

Voting For The President: The Supreme Court During War

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An extraordinary body of scholarship suggests that wars, especially major wars, stimulate presidential power. And central to this argument is a conviction that judges predictably uphold elements of presidents' policy agendas in war that would not withstand judicial scrutiny in peace. Few scholars, however, have actually subjected this claim to quantitative investigation. This article does so. Examining the universe of Supreme Court cases to which the US Government, a cabinet member, or a president was a named party over a 75-year period, and estimating a series of fixed effects and matching models, we find that during war Justices were 15 percentage points more likely to side with the government on the statutory cases that most directly implicated the president. We also document sizable effects associated with both the transitions from peace to war and from war to peace. On constitutional cases, however, null effects are consistently observed. These various estimates are robust to a wide variety of model specifications and do not appear to derive from the deep selection biases that pervade empirical studies of the courts. (*JEL* K0, K3, Z0).

Among stimulants to presidential power, war knows no equal. On this, consensus has reigned for quite some time. Wrote James Madison in *Helvidius* 4, "war is in fact the true nurse of executive aggrandizement." A century later, with the nation having fought two major wars and one catastrophic civil war, James Bryce (1995 (1888): 48–49) observed that "[Though] the direct domestic authority of the president is in time of peace very small...[in war] it expands with portentous speed."

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Reflecting on the two greatest wars of the 20th century, Edward Corwin (1957: 261) noted: “The President’s power as Commander-in-Chief has been transformed from a simple power of military command to a vast reservoir of indeterminate powers in time of emergency.” By Clinton Rossiter’s account (1956: 64–65), it is no less an “axiom of political science” that “great emergencies in the life of a constitutional state bring an increase in executive power and prestige.” And recently, John Yoo (2009: vii) observed that “War acts on executive power as an accelerant, causing it to burn hotter, brighter, and swifter.”

Courts, most concede, facilitate the expansion of a wartime presidency. Indeed, a massive legal literature on “crisis jurisprudence” takes as its operating premise the existence of heightened judicial deference to the president during times of war (for recent reviews, see Cole 2003; Tushnet 2003; Epstein et al. 2005; Gross and Ni Aolain 2006). Judges, it is postulated, employ laxer standards for evaluating presidential actions and policies during war than they do during peace. Consequentially, courts repeatedly uphold elements of wartime presidents’ agendas that, during peace, they invariably would overturn. On this, all scholars contributing to the crisis jurisprudence literature agree.

The overwhelming scholarly consensus that wars augment presidential power, however, belies a number of unresolved issues and ongoing debates within the crisis jurisprudence literature. Three, in our view, are especially amenable to empirical investigation. First, because the existing empirical basis for this literature consists nearly exclusively of case studies, we lack any estimates of the magnitude of the effect of major wars on the propensity of courts to rule in favor of the president. As Lee Epstein et al. lament (2005: 36), “empirical support for the crisis thesis in any one study is flimsy. It consists not of systematically derived data and carefully designed and executive analyses, but rather of anecdotal evidence from a handful of highly selected court decisions.” Though a few scholars recently have subjected some of the positive arguments of crisis jurisprudence to quantitative evaluation, we still lack clear estimates of war’s impact on the judiciary’s willingness to uphold the specific actions taken and policies supported by the president.

Legal scholars also continue to argue about the relevant bases for courts to side with the president during war. Some, such as Corwin (1947), point to a more expansive reading of Article II powers; and others, such as Issacharoff and Pildes (2004: 32), emphasize a more “process-based, institutionally-focused” judicial role. Finally, scholars disagree about whether the gains presidents make during war carry over into subsequent periods of peace. On this score, some, such as Rossiter (1956), posit the existence of “ratchet” effects; whereas others, such as Posner and Vermeule (2007), recognize periods of congressional and judicial retrenchment in the aftermath of major wars.

This article examines each of these unresolved issues and ongoing debates. Rather than engage the ongoing normative discussions about

whether judges ought to employ crisis jurisprudence, we scrutinize the prior evidentiary basis for believing that judicial standards for reviewing presidential actions predictably relax when the nation enters war. To do so, we survey the universe of Supreme Court cases to which the US Government, a cabinet secretary, or the president was a named party between 1933 and 2007, and we rely upon court appearances by the Solicitor General (SG) to identify those cases that most directly implicate the president. We find that Justices are roughly 8 percentage points more likely to side with the president during major wars (i.e., World War II, the Korean War, the Vietnam War, Gulf War, and post-9/11 era) than during peace. On statutory cases, however, Justices are 15 percentage points more likely to side with the president during peace than war, whereas on constitutional cases, Justices are no more or less likely to do so. Moreover, on the subset of cases where wartime effects are observed, the transition from peace to war appears only slightly more consequential than the transition from war to peace. These various estimates are consistent across a wide variety of statistical models, each of which is expressly designed to mitigate the deep selection biases that plague empirical studies of the courts.

We proceed as follows. Section 1 reviews the positive arguments of crisis jurisprudence and the existing studies that subject them to quantitative evaluation. Section 2 introduces our data and modeling strategies. Section 3 presents our main results, while Sections 4 and 5 subject them to a wide variety of robustness checks, evaluate the extent to which they reflect selection biases, and propose additional tests explicitly designed to reduce these biases. Section 6 concludes.

1. The Existing Literature on Judicial Decision-making in War

Scholars and statesmen have long argued that judges are particularly more prone to support the president's policy agenda during times of war. In terms of sheer volume, constitutional law scholars have written the most significant research on the topic. After briefly characterizing the positive (contra normative) elements of the crisis jurisprudence literature, this section recognizes important unresolved debates therein.

1.1 Crisis Jurisprudence Expressed

An extraordinary amount of legal scholarship emphasizes the relevance of war in judicial decision-making. Hundreds of jurists have contributed to this scholarship, which goes by a variety of names—most commonly “crisis jurisprudence” (Epstein et al. 2005), but also the “constitutional law of war” (Corwin 1947: 76), “executive expediency discourse” (Paul 1998), the “doctrine of constitutional relativity” (Smith 1951), and the “judicial deference thesis” (Posner and Vermeule 2007). Though its sources and appellations vary, this literature has a single purpose: to offer clear counsel to judges who are asked to adjudicate disputes about government actions during war.

Though most of the literature on crisis jurisprudence focuses on government abridgements of individual rights, a good deal of it implicates the president. It is the president, after all, who is charged with assessing foreign threats, formulating responses to them, and ultimately executing the nation's wars. And when attending to the domestic front during times of war, presidents do a great deal more than curtail civil liberties. They interfere in labor management disputes, seize domestic industries, set prices, ration scarce goods, create, kill, and redesign administrative agencies, and much, much more (for a review, see Howell 2011).

Among constitutional law scholars, a lively debate persists about how the courts ought to respond to these actions, and whether judges have a principled basis for incorporating concerns about war in their verdicts. Advocates of crisis jurisprudence respond decidedly in the affirmative. Indeed, the core thesis of crisis jurisprudence can be stated quite simply: when the life of the nation is in danger, the courts should grant presidents the latitude they need in order to prosecute wars; and consequentially, at least some presidential actions—both international and domestic—that do not survive judicial scrutiny during times of peace justifiably do so during periods of war. For at least as long as American troops are fighting and dying, a wartime jurisprudence, one that looks considerably more kindly upon exercises of presidential power, supplants a peacetime jurisprudence.

Crisis jurisprudence thereby constitutes a direct repudiation of the notion, often expressed by judges themselves, that the government cannot change “a constitution, or declare it changed, simply because it appears ill-adapted to a new state of things.”¹ Quite the opposite, crisis jurisprudence insists that the Constitution, if it is to survive, must adapt and evolve. The material context in which presidents operate crucially shapes the judiciary's assessment of the constitutionality of their actions. And as contexts go, wars legitimate presidential action like no other. As Justice Felix Frankfurter argued in *Korematsu v. United States*,² “the validity of action under the war power must be judged wholly in the context of war. That action is not to be stigmatized as lawless because like action in times of peace would be lawless.” Plainly, the president cannot round up thousands of citizens of a particular national background on a whim. But as long as the nation's security is at risk, Frankfurter reasoned, the courts ought to grant presidents a measure of deference that appropriately eludes their peacetime associates.

Positivist assessments of the pervasiveness of crisis jurisprudence, of course, should not be confused with a wholesale endorsement of its adoption. Indeed, vigorous debates continue about the efficacy of crisis jurisprudence—that is, whether the Constitution justifies it, whether it serves different notions of the larger public good, whether it corrodes our system

1. *South Carolina v. United States*, 199 U.S. 437 (1905).

2. *Korematsu v. United States*, 323 U.S. 214 (1944).

of checks and balances, and so forth.³ But while scholars dispute the legal justifications and social merits of crisis jurisprudence, all appear convinced of its existence. Indeed, at the very center of a literature defined by competing normative assertions resides a singular positive claim: as Posner and Vermeule (2007: 4) put it, it is “inevitable” that the courts will defer to the president during times of war.

- 1.2 Unresolved Empirical Issues and Ongoing Debates about Crisis Jurisprudence
That legal scholars are convinced of the existence of crisis jurisprudence does not mean that they speak with one voice, or even speak at all, about a number of key issues. Three, in our view, stand out: the magnitude of wartime effects on judicial decision-making; the relevant jurisprudence for resolving war-time disputes involving the president; and the existence of “ratchet” effects, which suggest that wartime gains to the president carry over, in full, to subsequent periods of peace. In the sections that follow, we present empirical findings that clarify all three of these unresolved issues and ongoing debates. But first, we briefly elaborate on the stakes involved.

1.2.1 Magnitude of Wartime Effects. Almost without exception, legal scholars ground their arguments about crisis jurisprudence in textual analyses of canonical court cases, *Korematsu* among them. They do so, moreover, with cause. In their majority, dissenting, and concurring opinions to these cases, Supreme Court Justices reflect openly and deeply on the relevance of war for either supporting or opposing the president. Only the most jaundiced legal realist would place zero weight on the evidentiary value of such ruminations, understood either as expressions of the motivations behind judicial votes or as outcomes worthy of analysis in their own right.

There is a downside, however, to maintaining an exclusively case-study approach to studying crisis jurisprudence. By scrutinizing only a handful of cases, legal scholars do not—indeed, cannot—gauge just how large the effect of war is on judicial decision-making. As Epstein et al. (2005: 41) point out, “to the degree that previous analyses have focused on a small number of decisions, we have no understanding of the magnitude of the impact of war.”

To be sure, a handful of scholars have begun to take a more systematic look at the positive assertions of crisis jurisprudence. Surveying the universe of Supreme Court cases involving civil liberties during the latter half of the 20th century, Lee Epstein et al. (2005) find substantial evidence that the courts do in fact take a narrower view of individual rights during periods of war, particularly when the courts are asked to overturn policies that directly involve a war effort and that infringe upon individual rights. These findings, however, speak only tangentially to issues involving

3. A sampling of recent publications includes Fletcher 2002; Gross 2003; Pushaw 2004; Wells 2004; Ackerman 2006; Posner 2006; Posner and Vermeule 2007; Fisher 2008; Fatovic 2009; Matheson 2009.

presidential power, as many of the cases in Epstein et al.'s data set concern challenges to laws enacted by Congress, which sitting presidents may or may not support. Hence, it is difficult to know whether any particular ruling supports Congress, the president, both, or neither.

In her survey of US tax cases, Nancy Staudt (2011) similarly finds that the judiciary is more likely to side with the US Government during times of war. During "cued" wars, which require Congress to take some positive action to prepare the nation for war, Staudt finds that the Supreme Court is about 4 percentage points more likely to side with the US Government. During actual wars, the Supreme Court does not appear any more likely to issue rulings that increase the federal fisc. Staudt provides no evidence, however, on whether the findings in these tax cases apply either to the broader range of policies advanced by the US Government or to the subset that particularly concern the president.

In a study of noncriminal appellate court cases to which the US Government was a party, Tom Clark (2006) finds no evidence of heightened judicial deference to the government during periods of war. In fact, Clark's findings suggest that appellate judges are significantly more likely to rule against the government during wartime, leading him (2006: 416) to conclude that "constitutional checks and balances placed on executive power do not necessarily collapse during wartime." But he neither presents any evidence about Supreme Court rulings, nor does he distinguish the subset of his cases that directly concern the president.

Collectively, these three studies yield mixed assessments of the relevance of war for judicial decision-making. It is not altogether clear, though, how any of them implicates the president, *per se*. Either because they focus on a particular policy issue or because they equate the US Government with the president, these studies do not yield clear estimates of the impact of war on the judicial support for the president's policy agenda. Moreover, each of these studies confronts the standard selection biases associated with empirical research on judicial decision-making. It is quite possible, probable even, that judges hear very different types of cases during times of war than during times of peace. On especially high profile cases, judges may delay rendering a decision until after a military conflict has subsided. And Supreme Court Justices may refuse to grant cert in cases that, sometimes by their own admission, would command their attention during periods of peace. Recognizing these challenges, Clark (2006: 416) cautions that "much further analysis [is required] before a broad claim may be staked about the nature of noncriminal adjudication during wartime."

1.2.2 Expressions of Crisis Jurisprudence. Legal scholars disagree not only about how Supreme Court Justices should resolve wartime disputes involving the presidency, but also about the relevant jurisprudence they in fact employ. On one side are scholars who insist that the Justices take a more expansive reading of Article II powers during times of war. Under this account, Justices recognize that constitutional meanings, rather than

being fixed, of necessity change as the nation enters and exits wars. As Corwin (1957: 80) puts it, “the principal canons of constitutional interpretation are in wartime set aside so far as concerns both the scope of national power and the capacity of the President to gather unto himself all constitutionally available powers in order to more effectively to focus them upon the task of the hour.” Wars, of course, do not altogether negate constitutional limits on presidential powers. But as Corwin goes on to argue, wars make such constitutional limits “considerably less stiff—the war emergency infiltrates them and renders them pliable.”

Other scholars, however, argue that crisis jurisprudence primarily expresses itself in procedural deliberations. Wary of enshrining their wartime rulings for the ages, Justices tend not to reason from the Constitution when upholding actions taken or policies supported by a wartime president. To do so, as Issacharoff and Pildes (2004: 12) argue, would “freeze into place, much more rigidly than is desirable . . . the institutional options that ought to be available to the government” in times of war. Hence, “in terms of actually defining first-order claims of rights, American courts show great reticence to engage the permissible scope of liberties in direct, first-order terms.” Rather, when recognizing the relevance of war for a policy dispute involving the president, the Justices tend to look to Congress for both guidance and cover.

This dispute concerns a great deal more than just the logical basis for crisis jurisprudence. It also speaks to its reach. If Corwin is correct, then the Supreme Court should appear more deferential to the president on all wartime cases. On the other hand, if Issacharoff and Pildes have a better grip on the truth, then evidence of crisis jurisprudence should be confined to the subset of cases that involve matters of statutory interpretation.⁴ For on this latter account, the incidence of war should not bear upon deliberations about constitutional rights and principles.

1.2.3 On “Ratchet” Effects. Whatever their magnitude or reach, there is the matter of wartime effects’ persistence. By some accounts, Justices renegotiate the appropriate boundaries of presidential power with each successive war; and that however the Justices rule, the resulting expansion endures when peace is restored. As Rossiter (1956: 65) puts it, wars augment presidential power “always at least temporarily, more often than not permanently.” Hence, over time presidential power is best characterized by a sequence of “ratchets” constituting something akin to a step function

4. Another test of Issacharoff and Pildes’s thesis would distinguish those wartime policies that are the product of joint presidential–congressional action from those that derive from the president alone. For two reasons, however, we do not pursue this line of inquiry. First, assessing congressional support for the president at the case-specific level is fraught with measurement problems. And second, as Issacharoff and Pildes themselves make clear, the willingness of Supreme Court Justices to rule in favor of the president on purely constitutional matters is itself a quantity of both theoretical and empirical interest.

in which transitions from peace to war are hugely consequential, whereas transitions from war to peace matter not at all.

Claims about ratchet effects, however, are not without their critics. Just as the outbreak of war inevitably induces an expansion of presidential power, say [Posner and Vermeule \(2007\)](#), so does the restoration of peace induce a retraction, as Congress and the courts recover whatever discretionary authority they had temporarily ceded to the Commander-in-Chief. In this sense, judicial deference—and by extension, presidential power—both flows and ebbs as a consequence of the nation entering and exiting wars.

2. Data and Methods

To investigate these enduring debates about crisis jurisprudence, we examine the universe of Supreme Court cases that explicitly name either the US Government, a member of the president's cabinet, or the president himself as a party.⁵ These selection criteria provide a straightforward and transparent way of identifying cases that concern the federal government generally, and the executive branch in particular.⁶ The time series begins when Franklin D. Roosevelt assumed office in 1933 and ends on December 31, 2007. Following Epstein et al., Staudt, and Clark, we identify the following military conflicts as “wars”: World War II, the Korean War, the Vietnam War, the Persian Gulf war, and the post-September 11 wars in Afghanistan and Iraq.⁷ During our time period, we have a total of 2193 cases, 685 of which occurred during war, and 1508 during peace.⁸

Our sample includes such staples of the existing crisis jurisprudence literature as: *Hirabayashi v. United States* (1943) and *Korematsu v. United States* (1944), which involved the treatment of Japanese Americans during World War II; *Hamdi v. Rumsfeld* (2004), *Hamdan v. Rumsfeld* (2006), and *Jose Padilla v. United States* (2006), which addressed the treatment of “enemy combatants” in the contemporary war on terror; and perhaps the most influential wartime case involving presidential power during the last century, *Youngstown Sheet & Tube Co. v. Sawyer* (1952). But our sample also includes a wide range of cases that do not explicitly involve war, such as *United States v. Nixon* (1974) that focused on the president's

5. To identify the universe of Supreme Court cases between 1953 and 2006 that named the US government, cabinet member, or president as a party, we used the *party1* and *party2* variables in ([Spaeth and Randazzo 2006](#)). During this period, we identified a total of 1513 cases. For cases after 2006 and before 1953, we searched the Supreme Court database available at: <http://bulk.resource.org/courts.gov/c/US/>. These searches yielded 667 cases for the period 1933 through the end of 1952, and 13 cases for 2007.

6. For similar selection criteria on a related study, see [Clark 2006](#).

7. The following dates delineate the beginnings and ends of US involvement in each of our wars: World War II (December 7, 1941 to August 14, 1945), Korea (June 27, 1950 to July 27, 1953), Vietnam (February 7, 1965 to January 27, 1973), Gulf War (January 16, 1991 to April 11, 1991), and post-9.11 (October 7, 2001 to December 31, 2007).

8. All statistics for cases and justice votes are from our estimating sample.

power of executive privilege; *Katz v. United States* (1967) which dealt with search and seizure issues associated with wiretapping; *United States v. Lopez* (1995) which focused on Congress's power, via the Commerce Clause, to prohibit the possession of guns near schools; *Clinton v. City of New York* (1998) which concerned the line item veto; and *Clinton v. Jones* (1997) which was a civil suit involving charges of sexual harassment.

The cases in our sample pit a wide variety of parties against the US Government, a cabinet secretary, or president (hereinafter, collectively referred to as the "government"). The most common are individuals, who appear in 59% of the cases. Corporations opposed the government in 27% of the cases, and the remaining 14% of the litigants comprised state governments, interest groups, and other parties.⁹ Though these groups may have been acting on behalf, or at least in consultation with, members of Congress, in none of the cases are members of Congress themselves listed as parties.

So to do, we observe significant variation in the substantive issue areas examined by the Supreme Court. Thirty-two percent of the cases pertained to criminal procedure, 22% to economic relations, 11% to federal taxation, 10% dealt with civil rights, 8% on judicial powers, and the remaining 17% covered such issues as first amendment rights, due process, and federalism.¹⁰

2.1 Identification: Implicating the President with the SG

Though all of our cases involve the federal government, they do not all implicate the president, or at least not equally so. Many of these cases concern disputes that do not directly concern the sitting president, either because they center on actions taken by one of his (someday her) predecessors, a bill enacted by the current Congress or a past one, or a policy pursued by an executive agency that the president may have little interest in defending. In principle, the Supreme Court may defer to the federal government as a whole during times of war. And in the analyses that follow, we explicitly test for this possibility. But because crisis jurisprudence most immediately concerns the nation's ability to survive, and because it is the president who, operationally, meets this challenge, we need to find a way of identifying the subset of cases that matter most to him.

9. The definition of these challenger types matches the classification of *party* type found in Spaeth and Randazzo 2006. For cases prior to 1953 and for the year 2007, we gathered this information from the original Supreme Court decisions and/or Lexis-Nexis legal search. For cases between 1953 and 2006, we collected this information from Spaeth and Randazzo 2006.

10. To identify the "type" of legal issue at dispute, we classified a case as belonging to one of 13 distinct issue areas: attorneys, civil rights, criminal procedure, due process, economic relations, federalism, federal taxation, first amendment, interstate relations, judicial power, privacy, unions, and miscellaneous. The definition of these issues is consistent with the categorization of the *value* variable found in Spaeth and Randazzo 2006.

To isolate the disputes that are most closely associated with the president, we focus on those cases for which the SG presented oral arguments before the Supreme Court. The SG's office, of course, is involved in every case in our sample. But, in slightly less than 13% of the cases did the SG himself (and recently, for the first time in the nation's history, herself) actually argue the case. For a variety of reasons, we have good reason to expect that these cases were of particular interest to the sitting president. For starters, these cases are especially likely to raise trenchant policy and/or constitutional issues. As Rebecca Salokar (2004) remarks in her in-depth examination of the SG's office, Solicitors General tend to argue cases that are objectively more important. Solicitors General also tend to argue cases that attract more attention of the media. Whereas 15.3% of the cases in our sample received coverage in the *New York Times* when the SG presented oral arguments, only 4.4% of the remaining cases did so.

Within both political science and the law, moreover, a substantial body of empirical research argues that the president and SG share the same political commitments (Bailey and Chang 2001; Koistinen 2004), and where differences emerge, the president can be expected to prevail. The SG serves at the president's pleasure, and it is the SG's job to present "the view of the Executive" to the judiciary (Perrett 1973: 1443). According to the former SG Francis Biddle (1969: 1447, note 16), the "Solicitor General does consult the Attorney General to learn the administration's position in politically sensitive cases." Hence, Michael Bailey et al. (2005: 77) argue that the SG might best be thought of as a "direct agent of the president."

It is little surprise, then, that decisions by Solicitors General to file amicus briefs to the Supreme Court systematically conform to the interests of the sitting president (Clinton et al. 2004; Bailey et al. 2005; Bailey 2007; Shor et al. 2010). More anecdotally, Solicitors General often express their acute awareness of their subservience to the president. Former SG Erwin Griswold (1969: 527) candidly observed that it is "unwise to lose sight of the reality that a SG is not an ombudsman with a roving commission to do justice as he sees it. He is a lawyer, though with special responsibilities, who must render conscientious representation to his client's interests." Subsequent Solicitors General have made similar observations. President Reagan's first SG, Rex E. Lee (1986: 599), plainly declared that representing the administration policy is "part of [the] job." Reflecting upon his own experience as SG, Kenneth Starr (2008: 144–145) recognized that "I was an 'inferior' or 'subordinate' officer in the executive branch. If I could not in conscience abide by the president's judgment, then I should resign."¹¹

We have, then, a reasonable basis for expecting that Solicitors General will *personally* argue those cases that matter most to the president, and will

11. This perspective, of course, plainly contradicts the view of Solicitor General as the 10th Justice, someone who is equally beholden to the Supreme Court as the president (Pierpaoli 2000). For an extended critique of this alternative view, see Iacus et al. 2008.

do so in ways that conform to the president's wishes. At times, this may be at the behest of their political superior (Bailey et al. 2005: 77–78). More commonly, though, Solicitors General will recognize the stakes involved in a particular case and take it upon themselves to argue it before the Supreme Court. As a general matter, therefore, the appearance of the SG in oral arguments serves as a useful signal—both to us as observers and to the Justices as participants—about the case's importance to the president.¹²

2.2 General Trends in the Volume of Cases Heard by the Supreme Court

Figure 1 plots the annual number of cases in our entire sample, as well as the subset of cases argued by the SG. For the first half of the time series (1933–69), the annual number of cases heard by the Supreme Court fluctuated around 33, whereas those argued by the SG hovered around 5. In the beginning of the early 1960s, and consistent with the Supreme Court's shrinking docket, the volume of both types of cases steadily declined. At the end of the series, each year the Supreme Court heard roughly 12 cases involving the government, just a couple of which were argued by the SG.

Around these trends, considerable variability is observed. The peak year was 1960 when the Supreme Court heard 56 cases to which the government was a party. In contrast, the Supreme Court heard just 15 such cases in 1940 and 1991, 13 in 1999, 2001, and 2007, and 12 or less between 2004 and 2006. Given his time in office, Roosevelt predictably occupied the White House for the largest number of cases in our sample: 415. Eisenhower, however, holds the top spot for the largest average annual caseload: 37, with Nixon coming in second with 34.7, and Kennedy close behind with 34. Given the secular decline in the time series, it is not surprising that the two Bush presidents hold the records for both the fewest total number of cases while in office (74 and 87 for the first and the second president, respectively) and the annual average (18.5 and 12.4, respectively).

The Supreme Court tended to hear fewer cases in war than in peace. On average, the Supreme Court heard 30.8 cases involving the government during peace years, and 26.3 cases during war years. As the shaded regions of Figure 1 illustrate, there do not appear to be any clear temporal trends within the Korean War, Gulf War, or post-9.11 era. World War II contains an early spike in the volume of challenges, but the time series quickly reverts to the mean for the period. The Vietnam War shows a steady, though not monotonic, increase in the annual number of Court cases involving the government. In 1965, the Supreme Court heard 24 cases. By the war's end in 1973, that number jumped to 47.

12. When the president is a named party to a case, of course, the appearance of the Solicitor General in oral arguments may be superfluous. It is worth noting, then, that the primary findings below carry through when identifying such cases as politically relevant to the president, regardless of whether the Solicitor General argued before the Court.

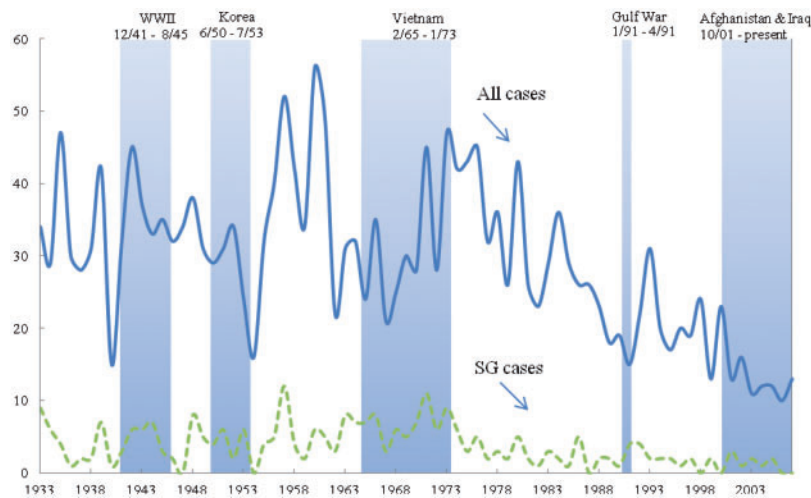


Figure 1. Trends in the Volume of Cases Involving the US Government.

Notes: Solid lines tracks the annual number of all cases from the estimating sample, and dotted lines track the annual number of these cases argued by the SG. Shaded regions denote periods of war.

2.3 General Trends in the Supreme Court's Support for the Government

Figure 2 tracks the variable levels of support given to the government by the Supreme Court during the period of study. The solid line shows the average win rates for all cases decided each year, and the dotted line shows the proportion of votes in these cases that broke in the government's favor. Not surprisingly, the two series track each other closely. During this time period, the government won, on average, 64% of the cases; and in these cases, the Justices sided with the government 52% of the time. The government received comparable levels of support on the subset of cases argued by the SG.

Surveying the full sample of cases involving the government, one finds little evidence of a relationship between war and success in the courts (Table 1). On average, the government won 63% of the wartime decisions and 65% of the peacetime decisions. Similarly, individual Justices voted for the government ~51% of the time in both periods. When turning to the subset of cases argued by the SG, however, some discernible differences emerge. Among these cases, the government won 67% of the wartime rulings and 63% of the peacetime rulings, a difference that just misses statistical significance ($p < 0.13$). On these cases, Justices voted for the government 53% of the time during war, and 48% of the time during peace, a difference that is statistically significant ($p < 0.02$).

A superficial examination of the data lends little support for the contention that crisis jurisprudence grips the Justices' attention only during massive military mobilizations on the scale of World War II. Though the government did win a slightly higher percentage of cases

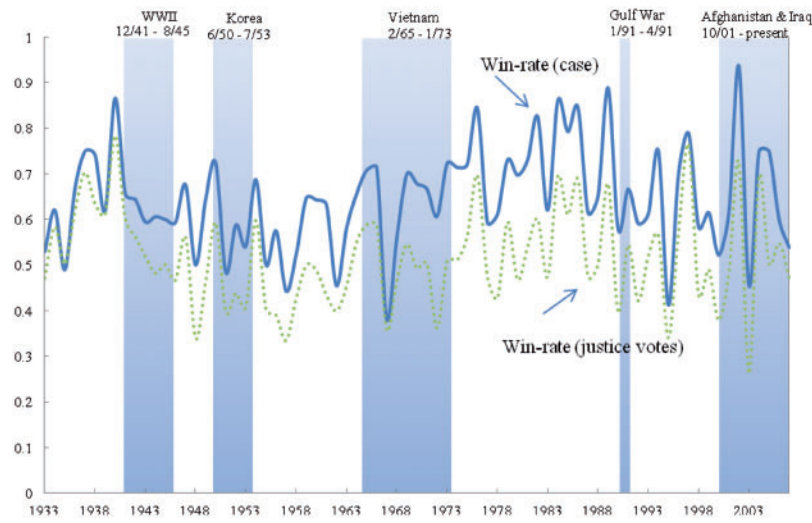


Figure 2. Trends in Judicial Support for the US Government.
 Notes: The solid lines tracks the annual percentage of all cases in the estimating sample won by the US Government, and the dotted line tracks the annual percentage of Justice votes in favor of the US Government. Shaded regions denote periods of war.

Table 1. Support for the Government by Case and Justice Votes

	Cases	<i>n</i>	Justices	<i>n</i>	Cases	<i>n</i>	Justices	<i>n</i>
Wartime decisions				Peacetime decisions				
All cases	0.63	576	0.51	5097	All cases	0.65	1618	0.52
SG cases	0.67	99	0.53	903	SG cases	0.63	183	0.48
World War II decisions				Post-WWII wartime decisions				
All cases	0.61	151	0.52	1354	All cases	0.64	425	0.50
SG cases	0.68	22	0.53	198	SG cases	0.66	77	0.53

Notes: Case and votes from estimating sample. SG refers to cases argued by the Solicitor General.

argued by the SG in World War II than in cases argued in subsequent wars (68% versus 66%, respectively), the proportion of Justices siding with the government on these cases were identical in World War II and in subsequent wars (53% for both).

2.4 Modeling Strategy

To gauge the impact of war on judicial behavior, we estimate variations of the following model:

$$Vote_{ict} = \beta_0 + \beta_1 War_t + \beta_2 SG_c + \beta_3 SG_c * War_t + \beta X_{c,t} + j_i + p_t + c_t + j_i * p_t + e_{ict}$$

where Vote_{ict} is a simple indicator variable for whether Justice i on case c at time t voted for the government.¹³ The primary covariates of interest identify whether a case is decided when the nation is at war (War_t), an indicator variable for whether the SG argued the case, and an interaction between the two ($\text{War}_t \times \text{SG}_i$). Background controls are included in the vector \mathbf{X} ; j_i , c_t , p_t , and $j_i * p_t$ identify fixed effects for Justices, presiding Chief Justices, presidents, and Justice by president combinations; and e_{ict} is an error term. So that the results may be interpreted directly as linear probabilities, we estimate our baseline models via least squares.¹⁴ To account for error dependence within each case,¹⁵ we cluster the standard errors (SEs) at the case level.

The key strength of these statistical models, and what sets them apart from most empirical work on judicial decision making, is the inclusion of such an exhaustive assembly of fixed effects. These variables account for all observable and unobservable time invariant characteristics of the Justices who cast votes, the courts over which different Chief Justices preside, the presidents then in office, and the unique relationships between individual Justices and presidents. Identification for the key covariates of interest comes from changes within each of these units. Hence, when including the full set of fixed effects, the coefficient for *War* identifies changes in the voting behavior of individual Justices facing the same president under the same presiding Chief Justice when the nation transitions either into or out of war.

As case-specific controls, the summary statistics for which can be found in Table 2, we include indicator variables for each of the policy domains that a case addresses, and each of the types of challengers faced by the government. To account for the well-documented higher win rates for petitioners to the Supreme Court, we include an indicator variable (*Petitioner*) that identifies those cases that name the government first. To account for the salience of each case, we identify whether it received any coverage in the *New York Times* within a 7-day window around the time of oral argument (*NYT Coverage*).¹⁶ We also include indicator variables that identify the different types of challenger—corporations,

13. Abstentions are dropped from the analysis. Information about Justice votes on cases between 1953 and 2006 are available in Spaeth and Randazzo 2006. For all other years, we entered each justice vote directly from the original Supreme Court decision.

14. The average value of the dependent variable (Justice vote for the government) is around 0.50, which falls on the linear portion of conventional maximum likelihood estimators (e.g., probit and logit). Thus, we can employ a linear probability model to estimate our model. Nonetheless, below we also show that these findings are robust to alternative estimation techniques.

15. The Justice fixed effects should account for dependence of votes across cases but within Justices.

16. Specifically, for each case, we checked whether the *New York Times* contained an article with the case name in the text within 7 days of the closing of oral arguments. For a defense of using the *New York Times* as a measure of case salience, see Epstein and Segal 2000.

Table 2. Summary Statistics

	Mean (Std Dev)	Min	Max
SG	0.13 (0.34)	0	1
War	0.26 (0.44)	0	1
SG*War	0.05 (0.21)	0	1
US Petitioner	0.47 (0.50)	0	1
NYT Coverage	0.06 (0.23)	0	1
Challenger type—corporation	0.23 (0.42)	0	1
Challenger type—individual	0.55 (0.50)	0	1
Challenger type—State Government	0.08 (0.27)	0	1
Challenger type—interest group	0.03 (0.16)	0	1
Challenger type—other	0.11 (0.32)	0	1
Amicus	-0.24 (1.49)	-27	26
Constitution	0.23 (0.42)	0	1
State Law	0.00 (0.05)	0	1
Federal Law	0.69 (0.46)	0	1
Judge Law	0.06 (0.25)	0	1
Divided Government	0.51 (0.50)	0	1
CoPartisan	0.56 (0.50)	0	1

Notes: Summary statistics for estimating sample. Number of observations: 19,453

individuals, interest groups, state governments, and assorted other¹⁷—that the government faced in each case.¹⁸ And finally, we identify whether a case raised issues that pertain to the US Constitution (*Constitution*), federal statutory law (*Federal Law*), state statutory law (*State Law*), or judge-made law (*Judge Law*).¹⁹

In three ways, we control for the levels of political support that a sitting president enjoys at the time that a case is being heard. First, we include an indicator variable (*Divided Government*) that identifies whether the president's party retains a majority of seats in both chambers of Congress. As a proxy for the relative amount of interest group support for the president, we inventory the amicus briefs filed in a case. The variable “Amicus”

17. This “other” category accounts for less than 2% of all the cases in our estimating sample, and identifies challengers, such as foreign governments and property claims.

18. The definition of these challenger types matches the classification of *party* in Spaeth and Randazzo 2006. For cases prior to 1953 and for the year 2007, we gathered this information from the original Supreme Court decisions. For cases between 1953 and 2006, we collected this information from Spaeth and Randazzo 2006.

19. We read through the Supreme Court's opinion and categorized whether the case pertained primarily to the US Constitution, federal statutory law, state statutory law, and judge-made law. In most instances, the primary legal issue is identified in the first or second paragraph of the majority opinion. If multiple legal issues are identified, we coded the first one listed. For cases from 1953 to 2006, our categorization of cases corresponds to the variables *LAW* and *LAWS* from Spaeth and Randazzo 2006. All of the core findings presented below carries through when cases concerning state statutory law are dropped from the analysis.

represents the difference in the number of amicus briefs filed in support of the government and the number filed against. Finally, to account for the Justice's own ideological commitments, initial models include an indicator variable (*CoPartisan*) for whether the Justice was appointed by a president of the same party as the sitting president.²⁰

3. Main Results

We present our main results in two phases. First, we examine the results from a host of fixed-effects models estimated on the universe of cases within our sample. We then distinguish those cases that deal with statutory matters from those that attend to constitutional issues.

3.1 All Cases

Table 3 presents the main results of the models that iteratively add each of the sets of Justice, presiding Chief Justice, president, and Justice-by-president fixed effects. All of the models also include policy and challenger fixed effects. The key results are remarkably stable across the various model specifications. In every instance, the main effect of *War* hovers around zero and never approaches standard thresholds for statistical significance. The interaction term $SG \times War$, however, is consistently positive and statistically significant. Depending upon the specification, the models predict that Justices are 7 to 8 percentage points more likely to side with the government, and by implication the president, during war than peace. Though substantively meaningful, this effect just misses standard thresholds for statistical significance on a two-tailed test.

For the most part, the background control variables behave in expected ways. As the first two models show, Justices are approximately 3 percentage points more likely to side with the government when they share the same party affiliation as the president. When the full complement of Justice and president fixed effects is included, however, this variable drops out of the analysis. The Justices are 16 percentage points more likely to side with the government when it is the petitioner in a case—an effect that is both large in magnitude and highly statistically significant. Indeed, among the time-varying regressors, only “Petitioner” registers effects that are consistently larger in magnitude than $SG \times War$.

The Justices are more likely to vote for the government in cases that raise constitutional, federal statutory, or state statutory issues than those that strictly concern judge-made law, which is the excluded category. Only

20. As a fourth measure of support for the president, we examined public opinion polls. Unfortunately, these data are not available for the first 4 years of our time series. When estimating models that include controls for the last measured approval rating for the president before a case was heard for the shorter time series, we recover results that broadly conform to those reported below.

Table 3. Probability of Siding with the President during War

	(1)	(2)	(3)	(4)
SG	-0.04 (0.03)	-0.04 (0.03)	-0.03 (0.03)	-0.03 (0.03)
War	-0.01 (0.02)	-0.03 (0.03)	0.01 (0.03)	0.00 (0.03)
SG*War	0.08 (0.05)	0.08 (0.05)	0.08 (0.05)	0.07 (0.05)
... <i>p</i> -value (two sided)	0.122	0.118	0.145	0.15
US Petitioner	0.16 (0.02)***	0.16 (0.02)***	0.16 (0.02)***	0.16 (0.02)***
NYT coverage	0.02 (0.03)	0.02 (0.03)	0.02 (0.03)	0.02 (0.03)
Amicus briefs	0.00 (0.01)	0.00 (0.01)	0.00 (0.01)	0.00 (0.01)
Constitutional	0.07 (0.03)**	0.08 (0.03)**	0.07 (0.03)**	0.07 (0.03)**
State Law	0.11 (0.22)	0.11 (0.22)	0.10 (0.22)	0.09 (0.22)
Federal Law	0.10 (0.03)***	0.11 (0.03)***	0.11 (0.03)	0.11 (0.03)
Divided government	-0.01 (0.02)	-0.01 (0.04)	0.02 (0.04)	0.02 (0.04)
CoPartisan	0.03 (0.01) ***	0.00 (0.01)	0.00 (0.01)	-
Fixed effects for				
Policy domain	Yes	Yes	Yes	Yes
Challenger type	Yes	Yes	Yes	Yes
Individual Justices	Yes	Yes	Yes	Yes
President	No	Yes	Yes	Yes
Chief Justices	No	No	Yes	Yes
Justice × President	No	No	No	Yes
<i>n</i> (Justice votes)	19,171	19,171	19,171	19,171
<i>n</i> (cases)	2244	2244	2244	2244
<i>R</i> ²	0.06	0.07	0.07	0.08

Notes: Ordinary Least Squares (OLS) estimation. Sample of all cases includes those listing the US government, the president, and/or a cabinet member as litigants. Robust SEs clustered by case reported in parentheses. *Significant at 10%, two-tailed test; **significant at 5%; ***significant at 1%. Dependent variable is Justice votes cast for the government (1 = yes, 0 = no).

the effects for “Constitution and Federal Law,” however, are significant. In none of the models, however, does the main effect SG appear statistically significant.²¹ The Justices do not appear to be especially deferential in all cases in which the SG makes oral arguments. Rather, the deployment of the SG has special salience only during times of war. Additionally, *NYT Coverage*, *Divided Government*, *Amicus*, and the challenger indicator variables all appear unrelated to how Justices vote on the cases in our sample.

We do not report the effects for all the Justice, president, chief justice, and Justice-by-president fixed effects. As one might expect, however, considerable variation is observed within the specified categories. The fixed effects for Justices and presidents are jointly significant, as are those for Chief Justices and Justice-by-president combinations. Crucially, though, the estimated effects for the main covariates of interest—in particular, $SG \times War$ —appear unaffected by the choice of which fixed effects to include in the model. To simplify the presentation of subsequent results, therefore, we restrict the analysis to models that include policy, challenger, Justice, and president fixed effects.

3.2 Statutory versus Constitutional Cases

As we have discussed, legal scholars continue to disagree about the logical foundations—and hence the empirical reach—of crisis jurisprudence. In Table 4, therefore, we distinguish the subsamples of cases that raise statutory and constitutional issues. The distinction turns out to be crucial for understanding patterns in judicial decision-making during periods of war and peace. On statutory cases, Justices were 14 to 15 percentage points more likely to side with the president during war—an estimate that is both large and highly statistically significant. Indeed, the magnitude of war’s impact on Justice votes on statutory cases is equivalent to that observed for Petitioner, the significance of which is well established in the literature; and is slightly larger than the estimate found in Epstein et al.’s study of civil liberties.

On constitutional cases, however, the point estimates hover around zero and do not approach standard thresholds of statistical significance.²² These findings weigh in favor of those scholars, like Issacharoff and Pildes, who emphasize the institutional and procedural foundations of crisis jurisprudence; and against those, like Corwin, who argue that war

21. This finding does not conflict with the existing literatures that document higher win rates for the Solicitor General. These prior previous findings compare the outcome of cases that go through the Solicitor General’s office with those that do not. In the sample here, the Solicitor General’s office is involved in every case. SG merely indicates whether the Solicitor General him or herself argued the case before the Supreme Court. We also estimated models that identify each unique Solicitor General. *F*-tests on the joint significance of these variables yield null results.

22. Though omitted, the background controls tend to perform similarly across the two subsamples.

Table 4. Probability of Siding with the President during War, Constitutional, and Statutory Cases

	(1)	(2)	(3)	(4)
Panel A: constitutional cases				
SG	-0.04 (0.05)	-0.03 (0.05)	-0.03 (0.05)	-0.03 (0.05)
War	0.06 (0.04)	0.10 (0.06)*	0.10 (0.07)	0.10 (0.07)
SG*War	-0.04 (0.09)	-0.06 (0.09)	-0.08 (0.09)	-0.07 (0.09)
... <i>p</i> -value	0.648	0.514	0.387	0.398
Number of justice votes	4388	4388	4388	4388
Number of cases	505	505	505	505
<i>R</i> ²	0.14	0.16	0.16	0.18
Panel B: statutory cases				
SG	-0.06 (0.04)	-0.05 (0.04)	-0.04 (0.04)	-0.04 (0.04)
War	-0.03 (0.03)	-0.04 (0.03)	0.01 (0.04)	0.00 (0.04)
SG*War	0.15 (0.07)**	0.15 (0.07)**	0.14 (0.07)**	0.14 (0.07)**
... <i>p</i> -value	0.021	0.028	0.033	0.036
<i>n</i> (Justice votes)	13,305	13,305	13,305	13,305
<i>n</i> (cases)	1567	1567	1567	1567
<i>R</i> ²	0.05	0.05	0.06	0.7
Fixed effects for				
Policy, challenger, justices	Yes	Yes	Yes	Yes
President	No	Yes	Yes	Yes
Chief Justices	No	No	Yes	Yes
Justice x President	No	No	No	Yes

Notes: OLS estimation. Robust standard errors, clustered by case reported in parentheses. *Significant at 10%, two-tailed test; **significant at 5%; ***significant at 1%. Dependent variable is Justice votes cast for the government (1=yes, 0=no). All models control for the following: *Petitioner*, *NYT Coverage*, *Amicus*, *CoPartisan* and *Divided Government*.

invites altogether new understandings of the Constitution itself. Given the stark differences in results for constitutional and statutory cases, we maintain this distinction in all of the empirical analyses that follow.

4. General Robustness Checks and Extensions

In addition to varying the particular set of control variables, we also have subjected our main models to a wide variety of robustness checks. In nearly every instance, the main findings hold: large and statistically significant effects that are observed for statutory cases, whereas null effects are observed for constitutional ones.

4.1 Alternative Estimation Strategies

Table 5 reports the estimated effects of war on the subset of cases argued by the SG from a variety of other statistical models. For the purposes of comparison, the first row reports the estimated effect of $SG \times War$ in our preferred linear probability model. In estimations via logit or probit, we find that Justices are 14 to 15 percentage points more likely to rule in favor of the government on statutory cases argued by the SG during times of war; and no more likely to rule in favor of the government on similarly argued constitutional cases (see Rows 2 and 3). When restricting the analysis to the subset of cases argued by the SG, we find that Justices are 23 percentage points more likely to rule in favor of the government on statutory cases, and no more likely to do so on constitutional cases (Row 4). When estimating models on case outcomes rather than Justice votes, we find that the Supreme Court as a whole is 16 percentage points more likely to side with the government on statutory cases argued by the SG during times of war, but 12 percentage points less likely to do so on similarly argued constitutional cases (see Row 5).²³ Finally, we have estimated models using a variety of matching techniques, which have the advantage of relaxing assumptions about the functional forms of the relationships between observed covariates and an outcome of interest. In models that exactly match on a subset of key observables, we find that Justices on average are 11 percentage points more likely to vote for the government on cases argued by the SG during war, and 4 percentage points more likely to do so on constitutional cases (Row 6).²⁴ Models that employ coarsened matching CEM techniques show a wartime effect of 12 percentage points on statutory cases, and 4 percentage points on constitutional cases

23. Since the unit of observation is the case outcome, these models do not include individual justice fixed effects, but do include chief justice fixed effects.

24. To preserve sufficient observations for analysis, these models match only on covariates that are observed prior to the decision. Namely, we match on indicator variables for whether the decision was rendered during war, the case was petitioned by the US Government, *War*, *Divided Government*, *NYT Coverage*, and the issue area of the case. Our choice of variables corresponds to those used by Epstein et al. (2005), who also employ matching estimators.

Table 5. Alternative Modeling Strategies

Modeling strategy	All cases	Constitutional	Statutory
FE models, LPM	0.07 (0.05)	-0.07 (0.09)	0.14 (0.07)**
FE models, probit	0.08 (0.05)	-0.09 (0.10)	0.15 (0.06)**
FE models, logit	0.08 (0.05)	-0.04 (0.09)	0.14 (0.06)**
FE models of subset of cases argued by SG	0.06 (0.08)	-0.05 (0.13)	0.23 (0.10)**
FE models with case as unit (LPM)	0.06 (0.06)	-0.12 (0.11)	0.16 (0.08)**
Exact matching	0.07 (0.03)**	0.04 (0.05)	0.11 (0.03)***
Coarsened exact matching	0.08 (0.03)**	0.04 (0.06)	0.12 (0.04)***

Notes: *Significant at 10%, two-tailed test; **significant at 5%; ***significant at 1%. Impact refers to the “treatment” effect of cases argued by the SG during war. Probabilities from probit and logit models recovered by keeping all continuous variables at their means and indicator variables at their modes. The treatment effect for exact matching (least squares) is the coefficient on the SG*War interaction term using the “MatchIt” program (see Ho et al. 2009) and a least squares model. Treatment effect for CEM procedure is the coefficient on the SG*War interaction term in the CEM-weighted regression. In the exact-matching models, the least squares regression controls for case-specific characteristics: case issue area, *Divided Government*, *Petitioner*, *NYT Coverage*, *CoPartisan*, *Amicus*, indicators for constitutional, federal, and statutory cases, and challenger fixed effects.

(Row 7).²⁵ Without exception, the estimates for statutory cases are statistically significant, whereas those for constitutional cases are not.

4.2 Alternative Definitions of War

Though conventional within the literature, our definition of war nonetheless warrants scrutiny. To do so, we adopt a randomization test approach advanced by Lax and Rader (2010) that is designed to test for the existence of jurisprudential regimes (e.g., war versus peace). Specifically, we randomize the “treatment” of war at the case level, such that 27% of the cases (which constitute the share of wartime cases in our sample) are designated as having occurred in war and the remaining 73% are designated as having occurred in peace. Subsequently, we interact this variable with another that identifies whether the case was argued by the SG, and we then estimate the specification in Table 3, Column 1. Finally, we record the coefficient, SE, and *t*-statistic on the interaction term, $SG \times War$. We repeat this procedure 10,000 times to generate a reference distribution of *t*-statistics for the interactive (treatment) effect. For statutory cases, our reference distribution has a mean of 0.00, standard deviation of 1.02, and minimum and maximal values of -3.81 and 4.96, respectively. Using its first and second moments, we can evaluate whether the *t*-statistic from our actual treatment effect remains statistically significant.

25. CEM models help re-weight the data using user-specified covariates to account for possible selection effects between control and treatment (i.e., those decided during war) cases (Iacus et al. 2008; Ho et al. 2009). We match on *Petitioner*, *CoPartisan*, *Divided Government*, *NYT Coverage*, and Justice specific fixed effects. Our treatment variable is whether the case was decided during war. This matching procedure allows us to generate weights, which we then employ in an empirical specification similar to our earlier Linear Probability Models (LPM).

In Table 4, Model 1, the t -statistic on the interaction term is 2.00. With respect to our reference distribution, this t -statistic remains statistically significant $(=(2.00 - 0.00)/1.02)$ with a p -value equal to 0.05. This test statistic implies that transitions from peace to war and from war to peace do not yield an artificially inflated relationship between war and judicial decisions (at least in cases involving the government). When performing this same procedure for constitutional cases, we again recover statistically insignificant results.

4.3 Disaggregating Wars

The behavior of Justices during World War II does not drive these results. When we replicate the main linear probability models, but disaggregate *War* into World War II (*WWII*) and subsequent wars (*Post-WWII*), the main effects for *WWII* and *post-WWII* hover around zero and are nowhere near statistical significance.²⁶ Both of the interaction effects for *WWII* and *post-WWII* and *SG*, meanwhile, indicate that Justices are approximately 15 percentage points more likely to vote for the government when the SG argues a statutory case during war. Moreover, because of the larger number of observations in the Korean, Vietnam, Gulf, and post-9.11 wars, the effect of $SG \times post-WWII$ is more precisely estimated than is the effect for $SG \times WWII$ —the former being statistically significant, the latter not.²⁷ For the subset of constitutional cases, the interaction effects for $SG \times post-WWII$ and $SG \times WWII$ are both negative and statistically insignificant.

4.4 Disaggregating Cases

Though the distinction between constitutional and statutory cases appears crucial, other kinds of distinctions appear less informative about the variable willingness of Justices to support the president during periods of war and peace. Indeed, the estimated effects appear roughly consistent in a wide variety of subsets of cases. When estimating separate models for criminal and noncriminal cases, for instance, we consistently find large and positive effects for statutory cases and null results for constitutional cases.²⁸ We also have estimated separate models on the subset of cases involving federal taxation, judicial power, and economic relations. Since these estimates rely on considerably smaller sample sizes, they are less precisely estimated and not always statistically significant. The point estimates, however, are consistently positive and large in magnitude for

26. These results are included in Supplementary Appendix Table A1.

27. In a pooled regression that includes interactions of each separate post-WWII war with the Solicitor General dummy variable, estimates appear positive but not identical in magnitude, suggesting a heterogeneous impact of cases argued by the Solicitor General across the four post-WWII wars. The average effect, however, remains positive and confirms our main findings.

28. See Supplementary Appendix Table A2.

statutory cases. For constitutional cases, meanwhile, the estimates are uniformly statistically insignificant.

5. Strategic Appeals and Cert Decisions

Two related problems continue to threaten the validity of our main estimates. The first relates to the omission of key control variables. None of the models presented thus far accounts for the strength of a challenge to the government—that is, the quality of arguments available to those parties who would challenge either the statutory authority or constitutionality of actions and/or policies advanced by the government. In principle, though, the Supreme Court may consider cases of very different quality during war and peace. If during war, presidents pursue policy initiatives that are more controversial, if would-be challengers to the government appeal only the most egregious actions taken by wartime presidents, or if Justices grant cert to only the strongest challenges to the government, then the estimated effects we have documented thus far understate the true extent of crisis jurisprudence. On the other hand, if during war a weaker set of challenges to the government makes its way to the Supreme Court, then the main estimates presented thus far overstate the true influence of crisis jurisprudence on the Supreme Court.

The second problem concerns the selection mechanisms that generate our sample of cases. When deciding whether to mount a challenge to the government, parties assuredly consider their chances of winning. And anticipating a loss, potential litigants may simply back off from a fight. Hence, the challenges against the government that actually materialize within the judiciary may differ from those that are strictly contemplated by a president's opponents. For our purposes, though, what matters most is whether war confounds the strategic calculations made by litigants who must decide whether to challenge the government and by Justices who must decide whether to hear them. If such strategies and calculations are unaffected by war, then the internal validity of our comparisons is uncompromised. But if litigants employ different criteria during war and peace when deciding whether to file and then appeal cases, or if Justices accept some cases during war that they would reject during peace (or vice versa), then our estimates of the impact of war on Justices' voting behavior may result from selection biases.

5.1 Gaging the Extent of Selection

These dual issues would appear intractable. There is no clear way to define, much less measure, the strength of a judicial challenge against the government. And the criteria that litigants use to determine whether to file a challenge, the government uses to decide whether to meet it, and Justices use to grant cert are all unobserved (and probably are unobservable). By tracking the volume of cases that appear before the Supreme Court during war and peace, however, we may be able to gage the

magnitude of the selection problem. If the Supreme Court's docket expands during war, we might worry that all the relevant actors in our analysis are relaxing their standards for challenging the government—and if so, then the positive effects of war that we have documented thus far may relate to unobservable aspects of the cases themselves rather than the adoption of crisis jurisprudence. On the other hand, if the volume of cases remains constant or even shrinks during war, then we have less reason for concern.

In Figure 1, we observe that during war, the Supreme Court hears fewer cases involving the government generally, but hears an equal number of cases argued by the SG. Among the universe of observations in our sample, the Supreme Court hears in an average month 2.2 cases during war and 2.6 during peace. Among those cases argued by the SG, the Supreme Court hears approximately 0.4 cases per month during war and 0.3 cases during peace. Moreover, there do not appear to be any differences in the number of cases argued by the SG. When estimating a variety of count models of the number of cases that come before the Supreme Court as a function of time-varying political characteristics and president and natural court fixed effects, the estimated effect of *War* is never statistically significant.²⁹ Additionally, in other models that predict whether the SG argues a case as a function of war, case-specific covariates, time-varying political characteristics, and president fixed effects, the estimated effect of *War* does not even approach statistical significance.³⁰ Neither the number of cases that come before the Supreme Court nor the probability that the SG himself assumes responsibility for making oral arguments on these cases appears systematically related to war.

Though reassuring, these facts say very little about the sample from which our cases are drawn. In particular, these facts do not rule out the possibility that the Supreme Court grants cert to very different kinds of cases during war and peace. To explore this possibility, we must look outside of the Supreme Court's docket. To do so, we rely upon the random sample of appellate court cases assembled in (Clark 2006). In all of these cases, the US Government was a named party. Hence, those cases in Clark's sample that were successfully appealed to the Supreme Court between 1933 and 2003 (when Clark's data end) reappear in our sample.

29. For example, when estimating a negative binomial count model that posits total number of cases decided by the Supreme Court in each year as a function of indicator variables for the incidence of war, divided government, and president and court fixed effects, the recovered coefficient on the war-year dummy is -0.08 with a p -value of 0.16.

30. Since the Solicitor General argues just over 10% of the cases, these models are estimated via logistic regressions. As regressors, the same set of time-varying covariates and presidential fixed effects are included. The case constitutes the unit of observations, and the dependent variable is coded as one, if the Solicitor General present oral arguments and zero otherwise. The point estimate for *War* is 0.13 and SE is 0.19.

We find little evidence that wars affect the chances that the Supreme Court will review an appellate court ruling. The probability that lower court rulings are appealed to the Supreme Court appears virtually constant in times of war and peace. Roughly 46% of the peacetime cases are appealed to the Supreme Court, as compared to 43% of the wartime cases.³¹ In 6% of the wartime appellate cases, the government files an appeal, as compared to 7% of the peacetime cases. And in 39% of the wartime appellate cases, the private party files an appeal, as compared to 37% of the peacetime cases. In every instance, the wartime and peacetime figures differ by no more than a couple of percentage points. None of the observed differences are statistically significant.

The frequency with which the Supreme Court grants cert also appears unaffected by war. Overall, the Supreme Court agrees to hear 8% of the appeals of peacetime cases and 11% of the appeals of wartime cases—a difference that is neither substantively nor statistically significant. As one would expect, the Court is much more likely to grant cert to an appeal made by the government than an appeal made by a private party. In these data, the probability that cert is granted increases six-fold when the petitioner is the government. But again, war is uncorrelated with the frequency with which the Court grants cert to cases appealed by either the government or its challengers. And to the extent that any differences are observed, they suggest that the Supreme Court is less selective during war than during peace.

5.2 Estimates that explicitly account for selection

Though neither the number of cases that come before the Supreme Court nor the probability that lower court rulings are successfully appealed to the Supreme Court varies much from times of war to times of peace, the types of cases that come before the Supreme Court nonetheless might. To address such sources of bias, scholars often estimate a system of equations that explicitly models the selection stage. To identify such a system, though, we need a valid instrument—in this instance, one that predicts the likelihood of a dispute coming before the Supreme Court but that is unrelated to Justices' actual voting behavior. Given the sheer number of plausible sources of selection—based on the strategic behavior of Justices, judges, litigants, and executive branch officials—we cannot even conceive of an instrument that satisfies the exclusion restriction. Instead, therefore, we explore three other econometric solutions: we consider subsamples of votes that are less likely to be contaminated by plausible sources of selection bias; we examine cases during temporal windows when the Court's docket is less susceptible to alteration; and we estimate Rosenbaum bounds.

31. Not knowing when appeals and cert decisions are made for each individual case in this sample, we compare wartime and peacetime decisions on the basis of when appellate court rulings were issued.

5.2.1 Subsamples. We first distinguish “majority party” from “minority party” Justices on the Supreme Court. We know that the party identifications of Supreme Court Justices, as inferred by the party identifications of the presidents who appointed them, constitute a powerful predictor of their voting behavior. If litigants decide whether or not to appeal a case on the basis of their likelihood of success on the Supreme Court, they should pay more attention to those Justices who constitute a partisan majority than those who reside among the minority. To win the case, after all, litigants must attract at least some votes of the majority party Justices. In principle, these litigants can win a case without persuading a single member of the minority party.

If our results reflect this type of selection bias, then we should expect to see powerful relationships between war and the votes cast by majority party Justices on cases argued by the SG, but weaker effects for minority party Justices. We do not. When estimating separate models for the two types of votes on statutory cases,³² the estimated effects of $SG \times War$ are statistically indistinguishable from one another. Whereas majority party Justices appear 12 percentage points more likely to vote for the government on wartime statutory cases argued by the SG, minority party Justices are 20 percentage points more likely to do so. On constitutional cases, meanwhile, we again recover negative and statistically insignificant results for both minority and majority party votes.

Of course, when deciding whether to appeal a case, litigants pay attention to more than just the Court’s partisan composition. Litigants also try to assess how each Justice’s personal background, legal philosophy, understanding of politics, and the like relate to the specific case at hand. It is of some importance, then, that the SG’s office knows more about such matters than nearly any other party, both because of the frequency with which it appears before the Supreme Court and because of the advice it regularly offers the Justices about whether to grant writs of certiorari and in filing amicus briefs. The SG’s office is deeply familiar with the Justices who sit on the Supreme Court, and the office may use this knowledge to select wartime and peacetime cases that stand the best chance of winning in the nation’s highest court.

We therefore estimated models that distinguish those cases where the SG, as the legal representative of the government, petitions the Supreme Court.³³ If our main findings derive from the careful selection of cases by the SG, then we should see large effects when it is the petitioner and weaker effects when it is not. Once again, though, we observe no differences across the two subsamples of cases. Though the effects are less precisely estimated, the point estimates themselves are indistinguishable from one another. Depending upon whether the government is the petitioner, Justices appear either 17 or 8 percentage points more likely to vote in its

32. See Supplementary Appendix Table A3.

33. See Supplementary Appendix Table A4.

favor on wartime statutory cases argued by the SG. For constitutional cases, null results are once again observed.³⁴

5.2.2 Temporal Windows. Rather than make explicit assumptions about the source of the selection bias, we can take advantage of the fact that the Supreme Court's docket is set months before cases are either heard or decided. Due to this lag, cases that are decided just before a war breaks out should be subject to the same kinds of selection criteria employed by litigants and Justices as those heard and decided just afterward. The cases that populate these short intervals may differ from those that inhabit other periods of time. But within these intervals, wartime and peacetime cases are less likely to be subject to unobserved sources of selection.

We face clear tradeoffs when deciding how long to define these intervals. Longer intervals permit the analysis of more observations, but they are at greater risk of selection biases. Shorter windows temper these selection biases, but they generate less precise estimates. We therefore estimate the effect of war on Justices' votes within multiple periods: 90, 120, and 150 days before and after each of our wars begins and ends. Given the paucity of cases argued by the SG during these short time spans, we cannot distinguish the subset of cases that directly implicate the president. Instead, these models assess whether Justices assume a more deferential posture toward the government as a whole when the nation transitions into and out of war.

Table 6 presents the results for all cases (Panel 1A and B) and just statutory cases (Panel 2A and B). (Given the small sample sizes, it is not possible to estimate separate models for just constitutional cases). Panels 1A and 2A compare the relevant sample of cases decided during each of the specified time intervals, and panels 1B and 2B limit the analysis to the subset of cases for which oral arguments always preceded the transition from either peace to war (in the odd columns) or war to peace (in the even columns). Depending upon one's preferred time interval and sample of cases, Justices appear somewhere between 26 and 36 percentage points more likely to rule in favor of the government immediately after a war begins than it was immediately before.

Transitions from war to peace also appear consequential, although the relevant impacts are less precisely estimated. When examining all cases, the point estimates are small in magnitude and never even come close to being statistically significant. For statutory cases, however, the results look comparable to those observed in the transitions from peace to war. Though, only statistically significant in one instance, the point estimates are consistently positive in sign and large in magnitude. Given the relatively large SEs and limited temporal scope, these findings obviously do not rule out the possibility of ratchet effects. But, they at least suggest that

34. Since the 95% confidence intervals from the estimates of the interaction terms in each regression overlap, these coefficient estimates appear indistinguishable from one another.

Table 6. Transitions from Peace to War, and from War to Peace

	Less than 150 days from war's...		Less than 120 days from war's...		Less than 90 days from war's...	
	Beginning	End	Beginning	End	Beginning	End
Panel 1A						
War	All cases	0.10 (0.08)	0.34 (0.11)***	0.06 (0.10)	0.26 (0.13)**	0.08 (0.11)
<i>n</i>	0.31 (0.10)***	898	652	670	511	520
<i>R</i> ²	0.17	0.16	0.21	0.13	0.24	0.22
Panel 1B						
War	All cases where oral arguments precede transition to war (beginning) and peace (end)	0.13 (0.15)	0.25 (0.10)**	0.13 (0.22)	0.20 (0.12)*	0.03 (0.22)
<i>N</i>	0.21 (0.11)*	555	372	452	309	361
<i>R</i> ²	0.35	0.18	0.39	0.18	0.44	0.22
Panel 2A						
War	All statutory cases	0.19 (0.11)*	0.35 (0.13)***	0.14 (0.12)	0.36 (0.15)**	0.24 (0.14)
<i>n</i>	0.27 (0.12)**	657	519	519	422	396
<i>R</i> ²	0.18	0.2	0.25	0.17	0.3	0.27
Panel 2B						
War	Statutory cases where oral arguments precede transition to war (beginning) and peace (end)	0.28 (0.19)	0.34 (0.12)***	0.33 (0.20)	0.35 (0.14)**	0.28 (0.20)
<i>n</i>	0.27 (0.14)*	403	305	345	259	272
<i>R</i> ²	0.36	0.21	0.38	0.21	0.44	0.26

Notes: OLS estimation. Robust SEs, clustered by case reported in parentheses. *Significant at 10%; two-tailed test; **significant at 5%; ***significant at 1%. Dependent variable is Justice votes cast for the government (1 = yes, 0 = no). All models include baseline controls (minus *Copartisan* and *Divided Government*) as well as fixed effects for Justices and presidents. Models in Panels 1B and 2B are restricted to cases in which oral arguments preceded the transition to war (**beginning**) or transition to peace (*end**).

the Court assumes a more critical posture in the immediate aftermath of war than it does in a war's waning hours.

5.2.3 Rosenbaum Bounds. Finally, we consider a bounding technique proposed by Rosenbaum (2002) that is specifically designed to determine how strongly an unmeasured variable must influence the selection process in order to weaken the causal inferences from regression. Given our binary outcome variable, we use a specific approach pioneered by Becker and Caliendo (2007),³⁵ and to evaluate the sensitivity of our estimates, we focus on the Mantel and Haenszel (1959) Q -statistic, which is commonly used in program evaluations within labor economics. The Q -statistic evaluates the conditional probability of mis-assignment to a treatment condition and thereby establishes a basis for assessing selection biases associated with unobservable factors.³⁶ For our purposes, treatment refers to a decision rendered during war, and the control condition is a peacetime decision.

We focus on two especially useful scenarios of the Q -statistic. The Q^+ test statistic measures the extent of over-estimation of the treatment effect due to unobserved selection bias. If one suspects that selection into the treatment status (i.e., war) is positively correlated with the outcome (vote for the government), then Q^+ is the appropriate statistic to gage the extent of potential upward bias in our coefficient estimate due to unobserved selection processes. Interpreting the Q^+ statistic is straightforward: a statistically significant value of the Q^+ statistic (i.e., $p < 0.10$) implies rejection of the null hypothesis of over-estimation. In contrast, the Q^- -test statistic measures the extent of under-estimation of the treatment effect with a similar statistical interpretation.³⁷ Given that the coefficient on the $SG \times War$ is positive, however, the value of the Q^+ statistic is especially informative about potential upward bias.

We estimate bounds for our most conservative estimate of war coming from matching models on statutory cases (0.11, as reported in Table 4). These models limit the sample to those cases argued by the SG and match votes on *War*, *Petitioner*, *Divided Government*, *NYT Coverage*, and fixed effects for justices and case issue areas. We evaluate the extent of over-estimation (Q^+) and under-estimation (Q^-) of the effect of War under various levels of uncertainty about unobserved selection bias (corresponding to higher levels of γ , which increase the odds of assigning decisions to wartime).

35. Recent applications of this approach (in particular in labor economics) can be found in (Aakvik 2001).

36. More specifically, this test compares the successful number of individuals (cases) in the treatment groups against the same expected number given the treatment effect is zero.

37. For more on the construction of the Q -statistic, see (Mantel and Haenszel 1959; Becker and Caliendo 2007).

For both statutory and constitutional cases, under the assumption of no hidden bias (i.e., $\gamma = 1$), we recover our main estimates.³⁸ Relaxing this assumption and considering higher values of γ , however, we do not find any evidence of selection bias. For instance, our treatment effect is insensitive to bias that would double the odds of assigning decisions to wartime (i.e., $\gamma = 2$). The extremely low p -value on the Q^+ -test statistic when $\gamma = 2$ implies, we can reject the null hypothesis of an over-estimate of our treatment effect. This conclusion also holds when we triple or quadruple the odds of assigning decisions to wartime. Moreover, we do not find compelling evidence of the under-estimation of our treatment effect either. Indeed, as values of γ increase, the p -values on both the Q^+ and Q^- -test statistics are persistently significant for statutory cases and null for constitutional cases.

6. Conclusion

Despite all that has been said and written about how judges should evaluate presidents during times of war and peace, almost no systematic evidence exists on what they actually do. An extraordinary literature on crisis jurisprudence notwithstanding, evidence of heightened judicial deference to the president during war remains fleeting. By analyzing over 2000 Supreme Court cases (yielding over 18,000 votes) that name the US Government, a cabinet member, or the president himself as a party between 1933 and 2007, devising new ways of identifying the subset of cases that matter most to the president, and addressing standard concerns about selection bias, this article offers some redress.

Our findings clarify a number of unresolved issues and ongoing debates about crisis jurisprudence. During the largest wars of the last 75 years, Justices were roughly 7 percentage points more likely to side with the government on the subset of cases that matter most to presidents. Evidence of a wartime effect, however, appears entirely consigned to statutory cases. Whereas Justices were roughly 15 percentage points more likely to side with the president on statutory cases during war, they were not more likely to do so on constitutional cases. Moreover, we find evidence not only that presidents fare better on Supreme Court cases in the immediate aftermath of a war's beginning; but also that, contrary to existing claims about ratchet effects, they also fare worse in the immediate aftermath of a war's termination.

These findings are remarkably robust. Indeed, the consistency of the estimated effects across the different fixed-effects specifications, the recovery of virtually identical effects from matching models, the roughly equal volume of cases argued by the SG during war and peace, the steady rates at which appellate court rulings are appealed to the Supreme Court and the Justices grant cert, and the estimation of models using a variety of

38. See Supplementary Appendix Table A5.

subsamples collectively suggest that these findings do not result from the well-known selection biases. Moreover, these findings do not appear to be an artifact of the USA's involvement in World War II, and they hold across numerous policy domains.

While clarifying important dimensions of the crisis jurisprudence literature, the findings on offer also reveal fruitful new lines of inquiry. Most importantly, they raise important questions about whether the Constitution can be said to constrain the Justices equally in war or peace. The Justices, after all, have discretion to choose the terms by which to adjudicate disputes that come before them. The null findings associated with constitutional cases, therefore, support either of the two contentions: first, that when a dispute raises matters of constitutional interpretation, the Justices do not give any credence to material considerations about war; or second, that when Justices are disposed to rule against a wartime president, they shroud their opinions in Article II interpretations. To distinguish between these two alternatives, future work should pay more attention not merely to the selection issues associated with cert, but also the Justices' subsequent, and quite possibly strategic, decisions about whether to rule on statutory or constitutional grounds.

Supplementary material

Supplementary material is available at *Journal of Law, Economics, & Organization* online.

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