Explaining peace and catastrophic competition during great power rivalries

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1 Introduction

Since 1850, the world has seen 256 inter-state rivalries, culminating in 411 militarized crises, 164 sanctions episodes, and 126 interstate wars. There are many cases of competition that scholars *could* study. But since Carr (1945), scholars have justifiably obsessed over 15 cases. Most books are motivated by the World Wars, the Cold War, the Anglo-American peace, and the Russo-Japanese conflict. Case studies into the causes of great power competition also focus on the Great Game, the Napoleonic wars, and NATO enlargement (i.e Germany's decision to remain peaceful) at the end of the Cold War.

Scholars focus on these cases for three reasons. First, they are important. They involve powerful states that make repeated, and interconnected choices over several years. They end in either the most devastating events in human history, or enormous territorial transfers that determine who governs hundreds of millions of people.

Second, they are policy relevant. Recent concerns over China have brought great power competition to the forefront of US national security policy.[1] Policy-makers want to know: why have Sino-American relations unfold the way that they have, and how will they unfold over the next three decades? American Policy-makers believe that Sino-American relations share similar features to past cases of great power rivalries. As a result, some worry that Sino-American relations will devolve into competition even though China may hold limited aims. Others fear that the United States will facilitate a powerful and exacting adversary by failing to compete at the right time. They want social scientists to explain instances and timing of competition across historical cases

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[1] They also dominated throughout the Cold War and in the 1990s when the Soviet Union collapsed.

so that they can better prepare to engage China no matter how the interaction unfolds.

Third, these cases are difficult to explain using the tools of rationalist social science. After a century of theory building, we should have an internally consistent theoretical framework that explains why some cases end in competition and the rest end in peace. That internally consistent theory should also be able to explain the timing of competition within each case to a reasonable degree of accuracy. However, the two dominant rationalist frameworks in international security—structural realism, and the bargaining framework—struggle to explain the instances and timing of competition in many of these cases.[2] Scholars have spent decades building more specific theories on top of these two frameworks. Thus, it is hard to summarize every nuance in a single paragraph.[3] But both realism and bargaining generate a handful of core predictions that serve as the basis for subsequent work. As applied to enduring great power rivalries, the bargaining framework emphasizes how shifting power can generate a commitment problem in which states cannot find a peaceful settlement today given their expectations about how future negotiations will unfold. As a result, bargaining theories predict that great power rivalries will end in war if power shifts rapidly in one states favor. Realism emphasizes a security dilemma that is driven by two-sided uncertainty about long-term intentions.

A quick review of the historical record suggests that the variables at the core of these theories—military power, territorial demands, and beliefs about the Challenger's motives—play an important role in many cases. But the devil is in the details. A closer look at cases shows that the way these variables influence state behavior does not perfectly align with what either framework expects. For example, both of these frameworks clearly predict competition between the United States and Britain in the 1800s. In 1823 president Monroe declared that the United States would remove European powers from the Americas over the next century. This demand for 20% of the world's land mass was followed by one of the fastest rates of economic growth in human history, rapid militarization and several militarized crises between the United States and Britain. But competition never came. Instead, the British conceded at Venezuela, then Alaska. Finally, United States and Britain forged a special friendship in 1907, leading to a long period of peace (Schake, 2017). Why didn't the United States' rapid rise and violent demands trigger competition, and how did it lead

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[2] As I detail later, these theories well explain crisis episodes, or other interactions. However, they struggle with long, interconnected great power rivalries.

[3] In chapter ?? I provide a comprehensive review of these frameworks.

to a lasting peace?

Even when these overarching frameworks correctly predict a case will end in competition, they often struggle to explain the timing of competition in that case. For example, British elites watched for 6 years as Hitler rapidly militarized, orchestrated the assassination of the Austrian Chancellor, broke several treaties and arms control commitments, and violently took territory. During this period, three consecutive British prime ministers were optimistic that long-run cooperation was possible. Competition did come. But it came after a relatively minor episode that involved no threats of force—Hitler violated the Munich Agreement—and at a point when Hitler's rate of militarization was relatively slow, and power was still unequal (Wark, 1985).

In yet another example, the Western Allies cooperated with Stalin for years at the onset of the Cold War. Following a week-long diplomatic meeting at Yalta, Churchill concluded that "poor Neville Chamberlain believed he could trust Hitler. He was wrong. But I don't think I'm wrong about Stalin."[4] With Hitler fresh in their minds, British and American elites remained optimistic that long-term cooperation was possible as Stalin defied the Yalta Accord over Poland, and orchestrated a coup in Romania. The western allies realized that Stalin's aims were vast following the Iran Crisis (1946). This inference drove the Western Allies to start the Cold War. Interestingly, British and American assessments of Soviet power remained constant across the entire period.

Based on these discrepancies, Glaser (2010) goes as far to call his rationalist theory normative. In his view, he explains how great powers *should* behave if they are rational. But he also accepts that states do not behave that way in most cases. There is now an enormous secondary literature that identifies a case that does not conform to rationalist predictions, then seeks to explain this non-conforming behavior as a mistake brought on by a unique configuration of individual misperceptions, or errors in aggregating beliefs.[5]

If these explanations are correct, it is difficult for US policy-makers to use social science to formulate their China policy. The reason is that social scientists know what behavior best serves the national interest (i.e. is rational). But they also acknowledge that political elites through history have systematically fail to follow that optimal behavior. Instead, in the most important

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[4]Nicholson Diaries, 27 Feb 1945. Discussion about Soviet strategic intentions.

[5]For example, Holmes (2013) explains misplaced Anglo-Soviet trust; Yarhi-Milo (2014) explains misplaced Anglo-German trust using such mechanisms. Critical to their theory are specific diplomatic interactions between specific elites (i.e. hold specific psychological profiles) from two different countries, and then specific institutional configurations inside countries that allow these misplaced beliefs to aggregate.

cases, elites depart from the best rationalist behavior even after we account for their individual-level foreign policy preferences (e.g. Britain delayed competition with Hitler and Stalin). What is more, claims that leaders make specific mistakes in specific cases are unlikely to generalize across cases because elite configurations are unique. Thus, we cannot well predict how, when, and why the United States will deviate from the best China policy. All we can know from observing the historical pattern is that our foreign policy decision-makers, no matter who they are, are likely to deviate from the policy that best serves their interpretation of our national interests.

An alternative position is that the core frameworks—realism and bargaining—are not a good place to start when we theorize about these 15 or so great power rivalries. It is possible that states and their leaders do make rational choices that serve the national interest; but scholars have not specified the most salient features of these strategic interactions.

1.1 Summary of Argument and Findings

In this book, I develop a new framework to study long-term great power rivalries. My assumptions are meant to stylistically represent Sino-American (1990-present), Anglo-American (1850-1907), Anglo-German (1932-1939), Anglo and American-Soviet (1940-1947), Sino-Soviet (1960s) relations and other long-term great power rivalries that scholars have wondered about since Carr. My framework is not adapted from economic models. Rather I generated my assumptions by asking real-world national security elites about the actions and beliefs that they seriously consider when they face a new great power rivals. My assumptions are a mathematical representation of their answers. Thus, my framework represents the choices and beliefs that are on the minds of national security elites as they reckon with long-term threats to their intrinsic interests.

The big difference between my framework and realism and bargaining is that I dramatically simplify the set of choices that I allow states to make. As a result, I pay much less attention to the role of power, haggling over marginal changes to the size of concessions, violent demands, and militarization as dominant causal variables in explaining competition and peace.[6] Rather, I make very realistic assumptions about state-motives. I assume that states are uncertain about each others strategic intentions, and explore how this uncertainty influences patterns of competition and peace.

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[6]Some of these variables appear, I just include simpler representations.

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Specifically, I theorize that a Challenger's specific foreign policy objectives depend on a combination of their intrinsic foreign policy motives—which I call a principle—and their historical context. For example, states that prioritize ethnic-nationalism (one principle) covet different concessions than states that prioritize security, prestige or uniting historical borders. Depending on the principle, the specific territories that they care about the most depend on their history and culture. States that want to restore their borders care about territories they historically controlled. Rather than focus on one principle at a time (such as security and greed), I assume that Challengers can be motivated by one of many principles, and each implies that the Challenger holds a specific set of core interests. I assume that Defenders are uncertain about the Challenger's true principle. Thus, they do not know which, and how many, core interests the Challenger holds. However, Defenders have a lot of information about the Challenger's history and culture, and can use that information to learn the Challenger's true principle over time.

By appreciating that states care about more than simply security or greed, and that there are many different reasons that states can hold limited aims, I illuminate new information problems, and new mechanism for learning in world politics. I call these mechanisms for learning *qualitative inferences*. They are *qualitative* because Defenders update their beliefs about the Challenger's motives based on how specific demands (a demand for Austria, not Poland) correspond to a principle, and not the scope of these demands or rates of militarization (a demand for half, not three quarters of Austria).

For example, American policy-makers believe that inferring "Chinese intentions is the single most difficult and important task we face."[7] For decades, the CIA could not assess China's long-term motives with high confidence. However, they could easily assess that Taiwan provided China with enormous historical and nationalistic benefits but few security benefits.[8] When China first intervened in Taiwan, the CIA made inferences about China's long-term motives based on its understanding of what principles could drive China to contest Taiwan.[9] From this inference, the CIA extrapolated to assess other territories China would and would not contest in the future.[10] More recent assessments are classified. But elite interviews confirm that as China took other

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[7]Quote from interview with former CIA Director for Analysis, Mark Lowenthal.
[8]Special National Intelligence Estimate (100-12-58) *Probable Developments in the Taiwan Straits Crisis,* p1.
[9]SNIE (100-4-59) *Chinese Communist Intentions and Probable Courses of Action in the Taiwan Strait Area*
[10]SNIE (13-3-61) *Chinese Communist Capabilities and Intentions in the Far East.*

specific actions (made demands in Tibet, published Defense White Papers, built One Belt One Road, etc) the CIA pieced together what principle China prioritized by examining how these actions fit together. I argue that qualitative features of China's actions (e.g. where they associated with China's historical claims, and therefore imply that China is really motivated by restoring its historical position) matter more than how often China fought, or how quickly it militarized, or how many demands it makes at once (the focus of the existing literature).

In the strategic context of a great power rivalry, qualitative inferences expose new insights about patterns of competition and peace. Drawing from an analysis of several formal models, my theory shows that rivalries unfold in one of two ways: delayed competition or delayed peace. A detailed look at my mechanism explains four of the most puzzling aspects of great power rivalries: (1) why Defenders are reassured by the Challenger's early diplomatic promises that the Challenger's motives are limited; (2) why Defenders are willing to make many repeated concessions even as the Challenger makes violent territorial demands, rapidly militarizes, or seeks to undermine the Defender in other ways; (3) punctuated (rather than gradual) changes in Defenders' beliefs about the Challenger's strategic motives; and (4) why some cases end in competition following a specific demand while others end in a stable peace.

The mechanism works as follows. Early on, Challengers with expansive and limited aims face different incentives. Challengers with expansive aims value many issues, thus they care less about which concessions they receive first. However, they want to under-state the scope of their motives to avoid competition for as long as they can. Challengers with limited aims each value a different (and specific) set of issues. They want to coordinate to receive valuable concessions first. Unlike past coordination theories (Trager, 2011), the Challenger's diplomacy reveals a principle (e.g. Nationalism) that motivates her territorial demands, and not a list of territories she claims to care about (Taiwan, Tibet, Uganda). Because diplomacy appeals to a reason for revision, it sets an expectation about the total set of territories that the Challenger will want as the interaction unfolds. By locking in this expectation, Challengers can exploit diplomacy to partially overcome incentives to under-state the scope of their motives for many periods.[11]

How the Challenger justifies her demands at the beginning has important implications for patterns of competition later on. Each principle implies that the Challenger cares about specific

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[11]The lock-in is strategic. There is no cost from norms (Goddard, 2018), or audiences (Kertzer and Brutger, 2016).

territories. Once she has captured those territories, she either makes another demand and reveals her initial justification was dishonest, or accepts the status quo forever. Challengers with expansive aims make another demand and the Defender infers that their motives are more extensive than originally claimed. This revelation triggers competition.

Applying the logic to Anglo-German bargaining, Hitler used diplomacy to explain he was motivated by nationalism, and therefore would only demand Germanic territories. This message was costless, but it altered how the British interpreted Hitler's future behavior. The British did not update their beliefs following the Austria (1934) and Rhineland (1936) crises because the British knew that Germans lived in these territories, and realized that these demands were consistent with Hitler's declared motives. However, when they observed Hitler demand the Slavic parts of Czechoslovakia, they assessed that this was outside what Ethnic-nationalist Hitler would seek. Thus, they ruled out that Hitler would stop once he captured territories populated by Germans. This, realization drove them to competition.

This is not the only case that my theory well explains. My predictions about the instances and timing of competition better fit the universe of great power rivalries since 1850 than the core predictions that follow from the main two alternative theoretical frameworks—realism, and bargaining. Not only does my theory explain the instances and timing of competition in more cases, it explains the cases that realism and bargaining theory have struggled with the most.

When I examine elite deliberations in historical cases, I find that the unique features of my qualitative signaling mechanism closely match how states learn about their rivals, and this learning is directly associated with competition choices. Using detailed archival evidence and a survey experiment with with real-world national security elites, I show that perceptions that past scholars could explained in specific cases using cognitive and social forces are well explained by my rational argument.

1.2   Contributions to Research

My theory seeks a rationalist explanation for the over-arching strategic process that drive the instances and timing of competition across long-term great power rivalries. To do that, I must makes extreme simplifications about the choices states can make at a particular moment, and about the actors who are making these choices. In real life, the world is more complicated. My

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review of case material will illuminate important nuances at specific stages of these rivalries that are not explained by my core theory. These deviations are largely consistent with more specific theories about costly signaling, personal incentives, norms and values, or bureaucratic politics that impact how specific crises unfold. My claim is not that I can explain every thought and action for every policy-maker at every moment of each great power rivalry. Rather, I claim that my framework provides a better starting place than the bargaining framework or realism for these more specific theories to build upon.

In this way, my theory compliments a vast body of micro-research that identifies the actions that states can take to signal their preferences because it brings their predictions in line with puzzling cases. These theories tell us which actions can facilitate learning. My theory explains what we learn and when we learn it. For example, military signaling theories starts with the premise that militarization and taking territory is costly (e.g, Kydd, 2005). Therefore, arming and taking territory (or failing to do that) reveals information about a state's motives. Many study the strategic dynamics that surround this insight within the realist framework (e.g Glaser, 2010). This restricts these scholars to thinking about states that either want security or are otherwise greedy. Thus, these scholars conclude that states can under-arm and forego territorial demands to communicate security-seeker aims. However, if states make violent territorial demands or rapidly militarize they signal greedy aims. I do not dispute that military actions communicate information, or that Defenders look for these actions as they assess the Challenger's aims. My claim is that Defenders draw inferences from these actions in the context of what the Challenger is militarizing over, and how it relates to a specific principle—not just security. Applied to my framework, these military actions mean different things depending on the issue and territory that the action relates to.

This is not just an academic distinction. It has real world implications. As discussed above, the CIA assessed that Taiwan provides few security benefits for China. Thus, military signaling theories built on realism tell us that during the Taiwan Straits Crisis, China revealed that it was not motivated by security and therefore must be greedy.[12] This was not the view of US policy-makers. Shockingly, many inferred from China's violent demands that China likely held limited (rather than

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[12]Signaling theorists who are committed to realism use a variety of tortured arguments to reconcile this difference. Some try to explain that Taiwan has huge security benefits. Glaser argues that the US and China 'disagree' about the status quo. My solution better fits how policy-makers think and generalizes across more cases.

expansive) aims and that China cared intensely about these specific objectives. If we start with my framework, then it is easy to explain why China could rapidly militarize and attempt violent revision over an issue that has nothing to do with security, and still hold limited aims: they could also be motivated by restoring their borders. By starting with my framework, the predictions from military signaling scholars take on new life that better fits this case. The US draws inferences from China's decision to militarize depending on the context of what China is militarizing over, and how that fits with the specific principles that China could value.

My theory also compliments theories of bureaucratic or domestic politics who suggest state preferences are the aggregate of micro-preferences. For example, **?** argues that elite factions vary in their hawkishness. He suggests that there is a best (rational) assessment, and that elites deviate from it depending on their institutional affiliations or lived experiences. These deviations are not irrational. Rather, they are responses to individual level preferences, and training. I do not dispute that individuals and domestic institutions (like the military) may deviate from an optimal baseline. What I will show in my analysis of British elites that assess Stalin's motives is that my theory provides a better rational baseline than realism and the bargaining framework around which these deviations fall. Consistent with **?** I find that Foreign Office staff are often more optimistic than military staff about Stalin's motives. But I can also explain how both camps start off reasonably optimistic that cooperation is possible and then both revise their assessment that Stalin's aims are vast.

My theory also compliments psychological and normative theories that explain the sources of state preferences and resolve. After all, my theory celebrates the fact that states can hold different preferences for specific normative or instrumental principles. These preferences could come from the psychological attributes of leaders or communities, or inter-subjective norms and values. My theory advances these insights by illuminating a strategic framework that can celebrate the far-reaching strategic implications of these insights. My theory explains *how* to take preferences seriously (Moravcsik, 1997) by identifying one mechanism through which specific preferences translate into specific foreign policy objectives. This mechanism is based on information the Defender has about the Challenger's historical, cultural and strategic context.

One class of micro-theories stands out as incompatible with my theory. These are theories that

assume realism[13] well describes rational behavior during rivalries, notice that states do not behave this way, then use that as evidence that states do not behave rationally. Starting from this premise, these scholars argue that states do not behave rationally because they succumb to social or cognitive processes that cause them to deviate from rational behavior.[14] I single out these theories because they speak to a broader question about what we should do in the face of evidence that dis-confirms a core rationalist framework (e.g. realism). In their view, evidence that realism systematically fails to predict how states behave is evidence that states do not behave rationally but realism is the model that explains optimal state behavior. In my view, evidence that realism does not predict cases is evidence that realists have not correctly characterized what rational behavior looks like. However, states could still behave rationally.[15] In the empirical section, I not only support my rationalist explanation with evidence, I will provide evidence that misperceptions are not driving aggregated elite choices in real life.

Although I treat repeated bargaining models designed to study great power rivalries as one alternative explanation, my theory also compliments this framework by providing a clear statement of the conditions under which my theory and bargaining theory are most likely to explain how cases unfold. Most notably, Powell (1999) argues that rapidly shifting power creates an insurmountable commitment problem. This commitment problem can drive competition under complete or incomplete information. As I explain in the theory section, I accept that commitment problems provide a scope condition on my argument. If the Challenger could militarize at an extremely rapid rate, then the commitment problem would dominate my information-based mechanism. The question, then, is: which explanation—whether states can, or want to take territory—is the most powerful for explaining patterns of competition? As we shall see in the medium-n analysis, my motives-based explanation covers more cases. Furthermore, I can explain critical cases that have puzzled power-based scholars for a century. However, I also accept that my theory does not well explain two cases that are consistent with Powell (1999). Consistent with the scope conditions detailed in my theory section, and with Powell, these cases fall in the extreme ranges for rates of shifting

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[13]or bargaining framework, but usually these theories start with realism.

[14]To be clear, these do not include all behavioral theories. Many behavioral theories describe why states are more sensitive to the cost of fighting. As described above, my theory compliments sub-state theories that explain preference deviations. The theories that I quarrel with claim that states make mistakes in how they process information and form beliefs.

[15]Here I use the term rational to mean that they do not succumb to misperceptions in how they process information and form beliefs and that they, loosely speaking, make choices after weighing alternatives.

power at the beginning of the case.

1.3 Road map and one Sino-American policy implications

Section 1 develops my theoretical argument. Chapter 1 explains the strategic setting that I focus on (i.e. what is a great power rivalry), details my assumptions, and explains the core strategic tension in these rivalries. Chapter 2 explains how my assumptions are different from bargaining theory and realism. It then explains that the best way to evaluate my theory relative to these alternatives is based on how well my theory explains historical cases of great power rivalries. Chapter 3 develops a theory about the instances and timing of competition in great power rivalries. It also illuminates important features of qualitative inferences that provides a novel explanation for how intelligence analysts assess the motives of their adversaries and the role of effective diplomacy in a reassurance setting.

Section 2 evaluates my argument. Chapters 5 and 6 evaluate my causal mechanism. Chapter 5 reports evidence from a survey experiment with national security elites. The experiment simulates an intelligence estimate exercise where members of the US and Australian national security communities evaluate the motives of an emerging Challenger to national security. The vignette allows for the possibility that the Challenger is motivated by different principles, and randomly varies the Challenger's diplomatic statements and military interventions. Chapter 6 reports an analytic narrative of a critical case: British assessments of Soviet motives at the onset of the Cold War. Chapter 7 examines my predictions about patterns of war and peace across cases through a medium-n analysis of all great power rivalries since 1850.

Section 3 examines modern Sino-American relations. Chapter 8 analyzes US policy towards China through the lens of my theory from 1990 until now. I argue that my theory explains why the United States tried so hard to cooperate with China in the 1990s and 2000s even as China rapidly militarized, instigated crises and sought to undermine American interests in East Asia. In short, the US assessed that China's choices to contest Taiwan and Tibet, rapidly militarize and invest in new weapons was consistent with a limited aim: restoring its historical position in Asia. Thus, they remained optimistic that long-term cooperation was possible, despite short-term contestation.

I argue that China's more recent actions caused them to update their beliefs about China's strategic aims. Specifically, China's military actions in the South China Sea alarmed many ana-

lysts.[16] While these look superficially similar to the Taiwan Straits Crisis, they are qualitatively different because they are inconsistent with China's long-standing claims of restoring its borders. As a result, China-watchers no longer believe that China is motivated by restoring its position in Asia. Looking back over the pattern of China's actions, they now believe that China's ambition extends to pushing the United States out of Asia at minimum.

This shift in assessment has grim implications for the future of Sino-American relations. In the past, American policymakers wanted to work through crises because they believed that long-run cooperation was possible. Now they believe that China's long-term motives are vast; and long-term cooperation is not possible. This assessment has filtered to policy-makers leading to a strategic shift to competition. Since the shift to competition is driven by a new assessment in China's motives, and not a single crisis, the United States is unlikely to turn back. In the conclusion I explore this and other policy implications for the practice of intelligence, and American foreign policy in the age of budget constraints and Sino-American competition.

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[16]Also, China's attempt to undermine Liberal Order and exert extreme influence outside of East Asia through predatory lending, shadow institutions and soft power.

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