Introduction

Contingency’s Challenge to Political Science

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At its starkest, contingency challenges the very possibility of science. By calling something contingent, at a minimum we are saying that it did not have to be as it is. Things could have been otherwise, and they would have been otherwise if something had happened differently. Science is usually seen as geared to uncovering laws that account for what must be the case. If the universe is law-governed, how can there be genuinely contingent events? Perhaps they seem contingent to us, but for the committed scientist this perception must mark our incomplete understanding. Either things are necessary and science is possible, or they are contingent and it is not. Contingency’s challenge is thus about the nature of reality, not just about the limits to our grasp of that reality. In the lingo: it is about ontology, not just epistemology.

Suppose that Israeli Prime Minister Yitzhak Rabin had not been assassinated by Yigal Amir in November of 1995, or that South African President F.W. De Klerk had been killed by a disgruntled white right-winger in January of 1992. Because no other National Party leader was willing to face down the Afrikaner hard right, as De Klerk did in March of that year by calling an unprecedented referendum on whether to conclude an agreement with the African National Congress, his death would almost certainly have derailed the negotiations between his National Party government and the ANC. This would have greatly strengthened NP reactionaries and ANC radicals, quite likely sending the country spiraling into chaos if not civil war.¹ By contrast, had Rabin escaped Amir’s bullet in 1995, he might well have concluded the agreement to which he was then close with Yassir Arafat and the Palestine Liberation Organization and which his
successor, Shimon Peres, did not pursue. The subsequent collapse of the Oslo accords and second intifada might have been avoided, ushering in agreements on Jerusalem, the status of refugees, and Jewish settlements that can scarcely be imagined today. We might have been looking today at a Middle East “miracle” while the ongoing the South Africa basket-case would have surprised no one.²

A bullet kills one leader, not another. Democratic settlements and civil wars occur in different countries as a result. Small contingencies with vast effects. History is replete with such instances of what might, or might not, have been. Had the just-appointed Prime Minister Winston Churchill—widely seen as a hothead at the time—not prevailed in his uphill battle with Foreign Secretary Lord Halifax in May of 1940, Britain would probably have capitulated to Hitler along with Belgium and France.³ How much twentieth-century history would then have been different?

Yet many social outcomes do not depend radically on contingencies in this way. My decision to vote or not to vote is unlikely to affect an election’s outcome. Any of the thousands of New Englanders who headed west in the nineteenth century could have stayed home without affecting America’s westward expansion. In these cases had a great many people acted differently—which they might have done—the results would have been different, but the actions of any given individual were inconsequential.

Choice is often the hallmark of contingency, but not always. Had an infected monkey not bitten someone in Africa in the 1980s, the AIDS pandemic might never have erupted.⁴ True, there is a connection with a human choice here—the person might have chosen to stay home on the day she or he was bitten. But contingency’s connection to human choice might itself be contingent. The monkey might have been eaten by a lion the day before it bit the person who became the initial human carrier. Then again, the lion might have been shot by a hunter the day before it would otherwise have eaten the monkey. And the hunter might have missed his flight to Africa the day before he would otherwise have shot the lion . . .

There are countless contingencies, massively consequential for human existence, that have nothing at all to do with human action—let alone human choice. Had a meteorite not struck what is now the Yucatán peninsula some 65 million years ago, the dinosaurs and thousands of other species of plants and animals might not have become extinct.⁵ How different would our world be in that case? And, of course, we might be rendered extinct by a future meteorite. We might be wiped out by a deadly microbe that is sitting under a rock somewhere and happens not to have been dis-
turbed for thousands of years—but could be knocked over at any time. In short, important contingencies for human affairs may be, but need not be, related to human choice, and many particular human choices, while contingent in that the person might have done otherwise, may not be especially consequential for larger social and political outcomes.

This means that when we consider the challenge of contingency to the possibility of political science, or even to the social sciences generally, the questions we have to ask, while a subset of contingency’s general challenge to science, are not easily quarantined. The social sciences might differ from the natural sciences in being concerned with human contrivances and the vicissitudes of voluntary action, but this is unlikely to map neatly onto the problem of contingency. Social and political arrangements can affect, and be affected by, the nonhuman world as well as the human world, and the degree, if any, to which these effects are law governed may not vary in any systematic way with the discipline of study. Indeed, as David Wootton’s discussion of the origins of contemporary debates about contingency in chapter 1 makes clear, early modern thinkers who wrestled with the idea of contingency did not work with the disciplinary terrain that most of us take for granted today.

Political science, conceived as that branch of the social sciences focused on the nature and dynamics of political regimes, may at times be in the inescapable vice of contingencies, yet immune to them at others. How much contingency has been thought to matter to the scientific study of politics has varied with theoretical fashion. Proponents of such architectonic theories as Marxism, elite theory, and modern rational-choice theory have been concerned to divine laws of politics, cousins of the laws of physics, that would render the apparent effects of contingency epiphenomenal or illusory, or relegate them to the error terms in powerful explanatory equations—to be minimized if not done away with entirely. Skeptics of these views range from those who see everything as contingent—partisans of the quip that history is just one damn thing after another—to those who recognize that much is contingent yet nonetheless aspire to develop a science of politics. Their strategies range from limiting attention to the noncontingent features of political life, to studying what is contingent in one context as necessary in another, to dealing with contingency through the lens of probability, to minimizing contingency—as Machiavelli urged—by understanding it and controlling it. The advantages and limitations of these various strategies for coping with contingency’s challenge to political science are the central focus of this volume.
It is tempting to see contingency’s challenge as an artifact of the Enlighten-ment—and in particular its erosion of religious understandings of natural law. As Wootton’s chapter makes clear, however, although contingency’s challenge was thrown into sharp relief by the eclipse of providential views of the universe, the core conundrum is traceable to older conflicts within the natural law tradition itself. These centered on two paradoxical tensions. One, arising from seeing God as all-powerful and natural law as universal, was captured in the question: Is God bound by natural law? If an affirmative answer seemed to threaten the idea of God’s omnipotence, then conceding that natural law might be altered by God’s will—or in formulations like John Locke’s that it simply is God’s will—undermined natural law’s universality. Put differently, if nothing in the universe is contingent on God’s will, in what meaningful sense can He be omnipotent? But if He is omnipotent, then how can there be laws that are truly universal? It seems that natural law must be, yet cannot be, binding on God.6

Closely related to this dilemma was the tension between the idea of an omniscient God and the intertwined doctrines of the fall and the possibility of redemption. If God created human beings with the capacity to choose between courses that lead to salvation or damnation, what they will in fact choose must in some sense be an open question. Yet this seems at odds with the idea of an all-knowing God who exists outside time and space and who knows, therefore, how everything turns out. How can the choices confronting humans really be choices at all if the outcome is not in doubt? We see the paradox at its starkest when we ask: Can an omniscient God create a world in which there are contingent outcomes? It seems that God must be able to do what it is impossible for him to do. Wootton’s chapter chronicles and illuminates the ways in which early modern thinkers grappled with these tensions.

The idea that all knowledge is corrigible, and that science advances not by making knowledge more certain, but rather by producing more knowledge, is a creature of the mature—post-Humean—Enlightenment.7 Philosophers of the early Enlightenment were wedded to the idea that only knowledge that is certain can be genuine. Sometimes this position was rooted in theology, sometimes in secular arguments like the Cartesian cogito. Whatever the source, indubitability was the gold standard for knowledge as distinct from opinion or speculation. Due to their identification of
knowledge with certainty, it would have been anathema to thinkers of the early Enlightenment to couple knowledge with contingency. Just as it seemed paradoxical to affirm that an omniscient God could exist in a contingent universe, so it seemed equally problematic to say that there could be knowledge of contingencies. Contingencies were conceived of as random, illogical, arbitrary, and impenetrable—by definition beyond the realm of knowledge. At the same time, however, they were not beyond the realm of experience. Under the circumstances, perhaps there could be an enterprise geared to reducing the influence of contingency in human affairs. This was one early modern approach championed by Machiavelli, who defended republics as superior to monarchies on the grounds that they are more flexible and therefore better able to adapt to unforeseen developments. Likewise, he contended that powerful polities, like Sparta and Venice, are comparatively likely to endure because they would be costly to attack. They were models of the kind of regime best suited to a world beset by contingencies.

Implicit in Machiavelli’s reasoning was the attempt to deal with contingency through the lens of probability: he was thinking about how to increase a regime’s chances of survival. As contingency displaced providence as the source of the unfathomable in human affairs, probability seemed to a number of early modern thinkers the best means to try to get a grip on it. Wootton notes thinkers like Pascal and Bayle who invented the notion of possible worlds, radically expanding contingency’s reach. Their attempts to bring order to unpredictability via the notion of probability were partly successful, but hampered by their almost congenital inability to entertain the idea that any proposition that was less than certain could rise to the level of knowledge. Hume, in particular, grappled mightily with this problem without resolving it.

In the course of trying to reconcile the realities of a contingent world with an epistemology that made certainty the hallmark of genuine knowledge, a number of eighteenth-century thinkers considered that there might be feedback mechanisms among the apparently disparate and chaotic events and—perhaps opaque—underlying forces that would render them coherent. This was the start of modern thinking about social processes by reference to such concepts as invisible hands and equilibrium processes for which Adam Smith is perhaps best known. But Wootton shows how several of Smith’s predecessors and contemporaries began thinking about politics in ways analogous to Smith’s *homo economicus*—the self-interested inhabitants of Mandeville’s *Grumbling Hive*. Not only
did they reason that apparently contingent events might be understood—and perhaps even predicted—by reference to underlying equilibrating mechanisms, they also carried this thought into the realm of institutional design. For it was but a small step from this thought to the Madisonian agenda that the selfish and aggrandizing actions of politicians could serve to offset and balance one another—that “[a]mbition must be made to counteract ambition”8—so long as their contingent actions were channeled through the right institutional structure.

Wootton’s history reveals that many contemporary debates about contingency were prefigured by early modern theorists. They were perplexed by contingency’s challenge to any ordered conception of the universe that the scientific outlook seems to require. Indeed, their distinctive epistemological assumptions, linking authentic knowledge to indubitable certainty, threw this tension into sharp relief. Their attempts to grapple with this tension involved taking a number of tacks, all of which would subsequently be explored more systematically. Mastering contingency so as to reduce its influence, using the ideas of probability and equilibrium to square the circle, and designing institutions to take advantage of contingency are all strategies taken in the contemporary literature that are explored in this volume. As a prelude to these considerations, we begin, in chapters 2 and 3, respectively, with analytical treatments of the meaning of contingency by Andreas Schedler and Philip Pettit.

What does it really mean to call something contingent, asks Schedler? His answer draws on a wide-ranging exploration of the term’s uses in both scientific and ordinary language. He finds that the meaning of contingency is partly, but only partly context-dependent. To call something contingent is to invoke a semantic architecture that rests on three pillars: indeterminacy, conditionality, and uncertainty. By indeterminacy Schedler means that when we call an event or phenomenon contingent, we invoke the idea of possible worlds alongside the actual world—what Max Weber once described as “objective possibility.” The world that exists might not have existed, and other worlds might have existed in its stead. “Things could be different,” as Schedler puts it. “They could be otherwise in the present. They could have been different in the past. They could be different in the future.”

But calling something contingent also invokes the notion of conditionality or causal dependence. Contingencies may often be what social scientists think of as dependent variables—they vary with the independent variables on which they depend. If there is no movement in the indepen-
dent variable, then there is none in the dependent variable. The distinction between dependent and independent variables is artificial inasmuch as an independent variable in one context may be a dependent one in another. Fertility rates might be contingent on women’s education and labor force participation rates, and these, in turn, might be contingent on social policies on education and child support subsidies. Conversely, changes in fertility rates might have an impact on public attitudes toward immigration. Whether everything—the existence of the universe and perhaps, even, the laws of physics—is in some ultimate sense contingent is a profound epistemic question, as we have seen. So is the matter of how well we can theorize about contingencies.

Often contingencies are unanticipated—think of the earthquakes and hurricanes once denoted “acts of God” in the contingency or force majeure clauses of insurance contracts. This is not to say that unexpected contingencies are uncaused; only that no theory is likely to anticipate them. They might be explained after the fact, but not predicted.

Or at least not predicted with certainty. At best we will be able to make probabilistic claims about future contingencies—such as that a major earthquake will very likely occur in California at some point in the next fifty years. Even when more focused predictions are made, as with the claim that when per capita incomes in democracies fall below $1,000 their chances of surviving for one year falls substantially, there is always an error term. It encapsulates contingencies—perhaps specifiable, perhaps not—that might forestall the predicted result in a given instance. Political scientists have yet to reach consensus about why India has survived as a democracy for decades against these odds. Contingencies produce outliers.

What do we mean when we call a contingent event more or less likely? Phillip Pettit illuminates this question in chapter 3 via the notion of resilience. Recognizing that contingent phenomena are conventionally understood as those things that are simply not necessary (that is, they may have occurred in the actual world but “do not figure in every possible world”), he finds such a definition lacks specificity. The traditional definition is a “catch-all category” that includes too many phenomena to be helpful. Pettit suggests, instead, that we focus on possible worlds. The larger the proportion of possible worlds in which a phenomenon would exist, the more resilient it is in Pettit’s sense. In other words, whereas necessary phenomena are those that occur in all possible worlds, their resilient (contingent) counterparts arise in many but not all such worlds. He goes on to argue that rational-choice and functionalist explanations ought
to be understood as explaining the resilience of a particular behavior or pattern rather than its emergence or historical continuance. That is, proponents of both these kinds of explanation end up proffering reasons why the behavior or pattern “continues across a certain range of contingencies.” The explanations in question tell us why particular phenomena withstand and persevere in the face of possible (but not yet actualized) disturbances, drifts, pressures, or crises. Institutions that are flexible in Machiavelli’s sense are resilient in Pettit’s.

II: Contingency’s Challenge

If Schedler and Pettit give us useful conceptual tools for thinking about contingency’s meaning, they do not tell us how serious a challenge contingency poses to the systematic study of politics. If contingent political outcomes can be predicted with high degrees of probability, if many political outcomes are strongly robust in Pettit’s sense, then the fact of contingency may pose no particular threat to the systematic empirical study of politics. In chapter 4, by David Mayhew, and chapter 5, by Jennifer Hochschild and Traci Burch, we find disquieting reasons for supposing that in politics contingencies do a lot more than lurk in *de minimus* error terms.

Mayhew makes a powerful case for the central importance of contingencies in accounting for many developments political scientists try to explain in other ways. The gravamen of his claim is that political scientists have been insufficiently attentive to causes that are “proximate, contingent, or short-term” rather than “basic, underlying, or long-term.” Whereas institutions, social forces, and enduring incentives predominate as explanatory factors in the study of American politics, Mayhew demonstrates the critical significance of such “chance events” as depressions, riots, assassinations, and wars, which often create “openings” that alter political outcomes decisively. There is a good chance that much New Deal legislation would not have been enacted but for the Great Depression. Ronald Reagan’s soaring popularity after the failed attempt on his life in March 1981 rescued his huge program of tax and spending cuts that had been headed for the rocks. The Great Society legislation was helped immeasurably by John F. Kennedy’s assassination. His legislative record had been so-so, but his death both sanctified and supplied impetus to his legislative program under Lyndon Johnson, as well as helping make 1964 “possibly the most productive legislative year since the 1930s.”
Nor are these isolated instances of “events as causes.” Mayhew cites a stunning array of major changes in taxes, tariffs, suffrage expansion, race relations, and veterans’ benefits that have been ushered in by wars. Indeed, the Second Bank of the United States and the Bureau of Internal Revenue (the ancestor of the Internal Revenue Service) were “durable institutions” brought about by the War of 1812 and the Civil War, respectively. At a minimum, Mayhew concludes that political scientists should pay more attention to the interactions among “underlying” causes and the types of events he identifies. Beyond this, they should focus less on elections and electoral competition in trying to explain political outcomes and more on what goes on between elections and how that interacts with electoral politics. They should also attend more to executive politics—which often drives and responds disproportionately to events—and less to legislative politics. Moreover, Mayhew suggests that students of American politics should invest less than scholars of Congress have recently done in devices like the Poole-Rosenthal left/right ideological scale for explaining congressional outcomes. Events can widen or narrow ideological differences in ways that are not captured by such ordinal scales. They can even shift entire ideological spectrums in unanticipated ways. Indeed, events can shape and reshape what the “right” and “left” wing positions are understood to be. Events can also swamp ideological differences—as with the near unanimous declaration of war on Japan following Pearl Harbor, the creation of NASA after the Soviet launch of Sputnik, or the Use of Force Resolution in the wake of the September 11, 2001, attacks.

In chapter 5, Jennifer Hochschild and Traci Burch reveal that political actors, no less than political scientists, can be victims of contingencies that they do not understand. Whereas Mayhew’s analysis calls into question much of the conventional scholarship in the study of American politics, Hochschild and Burch’s argument exposes the contingent nature of American law-making itself, an activity that presumably, by its very nature, should be subject to a certain amount of predictability. However, they show how in drafting even well-considered legislation, legislators can suffer from surprise. Here the surprise turns out to be the de-stabilization of America’s inherited racial categories. Specifically, they argue that the unintended and, indeed, unforeseeable effects of the Hart-Celler Immigration Act of 1965 (which overturned much of the 1924 rules on immigration) and the ability to “mark one or more” racial categories on the 2000 Census are leading to the disintegration of racial categories.

Given the context of the Cold War and the civil rights movement, legis-
lators and policymakers sought to remove the old system’s country specific quotas that had favored immigration from the British Isles, Ireland, and Germany. The goal was to ensure that the United States was seen as a “fair and meritocratic society.” Their primary goal in enacting the Hart-Celler Act of 1965 was not to increase immigration but rather to make it more fair—removing the bias in favor of immigrants from Northern Europe. If anything, these lawmakers thought that the act would only increase immigration from southern and Eastern Europe. As Hochschild and Burch argue, however, they were simply wrong—victims of contingency. The legislation was followed not only by an unexpected increase in immigrants from Asia, Mexico, the Caribbean, and South America, but also by an unanticipated decrease in immigrants from Europe. From 1970 to 2000, whites went from 83% of the American population to 69%, Asians and Pacific Islanders from 1% to 4%, and Latinos from 4% to 13%. These numbers stand in stark contrast to testimony given at that time by Attorney General Robert Kennedy (with the State Department and other experts concurring) that ending country specific quotas (passing the Hart-Celler Act) would not change the ethnic composition of America. The legislators could anticipate neither the complex incentives created by these actions nor the changes outside the United States about which they could not know.

More recently, in drafting legislation to permit one to mark multiple racial categories on the 2000 Census, lawmakers failed to see the combined effect of this decision with exogenous changes in immigration patterns. The debates in the 1990s to change the census reporting options involved little attention to immigrants or immigration law. They focused instead on multicultural issues of identity and choice, the artificiality of racial and ethnic categories, and the extension of anti-discrimination law to new realms. Even those who opposed the changes neglected the role of immigration, worrying instead about the possible impact on civil rights legislation. In fact, the two groups that stood at the center of the debate, whites and African-Americans, did not see it as related to immigration.

But as contingency would have it, most of those who turned out to define themselves by reference to more than one ethnic or racial category were immigrants, and the children of immigrants, from groups whose members had not been part of the public debate on multiracial categories. Moreover, since, as Hochschild and Burch argue, Anglo/Hispanics constitute the largest number of those who self-identify as mixed on the census,
an increase in Hispanic population will probably result in more people eschewing traditional categories of race. Here contingency took as its victim an entire group of political actors (lawmakers, analysts, lobbyists) who simply could not comprehend the effects of these two legislative changes, or how they would interact with other changes in patterns of immigration.

Whereas chapters 4 and 5 explore contingency’s challenge across time—events as causes of American development, the unintentional interactive effects of legislation, and various developments that are sometimes called path-dependent—the next chapter focuses on the contingencies of space. Just as certain temporal contingencies can have great political consequences, so can spatial contingencies shape politics as well. On this point, Susan Stokes demonstrates in chapter 7 that regions or locations can have significant effects on democratization.

Defining a region “as a contiguous and compact space constituted by a set of countries in the world or by an area within a country,” Stokes argues that spatial factors can affect the transition to and, more importantly, the consolidation of democracy. She distinguishes between those regional effects that are “spurious” from those that are genuine and “proper.” In the former case, rather than the role of region, it is some other underlying independent variable (such as income, age, education, housing quality) doing the actual explanatory work. True regional dynamics arise when a certain quality particular to the region in question best explains the relevant behavior. Moreover, these dynamics can occur within a political jurisdiction or between and among regions that are in close proximity to one another. Thus, a wave of democratization in nearby trading partners can exert more powerful effects than such a wave that is far away.

Stokes buttresses her case through an exploration of the regional unevenness in the consolidation of democracy in various parts of Argentina. Treating “regional quality of democracy” as an independent variable, she finds that out of four different regions within Argentina, one (the area of Mar del Plata) exhibited a more robust democracy (with, inter alia, less clientelism and a greater tilt toward programmatic politics) than the others. Rejecting factors such as income, education, gender, age, and party preference, Stokes contends that “distinctive regional characteristics” of Mar del Plata better account for this variation in democratic consolidation. These characteristics include more widely held beliefs in responsive government. But why do such regional differences arise? Here Stokes looks to the results of decades of distinctive types of regional leadership. In this
way, contingency is important in accounting for political outcomes in intersecting ways: regional differences have contingent effects on political outcomes, yet these regional differences are themselves the result of contingent factors.

III: What Is to Be Done?

The chapters in Part II establish the inescapable importance of contingency in accounting for political outcomes. Repudiating any notion that contingency is a minor error term to be quietly ignored, Mayhew, Hochschild and Burch, and Stokes establish that politics is influenced by the contingencies of time and space that scholars ignore at their peril. The study of politics is fraught with the vicissitudes of nagging counterfactuals, non-generalizable conclusions, and unexpected events. Acknowledging the omnipresence of contingency, how should political scientists proceed? The final chapters take up the challenge of contingency with an eye to providing some useful guidance. In a way, we end where we began. The early modern debates that Wootton explored in chapter 1 return in a contemporary idiom.

In chapters 7 and 8, Gregory Huber and Elisabeth Wood handle contingency by changing the terms by which the authors of previous chapters approached it. In their view it is misguided for political scientists to focus on the causes and consequences of particular events. They should not ask questions like: What would have happened had Rabin not been assassinated or if De Klerk had been? How much of the Great Society would have been enacted had J.F.K. not been shot in 1963? Would it have mattered if a particular individual had found himself living in a different region of Argentina? Instead, Huber and Wood think the focus should be on how strategic actors are likely to behave and what makes institutions endure, given the contingent political world.

Huber suggests that rather than attend to how “realized contingencies” influence political outcomes, political scientists would do better to study the strategic behavior of political actors “in the face of uncertainty.” Instead of approaching contingency as a backward looking enterprise—where the question is: Could this event have happened differently?—political scientists should address the forward-looking question: How are strategic actors likely to behave given the unavoidable existence of contin-
gency? From this perspective, concern with truly exogenous events (earthquakes, assassinations, etc.) does not further our knowledge of politics. After all, if something is not at all foreseeable, it will have little effect on the way people act. Simply put, Huber turns the tables on the apparent threat of contingency by charging political science to focus on political actors, not events or causes. Game theory, he contends, can aid in this endeavor.

Huber takes as his case study the effects of judicial elections on the behavior of judges. That is, how does the practice of selecting judges bear on the various kinds of sentences that judicial officers impose? Again, the wrong (and largely fruitless) question is to ask why did a certain newly elected judge impose a particular penalty on a criminal. The answer to this kind of question would depend on a host of contingent factors—the presence of one judge instead of another, the features of the case, securing an election win for the next race, to name but a few. But political scientists should leave such questions to others. Instead, the question should be: How should judges be expected to behave, knowing that they will be up for reelection? Here a probabilistic calculus becomes an important tool. As judges contemplate how harsh a sentence to impose, the chance the criminal may recidivate is always present. By using models of strategic action, we can hypothesize that, given the desire to be reelected, judges in certain situations will be more likely to impose higher than warranted penalties. For example, when faced with a high probability that the public will get wind of a sentence, a judge will probably dole out a harsher penalty, refusing to take a chance on the possibility that the criminal may not recidivate. In this way contingency’s effect on the strategic behavior of political actors becomes something we can study systematically.

Whereas Huber examines these effects of contingency on strategic actors, Wood is more interested in Machiavelli’s and Pettit’s question. She analyzes contingency’s impact on an institution’s robustness and how this robustness shapes the effect of contingent events. While some political arrangements (institutions, peace agreements, alliances, settlements) appear robust when confronted by unanticipated, contingent events, others unravel. Some emerge and persist independently in a variety of settings; others are rare or short-lived.

Wood invites us to think of an institution as an equilibrium pattern of interaction. An example of such an institution is a convention. Driving on the left side of the road, for example, is a convention that is characterized
by individuals driving on a particular side of the road. Contingent events—in this case a perceived need to attract business from neighboring countries in which they drive on the right—might disrupt such a stable pattern (or equilibrium), leading individuals to follow another convention, which might in turn produce a new institutional equilibrium. Wood uses stochastic game theory to model and analyze how contingencies cause such institutional shifts and give rise to new stable patterns. Her analysis seeks to ascertain why certain patterns endure or remain in the face of contingent challenges, which, she contends, can lead to insights about how to craft comparatively robust political arrangements.

Recognizing that not all contingencies are the same, she proposes three different models of for thinking about destabilizing and restabilizing equilibrium in institutional patterns: individuals randomly engaging in idiosyncratic behavior (a new stable pattern arises by simple error); exogenous shocks—Mayhew’s events as causes (an assassination or earthquakes); and organized, collective challenges by a group of political actors (a labor movement or sit-in). In Pettit’s terms, Wood offers three different ways of analyzing an institution’s resilience—depending on the kind of contingency involved. In each of the three models, she finds that the “basin of attraction” has much to do with an institution’s robustness or resilience. The larger the basin, the more likely it is that the institution will persist. By studying the resilience of institutions in the face of these different types of contingency, Wood renders tractable the seemingly intractable problem posed by theorizing about contingency itself.

In chapter 9 Courtney Jung brings comparable considerations to the study of democratic transition negotiations, but she refuses to punt so quickly on the challenge of accounting for contingency in politics. In comparing South Africa’s successful transition to democracy to its stalled Middle Eastern and Northern Ireland counterparts during the 1980s and 1990s, she explores the generalizable, though nonetheless contingent, features of attempts at negotiated settlements. The settings share in common the fact that neither the regime nor the opposition is powerful enough to impose a settlement unilaterally; they are also similar in that the regimes, while subject to an electoral constraint, are manifestly flawed democracies. In such quasi-democratic contexts, she argues, contingencies arise in certain patterns. There are opportunities and constraints in the various phases of potential peace negotiations.

Different actors might be more and less willing or able to take advantage of them for various contingent reasons, but one can still theorize
fruitfully about the types of circumstance that will lead windows of opportunity for settlements to open and to close, what is likely to happen if and when negotiators move through them—or fail to do so—at certain critical moments, and, indeed, what factors make it more and less likely that actors in these settings will be inclined to try to reach settlements. For instance, because politicians are critically reliant on popular support to legitimate their actions, one can tell a good deal about how likely they are to risk making agreements by studying their levels of popular support and the support for potential agreements. The very weak—low single digit—Palestinian support for a two state settlement as envisaged in the 1993 Oslo accords in the summer of 2000 made it vanishingly unlikely that Arafat would agree to such a settlement—no matter how strong the pressure from the Clinton administration and the Barak government in Israel. It would have finished him in Palestinian politics.

But the situation was very different in 1995. Had Rabin survived, or had his successor, Shimon Peres, called a snap election immediately following the assassination when there was strong Palestinian and Israeli support for the settlement, he would have won and been able to reach a deal. In this, he would have followed the model of De Klerk, who faced down his hard right flank by calling a snap referendum on the negotiations after he had lost a series of by-elections to the conservatives. That Peres lacked the appetite for risk, or perhaps the imagination, of De Klerk turned out to be a vital contingency that could not have been predicted. Nonetheless, the political science of transition negotiations advances when we gain a more accurate understanding, as we do here, of what the constraints and opportunities actually are, and what the costs and benefits of acting on them will be likely to be. We cannot predict when settlements will be reached, but Jung demonstrates that we can often get close to understanding conditions that are necessary for success. At a minimum, this will allow us to predict failure as with the Camp David attempt in 2000. But it also holds out the possibility of enhancing our understanding of what makes success achievable, and more and less likely.

In Chapter 10 Robert Shulman puts our deliberations about contingency’s challenge to political science into a larger perspective by recognizing its presence in the natural sciences, notably biochemistry. As political scientists we squirm at the thought that if events could always be otherwise, the very enterprise of generating law-like conclusions seems in jeopardy, but Shulman offers comfort in exploring the role contingency plays in the laboratory of the scientist. It is often said, as we noted at the outset
of this introduction, that science is impossible if things are contingent all the way down. Shulman rejects this formulation as wrongheaded, contending that contingency and science need not be mutually exclusive. If biochemists can successfully minimize contingency—which he argues that they can—then perhaps there is hope for political science as well.

Shulman highlights two ways in which scientists work to diminish the impact of contingency. First, in a spirit reminiscent of Huber, he illustrates by reference to the study of diabetes that it is the “well-defined medical question” that can overcome contingency. Scientists presently do not understand why some patients predisposed to diabetes fare better than others or why, despite the odds, some sedentary obese subjects remain healthy and some vigorous lean, subjects fall ill. Nonetheless, the focus on the question why for some the pancreas overproduces but glucose levels remain tolerable can turn such contingences into comprehensible outcomes. Knowing what to look for, then, is a simple, but vital task. Defining the parameters of the study—here the relationship between the functioning of the pancreas and the body’s glucose level—can aid in managing contingency. In this his claim is comparable to that of Huber and Jung who suggest that knowledge of politics can advance if we look at the relatively enduring incentives and constraints in different political circumstances, rather than at how a given individual in a given situation will respond to them.

Second, Shulman maintains that multi-method approaches can help tackle contingency. He sees this possibility at work in the study of the Brain/Mind. Here, contingency is inherent in the very definition of the subject, he argues. While some scientists appeal exclusively to a computational theory of the mind—maintaining a “top-down,” “fixed view” of the mind as a rational computer, others take a “bottom-up” approach, radically contextualizing mental concepts. Nevertheless, Shulman maintains that most of the time biological scientists actually reject both reductionism and holism. They move freely in both directions, looking for the interacting effects, and science is better for it. There might be lessons in this for the study of politics. Rather than run to deterministic theory (sweeping contingency under the “error term” rug along the way) or embrace contingency with the gusto of an event driven historian, perhaps political scientists should multitask more. In effect this is Mayhew’s advice in arguing for greater attention to the interactions between underlying dynamics and events as causes. Taken together, the chapters in this volume suggest that this is most likely to be achieved with a mix of methods—quantitative,
qualitative, and axiomatic. Taking our cue from Shulman, we should realize that we are more likely to domesticate contingency if we surround it rather than either run from it or chase it over the horizon.

NOTES

1. In an interview with Ian Shapiro on 8 December 2003, De Klerk confirmed that he consulted no one in the cabinet or National Party leadership on the decision to hold the referendum following a series of lost by-elections in conservative strongholds because he knew they would have opposed it. He described the choice as “the only unilateral act I took as President,” and speculated that had he been assassinated before the referendum, it would have been unlikely that any of the probable successors would have wanted to call such a referendum and, even if one did, the need to establish and consolidate his position as leader would have made it impossible to do so.


3. See Lukacs, Five Days.

4. For an exhaustive assessment of the “monkey bite” and other theories, see Hooper, The River. See also Barnett and Whiteside, AIDS, chapter 2, pp. 34–38.

5. See Frankel, The End of the Dinosaurs.

6. See Tuck, Natural Rights and Riley, Political Legitimacy, pp. 87–110.

7. See Shapiro, Moral Foundations, chapters 1–2.

8. Madison, “Federalist #51.”

9. One group of physicists has recently questioned whether the laws of physics might not be unchanging. See Webb et al., “Cosmological Evolution.”

BIBLIOGRAPHY


