Analyzing Grand Strategy as Empirical Phenomenon During the Trump Administration

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This paper argues that scholarship on grand strategy is evolving, and it proposes a framework to capture that evolution: Grand Strategy Analysis (GSA). It sets out the ways in which grand strategy has been studied in recent decades, and it shows that across different research traditions and scholarly fields, grand strategy is treated as a distinct, empirical phenomenon. Unfortunately, these traditions rarely communicate with one another. Further, in the larger academic and policy communities, grand strategy is typically assumed to be a vague organizing concept rather than a specific term of art. In part, these shortcomings flow from our lack of a shared analytical framework. The paper argues that foreign policy analysis offers a useful guide for the interdisciplinary and cumulative study of grand strategy. It then concludes with a proposal to study the first two years of the Trump administration’s grand strategy using this GSA framework.

The GSA Framework

Grand strategy is a distinct phenomenon. It may exist in the minds of a few leaders, or it may exist as a set of behaviors beyond any one group’s control. It may change frequently or rarely. It may be intentional or implied. Whatever grand strategy’s specific characteristics, it does affect foreign policy decisions, and it shapes long-term behavior. For those of us trying to understand grand strategy, this fact raises a set of familiar questions. Who—if anyone—shapes it? What is it? When does it change? Why is it that way? How does work? As we will see below, there already exists a growing set of answers to these questions. What we lack is a way to organize this research or to allow different approaches to “talk” to one another.

I argue that foreign policy analysis (FPA) provides the most sensible framework to unify scholarship on grand strategy. FPA scholars focus on agents—policy makers, government elites—making decisions. For international relations, this is crucial because, as Valerie Hudson observes, “all that occurs between nations and across nations is grounded in human decision makers acting singly or in groups.” For Hudson and many others, the FPA approach pays handsome dividends because it allows the scholar to directly evaluate “the point of theoretical intersection between the most important determinants of state behavior: material and ideational factors.” FPA focuses on decision makers. It allows us to observe how both human minds and impersonal factors affect international outcomes. By its nature, an FPA analysis will include many variables and may include multiple levels of analysis. It may also be interdisciplinary, and it is open to theoretical integration. Of course, it is also agent-oriented and actor-specific. Finally, “theoretical integration”—or integration across levels and types of analysis—is FPA’s most
general benefit. It allows the scholar to integrate many different types of variables and explanations. Admittedly, this can be difficult. Any one level of analysis, for example, includes many variables. Integrating different types of approaches can further multiply those relevant variables. Stated plainly: FPA can be complex. Nevertheless, by focusing on a discrete and specific unit—decision makers or decision groups—FPA allows great analytical flexibility without losing focus or rigor.

For grand strategy, the FPA approach is a natural fit. It allows the scholar to integrate international structures, decision processes, and ideas. As an approach, grand strategy analysis (GSA) makes no commitments about how to define grand strategy or the specific variables that may matter in any given case. Rather, it provides an organizing framework in which, for example, scholars from strategic and security studies can evaluate their different analyses. GSA provides a shared analytical framework to historians, policy professionals, political scientists and others. Specifically, GSA

- treats grand strategy as a unit of analysis (rather than a byproduct of domestic or international politics).

- is agent-oriented and actor-specific. This focuses empirical analysis on people without excluding systems and structures. Crucially, it also allows for human psychology, perceptions, and so forth.

- allows flexibility on how one defines grand strategy—whether as plans, principles, or behavior. We can observe, respectively, what policy makers do, their stated beliefs, and whether all of that fits existing patterns.

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1 Hudson 2007, 3 6-7, and 165 respectively.
• is multifactorial, multilevel, and integrative, as described above in the FPA overview below.

• assumes that both ideas and material factors can matter equally to grand strategy. Claiming that ideas and material factors both matter may seem noncontroversial. After all, in any given social or political situation, is this not what most of us assume? In fact, many—though certainly not all—political scientists have grown skeptical that scholars can observe and analyze ideas and beliefs. By contrast, many historians and policy makers tend to assume that ideas matter, but they often fail to rigorously separate the nature and effects of ideas and beliefs from other factors. GSA provides a vocabulary and a framework to do this.

• holds that grand strategy is distinct from—but intertwined with—foreign policy. Foreign policy might be characterized as all facets of a state that interact with and set behavior toward other states. By contrast, grand strategy is typically described as a specific plan or set of actions and positions to achieve high-order foreign policy goals.

In short, GSA is a framework to study grand strategy. GSA scholars assume that grand strategy can and does exist as a real phenomenon, and that grand strategy is best observed through the statements and actions of policy makers. Grand strategy may be a mode of thinking or a pattern of behavior. Either way, it can be shaped and changed by many different factors. A GSA approach allows the historian, political scientist, professional strategist, or student to compare these different factors by observing how they interact with relevant policy makers. In turn, GSA also gives these different types of analysts a shared vocabulary. Even if the outcome of this shared vocabulary is disagreement about the nature and practice of grand strategy, that disagreement can be sensible to all sides.
The following pages review different approaches to studying grand strategy before turning to how these approaches are converging. As international relations scholarship in particular has evolved, the study of grand strategy has emerged as a distinct phenomenon studied by scholars across fields and disciplines. The chapter further explains the notion of GSA itself, and then it turns to more recent and ongoing work that fits this category. It concludes with a return to the Trump administration and GSA’s relevance in understanding and analyzing this case.

The Problem and Promise of Studying Grand Strategy

In 2017, the American economist Richard Thaler won a Nobel Prize. The award committee lauded Thaler for applying “psychologically realistic assumptions into analyses of economic decision-making.”2 In other words, he and other economists assumed that humans are less than fully rational and they behave accordingly. “He has shown,” the press release explained, “how these human traits systematically affect individual decisions as well as market outcomes.” Thaler had joined a revolution. For decades, economists led a wave in the social sciences that valued abstract models of human behavior. Historians, theorists, and informed observers for centuries had drawn messy lessons about human nature or causes of war or monetary systems. By the 20th century, social scientists were attempting to clean up the clutter. They developed testable hypotheses tied to clear theories, and they worked to narrow possible explanations for given phenomena. Thus, for example, rather than “fanaticism” explaining extreme ideologies, specialists could point to economic conditions or to individual disaffection or to “us versus them” preferences. Though powerful, these approaches demanded a major tradeoff. They required specialization and narrow questions. General theories and unifying frameworks became

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harder to maintain. Even psychology veered into narrow, testable question sets. More importantly, these fields communicated with one another less and less even as their own, internal research silos subdivided into narrower specializations. Critics today delight in showing how far the social sciences and humanities have spiraled into irrelevance. This charge is not unfair, but it is partial. In fact, much insight and knowledge has flowed from these theories and the debates they have inspired. What this social science also has done is cycle our understanding of human behavior upwards. For example, vague ideas about “honor” leading to war have been discarded, but the notion of honor still exists in more precise formulations. In this case, perceptions and other psychological phenomena we might have traditionally called honor may make war more likely, though in slightly different ways than traditionally understood. By breaking big questions into pieces, social scientists can provide more precise analysis. Thoughtful scholars can also use these pieces to refine big questions and observe familiar phenomena in new ways.

Grand strategy is ripe for this kind of update. Models to explain human affairs always oversimplify. Nevertheless, they often interact with old wisdom, and new theories can more effectively explain well-established knowledge. Hindsight bias, for example, was a familiar experience long before modern psychology; however, systematic study and tested theories help us understand more precisely how this experience affects our thinking. Thus, what emerges from new theories may be a more complete, precise understanding of a given phenomenon.

In a similar way, grand strategy is a mature concept. Scholars and practitioners have studied and applied it along parallel tracks. We might divide these approaches to grand strategy into three rough categories. Understanding these categories is important to see how the concept of “grand strategy” is evolving and how it has been both useful and limited. It is also help to understand how and why there are actually several cross-cutting discussions about this topic, discussions
that do not always prove mutually comprehensible. It also reveals their shared weakness. What I will call traditionalists, theorists, and professionals. This book argues that these tracks remain fruitful, but if we expand the limits of our map, like a braided river, they are evidence that a more coherent, single stream exists.

The traditionalist approach—often associated with “strategic studies”—relies upon classic texts to frame historical analysis. Scholars working in this tradition are often historians or political scientists who focus on texts and historical methods. They draw upon writers like Thucydides or Clausewitz as frameworks. This approach allows a certain level of subjective interpretation and a freedom to draw lessons for current challenges.

Theorists, by contrast, are often political scientists—frequently in “security studies”—seeking to break down human behavior into simple variables. These variables in turn work within larger theories or schools of thought. This approach allows insight into the specific agents of grand strategy and offers a more precise and testable set of hypotheses than the classics of strategy. Some theorists may focus on foreign policy outcomes and decision making while others emphasize international systemic variables or bargaining models. They may even debate whether a premeditated strategy is possible and argue that a host of individual, domestic, and international variables drive strategic behavior.

In the third category, professionals, military and other policy planners are specifically focused on national security success. They draw upon insights from traditionalists and theorists—as well as their own training, intuition and experience—as they find them useful. Practitioners and professional strategists focus on identifying goals and optimizing outcomes. They are concerned about scholarship to the degree that it provides actionable insights into creating strategic
priorities and projecting success. Here, strategy is not an object of analysis but a policy agenda. The goal is a “good” strategy.

Notably, this professional approach almost always affects analysis among traditionalists and theorists. I argue that traditionalists, theorists and professionals muddy the very concept they are studying by mixing factual observation of *how grand strategy works* with prescriptions about *how to make grand strategy work effectively*. After all, why study grand strategy if you don’t want to understand and help implement an effective grand strategy? Theorists in particular are famous for working in a “basic research” vein. Their job, like a physicist or geologist, is to understand how the world works. Let others find the applications. In fact, though, theorists typically include extensive discussions about what constitutes an effective grand strategy. They may hope to make the world either a better or at least a more stable place. Perhaps they hope to join the professionals—or they themselves have been professionals—and directly shape government policy. And under scrutiny from a skeptical public and funding agencies, they also wish to make their work relevant. These are all worthy and understandable motivations.

However, these traditions fail to treat grand strategy as a stand-alone phenomenon. A phenomenon we can empirically observe separately from what we want it to be. To make this case, the book references behavioral economics as a model to illustrate its agenda. It then argues for grand strategy as a distinct, complex empirical phenomenon.

Behavioral economics is famous for insights everyone knew. It holds that human psychology mediates, shapes and distorts otherwise “rational” decision making. In a free market, people

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may pay more for a good simply out of habit. In the face of many options, people may feel overwhelmed and choose nothing. In a war, decision makers may weigh already lost lives and treasure over the prospect of future death and destruction and plow forward when all other evidence suggests that a negotiated settlement is optimal. In other words, even with full information, decision makers can choose the worse outcome. President Lyndon Johnson poured massive resources into Vietnam, “this bitch of a war,” because he had already poured earlier resources into the conflict. It was, as the common expression holds, throwing good money after bad. Senior officials like Robert McNamara realized that victory was not possible. Neither man did anything to stop the war. They believed the cost of “losing,” to them personally and to the United States, would be high and would be painful. The lesson, as behavioral economics holds, is that humans try to maximize preferable outcomes, but humans are also constrained by motivated reasoning, unconscious feelings, and a host of other psychological factors.

These insights are probabilistic. They indicate that no outcome is certain. Rather, outcomes are likely under given conditions. However, these insights also work in conjunction with rational models of human behavior. They support theories in which humans often behave in broadly predictable ways *once we understand their conditions and mental states*.

**Evolving Scholarship**

Implications of this work are clear. They are also difficult to apply to grand strategy. Political scientists and international relations scholars have long wrestled—often in career-defining debates—with how to balance intangible variables like beliefs, perceptions, culture, emotion and so forth with more concrete variables like rational optimization, institutions, wealth, and power.

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Results have been mixed but fruitful. Most relevant, here, are theoretical IR, foreign policy, security, and strategic studies. For instance, structural realists emerged in the 1960s and 1970s and offered a systematic, theory-based explanation for international relations. Inspired by economics, they argued for a limited set of premises: that states are self-interested and want to survive; that states are legally equal but vary based on capabilities; that no overarching international authority exists; and that states, for the first two reasons, cannot trust one another. Combining these assumptions with basic information about the particulars of who has what kind of power in the international system could allow scholars to explain and predict overall state behavior in that system. In this approach, state behavior is, on balance, rational. Or, to be more precise, state behavior fits the probabilistic constraints of a material system. States do not always follow the theory’s predictions, but more often than not, once we know a state’s power and how that power stacks up against other states, we can predict how it is likely to behave over time.

Missing from the structural realist account were human agency and domestic conditions. In other words, the people doing foreign policy the perceptions and beliefs that shape their thinking. The specific content of foreign policy and grand strategy.4 By the 1990s and into the new century, “neoclassical realists” were building a hybrid approach to compensate for this gap. Interested in helping policy makers apply academic theory, they integrated structural insights about the constraints of the international system with assumptions about human nature where those two met: foreign policy. Gideon Rose famously summarized this approach. It holds that both internal and external variables matter to explaining foreign policy. “The scope and ambition of country’s

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foreign policy,” he explained, “is driven first and foremost by its … relative material power capabilities.” However, “systemic pressures must be translated through intervening variables at the unit level.” Stated more simply, relative power drives most foreign policy, but relative power is interpreted by actual policy makers. These people make specific decisions. Leaders may be constrained by domestic politics and their own or group perceptions.

For many, this compromise effort proves unsatisfying. If we want to explain foreign policies, why not focus on the agents of decision and action themselves? In other words, the people. This is the argument of a wide array of approaches often collected under the heading “foreign policy analysis” (FPA). As realism arose, scholars like Columbia University professor Robert Jervis dissented from the pull of impersonal forces. To understand foreign policy behavior, Jervis wrote in his 1976 book *Perception and Misperception in International Politics*, observers should account for the human understanding of external and impersonal conditions. Often, Jervis argued, leaders tend to overestimate threats from adversaries and, in turn, drive exaggerated—sometimes dangerous—responses. This is a move from impersonal conditions to human psychology. And Jervis was not alone. As Texas A&M professor Valerie Hudson details in her 2007 book *Foreign Policy Analysis*, human minds and human agency remained a central concern among scholars for decades. Well-known figures working on these questions in the middle to late 20th century include Harold and Margaret Sprout, James Rosenau, Alexander George,

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Margaret G. Hermann, Ole Holsti, Kal Holsti, Yuen Khong, Deborah Welch Larson, Richard Hermann, and many others. In earlier eras, many writers argued that foreign affairs was really about diplomacy. Diplomacy, in turn, should really be about individuals and beliefs. This was not wrong. It was also incomplete and unsystematic. What these studies often lacked was a clear central organizing theory or framework. For instance, structural realism has remained prominent among IR scholars and students thanks in part to its simple-to-grasp framework. Challenged by this simplicity, Hudson points to decades of research on group decision making, comparative foreign policy, human psychology, and national-level conditions. She argues that, in fact, foreign policy analysis is really a single research agenda. It is an effort to account for human decision making in the context of both impersonal forces (like the balance of power) and individual perspectives (like perceptions). FPA also allows that many policy makers seek to behave rationally; however, that rationality is often compromised by phenomena such as psychological short cuts, cultural proclivities, and long-standing beliefs. In short, FPA provides a coherent focus on foreign policy decision makers. In turn, this allows scholars to study international relations with both flexibility and precision.

Amidst this spirit of innovation, a parallel school of thought emerged, an approach specifically targeting structural realism. This alternative was dubbed “constructivism,” and the problem it sought to address was a tendency among many scholars to explain international and political affairs in purely material terms. This is sometimes characterized as materialism or rationalism.

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9 Constructivism also critiques many versions of liberalism; however, I have set aside the liberal school of thought in this discussion for the sake of simplicity.
Realists, for instance, assume that observers can explain international affairs primarily—perhaps solely—based upon concrete and observable traits like economic and military power and policy makers’ attempts to maximize that power. The problem with this approach is that it ignores all the possible influences discussed above: perceptions, cultural understandings, and more. In fact, social expectations and repeated behaviors help to create norms. By “norms,” constructivists mean expectations about accepted behavior, and these norms are fundamental to how states interact in international affairs. Ideas, culture, and practices matter as much as economic capacity or military prowess. Indeed, economic capacity and military prowess can be applied in many ways; however, argue constructivists, we cannot understand or predict the specific ways in which power is used until we understand the norms surrounding it. Stated as an example, Americans worry far more about North Korean than British nuclear weapons based purely upon long-standing understandings and relationships. “Sovereignty,” argued Alexander Wendt in a famous article, “is what states make of it.” While sovereignty is very much real, it is real because all the relevant actors agree that it is real and important. In turn, it is not necessarily fixed. It can change over time based upon how it is applied, defended, and understood.

This book will not expand on the extensive debates surrounding constructivism. However, constructivism is necessary background to understand the book’s central argument. Specifically, constructivism demonstrates another avenue scholars use to navigate the influences and variables shaping state behavior and, for our purposes, grand strategy. Grand strategy may be a byproduct of the material facts of the international system. Or it may be the product of long-standing practices and beliefs. Or it may reflect an interaction of institutional and psychological factors.

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10 Citation. For other foundational constructivists, see Richard K. Ashley, Friedrich Kratchowil, Nicholas Onuf, Christian Reus-Smit, and John Ruggie.
Whatever its sources and influences, I argue that grand strategy is ready to be studied on its own terms as a unique, complex phenomenon.

Studies of war, peace and strategy is the other research area vital to our study of grand strategy. Really, this scholarship breaks into two areas: strategic studies and security studies. Scholar Joshua Rovner maintains footing in both areas, and he characterizes their shared agenda as “two approaches to the study of war and peace.”¹¹ Imagine you are tasked with analyzing World War I, and you have free reign to set your questions and approaches. What do you do? Do you focus on specific details to learn lessons, or do you try to apply and test abstract theories? Your answer likely tips you into one camp or the other.

Strategic studies is perhaps the older sibling, or at least the sibling steeped in history and the classics. Writers in this tradition often reference the ancient Chinese general Sun Tzu or the classical Greek historian Thucydides or the 19th century Prussian cavalry officer Clausewitz. More pointedly, work in strategic studies is historical. It seeks to observe the interaction of war technique and political agendas, and it uses those to draw lessons about effective strategy. Professional military strategists and academic historians most often work in this tradition. Their backgrounds and approaches allow them to draw insights about psychology and leadership as well as about broad historical trends and technological change. As historian Williamson Murray observers, “those who have been most successful at [practicing grand strategy] … have been willing to adapt to political, economic, and military conditions as they are rather than as they wish them to be.”¹² Crucially, Williams continues, “Perhaps the most important factor in the

development and execution of successful grand strategy has been leadership at the top.”

Examples of other work in this area include Paul Kennedy’s *Rise and Fall of the Great Powers*, Lawrence Freedman’s *Strategy*, John Lewis Gaddis’ *On Grand Strategy*, Russell Weigley’s *The American Way of War*, and Colin S. Gray’s *The Strategy Bridge*. To its detriment, however, strategic studies is also primarily focused on war, and as a field, it lacks central organizing theories or frameworks. In other words, it is not quite grand strategy, and its insights are not quite portable to other cases or categories. The midcentury strategic thinker Bernard Brodie recognized this limitation. In a 1949 article, he argued that “classical principles of strategy” are “too insubstantial.” Inspired by the social sciences, he called for a science of strategy, one studying how a state can maximize its resources for military preparedness and war.

Brodie was riding a wave, and he now saw the wider landscape. The general outlines of human behavior—studied for centuries from the trough between waves—could now be surveyed more clearly and systematically from the crest. Ever since, strategic studies scholars and professionals have wavered uncomfortably between strategy as wisdom or art and the modern demand for precision and underlying principles. They realize that policy makers require simplified takeaways and actionable points. For that reason, a “science of strategy” remains appealing. Indeed, business researchers and game theorists argue that they have isolated (quite different) versions of such a science. However, these approaches either tend to study business firms,

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13 Murray 2011, 21
which have a limited profit objective, or treat human motivation as a rational abstraction. For
strategic studies scholars, these approaches are fatally limited because they dismiss the rich
weight of history. Knowledge from careful study of history can serve as a guide to action and as
a constraint upon policy makers. Freedman closes his massive review of strategy deeply aware of
these tensions. His conclusion, however, is firm: “Strategy is art and not a science.” Done well,
it involves developing a sense of next steps as events unfold. Freedman also warns his readers to
be wary of stylized “stories,” or narratives that conveniently draw together clear cause-effect
relationships. Yet in his parting discussion, he also describes how humans rely on psychological
scripts and narratives to frame the actions. As I will argue, this is not surprising. Even if one
rejects strategy as science, a theory or framework of how strategies emerge and how people
interact with them is simply a matter of empirical observation.

Security studies, by contrast, is typically associated with political scientists. If not the younger
sibling, it is at least more comfortable with more recent analytical techniques and streamlined
theoretical models. Examples are extensive and cited throughout this text, but some of the more
general classic texts include Thomas Schelling’s *The Strategy of Conflict*, Kenneth Waltz’s
*Theory of International Politics*, Robert Axelrod’s *The Evolution of Cooperation* and Bruce
Bueno de Mesquita and David Lalman’s *War and Reason*. What these and other works share is
a concern for understanding the causes and outcomes (as well as the prevention) of war as
determined by a wide range of observable variables. For instance, in explaining Britain’s pre-

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Freedman 2013, 612

World War I changing security, Steven E. Lobell finds that psychological framing effects and prospect theory created the conditions for Britain’s ability and willingness to escalate its military commitments to the continent.19 Such studies also extend to deterrence and diplomatic strategies, both of which—notably—occur far more frequently than war. Lantis, for example, demonstrates how a “culturist turn” in security studies “provides a bridge between material and ideational explanation of state behavior.”20 He observes several studies of Australia, China, and several other Pacific rim countries. In those cases, security policies emerged from an interaction effect. The international structure and the state’s cultural predispositions were interpreted within the framework of long-standing historical narratives and political-military cultures. Of course, some approaches in this field oversimplify strategy by focusing narrowly upon rational calculation, military capacity and war motivations.

Now, reconsider the question at the top of this discussion: how to study World War I. If your impulse is to dig into documents, details of tactics and strategies, and leaders themselves, you are thinking like a strategic studies scholar. If your impulse is to develop a theory, isolate the key variables, and draw findings that apply across cases and between wars, you may be doing security studies.

As argued throughout this chapter, these approaches tend to study grand strategy as the byproduct of other phenomena, such as foreign policy decision making or international systems. Or they do little to separate actual grand strategy behavior from desired outcomes. They display

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no coherent study of grand strategy as a distinct phenomenon. There is no “grand strategy analysis.” Yet as I argue below, such a research agenda is emerging.

Grand Strategy Analysis

One of the more comprehensive analyses of grand strategy was penned by the late scholar and former national security official William C. Martel. His major work canvassed ancient and modern texts on foreign policy. It exhaustively cited major definitions. Martel argued that what we now call ‘grand strategy’ is a “broad consensus on the state’s goals and the means by which to put them into practice.”21 Grand strategy is a consensus about state interests and how to achieve them. Martel then traced this concept across the history of American foreign policy. His conclusion? “Only grand strategy provides the broad vision that helps policy makers conduct foreign policy.” Unfortunately, his argument continued, after the Cold War, the United States lacked a single vision or adversary. It “has been dangerously adrift for more than two decades.”22 Like most others in this grand strategy genre, it set out definitions and priorities, worked up a case study, and drew both diagnoses and prescriptions. The study ends with a set of recommendations for policy makers.

This is strategic studies developed to its fullest limits on grand strategy. Martel based his analysis in classic writings and then drew those insights forward in an historical case. The book focuses on policy makers who are free to shape and change grand strategy. Martel uses the word “theory” to describe the patterns of behavior that he observes, but as a set of lessons rather than formal propositions and predictions, what he developed is not quite theory the way most social

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22 Ibid. pp 351-2
scientists use the term. Martel’s approach was both too broad and too narrow. To simultaneously isolate a grand strategy, diagnose its problems, and build recommendations for leaders is necessary work. It is also general work. Actual policy makers may find that grand strategy raises an endless series of questions about target timeframes or domestic political sacrifices or military tradeoffs. In other words, even if it is thoroughly researched and defined, “grand strategy” may still leave observers with no clear idea about either the phenomenon or its application. The approach is also too narrow for merely seeking recommendations for effective grand strategy. Perhaps, for example, grand strategies are a phenomenon that can evolve a life of their own, a life outside any one set of leaders. Certainly US and Soviet strategies through the Cold War lived far longer than any one ruling regime. It also may be that leaders often cannot rationally or simply change grand strategy to make it better.

I highlight Martel’s work not because it is poorly done. In fact, it is quite thorough. Rather, I highlight it because even in the case of a former professional turned academic with an extensive historical study and an encyclopedic approach to grand strategy, the underlying approach has reached its limits. This is not unique to strategic studies or what I call the classic approach. As seen above, the theoretical and professional approaches also continue to produce real insights; yet, they are inherently limited. In the case of theory, grand strategy is rarely developed on its own terms as a phenomenon. Instead, it is often treated as the output of other variables, such as individual decision frameworks or international systemic constraints. Meanwhile, under time pressures, those working in the professional category often run with “what works,” which can easily shift from one crisis or policy or report or duty assignment to the next.

Still, as I argue below, new work illustrates how “grand strategy analysis” is emerging as a distinct field and how grand strategy observers can work within that field to make progress and
talk with rather than past one another. Realism, foreign policy analysis, constructivism, strategic and security studies—among other contributions—all shape grand strategy analysis. What emerges is a research agenda that looks something like behavioral economics, with its mix of classic theory and human psychology. Methodologically (in other words, how we study the topic), grand strategy analysis is deliberately patterned after foreign policy analysis.

An Emerging Research Program

Today, grand strategy scholarship bears the hallmarks of an independent research program. Academics and practitioners across different fields seek to understand it. They are producing an extensive and growing body of works focused primarily on grand strategy. The study and analyze grand strategy as a distinct phenomenon, one that can be defined and identified separately from other phenomena. They may even interact professionally, yet this work remains tracked into silos. The traditionalists, theorists, and professionals—as described above—have different agendas and different analytical frameworks. Their work is not quite cumulative beyond their own research streams. In short, a grand strategy research program has not quite coalesced.

Too often, says scholar Nina Silove in a pivotal study, scholars use the grand strategy concept loosely. They invoke it as a familiar concept, but they are not actually testing it as a dependent variable or as a distinctive phenomenon. Rather, they are focused on defining the term in ways that suit some other analytical agenda. Most debate about the concept itself, she says, “can be described as semantic.” Is grand strategy a fixed plan or a blind process? Is it an illusion or an empirical reality? Citing the well-known political scientist Gary Goertz, Silove observes that

debates about defining grand strategy prove sterile because they circle around who defines the language and how they define it rather than how to study the phenomenon itself. Imagine, for example, members of a police department debating how to define a street riot. Some insist that a riot cannot exist without ringleaders or at least a clear agenda. Others are more worried about the difference between civil disobedience and a riot. Beat cops, meanwhile, just want to know how to stay safe and clear the street. Can they define members of the riot as violent criminals? You may notice, however, that actual riot formation and evolution of riots themselves is lost in this debate. Everyone agrees that riots exist. Yet this understaffed police department is debating definitions rather than the phenomenon itself. In a similar way, the grand strategy phenomenon seems to exist. Trying to observe it on its own terms, however, is challenging. Grand strategies are seen as shorthand for other issues, such as a framework for specific historical analysis (i.e., why did that particular grand strategy emerge) or a set of prescriptions for national security (i.e., how do we shape the next grand strategy). This are important questions, but a well-rounded research program will also include studies of the phenomenon itself and how it behaves as an empirical or ideational phenomenon (i.e., why do grand strategies emerge in a certain form and how do they behave once under way). Just as our police department appears to lack a specialist devoted primarily to riots, the nascent grand strategy field involves observers from different traditions with cross-cutting agendas; however, they have yet to articulate a single or coherent common ground.

Silove is seeking to begin the process of “develop[ing] a theory of the concept of grand strategy.” She finds that grand strategy typically refers to one of three different types of phenomena. The first is grand plans. These are “deliberate, detailed plan[s] devised by individuals.” It is the scenario in which leaders devise a specific set of goals and steps to reach
them, and then they basically follow that plan.\textsuperscript{24} Second, \textit{grand principles} are an underlying set of ideas. Here, the scholar assumes that grand strategy is held in peoples’ minds but is really about a general set of priorities and goals rather than a limited plan. Third, \textit{grand behavior} is simply a pattern of state activity. Here, whether policy makers choose a grand strategy or explicitly hold one in their minds is not important. What matters is that, like the Roman or Han Empires or any other historical and current polity, the state displays a consistent set of strategic behaviors. What Silove suggests and this book argues is that, taken together, these approaches represent a proto-research program. They are different aspects of a single phenomenon.

\textbf{Current Work}

Other recent works exemplify these treatments of grand strategy. They also demonstrate how a “grand strategy analysis” research program is possible. Each advances our understanding of how grand strategies emerge, function, or change. On their own, however, with no ties to a general grand strategy theory or research program, these works represent a circular debate. Applying slightly different definitions, slightly different theories, and weighing priorities for a “good” grand strategy in slightly different ways, they make arguments that the reader cannot directly compare with alternatives. They also vary regarding agency and structure. Who actually makes grand strategy? Are leaders free to shape and change it as they see fit? Or does grand strategy

\textsuperscript{24} For details on these concepts, see Silove 2018, 29-44 and 49. Her summaries of each are quoted here. \textit{Grand plans}: “the detailed product of the deliberate efforts of individuals to translate a state’s interests into specific long-term goals, establish orders of priority between those goals, and consider all spheres of statecraft (military, diplomatic, and economic) in the process of identifying the means by which to achieve them. Given their level of detail, grand plans are likely to be—but are not necessarily—set down in written documents.” \textit{Grand principles}: “overarching ideas that are consciously held by individuals about the long-term goals that the state should prioritize and the military, diplomatic, and/or economic means that ought to be mobilized in pursuit of those goals. They tend to be expressed in single words or short phrases.” \textit{Grand Behavior}: “long-term pattern in a state’s distribution and employment of its military, diplomatic, and economic resources toward ends. In this context, the ends that receive the greatest relative resources can be deemed to be priorities, but the concept implies no inference that those ends were necessarily prioritized as a result of a grand plan, a grand principle, or any other factor”
emerge from domestic and international factors no matter the intentions and positions of individual policy makers?

Barry Posen, a political scientist at the Massachusetts Institute of Technology, created one of the most enduring definitions of grand strategy: it is a means/ends calculation, or a “state’s theory about how to produce security for itself.” When done effectively, grand strategy subjects military power to “the discipline of political analysis.” More recently, in his book Restraint, he argues that for decades the United States has pursued a grand strategy of “liberal hegemony.” Unfortunately, he argues, liberal hegemony is inherently expansionist and therefore unsustainable and unstable. Grand strategy, under this framework, is the output of policy maker calculations, national politics, and military conditions. It also can assume a life of its own and persist, in Posen’s view, even when it has grown ineffective and self-destructive. In another example, Patrick Porter writing in the journal International Security finds that US grand strategy has remained relatively stable for decades. Why? Porter points to what he calls “habit.” He says foreign policy elites after World War II conformed to professional and social pressure. There emerged among these leaders only one acceptable strategy—primacy—and all debates happened within its parameters. This created self-reinforcing norms and self-censorship. Porter argues that this consensus has led to US overextension and should be reconsidered. For our purposes, the takeaway is that—perhaps in the vein of Silove’s “grand principles”—both Posen and Porter

27 Ibid., 44
28 Posen (2014, 22) specifically cites four driving causes or theories for the book: realism, nuclear weapons, nationalism and identity politics, and the “costly and not easily controlled” prospect of war.
find that grand strategy ideas persisted on their own separate from any one individual’s or
group’s control. Grand strategy exists as a distinct phenomenon.

In his 2014 book *What Good is Grand Strategy*, historian Hal Brands studies this same period
with a view of grand strategy as a “conceptual logic.” For Brands, grand strategy sets fixed
priorities that guide decision makers. Brands extended this work in 2018 by arguing that US
grand strategy has actually remained consistent for decades. He objects to arguments like
Porter’s and Posen’s that call for US retrenchment or offshore balancing. Instead, Brands
believes that the raw facts of US power, alliance systems, economic investments, and military
experience mean that US internationalism will remain robust. Again, while Brands’ agenda is to
draw lessons about effective grand strategy, his approach assumes grand strategy itself as a
phenomenon. Finally, in his ambitious book *On Grand Strategy*, John Lewis Gaddis flips this
approach. Gaddis is a Cold War historian and foundational figure in Yale’s well-known Brady-
Johnson Program in Grand Strategy. His goal is to study grand strategy as “common sense.” At
first glance, this approach appears to completely negate grand strategy as a concept. After all,
why study grand strategy if it is indistinguishable from informed opinion? What Gaddis means,
though, is that effective grand strategy requires a reservoir of knowledge, knowledge that will
help decision makers confront specific challenges with general strategies. It is a mode of
thinking about a specific topic. At its best, it is wisdom. This is probably the least empirical
formulation of grand strategy possible. Still, it recognizes the phenomenon on its own terms.
Grand strategy is something that leaders of polities do. They may do it poorly or well, but the

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p 16.
moment they devise beliefs or make decisions or respond to existing conditions that involve expansive or long term implications, they are doing grand strategy.

This work on grand strategy extends beyond the United States. This scholarship also typically treats grand strategy as an empirical phenomenon. Hal Brands’ own work on Brazilian grand strategy under President Luiz Inácio Lula da Silva, for instance, shows a leader consciously driving strategy. Lula (as he is commonly known) worked with Brazil’s growing economy and built diplomatic bridges while the United States was distracted by the global war on terror.33 Yakov Berger, meanwhile, observes that Chinese professionals and researchers use “grand strategy” in its narrower form to refer to war making capabilities. Yet Chinese planners also develop their strategic thinking in direct response to developments in American strategic thought.34 Reaching further back, A. Wess Mitchell assembles a detailed history in his book *Grand Strategy of the Habsburg Empire*. It is not a work of explicit theory; yet, Mitchell’s (ix-x) underlying argument is that geography and resources shape and constrain grand strategic options. In other words, grand strategies exist and can be predicted, or at least explained. They are like other social phenomena, such as social revolutions, commercial trade, kinship practices, or party formation. They tend to emerge predictably under certain social conditions. Surrounded by other great powers, the Hapsburgs had to make hard choices about alliance, economic, and military commitments. Constrained by international and domestic conditions, leaders made early strategic

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decisions and once in place those investments were difficult to change. Elsewhere, political scientist Aaron M. Zack revives the insights of midcentury German historian Ludwig Dehio. In this framework, behavior in international systems depends on the potential of a state to draw external power into the system. Grand strategy, then, is a set of options constrained within or channeled by a government’s geopolitical environment. In a similar fashion, Geoffrey Parker’s *The Grand Strategy of Philip II* and John P. LeDonne’s *The Grand Strategy of the Russian Empire* also make the historian’s case for grand strategy. Both agree that leaders can and do hold broad strategic visions in their minds. They also implement them. Philips argues that “a ‘global’ strategic vision clearly underlay … initiatives undertaken by [Philip II’s] government.”

In these and other studies, patterns of strategic thought and behavior emerge over time. Overall, these patterns of strategic thought exist in leaders’ minds, but they often persist beyond any one leader. They are constrained by geopolitical realities. They may be justified by cultural agendas. In other words, grand strategies are observable, and they are comprised of an overlap between long-standing ideas, individual prerogatives, and material conditions.

Perhaps true to form, political scientists prove far more comfortable than historians with explicitly studying grand strategy as a phenomenon. In *Harmony and War*, for instance, Yuan-

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35 Political scientists call this series of narrowing decision options “path dependency.” For example, Manjeet Pardesi finds that leaders of early post-colonial India inherited and applied strategic views of their region originally developed by the British. Pardesi, Manjeet S. “Order in South Asia and the Indian Ocean Region: Indian Hegemony or Indian Primacy?” in A. Chong, ed. *International Security in the Asia-Pacific* London: Palgrave Macmillan. p 211.


Kang Wang applies a structural realist framework to the Song and Ming grand strategies.\(^{39}\) Not surprisingly, he finds that despite highly visible Confucian antimilitarist values, these empires displayed consistent grand strategies driven by traditional realpolitik calculations. Scholars studying strategic culture disagree with this realist framing. For them, security policies emerge from the opposite direction. Domestic historical narratives about a country’s role in the world shape how elites and the public interpret outside threats.\(^{40}\) Meanwhile, still other scholarship continues the long tradition of evaluating how policy makers can or should shape strategy.

Should the United States pursue deep engagement or offshore balancing? Why do governments like Iran and Russia espouse strategies that both challenge and engage the existing international system? What lessons can a rising China learn from earlier German and US grand strategies?\(^{41}\) In one exemplary study, Julian Germann studies West German grand strategy in the 1970s. He finds that leaders developed an economic approach that addressed domestic political challenges while integrating with the US-led economic order.\(^{42}\) Elsewhere, Christopher J. Fettweis argues that—despite arguments to the contrary—America’s post-Cold War allies are not passively freeriding on US security guarantees. Rather, they have developed “active, coherent, logical, rational grand strategies” to match a world with few serious threats.\(^{43}\) Again, overall, such work

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tends to emphasize grand strategy as fixed and seeks to probe its origins and effectiveness. Whatever their disagreements, these studies demonstrate the plausibility and substance of a grand strategy research program.

Another research area emphasizes how and when grand strategy changes. After 9/11 and particularly the 2003 US invasion of Iraq, the post-Cold War world was jolted. Leaders and academics around the world—though particularly in the United States—had spent a decade debating the nature of the post-Cold War World. Suddenly, the George W. Bush administration’s aggressive “war on terror” appeared to many observers to be breaking with long-standing US grand strategy. There emerged a wave of studies on grand strategy change. Is such dramatic change possible? When is grand strategy change likely? For instance, Jeffrey’s Legro’s 2005 book *Rethinking the World* focuses on widely-held foreign policy ideas. He argues that overarching strategic consensus only changes in rare situations. Specifically, when (1) an old idea is widely considered to have failed and (2) a single new idea pushed by an effective policy entrepreneur is available.44 With a more psychological focus, David Welch finds that major changes are rare because leaders, like all people, are motivated in part by loss aversion.45 By contrast, Kevin Narizny emphasizes domestic economic interests. Grand strategies shift, he says, depending upon the party or regime that holds office.46 Colin Dueck also maintains that grand strategies can and often are “adjusted.” However, rather than economic interest, strategic culture shapes the options available to decision makers.47 More recently, William R. Thompson argues

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that all this research struggles to balance “the interaction among ideas, external structures and developments, and domestic politics.” He proposes a solution. In his theory, major strategic changes grow more likely and more durable as certain variables—such as shocks and political entrepreneurship—experience changes. Finally, at the individual level, Popescu makes the case for “emergent strategy.” This framework returns us to the psychological insights of behavioral economics and the actor-centric focus of FPA. Popescu holds that many of the most effective strategies rely upon “emergent learning” rather than fixed plans. Under this framework, grand strategy change is incremental and is derived from a small group of elite decision makers. Clearly, scholars disagree about how easily or how frequently grand strategy can change, but they agree that the project of studying grand strategy change is vital.

In sum, scholarship on grand strategy is converging. It shows a distinct phenomenon that can be coherently and systematically studied. Still, approaches to studying and understanding grand strategy vary widely. Different fields and traditions struggle to communicate with one another or develop sustained or cumulative mutual growth. Grand Strategy Analysis offers a simple framework to draw these approaches together. It focuses on the individuals and groups who make and implement grand strategy, but it is agnostic about whether individual-, state-, or system-level factors drive grand strategy. It is also agnostic about whether grand strategies should be understood primarily as plans, principles or behaviors. Rather, it allows analysis and comparison amongst these understandings.

GSA and the Trump Administration

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GSA is a vital tool to analyze Donald J. Trump and his presidential administration. Throughout the two years this book covers, Trump himself displayed every indication that he wanted to break old patterns. Critics claimed that he was mercurial and lacked focus, to say nothing of implementing coherent strategic thought. Raw psychological needs, they said, drove all his decisions. Supporters pointed to Trump’s unexpected successes. Clearly, they countered, he is a rational deal maker. Meanwhile, journalists highlighted an increasing tendency for Trump’s senior officials to publicly commit to conventional foreign policy positions even as Trump himself picked fights with allies like Canada and praised the good sense of leaders like Russia’s Vladimir Putin. Players like Secretary of Defense Jim Mattis or UN Ambassador Nikki Haley appeared to make calls from a traditional Republican play book rather than jump with Trump’s every Tweet. In the international system, meanwhile, most leaders remained coy about their views of Trump and American grand strategy. Major issues like nonproliferation or the civil war in Syria or international trade continued to churn; yet no major realignments or crises emerged. Was US grand strategy basically stable and consistent despite Trump’s confrontational demeanor? How can we assess the relationship between individual psychology, bureaucratic priorities, national politics, and international structures?

Based on existing research, one possible grand strategy prediction is that Trump would have little impact on US grand strategy. After all, like ocean liners, grand strategies are hard to change course. They are determined by a mix of external and internal conditions. They are also held in the minds of many more people in and outside a given government. In other words, they take on a life beyond the control of any one president. In Silove’s parlance, we talking about grand principles or—perhaps more likely—grand behavior.
Based on a different set of assumptions, we could also predict the opposite. Because they are held in people’s minds, grand strategies can change with the individuals in power. As Hudson herself emphasizes, the “ground” of international relations is the people making foreign policy decisions. In the US system, presidents wield extensive executive power to change policies and use the “bully pulpit” to shape public and political opinion. In this approach, grand strategy is what policy leaders make of it. For the Trump administration, the president’s unique psychology itself may overwhelm others’ planning. Again, using Silove’s categories, we are talking about grand plans or possibly—grand principles.

GSA is well positioned to evaluate these competing accounts. “Do leaders matter?” is an old debate. For millennia, informed observers assumed that, yes, of course they do. But in the spirit of skepticism, nineteenth and twentieth century thinkers questioned conventional wisdom. After all, governments and practices like war persist for far longer than any one leader or group. Skeptics will concede that leaders may matter but only within clear parameters, or maybe even not at all. From that vantage, systems and institutions and cultures constrain or shape individual prerogatives. Individual decisions are the byproducts of larger structures. But just as assuming that individuals are the only units that matter to grand strategy, assuming that only structures matter can also bias one analysis. To illustrate, imagine a biologist and a geologist set out on a hike together. They cover the terrain and see some plants and animals. Later, a student asks what they saw and what is important to know about the ecosystem. Naturally, they will agree on the basics, but it is not hard to imagine that they will also give very different answers. They each carry their own assumptions about what is most important to understand the world.
Hudson argues that the real question in foreign policy analysis should really be “when do leaders matter” and “which leaders matter?” She observes that a number of variables matter: regime type; the leader’s interest in foreign policy; presence of a crisis situation; ambiguous or uncertain situations; degree of diplomatic training; leadership style; and group dynamics. In another influential study, political psychologists found that personality mattered to foreign policy outcomes. That influence depended on several conditions: the actor’s location in the political system; a situation that includes many elements open to restructuring; a delicate balance between internal and external forces; and an issue that requires active effort rather than routine role performance.

Studying grand strategy based on the impacts of individuals or domestic systems or international structures is important and offers insights. However, any one of those approaches can also bias the answer to a simple question, “What matters the most to the shape of grand strategy?” In contrast, GSA analyzes individuals and their decision making, but as a framework, it does not assume that those individuals alone drive grand strategy. Rather, individuals articulate and implement grand strategy. They are the agents of grand strategy. They are the basic units of analysis to understand grand strategy. A researcher may find, for example, that while individuals may believe that they alone drive a government’s security strategy, they are in fact profoundly constrained by prior strategic commitments, national bureaucracies and the current balance of power. Thus, GSA at its best is not prejudiced against either stability or change, structures or agents. Rather, it allows us to evaluate a number of possible variables and their different impacts.

50 Hudson 2007, 37-39
All this is useful to study the Trump administration. Trump himself seems to scramble our usual expectations about grand strategy. Where strategists might be careful and forward-thinking, Trump spent his first two years rushing into immediate, actionable issues. Where strategists might think about the world as a set of interlocking systems, Trump remained focused on one-off deals and individual state sovereignty. Where strategy must be executed in the context of an international structure, Trump’s temperament and loose management style kept his own personality close to the center of all policy planning. Is grand strategy really a viable concept under these conditions? Perhaps Trump’s first two years in office put the lie to our desire to see overarching order in foreign affairs.

In addition, public and professional debate over Trump’s first two years generated an important set of substantive questions.

- Was Trump trying to collapse the liberal internationalist, postwar order?
- Is nationalism a viable alternative as a grand strategy?
- Is he—or any head of government—alone sufficient to achieve such a change?
- Is “strategic ambiguity” (as one Trump supporter dubbed the president’s approach) Trump’s actual, carefully-considered approach?
- Is grand strategy, in any administration, relatively stable or ever-changing?

**An Actor-Centric Approach**

To untangle this braided set of topics, I propose two, simple analytical questions for Trump’s first two White House years. Each is derived from a simple causal question: why did US grand strategy take the shape that it did during 2017-18?

Q1: What was the Trump administration’s grand strategy?
Q2: How much did Trump himself shape that grand strategy?

This study is a “proof of concept” for GSA. It will apply the actor-centric approach set out above to explain grand strategy outcomes and answer those two analytical questions. Such study will then allow us to return to the general questions about Trump’s overall effect on grand strategy. The observations will be three case studies. They are chronological (rather than thematic) and divide the administration’s first two years of development into three roughly equal time periods. In each, I will focus on the most significant foreign policy, security, and strategic questions that the president and his senior officials are deliberating and on which they stake out policy positions. Picture a Venn diagram with two circles. One is “statements and debate” and the other is “policy positions.” We will be seeking their overlap. This allows us to include all three of grand plans, grand principles and grand behavior.

The methods will be simple. To answer Q1, we will apply a diagnostic rubric with three characteristics. This is a framework I have developed elsewhere.52 Sufficient to say here is that it is distilled from the extensive scholarship that seeks to define and then assess grand strategy. It is a summation of the most common characteristics used to identify a grand strategy. In each case study, we will seek to observe the administration’s position on each dimension.

Scope The geographic and conceptual extent of the strategy. What areas are considered relevant to nationals security? Who are allies, and who are adversaries?

Substance The content of the grand strategy. What are the stated core interests as well as the core interests as revealed by policy and other commitments? Is there a guiding ideology?

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**Assertiveness** The strategy’s relative aggression or posture toward the rest of the world. On one end of the scale is full military force, and on the other is diplomacy and institutionalism.

These dimensions are by design open-ended. There is no fixed set of possible grand strategies. Rather, they seek to faithfully represent a grand strategy as it was practiced and understood by those involved. Simple comparison or generalizability is more challenging with this approach, but it also provides a set of standards that we could apply to any case at any point in history.

To answer Q2, we will observe grand strategy decisions, statements and changes below the presidential level. This will compare the president’s statements and claims with the actual policies and actions of US government officials. The goal is to observe the responsiveness of overall US grand strategy relative to the president himself. As a secondary goal, it can shed light on just how constrained or free the overall administration was with regard to changing grand strategy. To make these assessments, we will observe three correlations: a) Presidential statements versus actual policy actions and agency statements, b) Trump’s positions versus those of his principals and Republican party leaders. For example, on contentious issues, did Trump regularly overrule or ignore his National Security Council principals or party leaders? c) The gap between the United States’ official policy before heading into a change and the US policy after the change. In other words, regardless of whose positions prevailed, did the grand strategic policy change, and how much did it change?

**Conclusion**
Behavioral economics proved transformative simply by uniting idealized models with observed human behavior. In a similar fashion, grand strategy has long existed as an ideal type in the minds of academics and professionals. A growing body of scholarship puts flesh on these ideals. Among those who study the topic, there is no question: grand strategy is real and observable. Those researchers just cannot decide whether what they are observing is a form of classic wisdom, a theoretical and empirical phenomenon, or an actionable policy agenda. Does it exist only in the minds of policy makers or does it take on a life of its own?

Rather than directly tackle these debates, this paper proposes a simple framework that allows these traditions to interact with one another. Grand strategy analysis (GSA) draws upon foreign policy analysis to construct this analytical approach. At base, this approach is “actor-centric.” It assumes that individuals and small groups are the baseline units of analysis to understand a state’s grand strategy at any given point in time. This focus allows a wide range of inputs or causal variables, including psychology, ideas, institutions, domestic politics, and international structures. Rather than preference one over the others, it provides a shared language and analytical focus that researchers from, say, security studies and strategic studies can apply for mutual comprehension.

The paper ends with a research proposal. Donald Trump seems to pose a serious challenge to the notion of grand strategy. He is brash and his pronouncements are often contradictory. He also promises a fundamental break with existing American approaches to the world. This case poses an ideal challenge for GSA. It raises the specter that grand strategies are really just illusions of order we impose on a stream of events, but it also raises the possibility that a given leader is, in fact, unable to fundamentally change his state’s grand strategy. I propose dividing Trump’s first two years into three case studies that are focused on statements and policies around the most
salient security issues that have emerged from Trump and his senior officials. We can then interrogate what is the grand strategy as well as the relative impact of the president himself. Again, the GSA approach does not necessarily bias these inquiries. Rather, it helps organize them around the actors involved. This approach will also be accessible to political scientists and historians, policy makers and students. However Trump’s grand strategy may (or may not) emerge from this study, the analysis itself will demonstrate the utility—and necessity—of GSA.