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Introduction by Dan Reiter, Emory University

The International Security Studies Forum (ISSF) of H-Diplo is very pleased to provide a roundtable discussion of Dr. Jessica Weeks's book, *Dictators at War and Peace*. The book offers an important answer to the centuries-old international relations question as to how the politics *within* states affect the politics *between* states? Since at least the Enlightenment, most observers have tackled this question by focusing on the differences between democracies and dictatorships, Immanuel Kant and others famously arguing that democracies are more peaceful. Realists have been skeptical of this claim, contending that all types of political systems conduct foreign policy similarly. Especially since the end of the Cold War, international-relations scholars have been consumed with the scientific exploration of the democratic peace proposition.

Dictators at War and Peace pushes the domestic politics/international politics research agenda in a new direction. It asks whether different kinds of dictatorships conduct foreign policy differently. The book proposes that they do, and intriguingly finds that some kinds of dictatorships exhibit foreign policy behavior that converges with democratic foreign policy behavior.

The three outstanding reviews in this roundtable, contributed by Drs. Alex Weisiger, H. E. Goemans, and Alexander B. Downes, find great value in the book, and also provide insightful and constructive critiques. Weisiger challenges some of the books' findings related to military junta regimes, notes that several dictatorships (such as Persian Gulf monarchies) do not easily fit Weeks' regime categories, and suggests the possibility that variables outside the theory, such as Communist ideology, might account for the observed findings. Goemans describes the limits that attend Weeks's development of a monadic rather than a strategic theory, argues in favor of making changes in the quantitative research design, and provides a competing account for one of Weeks's cases, the outbreak of the Falklands War. Downes proposes that Weeks's description of dictatorial regimes pays insufficient attention to variance in civil-military relations, demonstrating his critique through descriptions of Imperial Japan, Wilhelmine Germany, and Egypt under Nasser. The intellectual depth of the reviewers' discussions speaks to the magnitude of the contribution of *Dictators at War and Peace*. It is a book that will, for years to come, push forward old debates, create new debates, and inspire new and established scholars.

One issue that these three essays skirt, and which I wish to touch on here, concerns the policy ramifications of Weeks's argument. Especially since the end of Cold War, American foreign policy has stressed the importance of converting dictatorships into democracies, in part because, as Presidents Bill Clinton and George W. Bush reiterated, democracies do not fight each other, and dictatorships are belligerent. Weeks's book challenges this assumption, noting that some types of dictatorships, especially 'Machine' regimes led by civilian dictators who have to answer to elites, can be as peaceful as democracies.

This finding makes important recommendations for American foreign policy. Specifically, America need not democratize the entire world in order to create global order. If a regime is led by a civilian dictator and operates under elite constraints, that regime can be a peaceful member of the international order even if does not undergo democratization. For example, concluding that contemporary China is a Machine regime (Weeks generally classifies China after 1976 as being a Machine) would suggest that China is generally unlikely to initiate dangerous militarized disputes over issues such as the South China Sea and the political status of Taiwan. It also suggests that the U.S. and China ought to be unlikely to fight each other in the years to come.

Her argument also makes other suggestions. Regarding the problem of Russian aggression under President Vladimir Putin, Russian aggression could be substantially constrained if the Russian elite found a means of putting constraints on the power of the Russian president, even if such constraints fall short of effecting a full democratic transition. The U.S. also might view a nuclear Iran as posing a lesser threat, assuming that one views Iran as a Machine dictatorship, run by a constrained, civilian dictator.

Somewhat relatedly, Weeks's argument also speaks to the imperative of democratization. The past several years have demonstrated how difficult democratization can be. In Russia, the 1990s progress towards democracy was reversed in the 2000s by Putin. The Arab Spring has fallen short of its democratic aspirations, especially in countries like Libya, Bahrain, and Egypt, and perhaps even in apparent success stories like Tunisia. Afghanistan and Iraq are far short of the stable democracies into which the U.S. had hoped they might evolve. Of course, democracy should remain an important policy priority in its own right, if for no other reason than that democracy is the political system most likely to respect personal and political freedoms. But Weeks's argument does offer the reassurance that if despite the United States' best efforts it fails to spread democracy, or if it elects not to engage in a very costly democratization policy endeavor (such as war), Machine dictatorships should be as likely to support a peaceful world order as democracies. The significant caveat, of course, is that dictatorships such as North Korea that have no significant elite restrictions on their leaders remain dangerous.

The Goemans, Downes, and Weisiger essays that follow are constructive discussions of an important piece of international relations scholarship. We hope that ISSF readers find these essays, as well as Weeks's reply, to be stimulating and informative, and as encouragement to read *Dictators at War and Peace* itself.

Participants:

Jessica Weeks is Associate Professor and Trice Faculty Scholar in the department of Political Science at the University of Wisconsin-Madison. In addition to *Dictators at War and Peace* (Cornell University Press, 2014), she has published in journals including the *American Political Science Review*, *American Journal of Political Science*, and *International Organization*. She received a B.A. in political science summa cum laude from The Ohio State University, an M.A. in International History from the Graduate Institute in Geneva, Switzerland and a Ph.D. in political science from Stanford University.

Dan Reiter is the Samuel Candler Dobbs Professor of Political Science at Emory University. He is the author of *Crucible of Beliefs: Learning, Alliances, and World Wars* (Cornell, 1996) and *How Wars End* (Princeton, 2009), as well as coauthor, with Allan C. Stam, of *Democracies at War* (Princeton, 2002). He has also authored or coauthored dozens of scholarly and popular publications on international relations and foreign policy.

Alexander B. Downes (Ph.D., University of Chicago, 2004) is Associate Professor of Political Science and International Affairs at The George Washington University. His book *Targeting Civilians in War* was published by Cornell University Press in 2008 and won the Joseph Lepgold Prize awarded by Georgetown University for best book in international relations published in that year. His work has been funded by the Eisenhower Institute, the Harry Frank Guggenheim Foundation, the Smith Richardson Foundation, the Office of Naval Research, and the Carnegie Corporation of New York.

Hein Goemans is Associate Professor of Political Science at the University of Rochester. His first book, *War and Punishment*, was published by Princeton University Press (2000), and focuses on the role of leaders in war termination—with an empirical focus on World War I. His second book, *Leaders and International Conflict*, co-authored with Giacomo Chiozza, was published by Cambridge University Press (2011) and focuses on the role of leaders in war initiation. It was awarded the Joseph Lepgold Prize (Georgetown University) for best book in International Relations in 2011. His teaching focuses on international relations, with an emphasis on conflict and international relations history.

Alex Weisiger is an Associate Professor of Political Science at the University of Pennsylvania. His published research examines war termination, explanations for particularly destructive interstate wars, the democratic peace, and reputation. His book, *Logics of War: Explanations for Limited and Unlimited Conflicts*, was published by Cornell University Press in 2013.

Review by Alexander B. Downes, The George Washington University

Jessica Weeks has written a terrific book, one that unpacks authoritarian regimes into distinct types and demonstrates that these types differ systematically in their international conflict behavior. For Weeks, the key factors that distinguish authoritarian regimes from each other are whether they face a domestic audience that can hold the leader accountable for his or her actions, and whether the audience and the leader are civilians or military officers. Regimes where a civilian leader is vulnerable to removal by a civilian domestic audience are known as Machines, whereas Juntas consist of governments where a military leader faces an audience of military officers who are capable of removing him. Regimes in which leaders are not accountable to any audience differ only in whether the leader has a civilian (Bosses) or military (Strongmen) background. In general, regimes in which leaders are constrained by an audience are more peaceful, more likely to win the wars they enter, but more likely to be removed should they lose a war. Between Machines and Juntas, the latter are somewhat more conflict-prone and likely to lose wars because the military background of the leader and the audience create a more permissive environment for the use of force. By contrast, regimes in which leaders are largely unconstrained by other elites are more warlike, more likely to lose wars they enter, but more likely to survive in office in the event of battlefield defeat. Between Bosses and Strongmen, the latter are slightly more conflict-prone and more likely to lose wars, again because the military experience of the leader makes force a more attractive option.

Weeks is at the forefront of a new generation of scholars who seek to open up the black box of authoritarian regimes and use variation in their domestic characteristics to explain their foreign policy choices. Some of these arguments turn out to apply to all types of political regimes, whereas others are unique to autocracies. On the more general side, Hein Goemans argues that all regimes vary along two dimensions—the risk versus the cost of removal from office a leader faces for foreign policy failure—and that different combinations of these two variables determine leaders' decisions to initiate war as well as their wartime behavior.¹ In an argument more specific to non-democracies, Jessica Weiss maintains that authoritarian regimes can signal resolve by tolerating anti-foreign protests, which are costly to suppress and may spin out of control and potentially threaten regime survival. By allowing such protests to occur and continue, autocratic governments can credibly tie their hands on an issue and compel concessions from adversaries.²

Weeks's argument clearly puts her in the former camp. For Weeks, some autocracies are like democracies in that their leaders face audience costs for guiding their country into losing wars, or for backing down after issuing public threats of force.³ The nature of the audience is different—other civilian elites in Machines and

¹ On the latter, see H. E. Goemans, *War and Punishment: The Causes of War Termination and the First World War* (Princeton, N.J.: Princeton University Press, 2000). On the former, see Giacomo Chiozza and H. E. Goemans, *Leaders and International Conflict* (Cambridge: Cambridge University Press, 2011).

² Jessica Chen Weiss, "Authoritarian Signaling, Mass Audiences, and Nationalist Protest in China," *International Organization*, Vol. 67, No. 1 (January 2013): 1-35; and Jessica Chen Weiss, *Powerful Patriots: Nationalist Protest in China's Foreign Relations* (Oxford: Oxford University Press, 2014).

³ On autocratic audience costs in the context of threats of force rather than the use of force, see Jessica L. Weeks, "Autocratic Audience Costs: Regime Type and Signaling Resolve," *International Organization*, Vol. 62, No. 1 (Winter 2008): 35-64.

military officers in Juntas—but the effect is similar, inducing caution in leaders and causing them to avoid initiating disputes or wars they are not confident about winning. Machines and Juntas will thus not behave appreciably differently from democracies because their leaders cannot avoid being dislodged from power for their mistakes. Bosses and Strongmen, by contrast, should exhibit behavior quite different from democracies—and from their more constrained autocratic peers—because they are largely immune from domestic challenges.

Weeks finds much support for this theory. For example, in chapter 2 she shows that Bosses and Strongmen are significantly more likely than democracies and Machines to initiate militarized interstate disputes (MIDs). By contrast, there is no difference between the dispute initiation rates of democracies and Machines. In chapter 3, Weeks finds that Machines are just as likely as democracies to prevail in wars and MIDs, whereas Bosses and Strongmen are significantly more likely to lose. Unconstrained leaders, however, are far more likely than constrained leaders to survive defeats, further signaling their immunity from punishment by a domestic audience.⁴ Weeks also performs illuminating case studies of two Bosses—Iraqi dictator Saddam Hussein and Soviet leader Joseph Stalin—two Juntas—Argentina in the early 1980s and interwar Japan—and two Machines—North Vietnam and the Soviet Union under Khrushchev.

In short, this book is an excellent model of the ‘Triple Crown’ of Political Science research: an original and compelling theory, quantitative evidence across a large number of cases that shows that the theory is correlated with multiple dependent variables in the predicted way, and qualitative evidence from several cases demonstrating that the causal mechanisms posited by the theory influence the behavior of actual authoritarian leaders. The book constitutes a major leap forward in the study of authoritarian regimes and international security, and deserves the broadest possible readership both in academia and the policy community. The book’s relevance for U.S. foreign policy is obvious and timely as the United States grapples with several different types of authoritarian governments in China, Russia, Iran, Syria, North Korea, and elsewhere.

In the remainder of this review, I examine Weeks’s major contribution—her typology of authoritarian regimes. Specifically, I explore whether or not it is comprehensive: that is, whether it captures all authoritarian governments (with the exception of new and transitional regimes). Drawing on insights from recent literature on military effectiveness, I argue that the typology excludes an important category of regimes—civilian-led governments that face a military audience—that exhibit a distinct set of behaviors from civilian regimes with civilian audiences. This argument highlights a variable that is omitted by Weeks’s theory: civil-military relations. Weeks assumes that civilians exert strong control over the military in Machines, but the quality of civil-military relations is a variable, not a constant. Civilian-led regimes with a powerful, autonomous military audience likely behave in a more aggressive fashion than civilian regimes with a civilian audience for one of two reasons: (1) the military audience may (threaten to) remove leaders who resist their aggressive plans, or (2) the government does not control the military, which is able to commit the state to conflict without clear

⁴ The evidence in these chapters is ambiguous regarding Juntas, the second type of constrained autocracy. For example, Weeks finds that Juntas are significantly more likely than Machines and democracies to initiate MIDs, but not significantly more likely to win or lose wars. Leaders of Juntas also lose office at roughly the same rate as these other two regimes after losing a war. Juntas thus seem more constrained when it comes to war outcomes and the consequences of defeat, and less constrained in initiating military conflicts. See 44, 69, and 73.

regime approval. I develop this argument below using the cases of Imperial Japan, Wilhelmine Germany, and Egypt in the Six-Day War.

Civilian Leaders, Military Audiences: Comparing Theories

Comparing Weeks's theory with two others developed in the literature on military effectiveness fruitfully highlights the missing role of civil-military relations in her argument. An important assumption of Weeks's theory is that in constrained authoritarian regimes, civilian leaders are removed only by other civilian elites, whereas military leaders are removed only by the military.⁵ Although the latter seems plausible, the former is puzzling, and contradicts much work on civil-military relations that highlights the salience of leaders' vulnerability to military coups. It is now well established in the military effectiveness literature, for example, that regimes where the leadership fears a military coup take a number of steps—collectively referred to as 'coup-proofing,' and including such measures as purging competent military officers and replacing them with incompetent (but loyal) bunglers; creating multiple independent military and paramilitary forces; prohibiting communication between officers and adjacent units to inhibit anti-regime coordination; and allowing little if any realistic training—that decrease coup risk but vitiate the military's combat effectiveness.⁶ This literature suggests that even in civilian-led authoritarian regimes, the military may still be an important audience that can influence the behavior of the state. In other words, the argument that Machines are relatively unlikely to initiate forceful disputes or wars hinges on an unarticulated assumption that civilian control of the military is secure. If it is not, and thus the military constitutes a second audience in Machines, then these regimes may not face the simple incentive structure that Weeks lays out. On the one hand, the potential for removal at the hands of civilian elites for failed foreign adventures induces caution in the leaders of Machines. But these leaders may also face threats of removal from the military audience if they do not behave more aggressively.

Weeks's argument also differs from a second model of civil-military relations developed in the military effectiveness literature. Risa Brooks' theory of strategic assessment focuses on the balance of power between civilian leaders and military leaders (civilian dominance, military dominance, or shared power) and the extent of preference divergence between the two groups (high versus low).⁷ When civilians are firmly in charge and civilian and military officials have congruent preferences, states will be able to assess their strategic environment accurately and are likely to have positive military outcomes. When civilians share power with the military—that is, when the military can threaten the tenure of the leader directly or indirectly (in Weeks's

⁵ Weeks, for example, characterizes "nonpersonalist civilian machines" as being governed by "civilian leaders and elites." (19). Although I was unable to find an explicit statement that the leaders of Juntas must be military officers, Weeks strongly implies this when she writes, "the core domestic audience in Juntas is composed of *other* military officers" (6). (emphasis added). One could argue that the leader of a junta must, by definition, be a military officer.

⁶ Stephen Biddle and Robert Zirkle, "Technology, Civil-Military Relations, and Warfare in the Developing World," *Journal of Strategic Studies*, Vol. 19, No. 2 (June 1996): 171-212; James T. Quinlivan, "Coup-proofing: Its Practice and Consequences in the Middle East," *International Security*, Vol. 24, No. 2 (Fall 1999): 131-165; Caitlin Talmadge, "The Puzzle of Personalist Performance: Iraqi Battlefield Effectiveness in the Iran-Iraq War," *Security Studies*, Vol. 22, No. 2 (May 2013): 180-221; and Talmadge, *The Dictator's Army: Battlefield Effectiveness in Authoritarian Regimes* (Ithaca, N.Y.: Cornell University Press, forthcoming).

⁷ Risa A. Brooks, *Shaping Strategy: The Civil-Military Politics of Strategic Assessment* (Princeton, N.J.: Princeton University Press, 2008).

terms, the military constitutes an audience)—and the two sides have strongly divergent preferences, then strategic assessment will be very bad.⁸ Because they are competing for power, both civilian and military officials are reluctant to share information with each other, which in turn makes it difficult to coordinate strategic plans with political goals. Moreover, the military tends to focus on its internal rival, undermining its ability to assess its own (and its external adversary's) strengths and weaknesses, and it is unclear who has the final say on military strategy. Such states are prone to major strategic mistakes.

Civilian Leaders, Military Audiences: Cases

I argue that these alternative, civil-military based arguments, provide superior explanations for a number of important cases—some of which Weeks covers in depth, and some of which she does not. Imperial Japan, for example, provides an example of a civilian-led regime where civilians did not have control over the military, and thus the military constituted the key audience that could remove civilian leaders. Weeks codes Japan as a machine in 1931 but a junta by 1937 (76). It is unclear why the coding of Japan's regime type changes in 1937, and not in 1941, when Gen. Tojo Hideki became Prime Minister and civilian leaders were largely sidelined. The new Prime Minister in 1937, Fumimaro Konoe, was not a military officer, there was no coup or seizure of power by the military, and the structure of the regime remained unchanged.⁹ Weeks amply demonstrates that the preferences of the military were typically more aggressive and expansionist than those of civilian elites. But the regime she describes falls outside of her typology. That the military was free from civilian control is clear from 1931, when the Kwantung Army provoked a war in Manchuria against the Prime Minister's direct orders. After fighting erupted, writes Weeks, "Prime Minister Wakatsuki Reijir announced that the army was to follow a policy of 'non-aggravation.' But the civilian government was powerless to stop the army's advance. With the support of both the War Ministry and the General Staff, field commanders repeatedly ignored orders from Tokyo" (121). Similarly, army officers escalated the Changkufeng and Nomonhan conflicts against the wishes of the civilian government (124-125). As Weeks remarks about the latter episode, "the clash at Nomonhan was the product of poor civilian control over a Kwantung Army that represented the extremes of 'militaristic' thinking" (126).

The key fact that makes Imperial Japan from 1889 to 1941 a hybrid regime with civilian leaders and a military audience, and which Weeks mentions only in passing, is that under the Meiji Constitution of 1889, the military had the power to bring down any civilian government by withdrawing—or refusing to name—the Army or Navy Minister.¹⁰ The Constitution exempted the military from parliamentary control; the armed

⁸ Strategic assessment will also be poor when political and military leaders share power but have largely convergent preferences. Strategic assessment will be fair when political leaders dominate and preference divergence is low, or when the military is dominant (preference divergence is less important when civilians have no say in strategic assessment). Brooks, *Shaping Strategy*, 7 and 42-53.

⁹ Indeed, as I argue below, under the Meiji Constitution, the military already constituted the key audience that could remove civilian leaders.

¹⁰ Saburō Ienaga, *The Pacific War, 1931-1945* (New York: Pantheon Books, 1978), 35-36; and Ronald H. Spector, *Eagle against the Sun: The American War with Japan* (New York: Vintage Books, 1985), 33.

services answered only to the emperor, and “[n]o civilian control was ever allowed.”¹¹ Thus, even without recourse to force, the Japanese military could topple the regime, a prerogative it exercised on multiple occasions.¹² As a result, as Weeks notes, Japan’s civilian prime ministers were unable to prevent the military from starting or escalating wars, making Japan’s hybrid government much more warlike than Machines with civilian control of the military.

Wilhelmine Germany is another important case that seems to elude Weeks’s categorization. The structure of the German government in the years prior to World War I has long defied easy description. There were elections and universal manhood suffrage, but the Chancellor served at the pleasure of the Kaiser and did not require the support of a majority of the Reichstag. Moreover, command of the armed forces and the conduct of foreign policy were the sole prerogative of the throne rather than parliament. In addition to these regime characteristics, Wilhelmine Germany is perhaps the signature case of military autonomy in the literature. Brooks argues that civilian and military leaders shared power in the Wilhelmine system: “the German army...had substantial latent power and constituted a major force within the kaiser’s coalition.”¹³ At a minimum, civilian officials had no input on the formation of military strategy and it was unclear who had the authority to make strategic commitments on behalf of Germany.

One significant result of the military’s autonomy in the German system was the mismatch between German political goals—such as keeping Britain on the sidelines of a European war—and its military strategy (the Schlieffen Plan)—which was predicated on violating Belgian neutrality, making it highly likely that Britain would enter any such war. This contradiction stemmed from the near-complete exclusion of civilian elites from military planning. Instead of strategy being crafted to achieve political objectives, civilian policymakers had to mold their objectives to military strategy. As German Chancellor Bernhard von Bülow famously remarked about the Schlieffen Plan’s intended attack on neutral Belgium, “if the Chief of Staff, especially a strategic authority such as Schlieffen, believes such a measure to be necessary, then it is the obligation of diplomacy to adjust to it and prepare for it in every possible way.”¹⁴ Wilhelmine Germany thus appears to be another example of a civilian-led regime that shared power with and—in important ways—could not control its military, which led to the adoption of a military strategy poorly suited to the country’s political needs, with ultimately devastating consequences in the First World War.

A final example of a case that fits uneasily into Weeks’s typology is Egypt at the time of the Six-Day War. Weeks (78) codes Egypt’s President Gamal Abdel Nasser in 1967 as a Strongman—a leader with a military background constrained by no audience—but Brooks depicts Nasser as locked in a fierce competition for power with his military chief, Abdel Hakim Amer. Brooks describes Nasser as “effectively insulated from

¹¹ Ienaga, *Pacific War*, 35. By tradition, and sometimes by law (1900-1913, and from 1936 to the end of the war), both ministers had to be serving officers.

¹² Ienaga documents several, including in 1912, 1937, and 1940. *Ibid.*, 36, 40, and 41-42.

¹³ Brooks, *Shaping Strategy*, 182. Brooks codes preference divergence between political and military leaders as low, meaning that strategic assessment should be poor in this case. See page 181.

¹⁴ Cited in Jack Snyder, “Civil-Military Relations and the Cult of the Offensive, 1914 and 1984,” *International Security*, Vol. 9, No. 1 (Summer 1984), 125.

information about internal military affairs.”¹⁵ Nor was there good coordination between political and military officials, which is so necessary for effective strategy. “In fact,” writes Brooks, “political officials were so marginalized from the military command that they were often left in the dark about key decisions even when they had profound diplomatic implications.”¹⁶ The country’s intelligence agencies (controlled by Amer) were so consumed with spying on Nasser and his allies that information about the Israeli military was in short supply. Finally, Nasser’s efforts to gain control of officer appointments were repeatedly frustrated by Amer, and it was unclear who exactly had the authority to issue orders to the military. Amer, for instance, decided to send Egyptian forces into the Sinai without consulting Nasser, and considerable confusion exists regarding who was ultimately responsible for deciding to close the Straits of Tiran, regarded by Israel as a *casus belli*.¹⁷

Egypt failed miserably in the ensuing war, and Nasser managed to survive defeat—both of which are consistent with Weeks’s theory—but the mechanism that explains these outcomes is quite different across the two arguments. For Weeks, Strongmen like Nasser are the central decision-makers on war and peace, and Nasser could provoke war with Israel with little fear for his political fate given his personal control over the security forces. The picture sketched by Brooks could not be more different: out-of-control military officials essentially committed the state to war without the knowledge or consent of the Egyptian Strongman, whom they had deceived about the preparedness of the Egyptian army. Nasser survived the defeat, but only by confronting and purging his competitors in the military, and finally subordinating it to his control.¹⁸

In short, I argue that there are two types of civilian-led authoritarian regimes with audiences that vary depending on whether civilians control the military. In the first, civilian control is firm enough that military actors play no role in decisions to remove the executive. These are the restrained Machines depicted by Weeks. In the second, the hybrid type I have been describing, the military is outside civilian control and has the ability to remove the leader. These regimes are likely to be more aggressive, since civilian leaders may be removed for *opposing* the use of force rather than for going to war unsuccessfully, and civilian elites cannot prevent the military from taking action. Although it is difficult to say for certain, two pieces of evidence suggest that these types of regimes are not uncommon in authoritarian states: (1) according to the *Archigos* dataset, of all leaders who were ousted by “irregular” (i.e., violent) means, slightly more than half were

¹⁵ Brooks, *Shaping Strategy*, 80.

¹⁶ *Ibid.*, 83.

¹⁷ *Ibid.*, 85-92.

¹⁸ *Ibid.*, 112.

removed by military actors,¹⁹ and (2) military Juntas are the least common type of regime in Weeks's typology,²⁰ which implies that these coups are probably not limited to military-led regimes.

None of the foregoing should detract from Weeks's achievement in *Dictators*, which represents a substantial advancement in our knowledge of the behavior of authoritarian regimes on questions of war and peace. Consideration of civil-military relations, however, might have caused her to differentiate further among civilian-led regimes according to whether they face a civilian or a military audience, and identify those that are more and less prone to conflict. It also might have led her to different coding decisions on certain personalist regimes. Future work might seek to integrate civil-military relations to provide additional nuance to Weeks's typology.

¹⁹ Hein E. Goemans, Kristian S. Gleditsch, and Giacomo Chiozza, "Introducing Archigos: A Dataset of Political Leaders," *Journal of Peace Research*, Vol. 46, No. 2 (March 2009): 269-83. Of the 501 irregularly removed leaders for whom it is possible to code a specific manner of exit from power, 260 (52 percent) were overthrown by military actors. *Ibid.*, 275.

²⁰ Juntas constitute about 14 percent of authoritarian regime-years from 1946 to 1999. Weeks, *Dictators at War and Peace*, 40.

Review by H. E. Goemans, University of Rochester

Jessica Weeks's *Dictators at War and Peace* is an impressive and ambitious effort to re-invigorate studies that link domestic politics and international conflict, and to do so by shifting away from a focus on democracies to a focus on different types of autocracies. The book is getting a lot of well-deserved attention for its ambition and scope. Working towards these ambitious goals, Weeks introduces a significant amount of complexity in the analysis, but inevitably, introducing more complexity on some issues requires simplifying other issues. In this review I seek to highlight some of the costs and drawbacks of these tradeoffs.

The goal, as stated in Table 1.3, is nothing less than to explain the initiation of conflict, the probability of defeat, the probability of (post-conflict) punishment of the leader and/or regime members after a defeat, and the decision-making process in different authoritarian regimes (35). Building on Dan Slater's conceptualization of regime types – e.g., distinguishing Bosses, Strongmen, Machines, and Juntas – Weeks argues that leaders of these different regimes systematically come to different conclusions about the combination of four central factors in decisions about war.¹ “First, actors form views about the *benefits of winning* compared to continuing on a nonmilitary pathway. ... second, actors form perceptions of the *costs of fighting*, regardless of the outcome of the conflict. third, in addition to generic views about force, audiences and leaders have perceptions of the *costs of defeat* in a military challenge. finally, actors form estimates of the *likelihood of winning* a military contest” (15–16). Since these factors come together coherently, Weeks can use them to rank regimes from least to most likely on each of the proposed dependent variables, with for example Strongmen having the highest, Bosses, the second highest, Juntas the second lowest and Machines the lowest probability of conflict initiation.²

This ambitious research plan, however, comes at a cost. It requires some theoretical simplifications, and introduces significant difficulties in the empirics. I start with the theoretical simplification. Weeks explicitly uses a monadic design, and brackets strategic interaction (41). In the process Weeks bypasses the so-called bargaining model of war, with its emphasis on the inefficiency puzzle.³ As a result the analysis is weak when it

¹ Slater, Dan, 2003. “Iron Cage in an Iron Fist: Authoritarian Institutions and the Personalization of Power in Malaysia.” In *Comparative Politics* 36 (1): 81–101.

² The same rank ordering obtains for the probability of defeat. Machines have the highest probability of punishment, followed by Juntas, with Bosses and Strongmen, both personalist regime types, facing the lowest probability of punishment. I leave the full description of her predictions about the decision making process to the reader, it can be found in Table 1.3, page 35.

³ As Weeks writes, emphasis mine: “[The four] factors therefore can affect the probability that a country will seek to revise the status quo in its favor; giving rise to disputes in the first place. In the presence of a dispute, *additional* factors like secrecy, commitment problems, and indivisible issues may then prevent countries from locating solutions short of war” (16-17). Even though she claims that “these conjectures [about the four factors, HG] fit easily with theories viewing war as a bargaining process between two states, the links between the variation in these factors and the likelihood of war is unclear. Weeks is aware of the problem, as she writes in footnote 11, emphasis mine: “*Future* research could investigate whether authoritarian regime type fosters certain kinds of bargaining failures once a dispute has arisen” (187). See James D. Fearon, “Rationalist Explanations for War.” In *International Organization* 49 (3), 1995: 379-414.

moves from the onset of a dispute to the onset of war, since it takes only one to start a dispute⁴, but two to start a war. I am not convinced, however, that it is necessary to side-step the inefficiency puzzle of war to make predictions about the war-proneness of different authoritarian regime types. (In fact, in my work with Alexandre Debs I have proposed such a theory, built on the cost of replacing different types of leaders.⁵) Indeed, it seems to me that Weeks's theoretical arguments could, in principle, be linked to war through simple extensions of the logic of the bargaining model of war. Reading the book, it struck me that she seemed to suggest that different leaders may value the same good differently. If leaders value the good similarly, we are left with the typical unit-interval issue space, with a point on the line representing the probability of victory and intervals around this point that specify each leader's costs of war, firmly leading us back to James Fearon's unitary rational actor explanations, which leaves no room for different regime types.⁶

But it seems entirely plausible to propose that leaders value the same good differentially because it brings them different private benefits⁷ of war, thereby eliminating the bargaining range. Alternatively, variation in regime type could be linked to variation in private information, incentives to misrepresent, or commitment problems. It could be argued that personalist regimes, in particular Bosses like Saddam Hussein or Stalin, make decisions in such isolation that information about their preferences and calculations is limited to a very few individuals, leaving the dictator with more private information than other authoritarian leaders. In turn, the international opponent of such a leader might also have 'more' private information about his capabilities and resolve. Any information that contradicts the leader's beliefs about the international opponent may never reach a personalist leader who is surrounded by sycophants. Thus, even when dealing with a complicated four-way regime typology it seems by no means necessary to bypass the bargaining model of war. Explicitly building on the bargaining model of war and taking account of strategic interaction at both the domestic and international level, I would argue, might also lead to some countervailing hypotheses. Weeks writes that while "autocratic audiences may approve of the use of force if the benefits outweigh the costs, they are no less wary of the possibility of defeat than they democratic counterparts and do not see systematically greater gains from fighting" (22). In her view, such audiences by and large *restrain* leaders from going to war or initiating a dispute (22-23). Weeks differentiates authoritarian regimes that do not have an audience that can potentially punish the leader (personalist dictatorships) from authoritarian regimes that do have such audiences, but ignores the strategic interactions between domestic actors. As Giacomo Chiozza and I argue, it is the time-varying (an issue to which I return below) threat of domestic punishment that can make war a rational gamble

⁴ Note that in the statistical analysis of dispute initiation in Chapter 2 the dependent variable measures the "threat and use of force" (41).

⁵ When violence is more important in the replacement of leaders, the cost of replacing leaders is higher and the sensitivity of a leader's tenure on the outcome of international bargaining is greater (since international conflict can affect a leader's military power). We argue that this explains the democratic peace as well as differences among non-democratic regimes in the rate of involvement in war. Alexandre Debs and H. E. Goemans, "Regime Type, the Fate of Leaders and War." In *American Political Science Review*, 104, 2010: 430-445.

⁶ Fearon, James D. 1995. "Rationalist Explanations for War." In *International Organization* 49 (3): 379—414.

⁷ See the brief discussion on page 20 about excludable spoils of war and also the brief discussion on page 30 about the costs of conflict.

for resurrection.⁸ If the peacetime threat of domestic punishment is high, the use of force with the potential for political domestic rewards in the case of victory can be a rational gamble, even if defeat carries a high concomitant likelihood of punishment. The truncation of punishment is key. This suggests that the presence of an audience might prod leaders into wars they would not have selected if they had not had such an audience.

Leaving such theoretical issues aside, Weeks's framework also raises some issues for the empirical sections of her book, especially the case studies which are a significant addition to her previous published work.⁹ I focus here on these case studies, but it deserves note that Weeks adds a slew of new findings on the factors that affect war outcome and post-conflict punishment of leaders in Chapter 3. First, in the theory, all the variation that potentially drives variation in the use of force is between regime types; there are no time-varying factors or variables. It is a bit puzzling, thus, to see the directed-dyad-year as the unit of analysis in Chapter 2, the quantitative chapter. It is well understood that the inclusion of many additional (annual) observations inflates the number of observations and depresses the standard error. Although this approach is common in the literature,¹⁰ it puts a lot of weight on un-modeled common shocks that would push a regime type over the edge into conflict initiation and ignores the potential that a different regime type might not have experienced such a shock in the first place. I raise the issue because the case study chapters also do not *ex ante* identify threats to see whether domestic audiences punish leaders for failing to carry through on their threats. A recent literature argues that the initiation of a crisis is a poor proxy for threats.¹¹ Moreover, it is not clear that the outcome of ouster means that the audience punishes the leader for failing to carry through on a threat. The absence of time-varying factors also presents some difficulties in the case studies. Why do regimes sometimes stay at peace, but at other times go to war or in other ways choose to use force? It clearly cannot be the regime-type factors in isolation which lead to war; after all, these are constant. In other words, given a theoretical set-up with no time-varying factors, Weeks cannot explain why regime type A went to war in 1938, but not in 1933. With her focus on regime type, Weeks must explain why regime type A went to war in 1938, but regime type B would not have. Doing so would require a series of careful counterfactuals. To

⁸ Giacomo Chiozza and H. E. Goemans, *Leaders and International Conflict*. New York: Cambridge University Press, 2011.

⁹ For her previously published work, see Jessica L. P. Weeks, "Autocratic Audience Costs: Regime Type and Signaling Resolve," *International Organization* 62 (1), 2008: 35-64; and Jessica L. P. Weeks, "Strongmen and Straw Men: Authoritarian Regimes and the Initiation of International Conflict" *American Political Science Review* 106 (2), 2012: 326—347. For other work on the topic, e.g., Brian Lai and Dan Slater, 2006. "Institutions of the Offensive: Domestic Sources of Dispute Initiation in Authoritarian Regimes 1950—1992." *American Journal of Political Science* 50(1), 113—126., Debs and Goemans (2010), Chiozza and Goemans (2011)

¹⁰ For an exception, where we explicitly model the time varying threat to leaders, see Chiozza and Goemans (2011).

¹¹ Snyder, Jack and Erica D. Borghard. 2011. "The Cost of Empty Threats: A Penny, not a Pound." In *American Political Science Review* 105(2): 437—456. Downes, Alexander B. and Todd S. Sechser, 2012. "The Illusion of Democratic Credibility." In *International Organization*, 66 (3), July: 457—489. Trachtenberg, Marc, 2012. "Audience Costs: An Historical Analysis." In *Security Studies*, 21 (1): 3—42.

assess the explanatory power of the time-invariant regime-type specific factors requires an evaluation of the counterfactual behavior of the other potential regime types.

In other words, evaluation of the explanatory power of a Boss-type regime's decisions for the use of force must also compare the Boss's behavior against the behavior of a democracy, a Strongman-, a Junta-, and a Machine-regime in similar circumstances. Such a systematic evaluation of counterfactuals is missing. Case studies are a useful tool in tracing causal mechanisms, and in her book Weeks innovates relative to her earlier published work not just through an analysis of the factors that influence war outcome and post-conflict punishment in Chapter Three but also through presenting new qualitative evidence. She is particularly strong on detailing the relevant consultative processes. However, the case studies have a hard time nailing down the specific mechanism that leads a regime to the use of force. Since the four factors mentioned earlier neatly organize themselves into four regime types, it is not really possible to evaluate which factors drive decisions for conflict. Which of the four factors makes the Machines more peaceful than the Juntas? Is it cost of defeat, the benefits of winning, the costs of fighting, or the likelihood of winning? If it is all four of them together, how do they relate in the specific cases? Since there are no counterfactuals in the case studies to evaluate the explanatory power of the regime typology, there is no way to assess the underlying factors that drive the behavior of the different regimes.

As an example, let me discuss the details of one case about which Weeks and I disagree. In Chapter 5, Weeks dismisses diversionary explanations for Argentine behavior leading up to the 1982 Falklands War. The narrative is not fully convincing, however, since it does not drill all the way down into the mechanisms and evidence on the central points of contention of those who have argued that this was indeed an instance of diversionary conflict. Thus, Weeks dismisses fears of punishment as a motivator in Argentinean Junta leader General Galtieri's decision making, pointing out that few of his direct predecessors in Argentina and his contemporary colleagues from Latin America suffered from punishment after they lost office. I would point out that Galtieri was indeed imprisoned. Relying on Archigos 2.9¹², we also see that between 1945 and 1982, 121 South American leaders lost office. Of those, 60% lost office in a regular manner, and only about 10% of those suffered punishment in the form of exile. Roughly 38%, however, lost office in an irregular manner¹³, and 76% of those faced severe punishment in the form of exile (52%), imprisonment (15%) and/or death (2%). Two lost office as a result of suicide. With such numbers in recent history, it seems eminently plausible that Galtieri feared an irregular removal from office and subsequent severe punishment. As Levy and Vakili and Chiozza and I have argued, Galtieri's diversionary motives stemmed largely from a fear of an overthrow by Junta members, specifically Naval Minister Jorge L. Anaya.¹⁴ This is a different line of argument to link the Falklands War to the survival of Galtieri, in office and beyond, than the standard fear of a popular revolt.

¹² These results are obtainable by using the leader data set Archigos 2.9 and the following command line in Stata: `tab exit_tv posttenurefate_tv if ccode >100 & ccode<200 & year>1945 & year<1982 & posttenurefate_tv>=0, row col chi.`

¹³ The remainder retired because of ill health.

¹⁴ Levy, Jack S. and Lily I. Vakili. 1992. "Diversionary Action by Authoritarian Regimes: Argentina in the Falklands/Malvinas case." In *The Internationalization of Communal Strife*, ed. Manus I. Midlarsky, 118—46. New York: Routledge, p.32.

It is not discussed in the book.¹⁵ Overall, there is too little detail in the empirical chapters on both the novel mechanisms and on the mechanisms of the competing explanations to draw strong conclusions about the superiority of Weeks's approach.

These criticisms should in no way detract from the questions Weeks raises and from her insights into the logic of different authoritarian regimes at war and peace. Weeks deserves much credit for the originality of her contributions, and I hope and am confident that others will follow her lead.

¹⁵ Chiozza and Goemans (2011) cite David Pion-Berlin (1985, 70), *The Fall of Military Rule in Argentina: 1976—1983*. *Journal of Interamerican Studies and World Affairs* 27 (2): 55—76. “Under strong prodding from navy commander Almirante Jorge L. Anaya who had backed the new government on condition that they break the deadlock of the Malvinas, the junta decided, on January 6, to go ahead with the invasion.” See also Max Hastings and Simon Jenkins, 1983. *The Battle for the Falklands*. New York: W. W Norton & Company 46. Richard C. Thornton, 1998, *The Falklands Sting: Reagan, Thatcher and Argentina's Bomb*. Washington, D.C.: Brassey's, 74 and Oscar Raul Cardoso, Ricardo Lirschbaum and Eduardo Van DerKooy, 1987, *Falklands, The Secret Plot*. (trans. Bernard Ethll). Surrey, U.K.: Preston Editions 1-23, who write “But Anaya, however, had agreed to support Galtieri's access to the Casa Rosada, only in return for the *green light* on the Falklands. Any further postponement would finish this pact, bringing with it, sooner or later, deep dissension in the heart of the military regime.” All cited in Chiozza and Goemans (2011),

Review by Alex Weisiger, University of Pennsylvania

In her excellent book, Jessica Weeks advances a clear and generally compelling argument about how important variations among autocracies affect decisions about the use of force. International relations scholars have long been interested in the implications of democracy for foreign policy, whether in classical realist arguments that democracies are ill-suited to the effective conduct of power politics or in more recent arguments that democracies are both good at managing their relations with one another and particularly effective at war.¹ In this discussion, non-democracies have constituted a residual category, collecting together countries as varied as Tsarist Russia, communist China, and contemporary Somalia. It is only recently, however, that systematic analyses of variation among autocracies have emerged.

Weeks argues that we can best understand the foreign policies of these regimes by considering the implications of two central axes along which they vary: whether the leader is accountable to a domestic audience with the capacity to punish him, and whether the key policymakers are civilians or members of the military. At one extreme, Machines like contemporary China, in which leaders are accountable to a civilian audience, are effectively indistinguishable from democracies, if anything more cautious about the use of force and more likely to win the wars that they fight. Because leaders of juntas also must worry about punishment in the event of foreign policy failure, they too are relatively cautious about the use of force, though their military outlook leaves them more likely to see resort to violence as appropriate. By contrast, personalist leaders, both civilian Bosses and military Strongmen, face little prospect of punishment for foreign policy failure, and thus can afford to engage in speculative gambles.

Weeks tests these arguments both statistically, through tests examining conflict initiation, success in war, and punishment of leaders, and through a series of paired case studies. The statistical analysis builds on work by Barbara Geddes on the comparative politics of authoritarian regimes to classify countries in the period since 1945 into relevant categories.² In her analysis of conflict initiation, Weeks finds that Machines are comparable to democracies, that Juntas are comparatively more likely to use force, but that personalists (especially Strongmen) are particularly likely to do so. To examine war outcome, she extends her dataset back through the interwar period, again finding that Machines are comparable to democracies in their success in war, that Juntas are slightly less successful, and that personalists are particularly unsuccessful. Case studies of Soviet leader Joseph Stalin's decision to invade Finland and Iraqi President Saddam Hussein's decision to invade Kuwait make the case that each was a personalist leader who undertook a risky gamble that less megalomaniacal leaders with more concerns about possible punishment would have eschewed. She similarly argues that Japanese aggression in the 1930s and 40s and the Argentine Junta's decision to invade the Falkland Islands in 1982 reflect the mindset of military leaders but also the constraints imposed by the existence of an audience. Finally, case studies of North Vietnam and the Soviet Union after Stalin demonstrate the extensive consultation present even in Machines that have been involved in an unusually high amount of conflict, with what she argues were attendant benefits in identifying optimal policies.

¹ For examples, see Hans J. Morgenthau. *Politics among Nations: The Struggle for Power and Peace*. New York: Knopf, 1972, 146-148, and Dan Reiter and Allan C. Stam, *Democracies at War*. Princeton University Press, 2002.

² Barbara Geddes. *Paradigms and Sand Castles: Theory Building and Research Design in Comparative Politics*. Ann Arbor: University of Michigan Press, 2003.

The book has a number of strengths. The clearly articulated theoretical argument makes a compelling case for focusing on the two central variables – domestic audiences and civilian vs. military outlooks – as particularly salient determinants of autocrats' foreign policies. The integration of institutional and dispositional variables is a nice contribution that helps to advance the now well-established literature on the independent effect of leaders on foreign policy. The statistical analysis is thorough and sophisticated, yet simultaneously presented in a way that ensures that the reader can clearly assess the implications of important decisions about variable coding and statistical specification. The case studies complement this analysis nicely; for example, the carefully detailed review of the extensive consultation that North Vietnamese leaders undertook during the Vietnam War makes for a compelling case that responsibility to a broader audience was largely responsible for the effective North Vietnamese military strategy. In short, the book will be a standard for people interested in the foreign policy of non-democracies.

As with any work this ambitious, however, it is not above criticism. The remainder of this review discusses three areas that are either less convincing or that point to possible avenues for future work to further advance this research agenda. First, the evidence related to Juntas is open to questions and potential criticisms, which in turn raises some broader questions about the importance of military backgrounds. Second, the existence of a large residual category of non-democracies that do not fit any of the four regime types that Weeks identifies points to opportunities for further theorizing and raises some questions about the strength of some of the findings reported in the book. Third, relatively high degrees of overlap between certain regimes and other potential salient variables, such as the observation that most Machines were communist, raise potential alternative explanations for some key findings.

The least convincing results in the book concern Juntas, a limitation that is particularly salient because the Juntas provide the primary evidence of the independent effect of the distinction between civilian and military outlooks on foreign policy. In part, this situation reflects the relative rarity of Juntas, which are the least common of the four autocratic regimes. For example, Weeks reports that Juntas lost three out of eight wars in which they were involved, a rate that is lower than that for personalist regimes but higher than that for democracies and Machines. This finding is based on reasonable analysis, but different reasonable approaches would have produced quite different results. Imperial Japan is responsible for four of these eight wars (the Sino-Japanese War, the Pacific War, and the Chankufeng and Nomonhan border wars with the Soviet Union), and is coded as losing only the Pacific War, with the others coded as draws. Alternate reasonable approaches, such as coding Japan as losing in Nomonhan and in the Sino-Japanese War or simply collapsing all these conflicts into a single unsuccessful war, as Weeks does for Germany during the same period (193), would result in Juntas having a record comparable to that of the personalist regimes.

The Junta case studies are also less compelling than others in the book. While Japan provides a clear example of an aggressive military, its status as a Junta is more open to question given the importance of the civilian emperor and of civilian politicians. By contrast, the Argentine case makes a less compelling case for the argument that Juntas are generally predisposed to use force. One puzzle, which Weeks does not address, is why the Junta chose to invade the Falklands while accepting the diplomatic process that resulted in an unfavorable settlement to the Beagle Channel dispute with Chile during the same period. More importantly, the argument that the invasion was clearly going to be a diplomatic disaster but that the Junta's predisposition toward military solutions led it to miss this obvious fact (114-115) neglects what were in fact quite rational bases for the Junta to have been both pessimistic about the utility of diplomacy and optimistic about the use of force. In previous secret diplomacy, the British government had made clear that it was willing to cede the

islands if the inhabitants agreed, and moreover expressed frustration at the islanders' reluctance to go along.³ Moreover, retaking the islands posed a major military challenge, requiring the British to carry out amphibious landings thousands of miles from home with no local base from which to operate. All told, then, even if the Junta's strategy was risky, it was not unreasonable to believe that when confronted with a bloodless *fait accompli* the British government would simply walk away, while any attempts at a diplomatic resolution would likely be frustrated by the British government's refusal to force a transfer on the reluctant islanders.⁴

Second, while the categorization of authoritarian regimes into Machines, Juntas, Bosses, and Strongmen is well motivated and theoretically useful, it is worth noting that it is not exhaustive. In Weeks's data, which covers the period from 1945 to 2000, Machines are the most common of these regimes, constituting a total of 627 country years, compared to 584 years of Strongmen, 548 years of Bosses, and 294 years of Juntas. The most common category, however, is new and unstable regimes, with 1475 country years. Moreover, even setting aside these cases, whose recent changes may lead to distinct patterns of behavior that differ from the norm for the regime, there remain a total of 1,446 country years of other stable authoritarian governments. The majority of these states are monarchies in the Persian Gulf, Morocco, Thailand, and elsewhere, but the category also includes theocratic Iran, marginally democratic France in the 1960s, and a variety of different authoritarian regimes in the Americas, Africa, and Asia.

The existence of this residual category has important potential implications for the analysis. Likely the most important argument and finding in the book is that Machines are functionally equivalent to democracies. Weeks attributes this effect to the presence of a civilian audience, which holds leaders accountable for policy missteps and hence averts the kinds of risk-taking seen in more personalist regimes. That said, many of the "other stable authoritarian regimes," such as post-1979 Iran or the Gulf monarchies, could plausibly be categorized as Machines. In Iran, there clearly is an audience of clerics who hold leaders accountable, while in the monarchies the king's family constitutes a potential threat, as demonstrated for example by the 1995 coup in Qatar that elevated the crown prince to the throne. These countries are left in the residual "other" category not because of a clear argument that they do not fit the typology but because Geddes, on whose data Weeks relies, omitted them from her dataset. Whether these cases should be coded as Machines and whether doing so would substantially affect the results are both open questions whose answers will influence the future development of this research program.

Finally, there is the difficulty of sorting out the independent effect of regime type given possible confounds. While definitively eliminating concerns about reverse causation is close to impossible, Weeks (33-34, 52) makes a reasonable theoretical and empirical case against endogeneity criticisms (for example, that Juntas are involved in more conflict because they come to power in more dangerous neighborhoods). A possibility that she examines less is that certain autocratic regimes may covary with other factors that influence foreign policy. For example, by a rough count, over 60% (387 of 627 country years) of Machines in the dataset were communist states, which might plausibly also have adopted a more cautious foreign policy. Specifically, if

³ Lawrence Freedman. *The Official History of the Falklands Campaign*. London: Routledge, 2005, vol. 1, e.g. 116-118.

⁴ For similar interpretations of this case, see John Arquilla and María Moyano Rasmussen. "The Origins of the South Atlantic War." *Journal of Latin American Studies* 33:4 (2001), 739-755, and M. Taylor Fravel. "The Limits of Diversion: Rethinking Internal and External Conflict." *Security Studies* 19:2 (2010), 307-341.

Marxist-Leninist ideology convinced communist leaders that the tide of history favored communism and that their opponents were ultimately doomed, then avoiding risky wars is a natural implication. A preliminary statistical analysis suggests, contrary to this counterargument, that the result is actually being driven primarily by the non-communist Machines (countries like Mexico under the Institutional Revolutionary Party PRI, post-independence Kenya and Tanzania, and Malaysia under Mahathir Mohamad), but disentangling this relationship is not straightforward given the broad overlap.

In sum, *Dictators at War and Peace* is an excellent book, which makes a number of careful and interesting arguments about an important but understudied topic. Weeks makes the most convincing case yet that, like leaders of democracies, many autocratic leaders are accountable to domestic audiences who rein in riskier behavior and thus help prevent many of the worst foreign policy mistakes. The empirical analysis is admirably clear and consistently reasonable, even if the results are not always beyond dispute. Even where arguments are more open to question, whether in the analysis of Juntas or the existence of large numbers of autocracies that fall outside her empirical scope, Weeks's work will provide a starting point for important future research. In short, this book is a major contribution that deserves to be read both widely and carefully.

Author's Response by Jessica L.P. Weeks, University of Wisconsin-Madison

I am very thankful to H-Diplo/ISSF for hosting this discussion of my book. I am grateful that four extraordinary scholars—Daniel Reiter, Alexander Downes, Hein Goemans, and Alexander Weisiger took the time to provide such detailed and thought-provoking comments. Their essays raise helpful questions about my book, and suggest many productive avenues for future scholarship about dictatorships and foreign policy.

I will begin by responding to comments about the theory. Alexander Downes focuses much of his discussion on the book's typology of authoritarian regimes. As he notes, I differentiate regimes around two dimensions: first, whether or not the leader faces a powerful domestic audience, and second, whether the key decisionmakers in the regime are civilians or military officers. Leaders of personalist boss and strongman regimes do not face powerful domestic audiences and therefore face relatively few domestic constraints in their foreign policy decisions, while leaders of nonpersonalist civilian machines and military juntas are accountable to politically important domestic groups that shape their decisions about war and peace.

Downes's commentary focuses on the two kinds of nonpersonalist regimes: machines and juntas. Downes correctly points out that my argument assumes that machines feature strong civilian control of the military. Otherwise, the fear of a military coup or insurrection would cause leaders of machines to act more like the leaders of military juntas, in which a leader faces a domestic audience composed primarily of military officers. Leaders of juntas, I argue in the book, are more likely to use force because their audience can benefit from arms buildups and war, and because they tend to be more pessimistic about the efficacy of alternatives to war such as diplomacy.

Downes notes that I do not explicitly say that the leaders of juntas must be military officers themselves, though he points out that in one place in the text I imply this to be the case. Downes then points to Imperial Japan and Wilhemine Germany as examples of regimes that he thinks should be considered machines according to my typology, but which behave more like juntas because civilian elites shared power with, and often could not control, the military.¹

Downes's commentary raises an important question about the theory, and I appreciate the opportunity to clarify how I conceptualized and coded regime type. While the term "junta" typically evokes a team of military officers, I use the term slightly differently in the book. When leaders are constrained by a domestic audience, I code the regime as military versus civilian, based on whether the domestic *audience* was composed primarily of civilians (machines) or military officers (juntas). Therefore, by my definition, Japan is considered a junta regime when it went to war against China in 1937, even though the leader at the time, Prime Minister Konoe, was a civilian. It is clear that by that time, civilian leaders knew that their survival in office depended on the support of the military. In 1932, the civilian prime minister had been assassinated by a radical group of junior naval officers, and in early 1936, 1400 military officers attempted a takeover of the government,

¹ Downes also argues that Egypt under President Gamal Abdel Nasser is a regime that is difficult to code. I code it as a strongman regime in which a military officer presides over a personalistic regime, but he reads it as a regime in which Nasser actually faced a powerful domestic audience of military officers, i.e., a junta. I agree that this is a difficult case to code, though I leave it to country experts to decide whether the regime is better categorized as a junta or strongman regime.

resulting in the death of several top civilian leaders. Although the rebellion failed, it demonstrated the domestic coercive power of the military. Moreover, civilians were outnumbered by military officers in important ministries, and were increasingly excluded from important political and military decisions.² For these reasons, I coded the domestic audience in Japan as stemming primarily from the military. While the book does not delve into Wilhemine Germany, Downes's description suggests that the leadership of this period might, like Japan, be coded as a junta because of the domestic power of the military.

Thus, I believe that Downes and I agree that the military can exert influence even when the leader is a civilian. However, Downes raises an important issue that I do not address in the book: whether "civilian-led-juntas" like Japan are different from "pure" juntas like Argentina in the 1970s. Downes suggests that power-sharing between civilians and the military could introduce unique pathologies into the decisionmaking process. Drawing on research by Risa Brooks, he argues that competition for control between the military and civilians could lead to very poor strategic assessments, perhaps even poorer than those produced by a purely military government.³ This is an excellent point, and one I do not consider in the book. I hope to see it explored further in the burgeoning theoretical and empirical literature on civil-military relations.

Hein Goemans also comments in detail on the theory, though from a different angle. Goemans advocates building an argument more closely around the bargaining theory of war, which focuses on the puzzle of why states choose war rather than reaching a more efficient peaceful bargain. He writes that it is not necessary to "side-step the inefficiency puzzle of war to make predictions about the war-proneness of different authoritarian regime types," and points to some of his own excellent work that builds on the bargaining model to explain variation across dictatorships.⁴

I think, however, that to characterize my argument as side-stepping the bargaining model of war is not accurate. While the focus of my theory is not on how regime type affects private information, commitment problems, or indivisible issues, my argument dovetails well with the bargaining model in its attention to the size of the bargaining range between two countries. My argument suggests that the bargaining range is smaller when one leader is relatively immune to the costs of fighting or losing wars, gains private benefits from war, or has inaccurate assessments of the likelihood of winning, each of which is influenced by domestic regime type. A small bargaining range, in turn, makes it more likely for factors such as commitment problems and private information to cause bargaining to fail. Thus, I intended my argument to be easy to integrate with this important perspective on war, rather than side-stepping it.

² Downes points out that although I code the regime as a machine in 1931, the military had significant power during that entire period because the Meiji Constitution of 1889 gave the military the power to topple the government by withdrawing or refusing to name the Army or Navy Minister. This is an interesting point, and I agree that it underscores the difficulty of coding regimes into a four-category typology.

³ Risa A. Brooks, *Shaping Strategy: The Civil-Military Politics of Strategic Assessment* (Princeton, N.J.: Princeton University Press, 2008).

⁴ Alexandre Debs and H. E. Goemans, 2010. "Regime Type, the Fate of Leaders and War." *American Political Science Review*, 104: 430-445.

Nonetheless, I agree that there is still much to be learned by linking monadic arguments about regime type to (dyadic) theories of strategic interaction. My aim was to characterize the domestic politics of making decisions about war in non-democracies and to develop some core hypotheses, none of which are inconsistent with the bargaining model. Of course, that still leaves many future steps. One is to understand how the politics of different kinds of authoritarian systems might compensate for a small bargaining range, or lead to war even when the bargaining range is large. Another is to investigate how different types of regimes interact. Another, as Goemans points out, is to integrate time-varying factors into the model, allowing for within-regime variation rather than the across-regime variation on which I focus. I hope my book will spur future scholarship to engage in those theoretical tasks, whether by building on my work or critiquing it.

Other comments focused on the book's empirical sections. In Alex Weisiger's judgment, the results about juntas are less persuasive than those about the distinction between personalist and nonpersonalist regimes. As he points out, juntas are rare, so the findings about juntas' war outcomes in Chapter 3 rest on a small number of cases; had some cases been recoded, the success rate of juntas would have looked more like that of personalists. Like Downes, Weisiger also questions whether the Japanese regime is a pure example of a junta given that there were nominal civilian leaders, an issue I discussed above.

Weisiger also suggests that the explanation for the Argentine invasion of the Falklands is not persuasive because it was "rational" for the junta to have been pessimistic about diplomacy and optimistic about the likely success of a military operation. This again raises an important question about my argument. I tried not to suggest that military juntas are "irrational" in their decisions about war. Rather, I recognize that in the low-information environments that typically characterize decisions about conflict, many different decisions can be rationalized. The argument in the book is that all else being equal, civilian decisionmakers faced with the same situation would not have been as likely to use military force. Civilians, I argue, would have been more likely to raise the issue in a venue like the United Nations (UN), where many countries supported decolonization. Civilians, who, unlike military officers, might not have been preoccupied with the operational advantages of first strikes, would also have been less eager to attack first. The book also argues that civilians would have been more likely to stay put in the South Georgia islands, inviting the UK to look like the aggressor. Civilians would also not have looked only to local military capabilities as a signal of resolve, but would have been more likely to consider the domestic political picture in the UK. Thus, I agree with Weisiger that one can rationalize the junta's decision, but do not think that this undermines my conclusion that war was more likely because the decisionmakers stemmed from the military.

Goemans raises different questions about the conclusions I draw from the Argentine case. He argues that I do not engage enough with an alternative diversionary explanation for the war, namely that that General Galtieri had reason to fear severe punishment (such as death, imprisonment, or exile) if he lost office, which he expected would come at the hands of naval minister Jorge Anaya if he did not make progress on the Falklands.⁵ This, he points out, is different from the more common diversionary interpretation, in which the junta went to war because it feared a domestic revolt.

⁵ Chiozza, Giacomo and H.E. Goemans. 2011. *Leaders and International Conflict*. Cambridge: Cambridge University Press.

I disagree with Goemans's claim that the book does not grapple with this explanation. Goemans writes that I do not explicitly discuss the fact that Galtieri feared an overthrow by Admiral Anaya, who supported war enthusiastically. While I did not single out Anaya as uniquely threatening to Galtieri, my argument is that part of what drove Galtieri to war were his concerns about the preferences of his audience, including Anaya. This is because leaders of juntas must follow the wishes of their domestic audiences or face sanctions such as removal. According to my argument, Anaya was an important member of that audience, and I discuss his strong support for the operation explicitly through the case study.⁶

Moreover, it is important to note that Galtieri's fear of removal at the hands of fellow military officers for failing to invade the Falklands does *not* distinguish my explanation from that of Goemans. Rather, the key distinction between our explanations is whether Galtieri made what he believed to be a desperate gamble because he feared severe punishment, or chose war because he and members of his domestic audience, composed of other junta members and top military officers, believed that diplomacy would be fruitless and that war could further the military's parochial interests. As I argue in the book, members of the junta supported war in the Falklands even before coming to power; their preference for war was not induced by fear of violent overthrow at the hands of their peers. If anything, Goemans's focus on Admiral Anaya raises the question as to why he supported the war so fervently; Goemans takes this as exogenous, but in the book, I attribute much of Anaya's fervor to his naval background and parochial interests. Moreover, I did not find evidence that Galtieri gambled on an invasion of the Falklands despite believing to be hopeless endeavor – in fact, in his review, Alex Weisiger argues that the junta had reason to expect exactly the opposite.⁷ As for whether Galtieri expected severe punishment if he did not invade, I discuss at length the fates of previous junta members in Argentina and leaders of neighboring political systems similar to Argentina's, finding that deposed leaders of nonpersonalist juntas typically were not punished severely.⁸ Goemans counters that Galtieri was eventually if briefly imprisoned, but does not mention that this was only after the democratic turnover, and that the sentence was levied specifically because he had invaded the Falklands – not because he had stayed out.

The essays also provided very useful comments on the quantitative analyses of conflict initiation and conflict outcomes. Weisiger points out that in Chapter 2, there is a large residual category of non-democracies for which I did not have data. He points out, and I agree, that future scholarship should verify that the findings are not driven by the inclusion of particular regimes in this category. In my understanding, exciting ongoing data collection efforts by Barbara Geddes, Joseph Wright, and Erica Frantz will allow scholars to construct measures of regime type that are comparable to those I use in my book, and check whether the results

⁶ See 111-114.

⁷ Goemans does not address Galtieri's outlook for the war in his essay, or appear to discuss it in his other writing on the subject (Chiozza and Goemans 2011).

⁸ See 116-117. Moreover, as I note elsewhere in the book, the leaders of the most belligerent regimes are punished very rarely (74-75).

change.⁹ Similarly, I agree with Weisiger's comment that future scholarship should analyze whether the results are robust to controlling for possible confounding variables beyond those I studied.

Finally, Goemans raises the question of whether the research design I chose for Chapter 2, which focuses on directed-dyad-years as the unit of analysis, is appropriate for assessing my theory, which is monadic. It is true that my theory focuses on how domestic politics in one country affect its conflict behavior overall, rather than delving into how different authoritarian regime types interact with each other. It is also true that the unit of analysis in Chapter 2 is the directed dyad-year rather than the country-year. However, the dependent variable is conflict initiation by Side A of the dyad, and the explanatory variables measure regime type for Side A. Thus, the tests are about the monadic effect of regime type, regardless of the target's regime type. Why then, one could ask, not use a smaller, simpler dataset? The reason is that an enormous literature suggests that the strongest predictors of international conflict are dyadic in nature.¹⁰ For example, geographic proximity, shared interests, and trade levels have all been argued to affect interstate relations. To try to boil these down to the monadic level would leave the analysis open to critiques of omitted variable bias, which is why I opted for the dyadic approach that is the most common for analyzing the onset of military disputes (I also included fixed effects in the analyses). Nonetheless, I hope that future research will evaluate whether and how the conclusions change given different ways of approaching the empirical analysis.

Again, I thank these scholars for their commentaries, and I hope that this exchange will stimulate future research on dictatorships and foreign policy.

⁹ <http://sites.psu.edu/dictators/research/in-progress/>

¹⁰ Note also an emerging literature going in the opposite direction, suggesting that we should look at k-yads, rather than dyads, when analyzing some multilateral events such as alliances or major war. Paul Poast, 2010. "(Mis)Using Dyadic Data to Analyze Multilateral Events." *Political Analysis* Autumn 2010:403-425.