

LOST AMBITION: GRAND STRATEGY STABILITY AND ABANDONED CHANGE IN
THE JIMMY CARTER AND GEORGE W. BUSH ADMINISTRATIONS

Jacob Shively

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Jacob Shively

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This dissertation is about negative feedback, change and stability in United States (US) grand strategy. It specifically seeks to analyze and understand two seemingly dissimilar cases whose grand strategies have yet to be systematically compared: Jimmy Carter's attempt to redefine US grand strategy in more humanitarian and cooperative terms, and George W. Bush's attempt, under the "Freedom Agenda," to consolidate an assertive new strategic approach to the world. Though of different parties, operating in different geostrategic contexts and espousing different views on the role of American power, both presidents faced serious negative feedback and by ends of their respective terms had abandoned, if not contradicted, serious implementation of their earlier visions. Stated differently, their attempted changes failed. Comparing these two cases, with their apparent differences but ultimately similar outcomes, raises important questions about grand strategy change as well as when an attempted change fails or is abandoned.

To answer these questions, I will argue that despite their differences, the Carter and Bush administrations operated within the same dominant grand strategy—one established at the end of World War II—and that their efforts at change were likely constrained by the same overarching conceptual parameters. In turn, both administrations were susceptible to the same suite of negative feedback streams, and that these streams can be compared to determine which had the strongest effects in both cases. Identifying those streams will deepen our understanding of grand strategy feedback in all US administrations. Realist and liberal approaches to foreign policy, for instance, posit different explanations for strategic change, so this project aims to directly compare these competing accounts. The project's distinction between "dominant" and "tactical" level grand strategy also can help observers, no matter their theoretical commitments, separate the more stable elements of a grand strategy from those elements that are more flexible. For the broader study of foreign policy and political science, this research underscores the power of political and ideational inertia, and it systematically compares two cases that have, to date, never been compared in terms of comparative grand strategy and strategic abandonment.

Table of Contents

Chapter 1: Introduction

Chapter 2: The Constraints of Grand Strategy

Chapter 3: Case Study Format and Method

Chapter 4: From “Moral Turn” to Carter Doctrine

Chapter 5: Bush’s Second Term and Decline of the Freedom Agenda

Chapter 6: Comparing Feedback and Change between Carter and Bush

Chapter 7: Conclusion

Detailed Table of Contents

Chapter 1: Introduction

- Project Overview
- Research Overview
- Chapter Outline
- Grand Strategy: Current Research
 - The Concept
 - Schools of Thought
 - Ideas or Practices?
 - The Legro Model
- The Research Questions
 - The Problem of Change and Stability
 - Why “Failed” Change?
- Answering the Questions
 - Two-Level Approach: Dominant & Tactical Grand Strategy
 - A Framework for Change, Stability and Feedback
- Testing and Analysis
 - Assessing Dominant Stability
 - Comparing Alt. Explan. for Failed Change
 - Cases
- Conclusion
 - In Sum
 - Dissertation Outline

Chapter 2: The Constraints of Grand Strategy

- Introduction
- Method and Plan
 - Framework
 - Testing: Dimensions and Characteristics of Grand Strategy

- The Baseline: Post-World War II Grand Strategy
 - Grand Strategy Overview, 1941-1946
 - Dimensions of Postwar US Grand Strategy
- Carter's Moral Turn
 - Grand Strategy Overview, 1977-1978
 - Debating Carter's Foreign Policy
 - Dimensions of Carter's Grand Strategy
- The Bush Doctrine
 - Grand Strategy Overview, 2001-2005
 - Debating The Bush Doctrine
 - Dimensions of the Bush Grand Strategy
- Findings
 - Overarching Stability
 - The Dominant/Tactical Framework

Chapter 3: Case Study Format and Method

- Case Study Methods
- Descriptive Inference
 - Source Materials
- Congruence Method
 - Operationalizing and Testing
- Summary

Chapter 4: From "Moral Turn" to Carter Doctrine

- Introduction
 - Method and Plan
 - Salient Periods
- Inauguration and Early Precedents
 - Narration of Period
 - Analysis of Early Precedents
- Camp David to Treaty
 - Narration of Period
 - Analysis of Camp David to Treaty
- The Carter Doctrine
 - Narration of Period
 - Analysis of the Carter Doctrine
- Summary of Findings
 - Summary
 - Conclusions

Chapter 5: Bush's Second Term and Decline of the Freedom Agenda

Introduction

- Method and Plan

- Salient Periods

Second Inaugural

- Narration of Period

- Analysis

Katrina to Samarra

- Narration of Period

- Analysis

Midterm to Surge

- Narration of Period

- Analysis of the Carter Doctrine

Summary of Findings

- Summary

- Conclusions

Chapter 6: Comparing Feedback and Change between Carter and Bush

Case Comparison

- Comparable Cases

- Findings

- Assessment of Findings

Implications and Theory

- Legro Model (Slightly) Revised

- A "Snap Back" Thesis

Conclusion

Chapter 7: Conclusion

Overview

Theoretical Implications

- The Legro Model and Negative Feedback

- The Dominant/Tactical Distinction

- A "Snapback" Thesis

- Long Cycle Theory

Larger Contributions

- The Power of Inertia

- The Utility of Grand Strategy

Future Research

In Sum

Ch 1: Introduction

Project Overview

This dissertation is about negative feedback, change and stability in United States (US) grand strategy. It specifically seeks to analyze and understand two seemingly dissimilar cases whose grand strategies have yet to be systematically compared: Jimmy Carter's attempt to redefine US grand strategy in more humanitarian and cooperative terms, and George W. Bush's attempt, under the "Freedom Agenda," to consolidate an assertive new strategic approach to the world. Though of different parties, operating in different geostrategic contexts and espousing different views on the role of American power, both presidents faced serious negative feedback and by ends of their respective terms had abandoned, if not contradicted, serious implementation of their earlier visions. Stated differently, their attempted changes failed. Comparing these two cases, with their apparent differences but ultimately similar outcomes, raises important questions about grand strategy change as well as when an attempted change fails or is abandoned. Did the same types of negative feedback lead to these outcomes? Were these attempted changes even as radical as their advocates insisted? In terms of the general study of grand strategy, why and how do decision makers sometimes abandon or marginalize the very grand strategy change they attempted to implement? What constraints to change do they face that lead to this outcome? To answer these questions, I will argue that despite their differences, the Carter and Bush administrations operated within the same dominant grand strategy—one established at the end of World War II—and that their efforts at change were likely constrained by the same overarching conceptual parameters. In addition, both administrations were thus susceptible to the same suite of negative feedback streams, and that these streams can be compared to determine which had the strongest effects in both cases. Identifying those streams will deepen our understanding of

grand strategy feedback in all US administrations. Realist and liberal approaches to foreign policy, for instance, posit different explanations for strategic change, so this project aims to directly compare these competing accounts.

For the broader study of foreign policy and political science, this research underscores the power of political and ideational inertia, and it systematically compares two cases that have, to date, never been compared in terms of comparative grand strategy and strategic abandonment. As described below, distinguishing “dominant” from “tactical” level grand strategy also can help observers, no matter their theoretical commitments, separate the more stable elements of a grand strategy from those elements that are more flexible.

Research Overview

Much recent scholarship on grand strategy, catalyzed by dramatic turns like the end of the Cold War or the 2002 US National Security Strategy, tends to focus on the causes behind fundamental grand strategy formation and change.¹ By contrast, this work will study the logic behind policymakers’ decisions to drop an earlier change that they themselves (or at least their own political coalition) had implemented. In other words, it is exploring the abandonment or failure of a newly-implemented grand strategy change.

Specifically, the dissertation will involve two interrelated arguments. To begin, it draws upon Jeffrey Legro’s theory of great power strategy change as a framework. For Legro (2005, 13-16), major change only occurs, as it did for the United States after World War II, when an old strategy idea collapses after a shock, a single alternative arises, and that single alternative appears to be effective when first implemented. From this perspective, policymakers may tinker

¹ For example, see Brawley (2010, 3-4); Desch (1993, 9-12); Dueck (2006, 12); Grygiel (2006, x-xi); Ikenberry 2001, (3-4); Kitchen (2010, 136); Lobell (2003, 1-3); Miller (2010, 26-27); Narizny (2007, 17-18); Sampanis (2003, x); Trubowitz (1998).

with grand strategy after a shock, but they return to their old thinking because they perceive no good alternatives. Grand strategy is stable, in other words, because the conditions for change remain rare. While this theory offers a useful picture of why grand strategies remain broadly stable and may only change during epochal historical moments, whether and how lower-level strategic changes differ from higher order grand strategy transformation remains undertheorized.

I make two underlying arguments based upon a model advanced by Jeffrey Legro. First, overarching grand strategies—at what I will call the “dominant” level—remain stable for long periods partly because, without dramatically negative feedback, inertia carries forward the ideas, infrastructure, institutions, policies and other accoutrements of a grand strategy. This means that, as Legro asserts, dramatic or fundamental grand strategy change is difficult and rare.

Second, by contrast, changes *within* grand strategies—at what I will call the “tactical” level—remain relatively common because at this level, policy makers are making changes to the application of the dominant grand strategy rather than overturning its guiding parameters. This illustrates how the Carter and Bush grand strategies can be both similar and different in terms of key measures like content, aggressiveness, interests, and so forth.

In turn, while some of these tactical-level changes are considered successful and persist in policy, others are abandoned, sometimes by the very administrations that implemented them.

The key research question, therefore, involves Legro’s claim that elite perceptions of strategy failure—when policy makers come to believe that their new strategy is not delivering the kinds of outcomes they anticipated—explains why policymakers abandon an attempted grand strategy change. For instance, Legro argues that Bush’s attempt at strategic change was abandoned because outcomes diverged from expectations. Is this accurate? Other, possible explanations for abandoning a given grand strategy change include domestic electoral politics, bureaucratic

wrangling, parochial economic interests, and unexpected external events. In fact, I will argue that certain types of external and domestic feedback may matter as much or more to the decision for abandonment. For example, “foreign policy ideas,” as Legro puts it, are important in these cases because they set at least some of the parameters of possible change; however, the project’s findings suggest that such ideas are only part of the story. Further, I will argue that negative feedback is salient at the dominant level only when the power structure of the international system is perceived to be changing.

To summarize, this dissertation will compare the Carter and Bush administrations’ broadly similar approaches to grand strategy, and then it will focus in greater depth on the kinds of negative feedback that led both administrations to abandon ambitious changes *within* that overarching strategy. These issues are interrelated because, I argue, the Legro framework offers a useful model to understand both change and feedback, particularly when it is made more nuanced with the dominant/tactical bifurcation. While research on foreign policy and grand strategy change is extensive, no studies have compared these two administrations in terms of overarching stability, lower-order change and the effects of negative feedback.² While some models of grand strategy change include disaggregation into a hierarchical set of levels, the dominant/tactical model here is both unique and parsimonious.

Chapter Outline

The rest of this chapter reviews both the state of grand strategy research, including the concept of “grand strategy” itself, and the major schools of thought that apply the concept.

² These administrations have been compared to some extent on other issues. Berggren and Rae (2006), for instance, find that despite their personal and ideological differences, Carter and Bush were both displayed an “Evangelical Presidential style.”

It then details the two research puzzles that link this project to the larger fields of security studies, foreign policy and international relations: the problem of change and stability and the question of failed change. To answer these questions, the chapter then explains the dominant/tactical model as well as a framework for change, stability and feedback that is grounded in Legro's theory of foreign policy idea change. Finally, the chapter offers a methodological overview of the Carter and Bush cases as well as how they will be analyzed. The final pages summarize the project's relevance and outline the rest of the dissertation.

Grand Strategy: Current Research

The Concept

The notion of "grand strategy" typically involves any state with a plan to assess and apply specific means to achieve proscribed ends or national interests, especially security.³ Historians John Lewis Gaddis (1982, 88) and Paul Kennedy (1991, 5), for instance, have called grand strategies a means to promote a state's "long-term interests." Posen (1984, 13), however, offers perhaps the most-cited definition of grand strategy, calling it a "political-military means-ends chain, a state's theory about how it can best 'cause' security for itself." Clausewitz (1989, 605-609) is often credited with originating the concept by asserting that true military strategy inevitably extends beyond war, while later geographic thinkers like Alfred Thayer Mahan (1957) and Halford MacKinder (1904) described how powerful states might dominate the world with vast but carefully targeted geopolitical strategies.⁴ By the mid-twentieth century, influential strategist B.H. Liddell Hart (1954, 335–336 and 351) argued that "grand strategy" should

³ Luttwak (2009, 409) makes a similar case.

⁴ Spykman (1942) extended this logic to airpower. See Petersen (2011) for an updated application of these geopolitical theories.

coordinate and direct *all* resources of a nation, from the economic to the moral.⁵ Liddell Hart focused on winning wars to establish “a better state of peace,” but many grand strategists came to agree that truly effective strategy *governs the peace* and *prevents* conflict.⁶ More recently, along with a long tradition of identifying and advocating the most efficacious grand strategies for a given state, work on this topic in late 20th and early 21st centuries has also included cataloguing grand strategy types and even identifying historical grand strategies like those of ancient Rome.⁷ Thus, research on the nature of grand strategy falls into two interrelated categories: prescriptive and empirical. In terms of prescription, as major international actors began shaping the post-World War II peace, many grand strategists adopted a positivist stance: they argued that strategy was a kind of science that could be mastered and then deployed to serve leading powers.⁸ Impressed with disciplines like economics, grand strategists have contended that if policy makers can isolate the dynamics of strategy at all levels, they can advance the most effective grand strategy possible. Ultimately, writers on the subject typically seek to advance a given grand strategy they consider most efficacious to achieve national interests. This urge to isolate effective grand strategy and apply it in a contemporary setting persists, often under the umbrella field of “security studies.”⁹ Today, though, rather than advocating for a “scientific” grand strategy, scholars typically seek either to promulgate broad guiding principles for effective

⁵ Framing grand strategy as primarily military strategy did not end with Liddell Hart; see for example Williamson (1969) and Posen (1984).

⁶ See for example Brodie (1949, 468 and 478).

⁷ For grand strategy typologies, see Collins (1973, xx); Posen and Ross (1996/1997); Art (1998/1999). For historical grand strategies, see Luttwak (1976 and 2009); Joffe (1995).

⁸ Earle (1944, vii) introduces an early version of this approach, while Schelling (1960) set an early standard for rationally understanding war strategy. See also Osgood (1983) for a looser notion of “scientific” grand strategy.

⁹ Another subset of security studies, “strategic studies” maintains a more exclusive focus on “the role of military power” than most grand strategy research. Baylis and Wirtz (2010, 6). See also Gray (2006, 1-13).

strategy or to advocate grand strategy types appropriate for perceived national interests.¹⁰ This tradition unites the empirical and normative aspects of grand strategy. It focuses on identifying an immediate threat or troubling trajectory, such as a state's declining relative power, and prescribes a grand strategy along with a series of steps to address the problem.

In contrast, this project focuses exclusively on grand strategy as an empirical phenomenon—the observable nature and ideational underpinnings of grand strategy. Originally fueled by the Cold War focus on containment and nuclear weapons, empirical grand strategy scholars by the 1990s were wondering how both policymakers and existing paradigms would respond to shifting global balances of power.¹¹ Later, the George W. Bush administration's assertiveness raised intense discussion about how to explain when, why and how grand strategies form and change. This approach, however, diverges from the earlier “scientific” treatment of grand strategy. In that tradition, observers sought to isolate the precise conditions and variables that would, in proper arrangement, inevitably achieve security and other national interests. The more recent mode of grand strategy research, however, focuses on the grand strategies themselves: how are they constituted, how long do they last, from what sources do they arise, are they different from leading as opposed to middle powers, and so on. Scholars and policy makers might take answers to these questions and formulate grand strategy prescriptions, but the research agenda itself is focused on understanding grand strategies as a political and international phenomenon.

Schools of Thought

¹⁰ For principles of effective grand strategy, see especially Deibel (2007) and Luttwak (2001). For more recent examples of monographs prescribing grand strategies for the United States, see Art (2003); Bacevich (2002); Calleo (2009); Kupchan (2002); Layne (2006); Leffler and Legro (2008); Lieber (2005); Lobell (2003); Mead (2004).

¹¹ Chase et al. (1996, 33); Huntington (1993); Ikenberry (1998); Waltz (2000); Freedman (2003, 463). This push also includes efforts to revisit well-known cases of grand strategy formation and change. See, for example, Taliaferro, Ripsman and Lobell (2012) on the interwar period.

Today, major schools of thought with competing explanations for grand strategy change dominate empirical grand strategy research, especially among International Relations (IR) scholars. Skidmore, for instance, argues that there are two basic models regarding how states respond to international change. “Evolutionary” approaches emphasize rolling policy adjustments and are associated with structural realism, whereas liberal, “institutional” approaches emphasize domestic resistance to change and lags in adjustment.¹² None, though, offers a framework that adequately accounts for grand strategic stability, change and failure. The first of these, structural realism, tends to favor “strategic adaptation,” in which policies and strategies must regularly be adjusted to meet changing international circumstances to reflect a state’s relative material capabilities (Legro 2005, 44-45).¹³ Stephen Walt (1987, 2 – 5), for example, argues that states form defensive and balancing alliances that respond to systemic pressures.¹⁴ Other grand strategic types in this vein include “dominion” and “strategic engagement” (Art 1998-99, 79 and 101–103), but the overarching point here is that states’ grand strategies are constrained, possibly even dictated, by the international system.¹⁵ Works learning toward greater prescription by Charles Kupchan (2002, pp 1–3) and Christopher Layne (2006, 6–13) make arguments that, respectively, the United States should account for a shifting global distribution of power and the United States should adopt a grand strategy of “offshore balancing” rather than its current “extraterritorial hegemony.”¹⁶ Ironically, though, Kupchan (1994, p 16)

¹² Skidmore (1996, 4). In a similar vein, Dixon and Gaarder (1994, 187-191) set out structural realism, bureaucratic politics and elite change as the competing models of foreign policy change.

¹³ Related scholarship dealing directly and indirectly with grand strategy includes Brooks and Wohlforth (2008); Desch (1993); Jervis (1991); Mearsheimer (2001, 3); Kupchan (2002); Layne (2006); Miller (2010); Posen (1984, 220-237); Schweller (2006); Taliaferro (2004); Walt (1987); Yetiv (2008). Claims regarding external influences on the state, though, extend beyond structural realism. See for example Deutsch (1966).

¹⁴ See Mearsheimer (2001) for an offensive realist account of great power grand strategies.

¹⁵ See also Art (2003) and Posen (1996-1997, 5-53).

¹⁶ Other recent prescriptive texts penned by academics include Leiber (2005) and Shapiro (2007). Such works remain empirically grounded, but they explicitly advocate a given grand strategic track for the United States.

finds that strategic culture, once ensconced in popular and elite consciousness, can lead to “adjustment failure” as grand strategies persist despite major changes in the international system. This highlights a systematic contradiction in the realist work on grand strategy: it simultaneously explains how grand strategies *do* work as well as how they *should* work. In turn, despite Kupchan’s efforts, it insists that grand strategies can be unrelated to the ideological beliefs and strategic culture held by policymakers.¹⁷ In other words, readers are not certain whether grand strategies are structural outcomes or are chosen by free agents. Another issue is that structural realism makes few or no distinctions between levels of grand strategy. Containment, offshore balancing and bandwagoning might all be legitimate grand strategies, but do any of those fit within the others? When grand strategies change in the structural realist paradigm, does everything about the state’s foreign policy and security strategy transition? Liberals, by contrast, emphasize “parochial domestic interest” and assume that foreign policy and grand strategy changes are “a product of attempts by internal groups ... to hijack the state for their own purposes” (Legro 2005, 45).¹⁸ In other words, particularly in democracies, domestic interests seek state power in order to move policy to serve their own ends and then dictate the content of any given grand strategy.¹⁹ Narizny’s (2007, 17-18) theory of grand strategic formation, for instance, maintains that as domestic sectoral interests, domestic and international constraints, and party leaders change, grand strategies also inevitably change. However, tied to parochial interests, liberal theory lacks a broader view. With its focus on shifting economic and political interests, liberalism assumes, but struggles to explain, overarching, stable motifs in

¹⁷ Johnston (1995, 61-68) argues that Ming Chinese grand strategy is best understood not as structural realism but as “cultural realism,” or a set of beliefs that dictate realist behavior. This move, however, undermines the basic materialist premise of structural realism.

¹⁸ Trubowitz (2011, 2) calls these approaches *Real-* and *Innenpolitik*. Representative texts include Gourevich (1986); Narizny (2007); Snyder (1991); Trubowitz (1998).

¹⁹ For example, see Bacevich (2002); Ikenberry (2001); Krasner (1978); Lobell (2007); Narizny (2007); Rosecrance and Stein (1993); Welch (2005); Zakaria (1998).

foreign policy and grand strategy. For example, US grand strategy throughout much of the 19th century proved relatively stable, but from a liberal perspective, it remained volatile as administrations and policies constantly shifted in a kaleidoscope of interests. In addition, under this framework, if a new strategy is implemented but abandoned relatively soon, the most relevant variables are tied to domestic interests while individual and systemic level explanations are marginalized.

Neoclassical realists, meanwhile, seek to bridge the domestic/structural divide by insisting that grand strategy change arises from the interplay between domestic, exogenous and, sometimes, cultural factors.²⁰ While they maintain that structural realists overemphasize the rational and systemic aspects of foreign policies and grand strategies, they reject liberalism by asserting that the state is “relatively autonomous from societal actors” (Rose 1998, 144-147) as well as a mediator between domestic and systemic pressures.²¹ Furthermore, neoclassical realists incorporate strategic culture and ideas into their theories (Glenn 2009, 531). Dueck (2006, 11), for instance, argues that while material calculations drive the formation of grand strategy, strategic culture determines the specific policy options available to decision makers.²² Despite this flexibility, neoclassical realists focus on grand strategic change at the expense of accounting for stability, make no distinctions between degrees or levels of change, and lack explanations for grand strategy failure. For example, neoclassical realism offers no way to identify and explain overlapping grand strategies. Thus, when Brawley makes a case for domestic and international factors interacting to shape US grand strategy early in the Cold War, his account offers no

²⁰ See, for example, Rose (1998, 144-147); Glenn (2009, 531); Brawley (2010, 140). Mor (2006) looks at the role of public diplomacy in grand strategic success.

²¹ See also Brawley 2010, 140.

²² For similar approaches, see Christensen (1996, 3-16); Johnston (1995, 1-3); Lobell (2003, 1-3). In a similar vein, Kitchen (2010, 132-135) posits that ideas act as intervening variables between system pressures and elite decision-making; thus, grand strategies represent a combination of realist-style reactivity to material forces and constructivist-style perceptions.

insight into whether and how later iterations of containment, such as New Look, Flexible Response and Détente, may have fit within or contradicted that strategy. Finally, neoclassical realism lacks clear mechanisms to prioritize or evaluate multiple inputs, such as veto players or public opinion, or systematically parse overarching stability from lower-order change.²³

A final school of thought denies that grand strategy even exists. These skeptics charge that hindsight leads observers to falsely perceive grand strategy where none exists. In other words, grand strategy is imagined after the fact. Rather than pursuing sweeping plans and overarching strategies, states merely respond to immediate challenges as they arise, what Yetiv (2008, 11-12) calls “reactive engagement,” while bureaucratic inertia and institutional parochialism define foreign policy decision-making.²⁴ Here, what matters is that individuals are making foreign policy decisions, and these individuals are tied to institutional and political interests. They can only react to events and challenges as they arise. Devising, implementing and following a long-term strategy is chimerical. Though powerful critiques, these positions fail to explain cases of strategic consistence across decades. For example, British foreign policy in the 19th century was beset by political divisions and contradictory decisions, yet for decades it remained consistently focused on a core set of ideological and strategic objectives.²⁵ Ultimately, even if policy makers are not thinking explicitly about pursuing a grand strategy, they often appear to act as if such a strategy existed – and for researchers, this is sufficient to propose and test empirical theories (Dueck 2006, 11).

Ideas or Practices?

²³ Even Dueck’s account (2006, 18-20), which distinguishes between grand strategy type change and lower-level “strategic adjustment,” emphasizes strategy selection rather than explaining simultaneous stability and change.

²⁴ On the latter point, Majeski and Sylvan (2010, 2-3) insist that US foreign policy is “means- and not ends-driven.” More generally, this approach extends back to the bureaucratic politics model and the work of Graham Allison (Allison and Zelikow 1999) and others (Halperin 1975).

²⁵ See, for example, Bell (2007); Chamberlain (1988); Semmel (1986)

In turn, whether to define grand strategy as an idea or a practice also remains contentious. Fundamentally, some scholars argue, strategies are plans, not actions. Luttwak (2001, 207) in particular emphasizes that policymakers *first think* properly about, and *then apply*, effective grand strategy.²⁶ On the other hand, this approach raises familiar analytical problems: how can empirical scholars rigorously study an idea and its causal effects? Even in studying strategic culture, Dueck (2006, 12) maintains that material strategic outcomes such as military spending, alliance commitments, foreign aid, diplomatic initiatives and policy positions represent useful variables to assess a state's grand strategy.²⁷ Partly for this reason, strategists of all types tend to identify their target as the method or bridge between available means and a strategy's proscribed ends.²⁸

Consequently, grand strategies are best understood as *both* intangible plans—ideas—and patterns of behavior—practice. From this perspective, as an ideal type, grand strategy is formulated by one person or a small group of policy elites in an effort to achieve perceived state interests. After this formulation, the grand strategy is consolidated as policy and becomes a pattern of action. Envisioned as a Venn diagram, grand strategy is the space where ideas, plans and intentions overlap with policies, decisions and actions. Thus, this project treats both stated strategy beliefs and actual strategy choices as equally relevant for understanding grand strategy in each case study.

The Legro Model

²⁶ See also Foster (1985, 14).

²⁷ Other attempts at operationalizing grand strategy include Art (2003, 2-7); Christensen (1996, 13); Collins (1973, 1-7); Flint (2006, 55-56); Foster (1985, 18); Miller (2010, 36); Murdock and Kallmyer (2011, 550-552); Walt (1987, 17-25).

²⁸ In a revealing summary of prominent historical and recent writers, Baylis, Wirtz and Gray (2010, 5) show that most strategists understand their work as an “essentially pragmatic and practical activity,” or, as Bernard Brodie explained elsewhere, they are generating a “theory of action.”

To address the analytical gaps of the schools of thought described above as well as implement this definition of grand strategy, I adopt Legro's version of foreign policy change: it offers a model that is parsimonious and accounts for long-term stability, and it defines its target in both ideational and concrete ways. Specifically, Legro (2005, 4) develops a notion of "collective ideas," which he defines as shared conceptions among policymakers and citizens about how domestic and international order can best serve their state's interests.²⁹ In turn, Legro argues that ideational and institutional inertia mean that states only change their collective ideas about international politics when 1) a shock demonstrates clearly that the previous idea was a failure and 2) a dominant new idea has a clear advocate and proves effective when first applied.³⁰ Specifically, in the first stage, an international shock sparks collapse in the reigning orthodoxy, which has come to be seen as illegitimate, and "widespread agitation" arises to replace the failed strategy (Legro 2005, 4).

Collapse, however, does not ensure that a new strategy will arise. Thus, in the second stage, whether "consolidation" around a new change occurs depends on 1) the number of possible replacement ideas available (one is best), and 2) the perceived initial results (positive) of a new idea's implementation.³¹ For instance, Legro emphasizes that US grand strategy in the 1940s, rather than evolving through a series of incremental changes, shifted dramatically from standing aloof in the international system to active internationalism.³² Furthermore, this change was

²⁹ Here, according to Legro, ideas are "not so much mental as symbolic and organizational" and can be observed as embedded in "government procedures, education systems, rhetoric and statecraft."

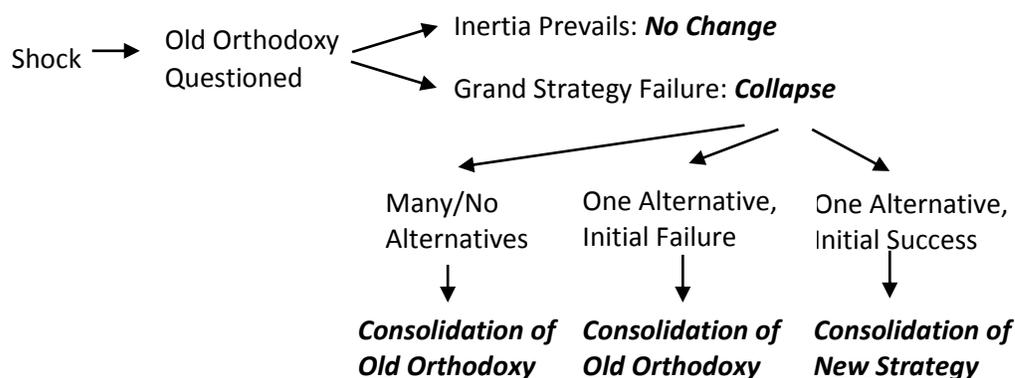
³⁰ Welch (2005, 8) makes a similar case by arguing that major foreign policy change is likely only when a leader "expect[s] the status quo to generate continued painful losses."

³¹ Here, three outcomes are possible: 1) The state is unlikely to consider change, no matter the strategy's perceived efficacy, if there are either *no* or *many* alternative strategies. 2) The state may revert to the previous strategy, which at least worked at some point, if there is one prominent alternative but its perceived efficacy is low. 3) The state will likely adopt a new idea if there is one prominent alternative that is associated with early positive outcomes (Legro 2005, 36 – 37).

³² In contrast, Dueck (2006, 82-108) argues that grand strategies change by degrees as policymakers constantly tinker with policies.

epochal: there has been no similar change for Americans before or since that period. In other words, according to Legro, grand strategy remains stable for decades—even generations—until a specific set of conditions leads to a collapse of the dominant ideas and a consolidation around new ideas. This approach offers an account for why grand strategies appear stable (the conditions for change occur infrequently), and it even posits explanations for when and why a newly implemented change might be abandoned (early, negative feedback suggests a gap between goals and reality). What it does not offer is an account of what happens between major changes or a division of grand strategy between overarching foreign policy ideas and lower-level, pragmatic implementation of those ideas.

Figure 1.1: *Legro's Theory of Foreign Policy Idea Change (adapted from Legro 2005, 14).*



The Research Questions

The Problem of Change and Stability

Overall, current research on grand strategy offers important insights into the variables that shape grand strategy formation and change, but it has largely failed to offer a systematic framework that distinguishes overarching strategic stability from lower order change; a framework that also identifies when and why grand strategy changes might fail; and, substantively, it offers little

comparative research on the Carter and Bush administrations, in which both of these questions are relevant. Given these shortcomings, the following sections detail the research questions that animate this project.

Little consensus exists regarding how easily and frequently grand strategies change. If they prove stable, then they can help analysts anticipate how a state will respond to changing international and domestic conditions. However, if grand strategies regularly or constantly change—if they are highly pliable—prediction remains limited because they may be as changeable as tomorrow’s headlines. On the one hand, policy makers rarely seem to change the fundamentals of a state’s overarching grand strategy. Historical examples include Roman (Kagan 2006), Russian (LeDonne 2004) and Spanish (Parker 1998) imperial strategies. Consider also, as mentioned above, British foreign policy throughout the middle nineteenth century, which involved a core set of ideological and strategic objectives justifying global trade and naval superiority.³³ In political science, Tsebelis (2002, 2-3), Welch (2005, 31-46), Schweller (2006, 11-12) and others offer various arguments that domestic barriers make changing strategy difficult.³⁴ Long cycle theories, meanwhile, hold that extended economic and military trends drive extended grand strategy lifecycles.³⁵ Many scholars of US foreign policy find that since World War II, strategies such as containment have remained embedded in a stable

³³ See, for example, Bell (2007); Chamberlain (1988); Semmel (1986, xi and 11).

³⁴ Strategic culture research typically finds similar results. See, for example Johnston (1995, 1-3); Jepperson, Wendt and Katzenstein (1996, 33-36). Not dissimilarly, Flint’s “geopolitical code” (2006, 55-56) may crop up quickly but can persist for decades. In a study of US-Soviet relations, Dixon and Gaarder (1994, 199-200) found that bureaucratic inertia constrained change and that US behavior toward the Soviet Union remained “remarkably unaffected” by changing administrations.

³⁵ See, for example, Doran (1991, 64-65); Modelski and Thompson (1988); Thompson (2006, 11-15). However, as discussed in Chapter 7, grand strategy remains a promising but underdeveloped topic in long cycle research.

internationalist US strategic framework, while realists such as Layne also observe a long-lived, if less liberal, postwar grand strategy.³⁶

On the other hand, research also suggests that foreign policies prove endlessly contested, which makes the notion of a stable grand strategy difficult to accept.³⁷ Again, British foreign policy proves instructive: those same nineteenth century decades of presumed stability also oscillated between conservative and liberal governments that held sometimes contradictory foreign policy positions. America's Cold War experience reveals a similar chain of competing strategic policies, while post-Cold War research offers no consensus regarding whether and how US grand strategy may have changed since 1991.³⁸ Not surprisingly, such research (Narizny 2007, 17-18; Dueck 2006, 12) suggests that grand strategies can change frequently, sometimes with each new administration or even within a single administration.

Why "Failed" Change?

This puzzle is intimately connected with the question of why a strategy change is short-lived. They are two sides of the same coin: presumably the same dynamics are in place, yet under certain conditions, a change persists while in others it is abandoned. According to the Legro model, change is rare and happens at the broadest levels of foreign policy, but attempted changes may occur more frequently. Foreign policy ideas and grand strategy can remain stable for decades—even generations—until a specific set of conditions leads to a collapse of the dominant ideas and a consolidation around new ideas. This is “the puzzling power of the status quo”

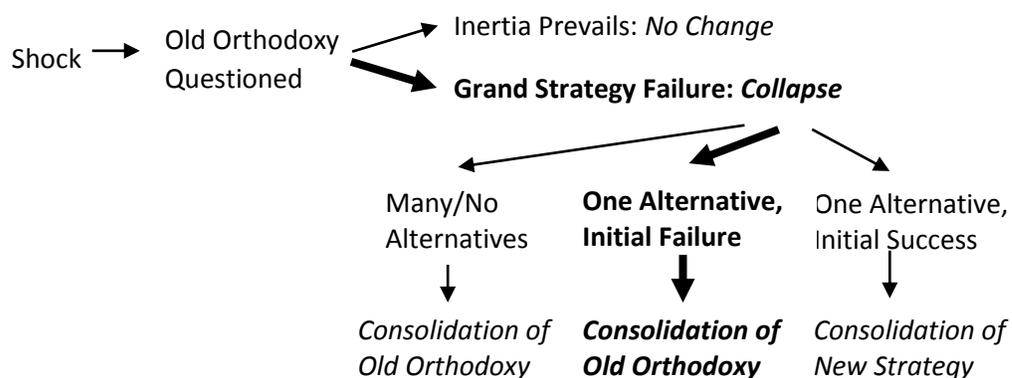
³⁶ Bacevich 2002, 4-6. A few liberal and other IR scholars view US strategy since World War II as broadly consistent and intertwined with democracy promotion, open trade and international institutions. See for example Ikenberry 2001, 255-256; Lake 1999, 193-201. Layne (2006, 6-15) focuses on “extraregional hegemony.”

³⁷ These arguments typically dovetail with the claim that grand strategy does not exist: all decisions are reactive and tied to bureaucratic commitments. For examples, see Betts (2000, 6-7); and Yetiv (2008, 11-12).

³⁸ Examples include extraregional hegemony (Layne 2006, 11); defensive and offensive liberalism (Miller 2010, 36); New World Order (Drezner 2007); selective engagement (Art 1998, 79-83); primacy (Posen 2003, 6); global revisionism (Haley 2004, 463); Bush Doctrine (Legro 2009, 52); and “no true grand strategy” (Brimley et al. 2007, 9).

(Legro 2009, 53). By contrast, some grand strategies are abandoned when they are implemented and policymakers interpret early feedback as negative. In Woodrow Wilson's attempt to internationalize US foreign policy after World War I, Legro finds that Wilson failed because, first, Americans grew disillusioned with their involvement in Europe and, second, internationalists offered too many competing visions for US foreign policy. (Legro 2005, 50) Elsewhere, Legro (2009, 55) argues that George W. Bush's attempted change represents an outcome similar to the one after World War I: the administration sought to shift US strategy from an emphasis on consensus and economic incentives toward an assertive stance grounded in military power. In the end, though, with mounting blood, treasure and political losses in Iraq and Afghanistan, that new policy was discredited by its own outcomes.

Figure 1.2: Legro's path of attempted but failed change. This occurs when policymakers perceive an old strategy to have failed. A single, prominent alternative is advanced and implemented as a change, but initial feedback seems negative and the change is abandoned in favor of consolidation around the old strategy.



For many other approaches, the causes of such failure must be inferred because all changes, whether of a long-standing strategy or a new innovation, are treated as fundamentally similar. Structural and neoclassical realist theory holds that policy makers will choose strategic adjustment when a change appears to be misaligned with a state's own relative power capabilities. Unfortunately, this approach suffers from the problems of oversimplification and predictive/prescriptive confusion discussed above.³⁹ By contrast, in a study of the Carter administration, Skidmore (1996, xviii-xix) contravenes realist theory by arguing that, rather than experiencing flexibility, hegemonic states like the United States can resist strategic adjustment for long periods; however, as they do, domestic interests and institutions rigidify and resist change.⁴⁰ Indeed, the primary domestic explanation for failure of a new grand strategy involves rejection or weak support on the part of some key player or constituency.⁴¹ In a broadly cast analysis, Jacobs and Page (2005, 121) find that business interests in fact shape officials' positions more than any other group's prerogatives; however, they also conclude that most research on this topic remains blinkered and needs to move from monocausal to multi-causal explanations. This reinforces the basic question regarding abandonment or failure of a new strategy: what is the single most important source of feedback? Answers to this question have depended upon the researcher's paradigmatic perspective or methodological focus. Far less research has evaluated cases of such "failure" as a distinct phenomenon. Is Legro's account of

³⁹ Doran's power cycle thesis (1991, 60-69 and 89) offers one of the more sophisticated arguments for how policy makers are both constrained by relative power yet also able to choose a new strategic direction. Even here, though, agency is uncertain and the "veto power" of domestic variables remains underdeveloped.

⁴⁰ In a competing explanation, Rosati (1993, 470) ascribes the change to a unique suite of factors, such as resistance to radical change from the national security bureaucracy, Carter's naiveté and bad luck, and the rise of a more assertive conservative coalition.

⁴¹ Along with grand strategy research from the liberal perspective, there exists an extensive body of work in this vein on US foreign policy. Institutionalists, for instance, trace specific policy outcomes to officials bargaining for political support with powerful interest groups (Keohane 1984; Gourevitch 1986; Frieden 1991), while median voter theory as well as public opinion research suggests that officials will mold their foreign policy positions to maximize electoral support (Downs 1957; Holsti 1996; Sobel 2001; Wittkopf 1990).

stability, change and (occasionally) failure the most effective? How can observers explain or characterize the apparent commingling of stability and change over time?

Answering the Questions

Two-Level Approach: Dominant and Tactical Level Grand Strategy

The following sections offer two interrelated frameworks to answer these questions, and the following chapters set about applying and testing these frameworks. To begin, this project proposes that grand strategy is best understood as a two-level phenomenon. This allows researchers to more precisely understand how grand strategies can experience both regular adjustment and long-term stability.⁴² Specifically, in the cases here, I maintain that US policymakers in the post-World War II era regularly sought to change grand strategy tactics without fundamentally questioning the dominant “internationalist” (as Legro defines it) grand strategy. In turn, though major periods of change and “failure” may appear to represent grand strategy revolutions or type changes from one grand strategy to a fundamentally new one, such as during the Carter and Bush administrations, they in fact reflect lower or tactical-level dynamics. Stated differently, most grand strategy changes, whether long-lived or abortive, occur at the tactical level.

Dominant Level

According to this bifurcated distinction, grand strategy’s *dominant parameters* are the higher order elements of grand strategy: they are *a stable set of guiding principles* that prescribe 1) the nature of state interests (and threats to those interests), 2) the type of international system or

⁴² This notion is based upon a hierarchy developed by Deibel (2007, 9-21). The least expansive strategies, says Deibel, are (1) tactical *military* strategies, while higher levels of strategizing include (2) waging war with all the tools available to the state, (3) developing a “national security strategy” that focuses on security but extends beyond war, (4) a “foreign affairs strategy” that unites all ambitions related to foreign policy and, finally, (5) “national strategy,” which unites domestic and foreign ambitions. See also Biddle 2007, 461-463 for an alternative hierarchy; Dueck (2006, 12) for an account of first- and second-order change.

world order as well as the menu of acceptable methods to best secure those interests. These are the ends of grand strategy. Rather than organizing military assets alone, dominant grand strategy involves “the coordinated use of all the instruments of state power to pursue objectives that protect and promote the national interest,” and, in effect, it can be seen as a kind of operational code functioning at the state level.⁴³ The United States after World War II, this paper will argue, offers a famous example. By the mid-1940s, US policymakers believed that their security and prosperity relied upon a stable, free-trade international system, a system composed, ideally, of fellow liberal democracies. In turn, the menu of acceptable methods to pursue this grand strategy included dominant military power and interventionism but not occupation; multilateral institutions in which the United States could exempt itself; open and integrated trade; and global financial solvency underwritten by, but also benefitting, the United States.

Table 1.1: *Distinguishing between dominant grand strategy and grand strategy tactics.*

Grand Strategy	
<u>Dominant Parameters</u>	<u>Tactics</u>
<p><i>A stable set of guiding principles.</i></p> <ul style="list-style-type: none"> • Nature of state interests • International order, ideological parameters and accepted means to best secure those interests 	<p><i>Active plans to secure state interests.</i></p> <ul style="list-style-type: none"> • Foreign policies • Security strategies • Mid-range goals • Responsive actions, statements, interventions and agreements.

⁴³ Adapted from Deibel (2007, 10). For the early standard on operational codes, see George (1969, 190). Flint’s “geopolitical code” (2006, 55-65) is even more similar to dominant level grand strategy as defined here, though Flint’s model is not specifically designed for grand strategy.

Tactical Level

The dominant level, then, defines both the grand strategy's ends as well as the parameters within which the grand strategy shall be contained, but the particular policies, behaviors and institutions policy makers pursue from year to year and from one administration to the next are worked out at the second level of grand strategy. In other words, tactical level grand strategy involves *active plans to secure state interests*.⁴⁴ These are the means of grand strategy. Grand strategy tactics range widely from sanctions and multilateral diplomacy to deterrence and containment. While containment pushes the limits of what observers might consider a traditional "tactic," it reveals how a major aspect of US grand strategy might change or disappear and yet the overarching strategy can remain stable. As a strategy, Cold War containment sought to constrain Soviet expansion through confrontations in peripheral states and global strategic alliances. However, from a broader perspective, containment remained only a means to support America's dominant-level ends: a physically secure US at the political center of a set of democratic, free market states. When containment ended with the Cold War, the Clinton administration sought to redefine US grand strategy tactics by prioritizing humanitarian missions and economic globalism; however, a key argument in following chapters is that America's dominant internationalist grand strategy persisted.

Most treatments of grand strategy either conflate possible levels into a single one or, at the other extreme, disaggregate strategy into a larger, unwieldy hierarchy.⁴⁵ Legro's model falls into the former category by effectively describing attempts at dominant level grand strategy change.

Thus, for instance, Legro (2009, 56) argues that the Bush Doctrine was a failed attempt at

⁴⁴ This notion resembles Posen's (1984, 13) "political-military means-ends chain, a state's theory about how it can best 'cause' security for itself."

⁴⁵ In an encyclopedic effort to define the practice of strategy, for instance, Gray (2010, 82 and 238) identifies "eight sets of binary distinctions," and 21 "dicta."

overarching strategic change that ended in “adjustment.” Unable to make distinctions between degrees or levels of change, Legro *must* characterize the Bush Doctrine as an attempt at dramatically breaking with the past. However, when the dominant/tactical distinction is applied, Legro’s model proves flexible enough to capture change at the tactical level. Observers might argue, under this framework, that while the Bush administration mouthed rhetoric of fundamental change, it never actually abandoned the core organizing principles undergirding US grand strategy since the 1940s. Further, these major attempts at change may have failed *because* they parted too dramatically from the dominant strategy. In other words, the dominant/tactical distinction can enrich existing theories of change by adding greater precision without falling into the trap of excessive detail.

A Framework for Change, Stability and Feedback

Implied in this two-level distinction is a testable framework about the frequency and parameters of grand strategy change. Specifically, it predicts that dominant-level transformations from one grand strategy type to another will be rare, whereas tactical adjustments within a dominant grand strategy are likely to prove much more common.⁴⁶ Stated differently, tactical grand strategic adjustments are more likely than fundamental grand strategy transformations. Specifically, tactical level changes will be constrained: they will not exceed the parameters set at the dominant level. Nevertheless, this framework makes few claims about *why* grand strategies change, so the dominant/tactical framework functions most effectively in conjunction with an outside causal theory of grand strategy change. The following sections, therefore, integrate the dominant/tactical framework with Legro’s theory regarding the causes of grand strategy change.

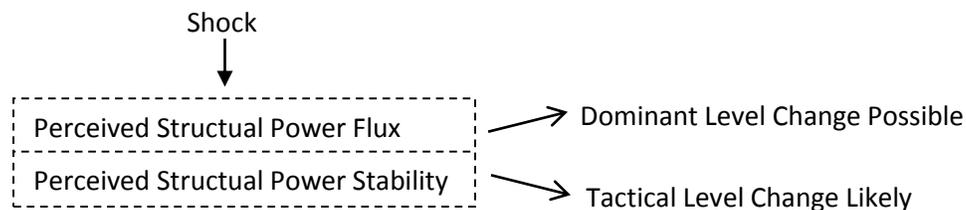
⁴⁶ This approach is not unlike the melding of *longue durée* and punctuated equilibrium described by Spruyt (1994, 22-25). In that formulation, basic institutional arrangements remain stable for long periods even as events, individuals, capabilities and other ephemera change within that structure. “Only when dominant coalitions change,” he explains, “Or interests and perceptions shift, will there be an opportunity for institutional transformation.”

Legro's theory of change in fact fits well with the dominant/tactical model. It describes a process of collapse, trial and reconsolidation (or rejection) that can transpire at both levels. Whereas Legro's model implicitly focuses on dominant-level change, this project argues that international systemic conditions will determine whether a change is likely to occur at the dominant or at the tactical level. Once change begins at one level or another, though, the process of change broadly follows Legro's model. To reiterate: the dominant/tactical theory predicts that change is constrained. Without favorable international conditions, grand strategy change is likely to occur at the tactical level. In turn, that tactical level change is likely to be constrained by parameters set at the dominant level. The following sections unpack this formulation. First, the dominant/tactical framework predicts that any given grand strategy change is more likely to be at the tactical than the dominant level. That stability depends on one variable: perceived relative geopolitical change. If a state's geopolitical situation (especially relative power) remains relatively stable, overall inertia will prevail even as the state responds to ongoing foreign policy and geostrategic fluctuations. If that same state's geopolitical situation is changing, a shock can initiate fundamental change. In other words, dominant grand strategy transformation remains unlikely unless a shock is combined with policy makers perceiving an underlying geopolitical shift.⁴⁷ Along these lines, in addition to World War II and the United States, Legro studies Germany's shift "from outsider to insider," Japan's Tokugawa/Meiji revolution, and the Soviet Union's perestroika. In each of those cases, the state's geopolitical position relative to the rest of the system was actually or believed to be in flux. To take the British example, the state's relative status as the lead naval and trading state remained stable throughout the nineteenth century, and its grand strategy remained relatively consistent through

⁴⁷ Doran (1991, 59-69) advances a similar theory.

that period. By contrast, relative decline in the twentieth century set the backdrop for fundamental changes—namely, retrenchment—in Britain’s grand strategy.

Figure 1.3: *Perceived international conditions help determine whether policy makers will pursue tactical- or dominant-level change. Legro’s theory essentially explains dominant-level change during periods of perceived change in the international system’s power structure, while the author’s framework seeks to add tactical-level changes, which are most the most common and likely during periods of perceived geopolitical stability.*



Tactical level changes, however, are more likely and occur in the presence of relative geopolitical stability. Confronted with a gap between existing strategy and new realities, policymakers move to adjust grand strategy after unexpected domestic or exogenous events, such as economic downturns, strategic surprises and—especially when ruling coalitions change—domestic elections. Whereas dominant-level change requires redefining the fundamental framework of state goals, tactical level change involves the (relatively) simpler task of seeking out new or adjusted means to pursue those goals. Faced with a gap between existing strategy and new realities, policymakers move to change grand strategy, but they are constrained by parameters set at the dominant level. Whereas dominant level change requires redefining the fundamental framework of state goals, changing grand strategy tactics involves the (relatively) simpler task of seeking out new or adjusted means to pursue those goals.⁴⁸ Furthermore, whereas dominant level transformation involves dramatic switching, such as from isolationism to

⁴⁸ Notably, some accepted strategies and approaches may persist for decades even as they waver in and out of favor or are periodically updated to address new conditions. Cullather’s research on modernization theory in US foreign policy, for instance, demonstrates how mid-twentieth century intelligence (2006, 143-144) and development (2002, 513-515) policies have experienced new life since the end of the Cold War.

internationalism, tactical level changes can range from introducing a major new strategy, like containment, to tinkering with aspects of standing strategy, like Obama's move to exit Iraq while instituting a "surge" in Afghanistan.

Finally, some of these tactical-level changes may "fail" in the sense that they are implemented but then abandoned or marginalized. As indicated above, Legro sets out a path for this type of failure, but as it stands, his model must treat all grand strategy changes as occurring at the same (dominant) level. Recognizing that any given strategy change is likely to be occurring at the tactical level offers a greater degree of precision. Specifically, Legro predicts that policy makers will abandon a new change if they rationally assess that strategy's performance and decide that it is not delivering promised outcomes.⁴⁹ Evaluating this process from a tactical-level perspective helps explain why policy makers might be able to abandon their own strategic change and yet maintain a relatively coherent grand strategy as opposed to leaving the state strategically rudderless. In the earlier case of the Bush administration, for example, marginalization of the Bush Doctrine and Freedom Agenda is not a curious case of aborted strategic revolution but the failure of a tactical-level change in response to a serious terrorist threat rather than systemic-level competition with peer competitors.

In sum, integrating the dominant/tactical distinction with Legro's two-stage model of change generates a more precise but equally parsimonious theory. The theory holds that domestic and exogenous shocks may initiate grand strategy change; however, whether that change constitutes a dominant- or tactical-level change depends upon whether policy makers perceive the power

⁴⁹ The term "rational" is the author's interpretation of Legro's framework. Notably, Legro does not engage the literature on rational choice; rather, his model resembles Gartner's (1993, 367-368) account of change during the Carter administration. In that account, policy makers follow "dominant indicators" and are "likely to act when they think that their current situation is becoming increasingly worse—going in the wrong direction at an accelerating pace." This effect is amplified if decision makers enjoy relative consensus regarding their foreign policy beliefs.

structure of the international system as stable or in transition. If they believe that their own state's relative power is fundamentally ascending or declining relative to peer competitors, then they are likely to initiate dominant-level change along the path described by Legro. However, if they perceive relative stability for their own state's position, they will still pursue tactical-level change that also follows the paths modeled by Legro. Stated differently, shocks open political space for change, but without serious exogenous pressure, the inertia of dominant level grand strategy beliefs and policies will define the parameters of that change. In some cases, such as those analyzed in this project, that tactical level change is abandoned, and Legro argues that this happens when policy makers observe a gap between what they expected the strategy would deliver and what they consider to be the strategy's actual performance. Thus, (dominant) grand strategies may remain stable for decades even as they also experience constant (tactical) change and adjustment as policy makers pursue their own initiatives and respond to unexpected events.

Testing and Analysis

The goals of this project are twofold: 1) to compare and analyze the Carter and Bush administrations in terms of grand strategic stability, feedback and change, and 2) to test the claim that the dominant/tactical framework, in conjunction with Legro's theory, is an effective tool to compare those cases and, more generally, better understand all grand strategies. This will involve testing the claims about dominant-level stability advanced above and then testing the theory's claims about which type of feedback, at the tactical level, leads to the "failure" of a new strategy. In other words, the proposed framework makes broad predictions about grand strategic change and stability, and some of these key predictions will be tested in the context of the Carter and Bush administrations.

Assessing Dominant Stability

First, the project must show that both the Carter and Bush cases represent attempted shifts *within* a long-lived dominant grand strategy and not just a series of distinct transformations among different grand strategies. Demonstrating this will verify the claim that at the dominant level all strategic changes since World War II, when Legro argues the switch to “American internationalism” settled into permanence, have occurred within a single, dominant grand strategy. It will also suggest that despite their differences, the Carter and Bush administrations operated within fundamentally similar strategic parameters (Legro 2005, 68-71). To compare this 1940s baseline with later grand strategy in the case studies, I will assess decision makers’ statements and decisions according to three dimensions: *scope*, *substance* and *orientation*. Respectively, these dimensions describe a grand strategy’s geopolitical extent, ideological and political content, and physical implementation.⁵⁰ They are described in greater detail in Chapter 2. This allows the researcher to operationalize dominant grand strategy and systematically compare grand strategy between two points in time. Scope, substance and orientation are *not* causal variables related to when and why grand strategies change. Rather, they are characteristics (not unlike height, weight and hair color among humans) used to identify specific grand strategies.

In effect, I predict that all three cases will demonstrate similar codings on scope, substance and orientation. If values on those three dimensions remain relatively stable in comparison to the baseline and each other, then the assumption of dominant level stability is more likely to be correct. However, the cases should also show movement *within* those dimensions as policy makers attempted to adjust to new conditions and implement a new, lower-order strategies.

⁵⁰ Unfortunately, no scholarly consensus exists on how to characterize and assess grand strategy types and type changes. Typically, researchers will tailor specified characteristics to fit their particular studies. The dimensions above represent the author’s best assessment of the most expansive but still discrete characteristics used throughout the relevant literature.

Alternatively, the more one or all of scope, substance and orientation diverge from the 1940s baseline, the more likely it is that grand strategy has undergone a dominant level change between those two cases. Such findings would undermine the argument that American grand strategy has remained basically stable since World War II. This approach is detailed further and applied in Chapter 2.

Comparing Alternate Explanations for Failed Change

The fundamental question, and the bulk of the research, driving this project is what exactly happens between adoption of a grand strategic adjustment and policymakers later abandoning it? The tactical-level interpretation of Legro's theory is one explanation, but other theories of foreign policy feedback offer alternative sources of rejection. To test these multiple explanations, the project applies a congruence method in which the researcher determines whether values on the independent and dependent variables align with a theory's predictions (George and Bennett 2005, 181-184). Under this congruence method, each case study will focus on the period between implementation of a grand strategy change and the abandonment of that strategic effort. If the Legro-style theory is correct that basically rational interpretations of failure lead policymakers to abandon a change, the observer should find high levels of perceived failure among policymakers. On the other hand, other types of initial feedback, compiled and then synthesized from a wide sampling of the literature on grand strategy feedback and change, include *shifting external conditions*; *parochial economic interests*; *veto players*; and *public opinion*.⁵¹ Overall, then, this project will seek to determine which source of initial feedback best

⁵¹ For example, Gourevitch (1978, 911) argues that international and domestic politics are so interrelated that they should be analyzed together as a single whole. Tellingly, this approach identifies two general sources of political feedback: the international system (in the form of state constraints, economic interests, and so forth) and domestic structures (particularly whether the state is "strong" or "weak"). Realist, liberal and neoclassical realist approaches, as mentioned above, also tend to divide along these lines. Public opinion, though, remains the least certain influence because "presidents sometimes lead, sometimes follow, and sometimes ignore the American public" (Knecht 2010, 3). See also Wittkopf (1990, 6-11).

accounts for policymakers abandoning a relatively recent, tactical-level grand strategy change. Rather than privileging one set of expectations or variables by testing for only those proscribed by Lego or any other scholar, this project pursues an inductive approach: I seek to assess the variables in each case without privileging one over the others (George and Bennett 2005, 45). Chapter 3 details this case study method and framework for analysis.

Cases

The first case study involves Jimmy Carter's attempt to remake US foreign policy and the priorities of US grand strategy. As a candidate and early in his term, Carter articulated a "systematic and far reaching" foreign policy response to the realism of the Nixon-Kissinger years as well as perceived failures such as Vietnam and OPEC's 1973-1974 oil embargo (Skidmore 1996; Jones 2008, 455-457).⁵² In his inaugural address, for instance, Carter (1977) downplayed arms racing with the Soviet Union and appealed to America's "moral sense" in order to support liberal states, deepen third world cooperation and pressure both leftist and rightist regimes that violated human rights. Carter also drew lessons from his experience on the Trilateral Commission and sought consensus with Japan and western European states on global economic and political issues, such as in the Middle East (LaFeber 1989, 648-652). Successes like the Panama Canal Treaty and the Camp David Accords embody Carter's vision of engaging the global south and emphasizing America's liberal foundations in order to ensure security and other national interests. Nevertheless, by the end of his term, Carter had shifted toward a more *realpolitik* approach in the face of militarized disputes in Africa and Latin America, the Iranian revolution and hostage crisis, a Soviet strategic build-up and the Soviet invasion of

⁵² Rosati (1993, 459) even calls it "the first post-cold war foreign policy."

Afghanistan.⁵³ In 1980, Carter's early liberalism gave way to policies like the "Carter doctrine," which declared that "an attempt by any outside power to gain control of the Persian Gulf region [would] be repelled by any means necessary, including military force (Carter 1980)."

Carter's attempt to shift grand strategy—at the tactical level, as this project will demonstrate—was abandoned. Specifically, while the administration's interpretation of overarching US interests remained consistent with earlier administrations, it sought to achieve those interests through a focus on interdependence and human rights as well as a minimization of the Cold War conflict. What type of feedback undermined this effort? The Legro-style theory suggests that an early, relatively rational calculation of failed outcomes led to the strategy's rejection and a return either to a previous grand strategy or, perhaps, a return to the search for a new strategy. By contrast, conventional wisdom typically faults Carter for proving too inexperienced, too idealistic, and too confused about priorities; meanwhile, more theoretically-grounded explanations credit the failure to domestic politics or system level realities (Mead 2010, 58). Which explanation carries the most weight, and does the dominant/tactical framework help researchers place Carter's grand strategy in proper context?

The second case involves George W. Bush's "Bush Doctrine" and, later, "Freedom Agenda," which some observers considered to be "the most important reformulation of US grand strategy in over half a century" (Gaddis 2002, 56). Specifically, after 9/11 and the subsequent invasion of Afghanistan, the Bush administration categorized all states as either "with us or ... with the terrorists" (Bush 2001). Months later, it released a National Security Strategy emphasizing a role for US military power actively shaping the world and presaged the 2003 Iraq invasion (Bush 2002, 29-31). An ambitious vision to preserve US security and expand democracy, this strategy

⁵³ Auten (2008, 1-2) observes this change in terms of defense spending, which the administration initially cut in high-profile areas but was expanding by the end of 1979.

broke with some of America's foreign policy traditions by relying explicitly upon military superiority as well as preemption and unilateralism (Gregg 2010, 237). While Legro (2009, 52) considers the doctrine to be a "brief experimentation" in America's long tradition of liberal internationalism, I hold that it should be characterized as a major tactical-level change more analogous to the adoption of containment than a fundamental change from "American internationalism."⁵⁴ Unlike containment, though, this effort at change quickly faltered. By the latter half of his second term, with neither the Afghanistan nor the Iraq Wars resolved, Bush (2008) himself had scaled back his ambitions to favor a less deadly "war of ideas" to complement great power cooperation.

Certainly specific Bush policies, such as detaining prisoners at Guantanamo Bay and targeting Taliban leaders with drone attacks, persisted into the Obama administration. Still, the major strategic overhaul initiated by Bush was abandoned. What feedback led to this outcome? Legro (2009, 58), in a brief article, argues that the Bush Doctrine was "undermined by results" thanks to failures in Afghanistan and Iraq, as well as general push-back from allies and adversaries. What about other sources of feedback, though, such as bureaucratic reshuffling, executive incompetence or electoral politics? Legro asserts that grand strategy "changes and alterations" are possible without abandoning "fundamental principles;" unfortunately, such claims about strategic adjustment do not appear in his original theory and may be ad hoc additions tailored for this case. This ill fit does not invalidate Legro's intuition about what information led policymakers to progressively drop the Freedom Agenda; however, it does demand that this project determine, first, whether the dominant/tactical distinction offers the kind of flexibility

⁵⁴ See also Dueck (2006, 147-162).

that the Legro model lacks; and second, whether Legro's theory most accurately accounts for which type of negative feedback truly matters.

Conclusion

In Sum

Scholar Andrew Bacevich (2002, 5) makes a case for stability in US grand strategy. He argues that "the politicoeconomic concept to which the United States adheres today has not changed in a century." Namely, US grand strategy remains defined by

the familiar quest for an 'open world,' the overriding imperative of commercial integration, confidence that technology endows the United States with a privileged position in that order, and the expectation that American military might will preserve order and enforce the rules.

Yet at a lower, tactical level, grand strategy also is constantly contested and frequently in flux.

As Carter's "moral turn" and Bush's Freedom Agenda demonstrate, policymakers regularly seek to change grand strategies after security, political and economic shocks. As those efforts demonstrate, though, changes can fail. This project, therefore, seeks to 1) demonstrate dominant-level grand strategic stability and, within that stability, 2) analyze what type of initial feedback most likely leads policymakers to abandon a tactical-level grand strategy change.

Jeffrey Legro's (2009, 54) theory offers a useful framework to explain grand strategic stability and failure. Foreign policy thinking, it suggests, is set during "searing" historical episodes and are then "embedded in national institutions and educational systems, and ... are protected by interest groups that benefit from them." Only a perceived failure of the status quo followed by implementation of a single alternative strategy can overcome this inertia. Crucially, this theory predicts that attempted changes will fail if their "early results" seem worse than those of the previous strategy. Unfortunately, Legro's theory only describes what I term "dominant-level" grand strategy change, and to date, Legro offers only one case study in which an attempted

strategic change failed: America's post-World War I decision for isolationism. To address these gaps, I offer a theory of "tactical-level" grand strategy based upon Legro's model. Here, attempts at grand strategic change are more common but failure of those changes is likely when early feedback is perceived by policy elites to be negative.

Beyond this model of *elite perceptions of initial failure*, though, a number of other potential early feedback mechanisms exist. These are categorized in more detail in Chapter 3 as *shifting external conditions*, *parochial economic interests*, *veto players*, and *public opinion*. In turn, by analyzing two cases in which policy makers rejected or abandoned their own tactical level strategy during the era of what Legro calls "American internationalism," I seek both to compare rigorously these competing claims and to explore a relatively understudied class of phenomena: failed grand strategy changes.

Thus, this dissertation argues that re-conceptualizing grand strategy as a two-level phenomenon helps resolve the puzzle of change and stability. Namely, it suggests that some elements of grand strategy remain stable for decades while other elements frequently change in order to adapt to shifting external and domestic conditions. By conflating these two levels, much empirical research on grand strategies attempts to draw conclusions and build theoretical models that in fact apply only to one level or the other. For instance, the dominant/tactical approach predicts that any given effort at grand strategy change is likely to remain bounded by the higher level, "dominant" grand strategy. Such insights, while ostensibly simple, hinge on a previously missing conceptual tool in grand strategy research, which has tended to rely upon either oversimplified or overly detailed models of grand strategy. Further, though the dominant/tactical distinction is integrated into Legro's model for this project, it does not necessarily espouse any particular school of thought and offers a framework generalizable to the entire field of enquiry.

Dissertation Outline

The following seven chapters apply this framework and seek to better understand grand strategy change by analyzing and comparing the Carter and Bush experiences. This introduction has set out the basic questions, reviewed the existing literature, described Legro's theory, defined the dominant/tactical distinction, and previewed the case studies. The second chapter tests the claim that both cases fit within the same postwar, dominant grand strategy parameters. It does this by setting out three dimensions that characterize any given grand strategy then comparing a baseline case, US grand strategy in the early to mid-1940s, to grand strategy in the Carter and Bush administrations when those strategic visions were at their most ambitious. The more those cases diverge or converge with the baseline, the more likely they are to have been either fundamentally similar or dissimilar with that baseline case. The chapter demonstrates that, despite key differences, the latter two cases appear to fit within the parameters of the baseline case. In turn, subsequent chapters focus on stability, change and failure at the tactical level.

Chapter 3 sets the stage for the rest of the project by setting out the method of analysis for the Carter and Bush administrations. Specifically, Chapters 4 and 5 each focus on a series of three salient periods during which, for each administration, periods of policy deliberation and outside events overlapped to push officials to clarify or recalibrate their strategic positions. During each salient period, the case studies will focus on whether and to what degree key variables related to *elite perceptions of failure, shifting external conditions, parochial economic interests, veto players, and public opinion* are congruent with predictions from competing theories and schools of thought. In other words, the case studies evaluate how much observed reality fits theoretical predictions regarding which types of feedback lead to a new strategy's abandonment. This analysis is carried forward in Chapter 6, which directly compares the two cases and then assess

both their individual and comparative results. Findings here suggest that not all theories are equally useful in predicting and understanding when a change “fails,” and that Legro’s model is strong but may not offer the most robust explanation for tactical-level change.

Finally, the conclusion in Chapter 7 will reintegrate the findings from Chapter 2 with the rest of the project to evaluate the larger dominant/tactical framework as a concept useful for grand strategy analysis. This chapter will also discuss the implications of this research for the study of grand strategy generally and for the Carter and Bush cases particularly. In addition, implications and lessons for policy makers are presented. Some of the most important outcomes of this research involve a clearer grasp of the long-term prospects of a given grand strategy and the limits of rhetoric and intentions in the face of political and governmental inertia. Policymakers can use such information both to distinguish lower-level grand strategy changes from overarching grand strategy parameters and to predict when and why such changes are likely to fail. Similarly, in projecting the behavior of other great powers, policy elites can better understand whether and how dramatic changes might be long-lasting. For scholars, the notion of “grand strategy” remains widely used but sparsely theorized. This research can expand our understanding of grand strategy as well as contribute empirical research to a specific phenomenon: feedback ending a grand strategic change.

This research also raises important questions. For instance, are the avenues of initial feedback the same in other states and other eras? When and where do institutions, like presidential elections, affect the pace of change for grand strategy tactics? More comparative research is required that encompasses Russian and Chinese grand strategies as well as those of middle-tier powers, such as Turkey and Brazil.

Academicians and policymakers routinely appeal to “grand strategy;” yet as an empirical phenomenon, this concept remains relatively understudied and undertheorized. This project aims to advance theory-building for grand strategy as well as to clarify the dynamics of grand strategy during important but puzzling periods in US foreign relations. It also aims to shed greater light on the underdeveloped but revealing parallels and comparative experiences of the Carter and Bush administrations. Such research contributes to IR as a field as well as to our general understandings of strategy, US foreign policy and international politics.

[CHAPTERS 2 – 6 OMITTED FROM THIS COPY. CONTACT AUTHOR FOR MORE DETAIL.]

Chapter 7: Conclusion

In late summer 2006, Israel launched a major offensive against Hezbollah. Hostilities flared after Hezbollah seized an Israeli soldier and, in response, the Israel Defense Forces activated preexisting plans to cripple the Iranian-backed organization. Bush administration officials needed to take a position. For them, this war became a brief tangent, but it raised important questions regarding the influence and flexibility of grand strategy.

Condoleezza Rice worried that Israel would be perceived as an aggressor, so she advised Bush to warn Israeli officials about excessive destruction and to call for a quick end to hostilities. Dick Cheney disagreed. He insisted that Israel should enjoy free reign to finish the operation as it saw fit. Bush sided with Rice and appealed to his grand strategy: “too much was at stake,” he explained, because the United States “can’t abandon the democratic forces and their foothold in Lebanon and sustain the Freedom Agenda” (Bush 2010, 414; Rice 2011, 480-481). Outside observers, though, remained unimpressed with Bush’s principles. Fareed Zakaria (2006, 49), for instance, had already noted that European diplomats interpreted Rice’s multilateralism as the “product of failure.” On one hand, Bush had launched the Freedom Agenda in order to consolidate major grand strategy changes. On the other hand, his less aggressive approach to the Israel-Hezbollah war conveniently dovetailed with an administration battered by strategic frustrations and domestic criticism. Does Bush’s decision here represent the result of negative feedback or the triumph of successful strategy? And if Bush was responsive to negative feedback, which source of that feedback carried the most weight? Did policy makers, for example, enjoy a degree of agency as they evaluated strategic options, or were they effectively coerced by external and domestic forces beyond their control?

This dissertation has set out to answer these and related questions. The following chapter, therefore, briefly reviews the project's goals and findings and then turns to evaluating this work's theoretical implications. In particular, it discusses implications for the Legro model, reiterates the dominant/tactical distinction as a parsimonious conceptual tool, expands on the "snapback thesis" proposed in Chapter Six and argues that this project's findings offer a natural fit with long cycle theory. The chapter then turns to larger contributions derived from this work. Namely, the role and power of inertia in domestic and international politics as well as the overall empirical utility of grand strategy as a concept. Finally, possibilities for future research are considered, and some of the questions raised in this episode during the Israel-Hezbollah war are reconsidered.

Overview

This dissertation has focused on periods of grand strategy "failure," defined here as when an administration abandons a strategy it has recently implemented. The Carter and Bush administrations prove especially relevant cases for this problem. Carter entered office explicitly declaring that he would change the tenor of US foreign relations by easing away from rivalry with the Soviet Union, expanding human rights as a security priority, and cooperating more deeply with allies and nonaligned governments. Such steps offered to break with the previous, Republican administrations. Though Carter's effort at radical change largely failed, the first two years of his effort provide a "tough case" for claims that grand strategies remain stable for long periods. On the other hand, the fact that Carter ultimately returned to long-standing strategic positions between 1978 and 1980 might suggest that, upon closer inspection, his major attempt at change was superficial, or at least not a fundamental break with the past. Similarly, the Bush Doctrine seemed to break with America's more cooperative foreign policy traditions by

explicitly relying upon military superiority as well as preemption and unilateralism. However, the doctrine might be considered a “brief experimentation,” a speed bump in America’s long road of liberal internationalism (Legro 2009, 52). However we interpret the Bush Doctrine, what remains clear is that, by the latter half of his second term, Bush himself had scaled back his ambitions and favored a less deadly “war of ideas.”

To evaluate the nature of change in these cases, the previous chapters evaluated both administrations in terms of, first, whether they represent variations within a dominant grand strategy and, second, which stream of negative feedback is most relevant to undermining a newly-implemented strategy change. In particular, the project proposes a dominant/tactical bifurcation of grand strategy. Here, dominant-level parameters remain relatively stable for long periods while tactical-level changes are easier to achieve; thus, tactical changes occur more regularly than dominant changes. This distinction also helps researchers observe and evaluate grand strategy change, a research agenda that, while productive, has been dominated either by overly simple or overly complex accounts of how to define grand strategy itself. In turn, the project applies this distinction to a model of foreign policy and grand strategic idea change developed by Jeffrey Legro. While Legro’s model is certainly not the only account of such change, it offers a compelling case for why grand strategies might remain stable for long periods and why, just as importantly here, an attempted change might fail. What Legro’s model fails to do, but the dominant/tactical distinction does contribute, is clarify at what level such changes might be occurring. In other words, his model is a good candidate for applying and testing the dominant/tactical framework as well as evaluating whether and how the Carter and Bush changes may be tied to the same overarching strategy as well as why those administrations implemented and then abandoned a new strategy.

The central empirical work of this study focused on those two questions, and the findings offer important insights into grand strategic stability, change and failure. Chapter Two demonstrated that even at the heights of their respective grand strategies, neither administration strayed from the dominant parameters set in the 1940s. Carter sought to elevate cooperative relations with all governments and emphasize human rights, whereas Bush leaned upon US military power and “coalitions of the willing;” nevertheless, neither questioned the goal of a world comprised of liberal democracies or the role of open trade and international institutions in bolstering that world. Further, the role of the United States as global leader remained fixed. Both administrations did test the limits of what Americans accepted as the necessary, prudent or proper place of military power in a US-led world, yet even on this issue, neither administration moved toward the kind of isolationism or aggression that would characterize a fundamentally new grand strategy.

Chapters Four and Five, meanwhile, analyzed each administration regarding negative feedback within those dominant parameters. They focused on six periods during which grand strategy was particularly salient for decision makers, and they sought to determine whether and to what degree predictions made by competing theories were present in the cases. Notably, Legro’s prediction of elite perceptions of failure (this author’s term) was apparent in the Bush case but not in the Carter case. By contrast, realist-grounded expectations that abandonment of a newly implemented strategy would follow perceptions of external pressure and/or power shifts in the international system were consistent across the salient periods. The influence of veto players was also relevant, but less prominent, as defense officials, Congressmen and other institutional players exerted pressure to adjust or change each administration’s grand strategy ambitions. Public opinion and even economic interests, however, factored little in the administrations’

immediate debates during the salient periods. Still, these last two variables cannot be fully dismissed because they characterize the political and economic context in which the substance of grand strategy took shape.

Overall, the findings suggest a hierarchical set of influences regarding negative feedback. External factors prove the most relevant, and the evidence suggests that shifting relative power in the international system or unexpected challenges to a state's standing are sufficient to undermine a new strategy. Nevertheless, abandonment of the new strategy in both cases was accompanied either by perceptions of failure or active veto players. In other words, negative external feedback was observed by policy makers who in turn acted to undermine or marginalize the new strategy. Those players cannot easily ignore external pressures, such as failed strategic initiatives or shifting balances of power; however, their prerogatives, interests and agendas determine how external pressure is interpreted, the content of a given strategy, and how quickly (or not) they will respond to such challenges. Officials and other players are the actual agents of change and shape how decisions and policies will respond to external factors.

Theoretical Implications

Such findings regarding stability and negative feedback offer important insights into the study of grand strategy. The following sections, therefore, evaluate these contributions, particularly as they relate to the conceptual innovations presented in this project. Overall, I argue that Legro's framework remains a useful model because it offers explanations for stability, change and failure, and it proves flexible enough to import the dominant/tactical distinction, which addresses a key weakness in Legro's original theory. Further, I argue that this research suggests a possible "snapback" thesis in which ambitious grand strategy changes are pushed back toward a median point determined by the dominant grand strategy, and I argue that all of

these dynamics fit more comfortably within a long cycle theory of change than that of, particularly, structural realism.

The Legro Model and Negative Feedback

To begin, the Legro theory is fundamentally about inertia. While Legro's study focuses on ideational change, his framework provides a fungible model that might be applied to many policy contexts in which events, interests, beliefs and other variables must align before inertia is overcome. (More on this below.) Legro's original framework was designed to capture major foreign policy idea change as a multi-step process in which several variables align before a major change occurs.⁵⁵ This is a powerful approach because it demonstrates why all types of changes may be difficult and when they are likely to succeed. Problematically, though, it remains vague about which sources of feedback lead policy makers to decide that a new strategy is successful or failing. Fortunately, the model does not logically rule out testing alternate types of feedback, even when those streams of feedback are derived from competing theories. Thus, in terms of application to the cases at hand, the researcher is able to directly compare the relative impacts of variables that are often analyzed separately as constituents of distinct theories. Here, that means perceptions of failure, external conditions, parochial economic interests, veto players, and public opinion. Stated differently, the Legro model allows space to test variables and clarify questions that it did not originally consider.

More generally, Legro's model offers a framework flexible enough that, even when its predictions about feedback fall short, the basic process of change appears to remain in place.

Recall that Legro's model anticipates the following lifecycle for a new grand strategy:

implementation → *trial period* → *consolidation OR abandonment*. As this project's findings

⁵⁵ These variables are as follows: perceptions of failure regarding the previous idea; availability of a single, viable alternative; and successful early implementation.

suggest, policy makers may not be rationally or objectively evaluating the performance of a new strategy during the “trial period” phase, as the Legro model seems to predict; nevertheless, there does appear to be a period in both case studies of early implementation and application during which the merits of a new strategy become apparent. Indeed, comparing alternate sources of feedback during that trial period phase has been one of this project’s central goals, and that comparison has demonstrated something not necessarily predicted by Legro’s model: hierarchical relationships among the feedback streams tested. In turn, that trial period is followed in these cases by a return to previous strategic positions rather than consolidation of the new strategy into a more permanent fixture. To reiterate, then, a new strategy was implemented, experienced what was in effect a trial period, and was then abandoned in favor of previous strategic positions. That timeline comports with the Legro model’s expectations regarding change and inertia.

For two years, to take the Carter example, the president and his administration attempted to calibrate their competing priorities and respond to, in particular, Soviet challenges to the president’s vision. Officials during this time displayed little willingness to seriously reconsider the grand strategy agenda that they had implemented; however, as challenges mounted through 1979 and 1980, Carter and his chief advisors found themselves walking back key initiatives, such as cuts to defense spending, and reasserting long established stances on American geopolitical interests around the world. In other words, even if they did not display the kind of objective evaluation predicted by the Legro theory, Carter officials did follow the theory’s predicted timeline for a newly implemented grand strategy change: a trial period followed by a period in which the administration, in this case, abandoned many of its earlier innovations. The Bush pattern is similar, though in that case, perhaps owing to built-in pressures from the first term,

policy makers more quickly descended from the heights of their vision to working toward scaling down their most ambitious plans.

In sum, the Legro model of change and inertia offers a generalizable and flexible framework. With the Carter and Bush cases, the specific question of abandoning a new strategy is under scrutiny. Here, Legro's model proves useful because it lays out broad expectations regarding the implementation and early reception of a new approach to grand strategy, and it allows the researcher to expand the model's original focus and compare competing streams of feedback. The findings suggest a hierarchical set of feedback streams. External conditions lead; however, the dissertation has also shown that much of this change and abandonment occurred within a single, dominant grand strategy. In other words, inertia, a fundamental phenomenon or concept in political behavior, remains a definitive context in which the implementation and evaluation of new strategies occurs.

The Dominant/Tactical Distinction

The dominant/tactical distinction is another of this project's key contributions. As the puzzle of change and stability raised in the first chapter suggests, many prominent theories, particularly in the realist and liberal camps, focus on explaining the causes of change. They emphasize the sources of strategy or the relevant variables of change, and they have yielded important insights into the influences of both structural conditions (realism) and domestic politics (liberalism/institutionalism) to shape grand strategy. Nevertheless, these approaches tend to take for granted the object of their study. Specifically, they fail to systematically distinguish a dramatic, fundamental change from lower-order changes. For example, as discussed in Chapter One, they remain unclear regarding whether a given grand strategy is likely to be stable for a long period, face abandonment as soon as permissive conditions arise or evolve

gradually toward some new type. For instance, structural realism holds that grand strategies will change in response to a changing power structure in the international system; however, the precise extent a given state's strategy response to these changes is unspecified.⁵⁶

The dominant/tactical distinction offers a generalizable framework to address such issues. As demonstrated in Chapter Two, by analyzing grand strategy as a two-level phenomenon, observers can separate long-term stability from more frequent lower-order changes. As the case studies in Chapters Four and Five demonstrate, for instance, negative feedback may drive tactical-level changes even as the dominant grand strategy remains in place. Though applied specifically to Legro's model for the current project, the dominant/tactical bifurcation can be integrated into any given theory of grand strategy change. This would allow, for example, a liberal theory of foreign policy change to contextualize the relentless pressure of competing domestic interests by evaluating those interests within long-standing, dominant parameters. Similarly, the dominant/tactical distinction allows researchers to compare tactical level changes across two different periods of dominant grand strategy. Researchers could interrogate, for instance, whether tactical level variables remained the same for American policy makers during both the nineteenth century and the post- World War II era.

Just as importantly, for policy-oriented studies, this framework allows more precise and realistic strategic analysis. Specifically, the potential for major, long-lasting grand strategy change is far more limited than many observers suggest. For example, current debates about whether and how the United States should respond to what is perceived to be a "rising China" should recognize that policy makers, even backed with broad-based support, are unable simply to implement a fundamentally new strategic approach or, more profoundly, "give up on grand

⁵⁶ As Keohane (1986, 180-181) points out in his classic critique of structural realism, the theory fails to explain change or account for efforts to mitigate conflict, to say nothing of calibrating levels or types of change.

strategy” (Zegart 2014). Much is required to overcome the inertia of dominant grand strategy; however, policy makers do hold an appreciable degree of flexibility regarding tactical level changes. They can shift basing priorities, trade arrangements and political agreements in ways that emphasize different aspects of the dominant grand strategy.⁵⁷ The United States, for example, might marginalize its cooperation with UN institutions and expand its alliance relations in the Pacific Rim, but neither of those moves would undermine the dominant strategic logic that has guided US engagement in the region since the 1940s. In turn, prognosticators should evaluate the likelihood of other states’ major strategic course corrections along these same lines. The line of argument, to take one mainstream example, that a rising great power like China “could well pursue a policy of coercion and brinkmanship” may be overstated (Colby and Ratner 2014, 12). The Chinese may enjoy tactical-level flexibility to expand their relative assertiveness, but the dominant/tactical framework suggests that they will remain constrained by extant dominant-level parameters for some time. In other words, some change is conceivable, even likely, but fundamental change toward a new role in the world or a dramatically new approach to peer competitors is far less credible without the necessary conditions to overcome inertia.

A “Snapback” Thesis

This project’s findings also suggest a possible “snapback” thesis regarding newly-implemented grand strategy changes. Though a preliminary proposal, this thesis concisely combines the effects of inertia with Legro’s model of change and “failure.” This snapback thesis holds that most ambitious new grand strategy changes are doomed to fail because they push too far from a median interpretation of the dominant grand strategy. Both Carter and Bush, though

⁵⁷ Thus, debates over American primacy versus retrenchment illustrate the fact that actors like the United States can make important grand strategy changes, but both sides overstate the possibility of fundamentally switching strategic postures. For example, see Thayer (2006, 32-33) and Mearsheimer (2011, 30-31).

in very different ways, sought to redefine how the United States interacts with the rest of the world. The challenge, according to the snapback thesis, is that Carter's effort to minimize force and confrontation and Bush's mirror-image effort to expand the role of US power both extended beyond a mainstream, or median, interpretation of the dominant grand strategy: what Legro has called "American Internationalism." Facing negative feedback at home and abroad, the administrations lacked sufficient political capital and historical precedent to implement something new. Not unlike the "median voter" thesis (Downs 1957) in which candidates for government offices are punished for representing positions far from the mainstream, grand strategies face increasing resistance the more they push boundaries, under the dominant grand strategy, of what is typically acceptable. They are forced to snap back into place.

As described in Chapters One and Two, dominant level changes are possible, but they are most likely after a political or similar type of shock occurs in conjunction with perceptions that that the power structure of the international system is changing. In most circumstances, though, the international system will not be seen to be in a period of fundamental realignment, and policy makers with ambitious grand strategy agendas will struggle to justify overturning extant military, political and ideological investments. The Bush administration, for example, faced a unique opportunity to change American grand strategy, and whatever the reasons for abandoning its ambitious agenda, the fact that it essentially returned to long-standing policies and priorities underscores the pull exerted by a dominant grand strategy. As Skidmore finds in his study of Carter's foreign policy, the President, "failed in finding a formula for establishing the domestic legitimacy of his new policies in competition with the institutional and intellectual legacies of past policies" (Skidmore 1993, 707). The administration, in other words, was forced to snap back toward a more familiar strategic position.

Long Cycle Theory

Finally, though external factors represent the largest single stream of feedback in these cases, as predicted by structural realism, I argue that this project's findings and innovations fit more naturally with another approach: long cycle theory. Taken broadly, long cycle theory observes that, driven by innovation and competition, states dominating a given era's leading economic sectors are likely to rise as "lead states" and seek to shape the international system according to their interests (Modelski and Thompson 1996, 3-7).⁵⁸ As innovations diffuse and new ones develop, though, a lead state's relative position declines and the system is likely to enter a transition period out of which a new leader may arise. This approach, like structural realism, does emphasize states as the fundamental units of the international system, and these units are competing in what is effectively a self-help environment. Further, these units' relative standing is determined by their material capabilities. Nevertheless, long cycle theory allows for a more nuanced treatment of grand strategy.⁵⁹

This treatment involves two key insights from long cycle theory. First, as discussed above, structural realism tends to treat all changes, no matter how limited or large, as essentially similar, whereas the long cycle approach suggests that some periods of change will be deeper than others.⁶⁰ Systemic transitions, for instance, allow a rising state more latitude to change its international role while the declining state may be forced to retrench its standing. Changes at

⁵⁸ See also Modelski (1987), Goldstein (1988) and Kurth (1999).

⁵⁹ Long cycle approaches also offer other benefits over the structural realist approach, including an account of modern state development, the heightened threat of general war during periods of great power transition, and the potential for hierarchical orders.

⁶⁰ For Waltz (1979, 201), the only real question of fundamental change involved whether or not powerful states could change the *system*—in other words, the very nature of international politics—rather than change the system's distribution of power. This may involve profound changes regarding the system's power polarity, but the strategic logic available to states remains the same. Neoclassical realism moves beyond this strait-jacket by evaluating elite perceptions and domestic constraints (Schweller 2003, 332-344). Nevertheless, this approach offers little systematic account of the actual power realities in which perceptions occur.

these moments are more likely to rearrange existing interests and strategic investments. During the long periods between transitions, though, changes will be more likely to face constraints from previous commitments. Even a state at the apex of its power can overreach its capabilities and domestic political will (Snyder 1991, 6). Doran (1991, 60) illustrates this dynamic by showing how policy makers at any given point along a state's arc of relative capabilities will inaccurately project their power growth or decline in a linear trajectory. Events, however, force them to draw their strategic goals back toward a more accurate understanding of the state's current capabilities. In other words, at any given point in time, changes are constrained within specific parameters.⁶¹

Second, structural realism offers little account of the substance of grand strategies. Because international relations is reduced to a set of premises about power competition, structural realism only describes approaches to balancing and alliance formation.⁶² This says nothing about the content used to justify and mobilize, and it cannot easily differentiate between, for example, a capitalist trading state like the United States and a more autarkic, state-directed economy like the Soviet Union. By contrast, long cycle theory maintains that factors differentiating state capabilities include their economic structures, domestic arrangements and international strategies.⁶³ Stated differently, long cycle theory allows researchers to evaluate both the material and ideational conditions of systemic leadership. For instance, early system leaders like 16th century Portugal and 17th century Netherlands played a two-level game at the

⁶¹ Skidmore (1996, xviii), for instance, finds that perceptions of hegemonic decline create political pressure to change grand strategy, yet the inertia of political and other investments to sustain hegemony make such change difficult.

⁶² After the Cold War's end, for example, realists rejected liberal and constructivist accounts of possible cooperation but struggled to explain why the United States did not face a balancing coalition. Leading explanations typically leaned on calculating the current balance of power rather than considering the ideas or substance of leading foreign policies and grand strategies (Wohlforth 2002; Mearsheimer 2001, 380-383).

⁶³ For example, smaller states frequently develop a reliance upon the global order and trading network generated by the system leader, while trading strategies may be adopted by frustrated land-based powers. And in every case, trading and democracy seem to exist as mutually reinforcing phenomena (Rasler and Thompson 2005, 24-25).

regional and systemic levels. They aggressively sought to organize international trade (as well as avoid significant territorial conquest) and work within the ideological and diplomatic framework of regional politics.⁶⁴ Later, Britain enjoyed two peaks as a system leader, and its second, in the 19th century, saw the state unite trading, industrial and naval power. Such singular economic and military capacity combined with global reach enables the leader to “reorganize the trading world along lines reflecting its preferences, interests and strengths” (Rasler and Thompson 2005, 22-23).⁶⁵ In turn, the 20th century saw the United States, after World War II, rise to systemic leadership and extend its own ideological and economic preferences across the world (Thompson 2006b, 11-15). In other words, long cycle theory suggests that the “hardware” of systemic leadership is accompanied by an equally important “software.”⁶⁶

These insights—constrained change and the “software” of systemic leadership—suggest that long cycle theory enjoys a conceptual advantage over structural realism. Specifically, the dominant/tactical distinction offers an effective model to describe how, during a long cycle, a new strategy is implemented and can remain largely stable for decades. It also shows how policy makers during that cycle might constantly seek to adjust and change their grand strategy but be constrained by material realities and political inertia. This approach to grand strategy also offers a precise account of how the “software” of leadership interacts with the cycles themselves. Notably, long cycle theory does not necessarily claim the kind of stability predicted by the dominant/tactical framework. Alternate approaches to grand strategy change in long cycle

⁶⁴ This is also similar to Rosecrance’s (1986) “trading states” thesis.

⁶⁵ Notably, winning great power wars plays a key role in this ability to impose a preferred order because, during systemic transition periods, such conflicts “represent disputes about how best to manage politico-economic problems and policies” (Rasler and Thompson 1989, 4). See also Rasler and Thompson (1994) and Thompson (2006a, 9).

⁶⁶ The author is indebted to William R. Thompson for this metaphor.

theory might include strategic adjustment or evolutionary adaptation; however, neither of those approaches distinguishes between levels of change, and neither fits as tightly as the dominant/tactical framework with the cyclical pattern of transition and stability.

This project's research agenda also offers important insights into the all-important strategies and international orders pursued by lead states. Despite a capacity to incorporate grand strategy as an ideational component of long cycles, little research has actively studied that substance. In other words, whereas the hardware of systemic leadership is well-documented, its software stands relatively unexamined. For example, ideational conflict, especially in the industrial era, may be tied to long cycles: Communists and Fascists sought to catch-up to and surpass liberalism, while Islamism may represent an attempt to resist, if not catch, liberalism (Thompson 2006a, 9). Similarly, the ideational strategies used to inspire successive generations to make sacrifices required by systemic leadership remain fairly under-examined (Thompson 2006b, 15-16). The dominant/tactical model offers to facilitate this research agenda. Specifically, a leader's strategic options at any given point in time are likely to be limited not just by systemic pressures but also by ideational inertia. In turn, "failed" efforts at change, such as those in the Carter and Bush administrations, may be a natural component of the long cycle experience. Specifically, as policy makers anticipate a certain degree of capacity to shape the system, they are likely to find that they have far fewer resources or far less ideological support or far more political resistance than expected. What remains, as expanded below, is to more explicitly extend this research agenda. Overall, this project's findings offer to deepen the ideational study of long cycles through grand strategies, and long cycle theory offers a larger context in which this research agenda finds relevance.

Larger Contributions

Out of this project, a pair of general lessons emerge for International Relations and Political Science as well as for all scholarship on interstate and foreign affairs. These are, first, the power of inertia and, second, the conceptual utility of grand strategy. Though not an immediate target of this project, these lessons embed the research in larger bodies of scholarship and offer theoretical and empirical insights for academicians and policy makers.

The Power of Inertia

Foreign policy researchers often focus on the origins of a policy, decision, or change. This is natural because understanding the sources of a given outcome or phenomenon drives scholarly enquiry and comports with a human urge to seek lessons and analogies from previous experiences. However, questions of non-change or inertia are less frequently studied. This bias in favor of change may be a function of the empirical limits of theory because non-change assumes counterfactuals. It assumes that some other outcome would have occurred under different circumstances, which is a claim notoriously difficult to support. Even if this problem is overcome (and, as argued below, studying failed changes is one way to do that) the relevance of inertia can easily be dismissed as a kind of methodological magic wand: inertia explains everything that is not change. Research on strategic culture, for instance, often faces this problem because scholars have to demonstrate that nonmaterial variables—culture and beliefs—prevented some hypothetical outcome (Johnston 1995, 13-14).

Despite these challenges, inertia arguably remains an all-pervasive and measurable reality of social and political life.⁶⁷ In this project, as discussed above, political, institutional and ideational inertia provide the logic of Legro's model, and the empirical chapters support this

⁶⁷ Organizational theory and research on firms, for example, suggest that “inertial pressures arise from both internal structural arrangements and environmental constraints,” and that “inertia, rather than plasticity, is the norm.” (Hannan and Freeman 1977, 931; Rumelt 1995, 101) See also Stinchcombe (1965) on “imprinting.”

proposal. Both administrations were constrained by a dominant grand strategy, and both, when abandoning a strategy that each had implemented, found themselves pursuing approaches basically similar to what had been in place previously. Similarly, one basic question at stake in this project has involved interrogating the types of feedback necessary to overcome implementation of a new strategy. In other words, what is required to overcome the personal, political and other investments made when elite policy makers implement a new strategy?

By evaluating both long-term consistency and lower-order “failures,” the project is able to evaluate inertia because the effects of inertia are real rather than hypothetical. This empirical evaluation includes observing stability on a proscribed set of measures across time; negative feedback that is congruent with change even when policy makers have strong incentives to maintain the new strategy; and a return to previous strategic positions when new efforts are abandoned. There are also other ways in which inertia can pervade every level and aspect of policy making. For example, activist groups, commercial lobbies and other special interests may dissent from a dominant grand strategy, but once they have carved out a political space within that dominant strategy, they may often have greater incentives to work within rather than work against that framework. Similarly, as Legro finds, a reigning ideology or “foreign policy idea” may grow less relevant or effective over time, but its previous acceptance and apparent utility often leave it ascendant even in the face of apparent failures. At a systemic level, states invest in given economic sectors and geostrategic interests, so change, while sometimes explosive or cascading, occurs along broadly predictable fault lines. Inertia is not the only story in understanding the nature of politics, but it is a fundamental one, and one that is often overlooked.

The Utility of Grand Strategy

Finally, this project underscores the utility of grand strategy as a concept in social science, historical enquiry and policy evaluation. Critics, as discussed in Chapter One, maintain that grand strategy is a chimera, a projection of order and intention where, in reality, policy makers are simply responding to immediate challenges or are constrained by bureaucratic interests. A second challenge to grand strategy is that, as a concept, it is hopelessly vague. It can represent a “kitchen sink” approach to foreign policy in which every decision made and every action taken can be considered an aspect of grand strategy. Indeed, even scholars working with the concept tend to redefine it to fit their own goals, and the common formulations that do exist, such as Posen’s or Gaddis’, remain thoroughly general. A third challenge is that, particularly in policy circles, grand strategy is treated as both an empirical phenomenon and a normative goal. It is something that exists and can be studied, but it is also something that is desirable to achieve. Often, contributions in this vein argue that many existential threats and challenges to the state would be resolved if the proper or most carefully calibrated grand strategy were adopted.⁶⁸ The typical problem with such approaches is that the empirical nature of grand strategy is not clearly defined or strategy is treated almost platonically, as if a strategic ideal exists that, like natural law, can be inferred and applied through careful observation and logical reasoning (Elkus 2014, 11-12).⁶⁹ Similarly, observes Kissinger, all administrations want to develop a clear strategy, but “the problem is that they never start with an analysis of what the world is, but what they think it should be.”⁷⁰ Without a clear definition of grand strategy’s

⁶⁸ To conclude a sober assessment of contemporary US grand strategy, Pape (2009, 34) offers a classic statement in this vein: “With the right grand strategy, however, America can mitigate the consequences of its relative decline, and possibly even reverse it.”

⁶⁹ Mearsheimer (2011, 30-32), for instance, assesses US grand strategy and offers prescriptions based upon a theoretical model, offensive structural realism. This offers a clear, if idealized, standard against which to compare a given grand strategy’s performance; however, it does not take seriously grand strategy as a phenomenon in its own right.

⁷⁰ Interview excerpt cited in Chollet and Goldgeier (2008, 71).

empirical dynamics, effective diagnoses and prescriptions are difficult or impossible to achieve.⁷¹

I argue, however, that this project directly addresses these concerns and underscores the ways in which grand strategy can be usefully operationalized and studied, and it demonstrates how observers can derive prescriptions that, while less ambitious, will be more precise and effective. First, that the term “grand strategy” is frequently applied in overly general or *post hoc* ways is undeniable. The important questions are, first, whether it can be used more precisely and consistently as well as, second, whether it can be observed as a phenomenon larger than the interests of foreign policy bureaucrats or the snap decisions of policy makers. This project has demonstrated that it can. The simple move, for example, of defining grand strategy as the overlap of intentions and actions offers a set of observable characteristics, such as presidential statements and decisions, which can be collected across time or cases to construct an empirical picture of grand strategy. In other words, rather than an artifact of clever definition or psychological biases, grand strategy can be a discrete, observable phenomenon. This is important because, in turn, grand strategy can directly affect policy makers’ options and choices. As this project has shown, though separated by time, context and ideology, both the Jimmy Carter and the George W. Bush administrations were constrained within parameters set after World War II. In other words, a dominant grand strategy existed separately from these policy makers’ immediate interests and intentions. Similarly, the question of stability and change—why states seem to project a given strategy over time as well as demonstrate constant foreign policy

⁷¹ Andrews (2004, 435), for example, advances a plausible trajectory for US grand strategy in Transatlantic relations; however, the work’s central logic implicitly rests on a normative assumption that amicable strategic relations between the United States and Western European states is desirable.

changes and adjustments—becomes more manageable empirically with a careful definition of grand strategy.

Further, a carefully-formulated notion of grand strategy allows researchers and policy makers, who frequently rely on historical analogies, to compare the phenomenon across eras and cases (Neustadt and May 1986; Jervis 1976; Khong 1992). For example, one of the ways in which the Carter and G.W. Bush administrations make sense as a comparison is under the rubric of grand strategy, and important insights regarding stability and negative feedback have been achieved through this comparison. As suggested above, though, this methodological capability also allows observers to interrogate historical grand strategies, compare current cases and analyze possible future trajectories. Rigorously applying grand strategy offers insights into the highest levels of a state's trajectory in the international system as well as insights into the lower-levels in which policy makers are deliberating and making decisions. In this sense, it is a fungible concept that can be applied even in contexts in which policy makers do not consciously subscribe to a grand strategy.

Finally, while many scholars conflate grand strategy's empirical and prescriptive applications, this project has demonstrated that isolating empirics is possible and need not undermine prescriptions. By studying the stability and feedback that affected both the Carter and G.W. Bush administrations, the preceding chapters separated out grand strategy as an empirical phenomenon and offered no comment on either what might be a "good" grand strategy or whether grand strategies should proceed along any given trajectory. In other words, like other political phenomena such as authority or cooperation, grand strategy exists on its own merits and can be studied as such. However, just as with authority or cooperation, if observers understand the empirical and historical dimensions of a phenomenon, they can develop theories and

prescriptions about the most desirable way to engage or direct that phenomenon. Often, grand strategy scholars start with a theory of how international politics works and how states should act in such an environment, or they start with a conviction that a standing grand strategy is failed and must be improved. This biases both the diagnosis and the prescription. Grand strategies can and should be studied on their own terms as empirical phenomena: this offers a clear foundation from which to project plausible, well-targeted recommendations and improvements. For example, both the Carter and Bush administrations seized what they considered to be pliable moments in American foreign policy to implement grand strategic change. Though perhaps naïve, as some critics charge, they were specifically ill-prepared to overcome political inertia and overly optimistic about their abilities to transcend negative feedback. A purely empirical understanding of grand strategy would have shed light on these limitations, and it now offers to guide future scholars and policy makers.

Future Research

These insights as well as the project's findings open questions and extensions for further research. First, perhaps the most controversial argument advanced here is that dominant American grand strategy has remained fundamentally stable since the 1940s, so more tests of that claim need to be evaluated. For instance, does the Truman administration's transition to Cold War and containment really represent a change *within* "American Internationalism"? Similarly, possible changes and transitions like détente and the collapse of the USSR need to be tested against the predictions made by the dominant/tactical framework. Looking backwards, is Franklin Roosevelt's strategic approach during WWII really the central point of transition? There is strong circumstantial evidence and existing scholarship to suggest that it was a

fundamental break, but does it fit the mold advanced here? Were there similar transitions at other points in US history?

Second, the central findings here offer important insights into negative feedback for a newly implemented strategy; however, as above, whether and to what degree these findings hold for other strategies and administrations needs to be examined. For instance, did efforts like George H.W. Bush's "new world order" or, more distantly, Taft's "dollar diplomacy" succumb to the same mix of pressures? This research also begs a mirror-image question: what array of conditions makes a strategy *more likely* to be adopted beyond its early implementation? What makes a new strategy "sticky"? Containment is perhaps the premiere example, here, but a lower-level case might include Washington's approach to Cuba after its revolution. Looking ahead, based on factors like veto players and external pressure, what are the odds of rejection of current strategic initiatives like the Obama administration's "pivot" to Asia?

Third, and finally, while this project has centered on US foreign policy and grand strategy, the frameworks and rubrics it applies can be used to evaluate and compare other cases. As suggested in Chapter One, comparative grand strategy remains an underdeveloped but potentially fruitful research vein. Though comparative foreign policy studies are well-established, this more specific focus has suffered from vague or imprecise operationalization of "grand strategy." As argued above, though, grand strategy can be empirically analyzed in a manner that is generalizable across cases. For instance, are other great power grand strategies really constrained by a dominant grand strategy? If the dominant/tactical theory is generalizable, then researchers can use it to broadly predict strategy outcomes when great powers experience crises or consider policy shifts. Thus, ascending states like China and India may rise in relative power and even adjust their strategic prerogatives in order to protect their expanding interests.

Yet those states are likely to remain bound by long-standing dominant grand strategies until a crisis occurs during a period of international, systemic transition. Notably, this type of enquiry dovetails with the long cycle research agenda. As suggested above, grand strategy, particularly as it is formulated here, offers an insight into the content that accompanies states in their relative competition with one another. Future research can take advantage of this project's operationalization of grand strategy to juxtapose different eras of long cycle transition, evaluate a single state at different points along its long cycle trajectory, or compare contemporaneous states across their relative capabilities and trajectories. Overall, more comparative research is required and will be facilitated by this project's conceptualizations and findings.

In Sum

Journalist Ezra Klein (2013) observes that a great myth pervades Washington D.C.: “everyone always thinks everyone else is efficiently and ruthlessly implementing long-term schemes.” In reality, he says, “no one can carry out complicated plans” and policy elites “don't stick too rigidly to plans or rely on some grand design.” This is good, says Klein, because it allows for flexibility in the face of failure. In a similar vein, Henry Kissinger allegedly observed that high political office does not build intellectual capital, it consumes it. There is, therefore, little time for building, referencing and evaluating a grand strategy. Such conventional wisdom seems to suggest that Bush's decision, at this chapter's outset, to side with Rice on the Israel-Hezbollah conflict was an expedient move. It had little to do with grand strategy and much to do with the administration's immediate need to put out a fire it could not control. What Klein and Kissinger elide, though, are the entrenched political, ideological and other structures that constrain decisions and offer short-hand guidance to harried policy makers. In reality, grand

strategy simultaneously shapes immediate policy decisions and, at a lower level, proves responsive to the pressures generated by failure or unexpected outcomes.

Condoleezza Rice (2011, 472) relates another revealing moment from her time as Secretary of State. When visiting Pakistan, a journalist asked why the United States had not called for “free and fair elections” in that country. “Frankly,” recounts Rice, “I wasn’t sure if he was right, and I didn’t want to blindside my host. But what else could I say? ‘The United States stands for free and fair elections and will help the Pakistani government to achieve them.’” She also observes, “I had no idea how elaborate my involvement would become” as she later brokered a political deal between Pervez Musharraf, the President, and Benazir Bhutto, a leading opposition figure. At first glance, Rice appears to be making policy off-the-cuff: faced with a decision, she stakes out a position broadly compatible with US foreign policy. Approached in a larger context, though, Rice appears to be implementing grand strategy. She is in Pakistan because it is a strategic partner, so investing in that state is neither random nor *ad hoc*. In an unscripted moment, Rice also leans on a common heuristic in US foreign policy—supporting liberal democracy—but that heuristic is ground in a larger strategic agenda that seeks to expand the “zone of democracy” as something good for the world’s and America’s security as well as both their bottom lines. Rice need not have thought explicitly about grand strategy, nor even believed that the concept is legitimate. Rather, grand strategy comprises the context in which decisions are made and challenges interpreted. At some previous point, of course, policy elites consciously adopted and implemented a dominant grand strategy, but subsequent leaders need not do the same to be constrained by the inertia of that earlier decision. Policy makers need not consciously ask “What’s our grand strategy?” to be responsive to or to have their decisions and

policies shaped by that strategy. At the tactical level, though, players like Rice enjoy more latitude, though their innovations are more prone to negative feedback.

Overall, then, this project has demonstrated that grand strategies are stable for long periods. Within that stability, they experience constant pressure and adjustment. The most likely agents of such change are policy elites—veto players and decision makers—but the single most important variable associated with change is external pressure. This typically occurs when a new grand strategy change is not producing results in the manner its architects expected, such as in the Middle East during Bush’s second term, or when some unexpected challenge compels policy makers to respond in a manner incongruent with their previous stance, such as the Iranian Revolution or the Soviet invasion of Afghanistan. These findings may seem intuitive, but this specific arrangement of inputs and constraints is neither universally accepted nor obvious. Economic interests and public opinion, for instance, create conducive or hostile general conditions for a given grand strategy, but their direct effects are more limited. Similarly, psychological phenomena, such as cognitive consistency, or domestic political pressures, such as party patronage, might conspire to undermine a new strategy. More broadly, little consensus exists regarding the nature of grand strategy change, which conceivably could change quickly or easily or constantly evolve rather than remain fixed to a “dominant” understanding of a state’s interests and goals.⁷² The preceding chapters, by contrast, have proposed and evaluated a specific and generalizable framework to understand grand strategy and the types of feedback that might undermine it.

⁷² Rosenau (1981, 3), for example, argues that adaptation occurs regularly and in the long term is not fixed, though it is constrained in the short term to political “acceptability.” By contrast, Andrews (2004) argues that grand strategy must be constantly managed if it is to remain stable.

Several general lessons and research questions flow from this project. First, grand strategy is shown here to be a useful analytical concept. Research on the topic tends to be conducted with an eye toward making recommendations and prescriptions or evaluating “good” and “bad” strategies. However, grand strategy can be studied primarily as an empirical phenomenon. This move offers more nuanced insight into the constraints and possibilities of grand strategy, and it may even generate more careful and effective prescriptions than projects that start with an end product and reverse engineer empirics and prescriptions. Second, inertia proves to be a powerful force at all levels. Dominant grand strategies, for example, appear to be the stable beneficiaries of sunk costs, institutional commitments, political investments and ideational consistency. Further, at the tactical level, a “snapback thesis” may be an effect whereby ambitious grand strategy changes are undermined and forced back toward a median point when they violate key aspects of the dominant grand strategy. Looking ahead, this project raises opportunities to develop a deeper and wider literature on comparative grand strategy, and it offers effective conceptual tools to evaluate grand strategies in a long cycle context as they affect or are deployed by leading- and middle-tier states. Addressing such questions will advance our understanding of US grand strategy as well as offer a framework to better understand the options for change and adjustment facing all great power policy makers. Such research would promote a deeper theoretical and empirical understanding of grand strategy, a concept often applied in policy debates but less often studied as an empirical phenomenon.

[APPENDICES OMITTED FROM THIS COPY. CONTACT AUTHOR FOR MORE DETAIL.]

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