insulated states.)

Overall, events matter, but they matter in two very different ways. First, they matter because other strategic actors have a difficult time properly assigning blame and awarding credit for the manifestation of contingencies. Whose fault is it? Could we have avoided it? Second, they matter because strategic actors will seek to avoid events, realized contingencies, which will be damaging to their interests. In both cases, the best way to understand these dynamics is not to focus on events as they take place, but to consider broadly the implications of uncertainty and potential contingencies for the strategic decisions that shape human political interactions. Taking contingency seriously demands incorporating theories of strategic behavior in the face of future uncertainty into both theoretical and empirical work in political science. The alternative, to ignore uncertainty and strategy, is to overlook one of the basic underpinnings of conflict in all political interactions.

Some Counter Arguments Addressed

So far, this essay suggests a rethinking of how political scientists should approach the “problem” of contingency in political interactions. By bringing to the fore the idea of anticipatory behavior, it generally suggests a greater focus on the logic of strategic action in anticipation of foreseen contingencies. It particularly advocates for the application of game theoretic models of these complex interactions. Such an approach is not without its limitations, however. Here I briefly explore the confines of this approach by considering three types of objections to the arguments made above.

First, consider the objection that strategic models of interaction require individuals to be supremely rational and selfish utility maximizers. In the example developed above about judicial sentencing behavior, one might object to the characterization of incumbent judges as concerned with retaining office. After all, judges have many motives, including both the desire to “judge” and to be seen positively by other members of their social community. Narrow expressions of judicial preferences as concerned with reelection necessarily understate the true complexity of the “utility functions” judges seek to maximize. Arguments of this sort are not, however, attacks on the idea of modeling anticipatory behavior. Rather, they are criticisms of particular expressions of actors’ utility functions and maximization behavior. If strategic actors are other regarding (Pettit, this volume) and concerned about their peers, stating that judges first value reelection may miss the fact that some judges would rather lose than be perceived as punitive. Alternatively, actors may be bad at math, more or less risk averse, or systematically biased in their acquisition and processing of different types of information. Insofar as incorrect formalizations of actors’ preferences and decision-making processes are wrong, these are weaknesses with particular models, however, and not the approach of modeling as a whole.³

A second and related criticism is that game theoretic models of behavior often overlook the systemic or organizationally induced sources of behavior and outcomes. Bureaucracies, for example, structure decision-making. Therefore, they generate both standard operating procedures and create interests for their participants that are internal to the organizational apparatus (Jervis 1997; Perrow 1986). Beyond noting that such organizational incentives are easily incorporated as

³ For an extended discussion of the difficulty, and promise, of modeling social and peer effects, see Manski 2000.

revisions in actors' preferences, a broad focus on anticipatory behavior suggest stepping back further to recognize the strategic and anticipatory origins of these systems and organizations themselves. (See the above comment on the design of institutions in newly democratizing nations.)

Finally, one might suggest that recognizing contingency means embracing uncertainty about political outcomes above simple deterministic theories of outcomes (Hacker 2001). Reading this essay as embracing outcome determinacy (even when it originates in strategic behavior) is, however, a mistake. Rather, I am suggesting that acknowledging the uncertainty of outcomes requires us to admit that strategic actors are similarly confounded by uncertainty in making their own choices. Their strategic choices, made in light of uncertainty, in turn affect the outcomes of important political interactions, the realized contingencies that have been the focus of much previous scholarship. Overall, contingencies may be unexplained *ex ante*, but they are not exogenous. Furthermore, their long shadow shapes important political behavior in anticipation of their occurrence and in ensuring their avoidance.

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