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David A. Welch: Painful Choices

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Introduction

DURING THE 2000 U.S. presidential campaign, Republican candidate George W. Bush criticized the Clinton administration for overextending American commitments, failing to focus on key American interests, and burdening the U.S. military with tasks unrelated to its core mission. A president should authorize the use of military force, Bush insisted in the first of two presidential debates with Democratic candidate Al Gore, only “if it’s in our vital national interests,” when there is “a clear understanding as to what the mission would be,” when the United States is “prepared and trained to . . . win,” and when there is a clear exit strategy. “I would take the use of force very seriously,” Bush asserted. “I would be guarded in my approach. I don’t think we can be all things to all people in the world. I think we’ve got to be very careful when we commit our troops. The vice president and I have a disagreement about the use of troops. He believes in nation-building. I would be very careful about using our troops as nation-builders.”¹

In the second debate, Bush insisted that the United States should be “humble in how we treat nations that are figuring out how to chart their own course”—more respectful of the principle of state sovereignty, in other words—and he lambasted the Clinton administration for allowing the international coalition containing Saddam Hussein to weaken. “The coalition that was in place isn’t as strong as it used to be,” Bush complained. “He is a danger; we don’t want him fishing in troubled waters in the Middle East. And it’s going to be hard to—it’s going to be important to rebuild that coalition to keep the pressure on him.”²

With respect to the general principles of American foreign policy, in short, Bush promised restraint and selectivity. Bush would be a domestic politics president, most analysts surmised, as one might expect from a man without much experience of, interest in, or even knowledge of foreign affairs. Few international politics issues captivated him. Iraq was an exception. When host Jim Lehrer asked during the second debate whether Bush thought he could “get [Saddam] out of there,” Bush replied, “I’d

¹ “Presidential Debate between Democratic Candidate Vice President Al Gore and Republican Candidate Governor George W. Bush, Clark Athletic Center, University of Massachusetts, Boston, MA, 3 October 2000.”

² “Presidential Debate between Democratic Candidate Vice President Al Gore and Republican Candidate Governor George W. Bush, Wait Chapel, Wake Forest University, Winston-Salem, NC, 11 October 2000.”

like to, of course, and I presume this administration would as well.” But the main thing was containment. Bush would redeem Clinton’s failed Iraq policy by reinvigorating multilateralism.

By the spring of 2003, the U.S. military had toppled Saddam’s regime, having already toppled the Taliban regime in Afghanistan, and was heavily engaged in nation building in both countries.³ Bush’s call for humility, selectivity, restraint, and reinvigorated multilateralism had morphed into a doctrine of preventive war near-universally condemned abroad as dangerously unilateral.⁴ While the American action in Afghanistan enjoyed considerable international support, the war in Iraq did not—in fact, the United Nations Security Council was strongly opposed. A domestic-politics president had suddenly become a foreign-policy president, indeed a national-security president broadly construed, largely unconcerned with multilateralism, and anything but humble. What had happened to provoke such a change?

The answer, of course, is 9/11.⁵

Now, at one level there is nothing surprising about this. 9/11 was the deadliest terrorist attack in history. By toppling both World Trade Center towers in the most horrific way imaginable—using hijacked airliners as weapons of mass destruction—Osama bin Laden’s al-Qaeda network dealt the United States a psychological blow rivaled in American history only, perhaps, by the Japanese surprise attack on Pearl Harbor on December 7, 1941.⁶ Suddenly the American people—and, of course, their president—felt acutely vulnerable in a way they had never felt before, and to an enemy they had hitherto underestimated. It would have been odd indeed if American foreign and domestic policy had not reflected the widespread perception in the United States that the world had changed both suddenly and profoundly. “A faceless enemy has declared war on the United States of America,” Bush declared on September 12; “So we are at war.”⁷

³ “The theoretical pronouncements Bush had made about not nation building have been discarded almost wholesale in the face of the need to keep Afghanistan together. He was at times acting like the Afghan budget director and bill collector.” Bob Woodward, *Bush at War*, p. 339.

⁴ “National Security Strategy of the United States of America”; Ivo H. Daalder, James M. Lindsay, and James B. Steinberg, *The Bush National Security Strategy*, pp. 1–9.

⁵ Throughout I will use this particular popular shorthand for the attacks mounted by al-Qaeda against the World Trade Center in New York and the Pentagon in Washington, DC, on September 11, 2001.

⁶ As Bush confided to his diary on the evening of 9/11: “The Pearl Harbor of the 21st century took place today.” Woodward, *Bush at War*, p. 37.

⁷ *Ibid.*, p. 41.

The problem, though, is that Osama bin Laden had declared war on the United States more than three years earlier—on February 23, 1998.⁸ Al-Qaeda's first attempt to topple the World Trade Center had taken place eight years earlier. That attempt killed 6, injured 1,042, and caused "more hospital casualties than any other event in domestic American history apart from the Civil War."⁹ American officials knew that the mastermind of the first World Trade Center attack, Ramzi Yousef, had plans to fly aircraft into government buildings.¹⁰ Bush's own director of central intelligence, George Tenet, had told him even before the inauguration that al-Qaeda posed a "tremendous" and "immediate" threat to the United States.¹¹ The real puzzle is why American policy had not changed earlier to reflect the growing menace al-Qaeda had posed for almost a decade. "The question that would always linger," Bob Woodward later mused, "was whether they had moved fast enough on a threat that had been identified by the CIA as one of the top three facing the country, whether September 11 was as much a failure of policy as it was of intelligence."¹²

What is striking and surprising in retrospect, in other words, is not how dramatically American policy changed after 9/11 so much as how little it changed beforehand. Until 9/11, antiterrorism was a relatively minor concern in American foreign policy, largely handled as a routine security-intelligence issue—a matter of mundane police-work more than anything else. Terrorism was not an issue in the 2000 presidential campaign. Prior to 9/11, Bush had never even been briefed by his chief counterterrorism expert. The very first principals' meeting on the subject in the Bush administration took place only on September 4, 2001.¹³ Everything changed after 9/11. The "War on Terror" became *the* central concern not only of U.S. foreign policy, but of American politics as a whole—a matter of the highest of high politics, to which everything else suddenly became secondary.

American policy toward Iraq changed fundamentally after 9/11, too.¹⁴ Despite Bush's preelection hostility toward Saddam's regime, and despite the enthusiasm some of his officials felt for toppling Saddam from the very beginning of his administration, the fact is that Bush's Iraq pol-

⁸ Simon Reeve, *The New Jackals*, pp. 194, 268–270. For further discussion, see National Commission on Terrorist Attacks upon the United States, *The 9/11 Commission Report*, pp. 47–48.

⁹ Reeve, *The New Jackals*, p. 15. For further discussion, see *The 9/11 Commission Report*, pp. 153–173.

¹⁰ Reeve, *The New Jackals*, p. 8.

¹¹ Woodward, *Bush at War*, p. 34.

¹² *Ibid.*, p. 36.

¹³ Richard A. Clarke, *Against All Enemies*, pp. 26, 237–238.

¹⁴ Todd S. Purdum, *A Time of Our Choosing*, pp. 4–10.

icy remained Clinton's Iraq policy for more than a year and a half.¹⁵ Officially, both presidents favored regime change, but in practice, neither felt the kind of urgency, and neither had the kind of unified advice, that made an energetic policy of regime change possible. Both simply hobbled along. But 9/11 shook everything up and gave the Iraq hawks the window they needed. "September the 11th obviously changed my thinking a lot about my responsibility as president," Bush later told Woodward. "Keeping Saddam in a box looked less and less feasible to me."¹⁶ Once Bush had taken care of business in Afghanistan, he turned his sights on Saddam, despite the fact, with which Kenneth Pollack begins a book advocating a war with Iraq, that "[a]s best we can tell, Iraq was not involved in the terrorist attacks of September 11, 2001."¹⁷

While it is doubtful that anyone on September 10 would have agonized over the question of why American foreign policy had not changed significantly to meet the new transnational terror threat, anyone watching the news on the morning of September 11, 2001, would have felt that a dramatic change was certain. Anyone listening to the radio on December 7, 1941, would have felt the same way. But who would have anticipated Japan's fateful decision to attack Pearl Harbor in the first place? That signaled a change in Japanese policy of the first magnitude. Often the most interesting and the most momentous changes catch us by surprise. Moreover, they surprise us not merely because they seem to be such sudden, radical departures, but because radical departures strike us as so rare. Is there any way to improve our ability to anticipate such changes? Is there any way to enhance our understanding of dramatic changes (surprising or otherwise) and of the long periods between them?

My purpose in this book is to find out. I do so by developing and test driving a theory of foreign policy change. In this endeavor I was motivated by curiosity about three superficially different, but, at the end of the day, quite closely related concerns. The first was to see whether there is anything we can know with tolerable reliability about international politics *in general*. General, portable, parsimonious International Relations theory has been something of a Holy Grail at least since the behavioral revo-

¹⁵ As Ken Pollack puts it, "the Bush administration turned out to be not too dissimilar to the Clinton administration in its final days." Kenneth M. Pollack, *The Threatening Storm*, p. 105.

¹⁶ Bob Woodward, *Plan of Attack*, p. 27. On the internal disputes over Iraq policy and their evolution, see *ibid.*, pp. 9–30, esp. p. 23: "The deep divisions and tensions in the war cabinet with Powell the moderate negotiator and Rumsfeld the hard-line activist meant no real policy would be made until either the president stepped in or events forced his hand."

¹⁷ Pollack, *The Threatening Storm*, p. xxi. See also Clarke, *Against All Enemies*, p. 33.

lution, both in terms of its desirability and its elusiveness.¹⁸ The failure of the grand-theory project to date has generated considerable criticism of the very idea, both among those who would describe it as desirable in principle but impossible in practice and among those who, in rejecting its very philosophical and methodological foundations, would insist that it is impossible even in principle. I began this project with a hunch that our disappointment with the search for general, portable, parsimonious IR theory may simply be an artifact of our having asked the wrong questions and having attempted to solve the wrong problems.

My second concern followed directly from the first. Suppose that a general (or nearly general) IR theory were possible after all; how far might we take it? What are its limits? What limits it? What might these limits say about the very philosophical questions that underlie many of the most active debates in IR theory today? I had a second hunch that, while a general IR theory might be attainable, it could never be perfect, and that as a discipline we might benefit from pondering its imperfections. In a very literal sense, in other words, I sought to conduct a study both of the possibilities *and* limits of general IR theory.

My third concern was whether a general IR theory could be useful both to analysts and to leaders. Here my feeling was that there are three quite different standards of usefulness. The gold standard is the ability to see into the future. The bronze standard is the ability merely to understand the past. There are good reasons to think the gold standard unattainable, but my third hunch was that we can do better than simple post-hoc explanation. The silver standard, as it were, would be to reduce uncertainty about the future. I wanted to see if a general IR theory could help us, if not predict important events in world politics, at least anticipate them, or be sensitive to the conditions under which they become more likely.

In the course of working on this project, not surprisingly, I encountered both gratification and disappointment. My own view, having now completed it, is that the effort proved largely successful and the disappointments instructive. Readers will, of course, judge for themselves. But I believe it shows that there are indeed grounds for thinking that we can

¹⁸ I will embrace Hollis and Smith's useful practice of capitalizing the phrase when referring to the discipline (or using the abbreviation IR) and using lower case when referring to its subject matter. Martin Hollis and Steve Smith, *Explaining and Understanding International Relations*, p. 10. Behavioralism is a form of theory-driven empiricism employing the epistemology and methods of the natural sciences to seek lawlike generalizations, both explanatory and predictive, from quantifiable data. The phrase "behavioral revolution" denotes the social sciences' enthusiastic embrace of behavioralism in the 1950s and 1960s. Of course, some scholars working in a more traditional historical/interpretive mode sought general, portable theories as well; see, e.g., Hans J. Morgenthau, *Politics among Nations*.

generalize about important aspects of international politics in a way that helps us reduce uncertainty about the future as well as better understand the past.

I should note at the outset that this book constructs and explores *a* (fairly) general theory. The subtitle is *A Theory of Foreign Policy Change*, not *The Theory of Foreign Policy Change*. I do not wish to claim that this is the only one possible, even with respect to this specific problematique. The problematique itself is, of course, something that some readers will object to as falling outside the parameters of IR theory properly understood. Kenneth Waltz, for example, is keen to distinguish a theory of international politics from a theory of foreign policy.¹⁹ I am less keen to make this distinction because if by a “theory of international politics” we mean a theory of behavior, this is precisely the wrong game to stalk, and our having stalked it thus far unsuccessfully, I argue, is the main reason for the widespread disenchantment with general IR theory. I am interested in a theory of change in behavior, not of behavior per se, so I am not in search of the conditions under which states will do specific things—thus I am not in pursuit of a theory of foreign policy strictly understood. But I am interested in changes in the behavior of states, not of systems, and so if we strictly insist that only purely systemic theories qualify as general IR theories, I would be wrong to characterize my project as I do. Lacking the capacity to imagine the justification for such a restriction, I feel comfortable with the characterization. A general theory gives us an explanation for why things happen that is not bound by time or place, and that is precisely what I offer here.

In addition to being quite general, the theory is both portable and parsimonious. A portable theory enables us to apply the explanation to a wide variety of specific cases that might seem on the face of things to differ significantly from one another. A parsimonious theory explains a lot of things in terms of a relatively small number of things. It is easy to see the attraction of this kind of theory. Thus when I suggest that Kenneth Waltz may have erred in seeking a general theory of international politics as he conceived it, I do not mean to quibble with his understanding of theoretical virtues. I merely mean to suggest that the quest for a general, abstract, parsimonious theory of international politics as he conceived it was hopeless from the beginning.

Why do I suggest this? And why do I seek instead a theory of foreign policy change?

There is too much variation in the things leaders of states seek, and in the imperatives to which they respond, for any spare theory to capture either the dynamics of international politics in general or the mainsprings

¹⁹ Kenneth N. Waltz, *Theory of International Politics*, pp. 121–122.

of foreign policies in particular. This is because of the enormous complexity of, and variation in, the things that affect leaders' choices. Systemic imperatives, domestic political considerations, cultural attributes, ideologies, normative commitments, bureaucratic factors, and idiosyncratic personality traits can all influence the choices leaders make. Moreover, they do so haphazardly. This is not necessarily bad. It is one of the things that makes international politics so interesting. But it does mean that the kind of grand theory that presupposes the inputs of leaders' choices is bound to fail.²⁰

Are there any constants of international politics, any fairly hard-and-fast regularities, upon which to build international "theory"? Two things stand out. First, people everywhere process information in more or less the same way. Human psychology in Japan is the same as human psychology in Europe. If we can identify the things leaders take into consideration in their deliberations, we can fairly confidently anticipate (within certain limits) how they will process them. Second, the ship of state is ordinarily ponderous, not nimble. It is insensitive to minor changes in the environment. It alters course only with great effort, and only when absolutely necessary. The state does not—because it cannot—constantly sample the outside world, adjusting and fine-tuning its behavior so as to optimize something (such as "power" or "the national interest"). The state is not like a variable-timing engine that continuously monitors and adjusts to its environment. Instead, it goes about its business today more or less the same way it went about its business yesterday, unless it has some very compelling reason to do something dramatically different. American policy toward Iraq between 1991 and 2003 provides a classic illustration. "We just fly missions and drop bombs from time to time," retired Army Colonel Andrew Bacevich mused in the year 2000, "because we've been doing it for ten years and no one can stop us from doing so."²¹

If we can be fairly confident most of the time that states will do today what they did yesterday, and that they will do tomorrow what they do today, it is not terribly important to know why any particular state does

²⁰ For further discussion, see Masato Kimura and David A. Welch, "Specifying 'Interests.' "

²¹ Quoted in Thomas E. Ricks, "Containing Iraq: A Forgotten War," p. A01. If we wish to think in terms of similes, the state more closely resembles an interrupt-driven computer than a variable-timing engine. A computer processor operating at a 60 MHz clock speed will do precisely the same thing the next clock cycle that it did during the last—60 million times per second—until it receives a signal from an attached component, such as a keyboard, to do something different. It will then rise from its electronic slumber to comply. Even the fastest typist will convince a 60 MHz processor to do something different only once every 6 million clock cycles or so. While computers and states are both very status quo oriented, however, there is an important difference between them: to rouse the state, one must often pound both hard and repeatedly.

what it does, and there is no reason at all to try to determine why states in general do what they do in general. What we really need to know is: when can we expect them to do something noticeably different, and why?

I characterize the theory I develop here as a “loss-aversion” theory of foreign policy change because, as I will argue, foreign policy is most likely to change dramatically when leaders expect the status quo to generate continued painful losses. States will not alter their behavior simply to try to realize some marginal gain. The clearest signals of an impending change are desperation, stridency, and distress. The choice for change will often carry with it a risk of even greater loss—a risk of loss so great that, in many cases, no rational actor would accept it. The choice for change, in short, is commonly a painful choice.

I begin in chapter 1 by spending a little more time justifying some of these bold claims. In particular, I lay out a more careful defense of my view that a theory of foreign policy change is the appropriate game to stalk. In the course of this discussion I delve more deeply into meta-theory—the theory of theory, as it were—than some readers may care for, and if you have no more than a passing interest in this, you may wish simply to skim it. It is a necessary part of my argument, however, so I cannot afford to treat it too hastily. In chapter 2, I develop the theory of foreign policy change, drawing upon relevant work in organizational behavior, cognitive and motivational psychology, and behavioral decision theory. It is here that I begin paying close attention to limits. In the following three chapters, I “test drive” the theory in a series of structured, focused, comparative case studies.²² In these chapters I try to bring the abstractions down to earth. Finally, in the conclusion, I comment on the theory’s performance and limits and briefly address the practical policy implications of the study, which primarily concern strategic warning (in the security domain) and assessing the ripeness for negotiation and resistance points (in all domains).

While the book is clearly animated by an enthusiasm for the scientific project, my exploration of its limits reflects a pervasive theme: namely, that the science of international politics must somehow make allowance for the art. We must rediscover what scholars of International Relations knew before the behavioral revolution: that what happens in world politics is powerfully affected by the humanity of those whose choices shape it. Leaders have right brains as well as left. They are driven as much by passion as by reason. Moreover, international politics is a strategic enterprise not merely in the economic sense modeled so well by game theorists,

²² For methodological discussion, see Alexander George, “Case Studies and Theory Development”; Alexander L. George and Timothy J. McKeown, “Case Studies and Theories of Organizational Decision Making,” pp. 21–58.

but also in the classical sense, in which leaders sometimes attempt to mislead, lull, and outfox each other in high-stakes battles of wits. The kinds of events I seek to explain—and to help leaders anticipate—are therefore not the fully determined resultants of immutable physical forces, with which natural science grapples so well, but the willful choices of people acting in what they themselves perceive to be a fluid, dynamic, sometimes unpredictable environment. This places limits on the precision with which we can identify, measure, and forecast. It gives an advantage to the analyst who can combine knowledge and reason with less tangible abilities such as observational skill, insight, and empathy. It means that we must moderate our hopes and expectations for “testing” theory—Newton’s experiments would have consistently failed if the bodies he pushed and dropped and threw had the will, capacity, and malice to misbehave—but since international politics is not an entirely capricious domain, and human nature is not infinitely malleable, we can still use evidence to distinguish better from worse statements about how the world works. We are not confined merely to interpreting it, or bearing witness to it. The middle-ground approach to epistemology I promote and defend in this book—“test driving” rather than “testing” theory; judging its performance, its comfort, and its fit-and-finish, rather than its “truth” or even, in comparative perspective, its relative usefulness—is as much, I contend, as the subject matter will allow.

This book, in short, is an attempt to make the case that we should be as scientific about the study of world politics as possible under the circumstances, and that while circumstances may not permit perfection, they are not quite so debilitating as many believe. As between theory and policy, in other words, we do not necessarily have painful choices to make.