Interbranch Conflict and Executive Action *

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Abstract

Unilateral power is central to the modern presidency and generates considerable scholarly and popular debate. We offer the most comprehensive assessment of presidential unilateralism to date and test the proposition that interbranch conflict is associated with the use of unilateral power. Using new data on more than 30,000 unilateral directives issued between 1953 and 2016 and machine learning techniques to distinguish those with substantive significance, we produce several new findings. First, the rate of significant unilateral action has increased markedly over the last 30 years. Second, we find no evidence that adjoining institutions constrain presidential unilateralism. Presidents issue greater numbers of significant unilateral directives as ideological disagreement increases with Congress, while their use is not responsive to the ideological composition of the judiciary. Our findings suggest that adjoining institutions may be less effective at constraining presidential unilateralism than commonly posited and have normative implications for evaluating the separation of powers.

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Just as in 1787, contemporary debates over the separation of powers in American government often concern the proper scope of presidential power. But though the Federalists sought to ensure that the presidency possessed sufficient tools to protect against Congress’s tendency to usurp executive authority (*Federalist* #48 and 51), modern-day observers from across the political spectrum frequently lament that contemporary presidents have become substantially more powerful relative to their predecessors and the Federalists’ intentions. These lamentations often center on the president’s unilateral powers. Heightened expectations of American presidents combined with constitutional ambiguities provide incentives and opportunities for modern presidents to deploy unilateral prerogatives to create policies and reinterpret existing ones without involving Congress. According to Moe and Howell (1999a, 132), unilateral action “virtually defines what is distinctively modern about the modern presidency.”¹ Accordingly, many observers note that unilateral powers have substantially empowered the presidency vis-à-vis the adjoining branches, amounting to a “new imperial presidency” (Rudalevige 2005), an “executive unbound” (Posner and Vermeule 2010), and a “takeover” of American government (Savage 2007).

The contemporary salience of unilateral power has motivated a burgeoning literature on its use. Theoretical accounts emphasize the strategic incentives for presidents to exercise unilateral powers, including electoral incentives, legacy considerations, and personal policy motivations, and identify relevant constraints on their use, including bureaucratic compliance, legislative retaliation, and judicial review (e.g., Chiou and Rothenberg 2014, 2017; Howell 2003; Judd 2017; Lowande 2018; Moe and Howell 1999a,b). Quantitative empirical scholarship studies the frequency with which executives exercise unilateral powers and identifies the correlates and consequences of its use by American presidents (e.g., Bailey and Rottinghaus 2013; Belco and Rottinghaus 2017; Bolton and Thrower 2016; Christenson and Kriner Forthcoming; Deering and Maltzman 1999; Krause and Cohen 1997, 2000; Lowande 2014; Mayer 1999, 2001; Mayer and Price 2002; Rottinghaus and Warber 2015; Warber 2006; Williams Forthcoming) and governors (e.g., Barber, ¹For a historical perspective on unilateral power, see Dodds (2013).
Bolton, and Thrower Forthcoming; Cockerham and Crew 2017) and in presidential systems outside the U.S. (e.g., Neto 2006; Pereira, Power, and Rennó 2005). Archival and qualitative studies have further detailed the processes by which unilateral directives are developed and implemented (e.g., Dodds 2013; Rudalevige 2012, 2015).

In this paper, we make new theoretical and empirical contributions to scholarship on unilateral action and American political institutions. We test the proposition that presidents make greater use of unilateral powers during periods of conflict with the legislative and judicial branches. Drawing from Lowi’s (1985) account of the plebescitary presidency, we argue that unilateral powers play a central role in allowing modern presidents to meet the vast expectations placed upon them. The prominence of presidents in American governance, we argue, renders institutional constraints less binding than conventional theories posit. In evaluating our argument empirically, we offer the most comprehensive assessment of presidential unilateralism to date. Though scholars recognize that presidents exercise unilateral powers through a variety of tools, existing research focuses overwhelmingly on numbered executive orders to the exclusion of other directives.\(^2\) The omission of directives beyond executive orders raises the possibility that prior research may have reached inappropriate empirical conclusions regarding the frequency of unilateral action and predictors of its use.

We introduce new data on more than 30,000 executive orders, proclamations, memoranda, and other directives issued between 1953 and 2016 and use machine learning techniques to identify those with substantive significance. Descriptively, our data show that presidents have issued policy-significant unilateral directives at substantially greater rates over the last 30 years. This increase, we find, is driven largely by presidents’ increasing reliance on directives other than executive orders, such as memoranda. Incorporating the full range of substantively significant

\(^2\)Several studies have focused on other tools, such as proclamations (Bailey and Rottinghaus 2013; Belco and Rottinghaus 2009) and memoranda (Lowande 2014), but this research generally does not study the joint distribution of unilateral directives.
unilateral directives, we find no evidence that institutional conflict between the president and Congress is associated with lower rates of unilateral action. Instead, and in contrast with most existing literature, we find that presidents issue significantly greater numbers of unilateral actions as ideological divergence increases between them and Congress. We also find no evidence that the possibility of judicial review deters rates of unilateral action as we find consistently null results when testing whether presidents’ use of unilateral power is responsive to the ideological composition of the Supreme Court. These results are robust across a wide range of model specifications, measurement strategies, subsets of directives, and document coding strategies. Altogether, our findings provide a fuller characterization of the unilateral presidency and suggest that institutional constraints may be less effective at taming presidents’ unilateral ambitions than commonly posited. Our results therefore provide support for normative concerns about the potential for unilateral power to distort the separation-of-powers system.

**Unilateral Action and the Presidency**

Canonical perspectives on presidential power focus on the president’s ability to successfully bargain with other political actors (Neustadt 1990). According to this view, presidents are powerful to the extent they can persuade others that they share the president’s interests and act to advance them. This characterization of presidential power generated decades of research investigating the correlates of presidents’ success in achieving their preferred legislative outcomes (e.g., Bond and Fleisher 1990; Canes-Wrone 2006; Edwards 1976).

More recently, scholars have dedicated increased attention to presidents’ capacity to realize policy achievements through the exercise of unilateral powers (Belco and Rottinghaus 2017; Bolton and Thrower 2016; Chiou and Rothenberg 2014, 2017; Howell 2003; Moe and Howell 1999a,b; Warber 2006). Contemporary presidents have used unilateral powers “to implement many of their most important policy initiatives, basing them on any combination of constitu-
tional and statutory power that is thought to be available" (Shane and Bruff 1996, 131). By taking action at their own initiative, unilateral powers allow presidents to secure policy outcomes which could have eluded them otherwise.

From the president’s perspective, unilateral action offers clear benefits. They can act alone to effect policy change that does not require them to expend political capital on persuading reluctant legislators, allowing presidents to act swiftly when the times call for it and freeing them to develop legislative strategies that account for the items presidents can achieve on their own. Unilateral action also affords presidents opportunities to claim sole credit for policies enacted with it, as they do not have to share credit for a bill they did not write with the legislative coalition that passed it. Perhaps most importantly, unilateral action may enable presidents to achieve policy outcomes more to their liking than a similar legislative initiative—and to accomplish policies that Congress would have refused to consider or opposed altogether. This latter point—which scholars have labeled the “evasion hypothesis” (Martin 1999), the “strategic model” (Deering and Maltzman 1999), and the “strong form” of unilateral action (Mayer 2009)—is commonly cited by normative concerns that unilateral action imperils the separation of powers by enabling the president to act outside constitutionally-prescribed processes and despite the objections of adjoining branches. Accordingly, some legal scholars argue that “the ambitions of the unilateral presidency cannot be squared with … the presidency envisioned by our Constitution” (Shane 2009, 5).

Many theoretical accounts of unilateral action, however, emphasize the institutional constraints that bear on a president’s decision. In contrast with the evasion hypothesis, these models posit that presidents consider unilateral action in the shadow of potential responses from Congress and the courts. For instance, when the president and Congress disagree ideologically, members of Congress may be especially inclined to reverse the president’s unilateral action (Bolton and Thrower 2016; Howell 2003; Moe and Howell 1999a, b), potentially resulting in a worse outcome.

Howell (2003, 70) argues that the evasion hypothesis “ignores the constraining effect of Congress.”
policy outcome for the president than if no unilateral action had been taken. As a result, these models suggest that presidents strategically scale back their unilateral ambitions as the likelihood of congressional reversal increases.⁴ Similar logic applies to the judiciary. Presidents anticipate the potential for the courts to strike down a unilateral action and will modify the scope of their intended action or else choose not to act at all (Fox and Stephenson 2011; Howell 2003; Thrower 2017).⁵ On the whole, this theoretical perspective accounts for how adjoining political institutions through the separation of powers constrain presidential use of unilateral prerogatives.⁶

These competing accounts have motivated a growing literature on the predictors of unilateral

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⁴In principle, Congress can also constrain unilateral power through statutory means by limiting how presidents exercise enumerated powers or formulating policy with limits on executive discretion. However, scholars generally dismiss the strength of this constraint because legislators may sometimes prefer to delegate authority to the executive branch and at other times are ill-equipped to wield precise control over the executive branch’s policy implementation (Moe and Howell 1999a, 141). Presidents can also use unilateral action to frustrate congressional activity without creating dramatic new policies by, instance, preempting legislation or modifying bureaucratic structures (Mayer 1999).

⁵Presidents may also consider the potential judicial response to other actions such as signing statements (Thrower 2019).

⁶Congress and the courts may not be the other relevant institutional constraints on unilateral action; indeed, the potential for bureaucratic noncompliance may also shape presidential decision making (Kennedy 2015; Lowande 2018; Rudalevige 2012, 2015). Moreover, institutions may not be the only potential constraint on unilateralism. Several studies link unilateral action to public opinion. For instance, Reeves and Rogowski (2015, 2016) document the public’s opposition to unilateral action, Rottinghaus and Warber (2015) studies how unilateral action allows presidents to cultivate support from particular constituencies, and Judd (2017) models how unilateral action reflects a president’s electoral motivations.
action. The empirical record generally provides greater support for the latter perspective, particularly in the presidential-congressional context. On the logic that divided government characterizes periods of disagreement between presidents and Congress, existing scholarship overwhelming reports either null or negative findings regarding divided government and the use of unilateral powers in the modern era (Bailey and Rottinghaus 2013; Belco and Rottinghaus 2017; Bolton and Thrower 2016; Deering and Maltzman 1999; Fine and Warber 2012; Howell 2005; Krause and Cohen 1997, 2000; Lowande 2014; Mayer 1999, 2001; Mayer and Price 2002; Rottinghaus and Warber 2015; Warber 2006).7 This near-unanimity weighs against the evasion hypothesis by indicating that presidents do not appear to use unilateral action to circumvent a politically-hostile Congress, and suggests that presidents may be constrained rather than empowered by legislative opposition. While some scholarship considers how judicial review affects behavior within the executive branch (Canes-Wrone 2003; Fox and Stephenson 2011; Wood and Waterman 1993), few empirical studies directly examine how patterns of unilateral action are responsive to the ideological composition of the court vis-à-vis the president.8 In the only study that does, Thrower (2017) presents evidence consistent with the evasion hypothesis by showing that presidents issue more

7Two important exceptions are Deering and Maltzman (1999), who show that presidents issue more executive orders when facing ideologically-opposed congresses, and Williams (Forthcoming), who finds that presidents from 1945 to 2016 issued more proclamations under divided government.

8Howell (2003, chapter 6) shows that the courts systematically uphold unilateral directives and suggests that this deference indicates the courts may not meaningfully constrain presidential behavior. However, analogous to Cameron’s (2000) analysis of vetoes, the absence of judicial reversals of unilateral actions does not mean that the courts do not constrain presidential action. If presidents anticipate circumstances in which court reversals are likely, presidents would not issue unilateral actions that could not sustain court challenges and thus we would not observe judicial reversals of them.
executive orders as ideological distance with the courts increases.

**Congress, Courts, and the Incentives for Unilateral Action**

We revisit the nature of institutional constraints on presidential unilateralism. On both theoretical and empirical grounds, we argue that existing accounts of unilateral power incompletely characterize the context in which contemporary presidents govern. As a starting point, consider that contemporary presidents confront extraordinary expectations from the public, organized interests, political observers, and historians (Edwards 1983; Howell 2013). If, as Neustadt (1990, xix, emphasis in original) argued about the modern presidency, “*weak* remains the word with which to start,” presidents have strong incentives to attempt to meet these expectations through the prerogatives available to them. Moreover, the modern context is decidedly president-centered such that Americans “identify directly with the presidency” (Lowi 1985, 20). Given the expectations for presidential action and the exaltation of the presidency in the American political system, it is unclear theoretically why presidents would be expected merely to yield to adjoining institutions in the context of political disagreement.\(^9\) Therefore, we expect that the electoral and legacy costs of failing to act (or being perceived as such) outweigh the short-term policy costs that may be imposed by adjoining institutions.

Instead, with respect to presidential-congressional relations, we argue that presidents have the strongest incentives to advance their objectives via unilateral action when their preferences diverge from Congress’s. Under these conditions, presidents are unlikely to achieve their key policy objectives legislatively.\(^10\) Moreover, due to supermajoritarian requirements in the legislative

\(^9\)Howell (2013, 125) makes a similar point when arguing that “opting not to act—indeed, merely being perceived as not acting—comes at a great political cost.”

\(^10\)For the purposes of this discussion we consider the preferences of the president and a median legislator, though our argument can be applied to any member believed to be pivotal in the
process, growing ideological divergence between presidents and Congress likely reduces the volume of legislation Congress produces (Krehbiel 1998). Presidents therefore face a choice between presenting to voters a lackluster record of accomplishment, or else seeking alternative means of achieving their goals. Because the latter option offers greater benefits, we expect that presidents make greater use of unilateral powers as ideological disagreement increases between presidents and Congress.

In advancing this expectation, we do not argue that presidents ignore the potential for congressional retaliation when contemplating unilateral action. Instead, we argue that this scholarship overstates its importance. For instance, collective action problems hinder Congress’s ability to reverse a president’s unilateral action (Howell 2003; Moe and Wilson 1994)\(^\text{11}\) and legislators lack individual incentives to assert Congress’s collective institutional power against a president they believe has overstepped his authority (Moe and Howell 1999\(^a\)). These challenges are compounded by growing congressional polarization along party lines over the last four decades (McCarty, Poole, and Rosenthal 2006) and increasingly narrow partisan majorities in Congress (Lee 2016). While congressional capacity may have grown during the postwar era (Bolton and Thrower 2016), so too has the president’s capacity. The scope and role of the Executive Office of the President has expanded dramatically over the last half-century; for instance, between 1962 and 2017, its budget increased from $97 to $411 million (in 2017 dollars) and outpaced the growth in outlays for the legislative branch over the same time period.\(^\text{12}\) These data all suggest that presidents have significant capacities of their own to identify opportunities for using unilateral legislative process.

\(^{11}\)According to Moe and Howell (1999\(^a\), 146), “the veto-filled process of generating legislation remains incredibly difficult and costly” such that “Congress is unlikely to reverse” presidential actions that shift the status quo unilaterally.

\(^{12}\)These data were obtained from https://www.whitehouse.gov/wp-content/uploads/2018/02/hist04z1-fy2019.xlsx.
powers when faced with a hostile Congress and that this capacity may significantly weaken or effectively eliminate the potential for legislative reversal to dissuade a president from exercising unilateral powers.

In the context of judicial review, presidents may have little incentive to consider whether a given directive can withstand judicial scrutiny. To be sure, insofar as decisions issued by judges and justices are shaped by their policy attitudes (Segal and Spaeth 2002) and presidents issue unilateral actions that reflect their sincerely-held preferences, a president’s unilateral action may have a greater chance of surviving challenges in court when the president and the courts share similar political views. But such action by the courts may not come at much cost to contemporary presidents. Instead, presidents may simply receive credit for acting irrespective of the judicial disposition. Extending the insights offered by Whittington (2007), in some circumstances, this could give rise to “posturing” (Fox and Stephenson 2011) or “grandstanding” (Judd 2017) in which electorally-motivated presidents take decisive action expecting that the courts may strike it down. This account, combined with patterns of judicial deference to existing unilateral actions, suggests that presidents may have little reason to consider the ideological composition of the judiciary when contemplating unilateral action.

Empirically, existing research provides a consistently incomplete characterization of presidents’ use of unilateral powers. Though presidents can exercise unilateral powers through a variety of means, existing scholarship focuses overwhelmingly on a single directive: executive orders (e.g., Belco and Rottinghaus 2017; Bolton and Thrower 2016; Chiou and Rothenberg 2014, 2017; Fine and Warber 2012; Howell 2003, 2005; Krause and Cohen 1997, 2000; Mayer 1999, 2001; Mayer and Price 2002; Thrower 2017; Warber 2006; Warber, Ouyang, and Waterman 2018). Some scholars have studied presidents’ use of other unilateral tools such as proclamations or compared patterns of activity across several classes of directives (Bailey and Rottinghaus 2013; Belco and Rottinghaus 2009; Lowande 2014; Rottinghaus and Lim 2009), these studies generally overlook potential interdependencies between them and do not investigate how presidents choose
among them. The potential for strategic substitution between executive orders and other forms of unilateral action (see, e.g., Lowande 2014) suggests that executive orders alone may provide an incomplete and potentially biased assessment of presidents’ use of unilateral powers.\textsuperscript{13} Finally, because the president’s unilateral toolkit has expanded over time and the interpretation of particular forms of unilateral action has evolved along with it, the concerns outlined above may be particularly acute for studies of unilateral action that span several decades.

Data and Measurement

We address these research questions and limitations of existing scholarship by assembling the most comprehensive dataset to date on unilateral directives issued by presidents. These data are obtained from the \textit{CIS Index to Presidential Executive Orders & Proclamations 1987} (CIS) and have been extended through 2017 by \textit{ProQuest Executive Orders and Presidential Proclamations 1789-2017}. This database contains “a complete collection of numbered and unnumbered Executive Orders and Presidential Proclamations” including approximately 100,000 presidential actions issued since 1789. We draw upon the full text of each of these documents, which we convert into machine-readable format using an optical character recognition system (Smith 2007).

These data significantly expand access to the nature and content of presidential action. First, this database includes a number of unilateral directives that have lacked formal designation as such and thus are routinely excluded from existing studies. As the CIS \textit{Index} notes (1987, ix), “various categories of Executive Orders and Proclamations were routinely excluded from the Numbered Series despite their similarity to materials included in the series.”\textsuperscript{14} Therefore, the in-

\textsuperscript{13}Executive orders and other forms of unilateral action are largely (though not wholly) interchangeable. As Mayer and Price (2002, 34) writes, “The lack of any agreed-upon definition means that, in essence, an executive order is whatever a president chooses to call by that name.”

\textsuperscript{14}The CIS \textit{Index} (1987, ix) notes that these omitted directives include the following: “Execu-
clusion of these directives—which function as “a form of executive lawmaking used in instances in which the Constitution or Congress directly or indirectly permits the President to take action” (CIS Index to Presidential Executive Orders and Proclamations 1987, vii) though they may not have been formally labeled as a particular kind of unilateral action—provides a dramatically fuller characterization of the extent of direct presidential action.¹⁵ Second, these documents suggest that the data generating process may have been less distinct across various forms of unilateral directives than implicitly assumed by previous scholarship. For instance, as the CIS Index (1987, viii) reports, “Although technically, Executive Orders differ from Presidential Proclamations, this distinction has been imperfectly observed over the years. There are items in each category that seem to belong in the other.” The potential for substitution across unilateral tools suggests the importance of characterizing presidential unilateral action using executive orders and proclamations in addition to documents such as “memo orders” (Woolley and Peters 2017) and other directives that

tive Orders issued by Secretaries of Federal departments with the approval of the President that, while sometimes compiled and numbered in a separate series, were not included in the Executive Order numbering system; Orders issued at the behest of the President to effect the reservation, sale, or other disposition of Federal lands; Military Orders, usually involving personnel and other administrative matters in the military departments; Presidential directives, decisions, determinations and other types of Presidential orders other than those formally labeled Executive Orders or Presidential Proclamations.” These unnumbered EOs and EO-like documents were found in: presidential papers and collections, files of correspondence and orders maintained by Federal departments and agencies, and “other Presidential documents” contained in the Federal Register (1987, ix-x).

¹⁵The Federal Register notes that “The President of the United States issues other types of documents, including but not limited to; memoranda, notices, determinations, letters, messages, and orders. After they are signed, the White House sends it to the Office of the Federal Register.” Available at https://www.federalregister.gov/presidential-documents (accessed June 4, 2019).
have not been systematically cataloged as such or classified according to their policy significance.

The CIS *Index* identifies classes of documents according to 41 Source Record Groups (SRGs). These SRGs describe the original materials from which presidential directives were sourced and range from Numbered Executive Orders and Numbered Proclamations to various agency and department records and presidential papers. According to the CIS *Index*, each of the 41 sources distinguished by the SRGs was searched for presidential directives and orders, and cross-checked against documents contained found in other SRGs to eliminate duplication. We omit from our study documents from several SRGs which do not appear to be of policy consequence and/or address matters other than “executive lawmaking” (such as pardons and Statements of Administration Policy). We recategorize documents from the remaining SRGs into three groups of directives: *Executive Orders, Proclamations*, and *Memoranda*. A complete accounting of the 41 SRGs, and justifications for their categorization, is shown in Appendix A. Altogether, our dataset consists of 30,162 unilateral directives issued between 1953 and 2016.

Figure 1 displays frequencies of our raw data and shows the number of documents in each category from 1953 through 2016. While executive orders may be the most publicly salient form of unilateral actions, presidents quite often make more frequent use of other unilateral tools. Consistent with Lowande (2014), by around 1970 presidents occasionally made more frequent use of memoranda relative to executive orders, and by the mid-1980s this became a common occurrence. The number of proclamations issued annually, meanwhile, has gradually yet steadily increased in recent decades, while the number of executive orders has declined. Descriptively, the data indicate that executive orders alone do not provide a full picture of presidents’ use of unilateral action. Instead, understanding unilateral action in the modern presidency requires considering other directives, including memoranda and proclamations, in conjunction with executive orders.
Figure 1: The annual number of documents by category from 1953 to 2016. The documents fall into three categories: Executive Orders, Proclamations, and Memoranda.
Measuring the Significance of Unilateral Action

Though the raw data shown in Figure 1 provide a dramatically fuller characterization of unilateral actions than measures found in existing scholarship, all unilateral actions are not created equal. Instead, substantial numbers of unilateral actions are purely ceremonial or concern mundane administrative affairs. Since theories of presidential behavior are concerned with explaining when presidents exercise significant policy influence, we distinguish unilateral actions that are of some policy consequence from those that are administrative or ceremonial in nature (Cameron 2009; Howell 2003). One approach distinguishes significance based on the content of executive orders; for example, Bolton and Thrower (2016) distinguish “nonceremonial” orders while others classify executive orders based on whether they address major policy, routine administrative affairs, or are ceremonial in nature (Fine and Warber 2012; Warber 2006). An alternative approach identifies executive orders based on media salience by distinguishing those receiving significant media attention from newspapers such as the New York Times (Howell 2003; Williams Forthcoming). Chiou and Rothenberg (2014, 2017) expand upon this approach by including a wider range of media sources as well as exogenous measures of the political environment.

We employ a text-based approach to estimate the policy significance of the unilateral actions in our data. Our approach improves upon existing approaches to measuring the significance of presidential actions in several key ways. First, in contrast with news media-based approaches, our estimates are based substantially on the text of the actions in question rather than indices of media coverage. While media mentions may be correlated with the policy significance of a particular action, the correlation likely is fairly noisy and variable over time. Moreover, to the extent that actions which are merely ceremonial but of popular interest receive substantial me-

16This approach is similar to that in existing research (e.g., Hillard, Purpura, and Wilkerson 2008), which has produced document classifications similar to human judgment with high accuracy and at low cost for a much broader range of documents.
dia attention, and actions which are substantively important but highly technical do not receive media attention, this approach may not meaningfully distinguish unilateral action significance along a continuous scale. Instead, because we use coarsening to identify significant actions above a given threshold of significance (and without making inferences about cardinality), we will fail to identify a significant action as significant only when a less impactful unilateral action receives so much attention that it moves from below our threshold to above it, or when the media fail to discuss a significant policy document. This reduces measurement error considerably vis-à-vis Chiou and Rothenberg (2014, 2017) and Howell (2003). Our approach further relaxes the assumption that all equally significant unilateral actions are likely to receive equivalent media attention, as levels of popular interest may vary with the policy in question and the nature of the media environment may shape uptake of particular actions. Second, our approach is easily scalable and can evaluate the text of tens of thousands of presidential documents automatically and cheaply. And third, by using machine learning rather than relying solely on human coders, we avoid many potential biases associated with subjective evaluations.

We use the text of presidential unilateral directives and a supervised learning approach to text analysis (e.g., Colleoni, Rozza, and Arvidsson 2014; Gentzkow and Shapiro 2010; Hopkins and King 2010) to estimate the substantive significance of each action.\textsuperscript{17} We follow a standard methodological procedure in the literature to perform supervised learning consisting of six steps: (1) Collect a training corpus which differs along the dimension of interest, (2) label each document in the corpus corresponding to its location along the dimension of interest, (3) convert the corpus to a data set, (4) train a supervised model on the data set and training labels, (5) use the model to predict the labels for out-of-sample documents, which are the ultimate quantity of interest, and (6) examine the results for face validity, internal validity, and external validity. We briefly

\textsuperscript{17}Supervised learning requires labeled example documents from which to learn a relationship between words and a label. Tools like regularized regression, support vector machines, random forests, and neural networks then use those examples to estimate a complex functional form.
describe these six steps below and provide more extensive documentation in Appendix B.

**Training Data.** We begin with the CIS Index to Presidential Executive Orders and Proclama-
tions, as described above, which includes the text of each directive.

**Document Labeling.** We create training labels in three ways. For numbered executive orders
from 1953 to 2002, we match significance estimates from Chiou and Rothenberg (2014) to the text
of the corresponding executive order.\(^1\) The estimates from Chiou and Rothenberg (2014) are
continuous from roughly -1 to 3; we coarsen them to create a binary indicator of significance at
the threshold of 0.5.\(^2\) Secondly, we use trained undergraduate research assistants to label each
numbered proclamation in a dichotomous fashion as either “substantive” or “ceremonial.” Finally,
for a subset of non-numbered proclamations, executive orders, and other documents, we identify
in-text references to numbered executive orders and assign a new document a significance score
equal to the average significance of all numbered executive orders it references. These 9,531
documents matched to their significance estimates constitutes the training set; the remaining
20,631 documents without significance estimates constitute our test set.

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\(^{1}\)Chiou and Rothenberg (2014) do not estimate the significance of executive orders issued after
2002.

\(^{2}\)Here and in one other step in our measurement strategy we coarsen a continuous score to
produce a dichotomous measure of significance. While we perform a series of robustness checks
on these choices (see Appendix B), we acknowledge that readers may disagree with our choices.
We suspect that variations in measurement choices would have minimal effects on our substan-
tive findings. In the interest of transparency, however, upon publication we will publish our full
replication file for generating estimates of document significance on the journal Dataverse, indi-
cating where in the code we make these choices, so that curious readers can explore alternative
threshold choices.
**Text to Data.** We convert the training set and test set into a data object that can be analyzed quantitatively by using the documents in our corpora to create term-document matrices. Each document is a row in a matrix, and each unique lexical feature is represented in a column. Entries in this matrix indicate how many times lexical object $j$ occurs in document $i$. In this case, columns of the data set include unigrams, which are single words, and bigrams, which are ordered pairs of words. Consider Executive Order 13123, the Preamble of which begins: “The Federal Government, as the Nation’s largest energy consumer, shall significantly improve its energy management in order to save taxpayer dollars and reduce emissions that contribute to air pollution and global climate change.” The unigrams in this document include, among others, “energy”, “consumer”, “management”, “save”, “dollars”, “emissions”, “pollution”, and “climate”; bigrams include “energy consumer”, “energy management”, “taxpayer dollars”, “air pollution”, and “climate change.” As additional preprocessing, we remove all terms which do not occur in at least 1,000 documents. In total, our training term-document matrix has 10,746 unique documents and 8,863 terms.

**Modeling.** Next, we apply standard machine learning tools to model the relationship between lexical features and document significance. We are methodologically and theoretically agnostic as to which model will work best, so we test a variety and measure their success using $k$-fold cross-validation (Mozer et al. Forthcoming). This procedure involves partitioning a training set into a number of non-overlapping random subsamples, training a model on all but one of them, predicting the outcome measure for the omitted subsample, and comparing the model’s predictions to the true outcome labels. Stronger correlations with the known estimates generate greater confidence in its predictions for the training set.

We find that the random forest model performs best. It successfully identifies whether a document is below or above our significance threshold 98.76% of the time, with little observable heterogeneity across significance levels and with roughly equal false-positive and false-negative rates (for more details, see Appendix B). This provides confidence that although we are estimat-
ing documents’ significance with error, that error is unlikely to produce bias in our substantive results.

**Estimation.** Finally, we use the random forest model to estimate the policy significance for the remaining 20,631 documents in our data. The random forest model produces for each document in the test set a *probability* that it is significant.\(^{20}\) We distinguish significant unilateral actions as those whose scores are greater than 0.355, the value that equals the false-positive and false-negative rates. This threshold distinguishes 5,362, or roughly 18 percent, or the directives in our data as policy significant.\(^{21}\) Because our scores are measured with error, we do not interpret them in a cardinal way. In our analyses below, moreover, we explore the robustness of our findings across varying thresholds used to distinguish significant unilateral actions.

**Face Validity.** We examine the distribution of significance probabilities in our test set, as well as individual documents assigned very high or low probabilities, as an exercise in face validity. Table 1 presents a selection of unilateral actions from our test set, ordered by estimated significance, along with a description. The documents and their accompanying significance estimates provide evidence that our measurement approach effectively distinguishes directives on the basis

\(^{20}\)Figure A.1 displays the distribution of significance scores across document types.

\(^{21}\)This appears to represent an appropriate level of selectivity given related scholarship. To contextualize this figure, Howell (2005) characterizes 8% of executive orders (290 of 3,749) issued between 1945 and 2001 as significant, while 97% of executive orders issued between 1945 and 2013 (4,045 of 4,153) are included in Bolton and Thrower’s (2016) study of “non-ceremonial” orders. Mayer and Price (2002) use six criteria to identify significant executive orders from a sample of those issued between 1936 and 1999 and classify 14% of them (149 of 1,028) as such. Finally, based on Chiou and Rothenberg’s (2014) continuous estimates of order significance, 14%, 26% and 44% are classified as significant based on thresholds of 1, 0.5, and 0, respectively.
of their policy significance. Purely ceremonial directives, such as those related to National Mentoring Month and the Minnesota World’s Fair 2023, have significance estimates that are close to zero. More substantive directives, such as those related to the conflict in Libya, the federal government’s response to cyber attacks, and interagency policy on carbon emissions, have significance estimates closer to one. Notably, the estimates in Table 1 also showcase the consistency of our estimates. For instance, President Obama issued two executive orders related to the order of succession within the Environmental Protection Agency; though these orders were issued four years apart, they have virtually identical significance estimates (both 0.22). Moreover, both of these estimates are extremely similar to the estimated significance of an executive order that established the order of succession within the Department of Commerce. While we do not interpret our significant estimates as cardinal in nature—that is, we do not infer that EO 13726 was twice as important as EO 13435—these estimates appear to do a reasonable job of distinguishing presidential actions according to their policy significance and support our use of a binary indicator to identify them.

Model Accuracy

We also evaluated the accuracy of the model we used to generate our estimates. Our approach has three key advantages over human coders: accuracy, consistency, and scalability. A statistical model will produce the same (or very similar) codings for a single document each time it is queried, while humans may not. Machine coding can also produce labels for an enormous number of documents simultaneously, while human coders may take months or years to do the same. Despite these advantages, machine coding may be less desirable if it is substantially less accurate than human coders. We evaluated the accuracy of our model through two main procedures. In the interest of space, we present the details in Appendix B. In short, we find that our model has predictive success in classifying significant documents that compares well with many of the best results in the field of machine learning. Moreover, we compare our model’s results
<table>
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<tr>
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<td>0.01</td>
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<tr>
<td>May 21, 2012</td>
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<td>EO 13614: Providing an Order of Succession Within the Environmental Protection Agency</td>
</tr>
<tr>
<td>Aug. 12, 2016</td>
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<td>EO 13737: Providing an Order of Succession Within the Environmental Protection Agency</td>
</tr>
<tr>
<td>June 20, 2007</td>
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<td>EO 13435: Expanding Approved Stem Cell Lines in Ethically Responsible Ways</td>
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<td>Public Land Order No. 7857; Extension of Public Land Order No. 7254, Montana</td>
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<tr>
<td>May 14, 2007</td>
<td>0.54</td>
<td>EO 13432: Cooperation Among Agencies in Protecting the Environment With Respect to Greenhouse Gas Emissions From Motor Vehicles, Nonroad Vehicles, and Nonroad Engines</td>
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<tr>
<td>July 26, 2016</td>
<td>0.62</td>
<td>Presidential Policy Directive – United States Cyber Incident Coordination</td>
</tr>
<tr>
<td>Apr. 19, 2016</td>
<td>0.70</td>
<td>EO 13726: Blocking Property and Suspending Entry Into the United States of Persons Contributing to the Situation in Libya</td>
</tr>
</tbody>
</table>

Table 1: Significance estimates for selected unilateral actions from the test set. Directives judged to be policy significant are shown in **bold.** Note: *PR* = proclamation; *EO* = executive order.
with what might be obtained from a more conventional approach to document coding and show that the model’s success rate dramatically exceeds the predictive success rate from well-trained undergraduate coders.

Even so, we acknowledge several potential threats to our model’s applicability. These threats relate to inaccurate document transcription, changes in language over time, unrepresentativeness of the training set, and heteroskedastic prediction accuracy. We detail these threats, and provide a suite of robustness checks to evaluate them, in Appendix B.

Patterns in Significant Unilateral Action, 1953–2016

Based on the criterion we adopted for distinguishing policy-significant unilateral actions, Figure 2 shows the annual distribution of significant unilateral actions from 1953 to 2016. Across the entire time period, presidents issued an average of 84 significant directives per year. As other research has detailed (e.g., Howell 2003), presidents have generally made increasing use of unilateral powers to achieve significant policy outcomes over the latter half of the twentieth century. Presidents between Eisenhower and Carter (inclusive) issued an average of 47 significant directives per year, while this figure nearly triples (to 113) for presidents Reagan through Obama. Despite the common association of unitary executive theory with the Reagan and George W. Bush presidencies, our data show that the most significant increase in the use of unilateral power occurred during the George H. W. Bush and Clinton presidencies. No president had issued more than 100 significant directives prior to the last year of Bush’s presidency, when he issued 130. Clinton maintained a similar rate, issuing between 144 and 168 in each year of his first term. Clinton further increased the use of unilateral power during his second term, reaching a peak of 201 in 1998. Since then, however, the annual number of unilateral actions has declined somewhat, though not to pre-H.W. Bush levels.

Figure 3 compares the patterns from our data to other commonly-used measures of significant unilateral action with data from Bolton and Thrower (2016), Howell (2005), and Lowande (2014).
Figure 2: Annual number of significant executive actions from 1953 to 2016.
Two patterns stand out. First, with few exceptions our measure identifies substantially more instances of unilateral action than characterized by the use of specific unilateral tools. Second, the comparisons also suggest that our measure provides new information about the use of unilateral powers, particularly in recent decades. Before 1980, our measure of the volume of significant presidential activity was correlated relatively highly with related measures. The correlations between our measure and the number of nonceremonial executive orders, significant executive orders, and memoranda, respectively, are .50, .22, and .43. Since 1980, however, the correlations are negatively signed and small in magnitude (-.10, -.10, and -.02, respectively, suggesting that the use of one specific directive type is relatively uninformative about the overall volume of unilateral activity. Our data thus provide new descriptive information about presidents’ use of unilateral powers that is not fully captured by existing data sources and suggest that empirical studies of unilateral action by contemporary presidents may risk potential biases by focusing only on specific tools.

Figure 4 shows how the distribution of significance varies across various unilateral tools. Overall, between 1953 and 2016, executive orders comprised 40 percent of significant unilateral actions, while memoranda and proclamations accounted for 41 and 19 percent, respectively. These aggregate statistics obscure substantial variation across time, however. Consistent with Lowande (2014), the figure makes clear that the dramatic rise in significant unilateral action in the early 1990s was driven largely by increased use of memoranda. Before 1980, memoranda comprised an average of 11 percent of the annual number of significant actions; since then, however, memos have comprised an average of 43 percent of significant actions. The increased prominence of memoranda among significant unilateral directives has come largely at the expense of proclamations, which comprised 47 percent of significant unilateral actions before 1980 but only 13 percent since then.

Our data and measures thus generates several new descriptive findings about patterns of unilateral activity by modern American presidents. We now use these data to test our hypotheses
Figure 3: Annual number of significant unilateral actions, 1953 to 2016

The solid line shows the annual number of significant unilateral actions. The dashed line shows the number of nonceremonial executive orders using data from Bolton and Thrower (2016) and the dotted line shows the number of significant executive orders from Howell (2003). Data on memoranda are from Lowande (2014).
Figure 4: Significant Actions by Unilateral Tool, 1953–2016
about institutional conflict and presidents’ use of unilateral power.

Modeling Presidents’ Use of Unilateral Power

Using the significance scores developed above, we test our hypotheses about the institutional factors that shape presidents’ use of unilateral powers from 1953 to 2017. Our modeling approach follows previous research in this area (e.g., Bolton and Thrower 2016; Howell 2003). The dependent variable in our analysis is the number of significant unilateral actions issued by a president in a given year.

Our primary independent variables follow our main hypotheses. First, our argument predicts that presidents make greater use of unilateral powers as disagreement with Congress increases. We use common space DW-NOMINATE scores (Carroll et al. 2009) to characterize the preferences of presidents and median members of the U.S. House and Senate. Using these scores, we calculate the ideological distance between presidents and the median of each chamber, and characterize Distance from legislature as the greater of the two distances. This specification follows the logic that the chamber more distant from the president is the relevant constraint on a president’s ability to secure policy accomplishments through the legislative process.

Our focus on preference-based disagreement between presidents and Congress is consistent with the theoretical approach used in (Chiou and Rothenberg 2014, 2017) but departs from other research in this area which studies interbranch conflict on the basis of divided party government (e.g., Bolton and Thrower 2016; Howell 2003). As political parties have polarized over the last several decades and the distribution of legislative preferences within parties has changed, it is likely that divided government would not have the same consistent effect on a president’s unilateral power.

22These scores were obtained from https://legacy.voteview.com/dwnomin_joint_house_and_senate.htm (accessed April 28, 2018) Reasonably reliable estimates for presidents are available beginning in 1953.
eral calculations in analyses that cover longer periods of time. Our focus on preference-based disagreement helps avoid these issues. If our argument is correct, we expect to find a positive relationship between Legislative conflict and the annual number of unilateral actions.

Second, we test the hypothesis that presidents scale back the use of unilateral powers as ideological disagreement with the courts increases. We measure Distance from judiciary using judicial common space scores (Epstein et al. 2007) and presidents’ common space DW-NOMINATE estimates, both of which are intended to be comparable and are estimated in the same ideological space. In our main analyses, we include the ideological distance between the president and the median member of the U.S. Supreme Court. Of course, the Supreme Court will not review all unilateral actions; indeed, Howell (2003) reports that only about 16% of federal challenges to executive orders in the modern era were reviewed by the Supreme Court. If the courts operate as a constraint on presidential activity, however, presidents must anticipate the likely reception the Court would provide to a given unilateral act, and thus the Supreme Court may be the final judicial arbiter of unilateral activity. However, we recognize that other federal courts may also serve as relevant constraints given the few cases heard by the Supreme Court, and thus in additional analyses described below we characterize Distance from judiciary using the ideological distance between presidents and the D.C. Circuit and the average distance between presidents and the medians of all federal circuits. If judicial review constrains unilateral activity, we would observe a negative relationship between Distance from judiciary and unilateral action.

With 64 observations at the annual level, we are cognizant of the limits to statistical power.

---

Consistent with this measurement concern, the correlation between the presence of divided government and our measure of Distance from legislature is less than half as large prior to 1981 ($r = .42$) as it is for 1981 to 2016 ($r = .88$). We have also estimated our main results shown below while substituting Divided Government for Distance from legislature, and we find no evidence that divided government is associated with the issuance of fewer significant unilateral actions. See Table C.1.
that accompany our modeling approach. Nevertheless, we seek to account for other factors to address potential confounding. First, unobserved characteristics of presidents and their administrations may be associated with the varying propensity to exercise unilateral powers. While prior scholarship has differed in its use of president fixed effects when modeling unilateral action—for instance, Bolton and Thrower (2016) and Howell (2003) include them while Chiou and Rothenberg (2014, 2017) do not—we split the difference and estimate both model specifications. The inclusion of president fixed effects means that the primary coefficients of interest are identified with variation in within a given presidential term and comes with both strengths and limitations. On the one hand, this modeling strategy accounts for potential president-specific confounders and implicitly addresses secular trends; on the other hand, the within-president variation for our main independent variables is relatively small which raises the possibility that minor changes in the ideological alignment between presidents and other institutions could produce coefficients that are large in magnitude. Second, we include an indicator, Administration change, for new presidential administrations that represent a change in the president’s party. Because a change in the party occupying the White House has implications for the distribution of status quo policies which can be changed by the incoming president, this variable is expected to be positively signed (Chiou and Rothenberg 2014, 2017; Howell 2003). Third, we account for factors which may create greater demand or incentives for presidents to issue unilateral actions by including indicators for years the country is involved in major War.24 In all our models, standard errors are clustered on president.

At the outset, and as with related scholarship in this area (Bolton and Thrower 2016; Chiou and Rothenberg 2014, 2017; Howell 2003), we note the limitations of our empirical approach for identifying the effects of institutional conflict on presidential unilateralism. We cannot randomly assign presidents to varying levels of ideological conflict with adjoining institutions, and while

it may be possible in principle to consider regression discontinuity approaches for studying the effect of the ideological composition of Congress on presidential behavior it would require a significantly longer time series than we possess. Moreover, our approach requires us to assume that (for instance) patterns of unilateral activity in years with high institutional conflict would have otherwise resembled patterns of unilateral action in years with low institutional conflict if there not had otherwise been high levels of conflict. In a more ideal scenario, we would distinguish instances where presidents issued unilateral directives from other times in which they could have plausibly done so (or desired to do so) to evaluate potential explanations for their restraint. Lacking these data, we instead follow approaches used in scholarship on the passage of major laws (e.g., Clinton and Lapinski 2006; Mayhew 1991) and interpret our results as evidence of the predictors of presidential productivity.

Results

Table 2 displays the results of our analyses. In column (1) and (2) we report results from testing our hypotheses in bivariate settings. In column (3) we test our hypotheses jointly, to which we add president fixed effects in column (4) and additional covariates in column (5).

We first describe the results for our hypothesis regarding unilateral action and presidential-congressional conflict. Across each of the four models in which this variable is included, the coefficient estimate is positive. Contrary to accounts that argue that ideological conflict with Congress leads presidents to issue fewer unilateral actions than they otherwise might, this result suggests that presidents issue more unilateral actions when the legislative route may be less available for accomplishing their political goals. Moreover, the coefficient estimate is statistically significant in models (3) through (5). In contrast with a large empirical scholarship which uses indicators of divided party government to emphasize the strength of legislative constraints on unilateral action, our results indicate that presidents make greater use of unilateral powers during periods of greater ideological disagreement with Congress. To our knowledge, this is the
among the first findings in the literature consistent with the evasion hypothesis, although our argument implies a somewhat different interpretation.

The magnitudes of the estimates in Table 2 are substantively important. Based on the results shown in model (5), we estimated the predicted number of significant unilateral actions issued at the mean level of ideological conflict (0.437) and one standard deviation (0.183) above it. Recent presidents have frequently experienced a one-standard deviation increase in ideological conflict with Congress during their time in office; for instance, the values of this variable increased by 0.3 or more during the administrations of Presidents Bill Clinton, George W. Bush, and Barack Obama. A one-standard deviation increase in ideological conflict is predicted to increase the number of significant unilateral acts by about nine, from 83 to 92, which corresponds to an 11 percent increase. The predicted increase in unilateral activity is even greater at the levels of within-president change in ideological conflict experienced under President Obama (increase from 0.11 to 0.58). With this change in the level of presidential-congressional conflict, our model predicts an increase in significant unilateral activity from 69 to 90, representing a 30 percent increase.

We find no evidence, however, that ideological conflict between the president and the judiciary is associated with less unilateral activity. The coefficient for Distance from judiciary is consistently positive and is statistically significant in some models. These provide no support for the hypothesis that presidents scale back the use of unilateral power as their preferences conflict with the judiciary. Extending the findings from Howell (2003) about the conditions under which courts defer to presidents when their unilateral actions are challenged, we find that the courts do not appear to exercise a meaningful constraint on presidents’ decisions to exercise unilateral power. While presidents may not issue more unilateral actions during periods of heightened conflict, as the coefficients do not reach statistical significance in either model (4) or (5), nor do any of our results suggest they issue fewer during these periods.

Additional analyses show that our results for ideological conflict between presidents and adjoining political institutions are robust to a number of alternative measurement strategies and
Table 2: Institutional Conflict and Unilateral Action

<table>
<thead>
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<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
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<td>0.527*</td>
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<td>(0.728)</td>
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<td>(0.238)</td>
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<td>(0.322)</td>
<td>(0.203)</td>
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President Fixed Effects ✓ ✓
Controls ✓
Observations 64 64 64 64 64

Robust standard errors clustered on president are in parentheses. * p < 0.05.
Dependent variable is the number of significant executive actions in year $t$.
President fixed effects are included where indicated.
Control variables include indicators for administration change and war.

model specifications. First, rather than use an indicator of significant unilateral action that is based on the value which equalizes the values of the false-positive and false-negative rates, we have also estimated models when significant unilateral actions are those that are above the median value of estimated significance. Because this threshold (0.156) is substantially smaller in magnitude than our chosen threshold (0.355), this results in the inclusion of many more unilateral actions. While this may introduce additional noise through the incorporation of less significant unilateral action, we continue to find similar results to those shown above.25

Second, we have estimated models when replacing our measure of Distance from legislature with the ideological conflict between the president and the House, and the president and the Senate. These results are shown in Tables C.3 and C.4 in the Appendix and are similar to those shown above. In neither of them do we find that ideological conflict with Congress reduces the production of unilateral action. We have also estimated models where we measure Judicial conflict based on the ideological distance between the president and the median of the D.C. Circuit

25See Table C.2.
Court of Appeals, where challenges to executive branch decisions are often filed.\textsuperscript{26} In addition, we have used the average ideological distance between presidents and the median of each of the 12 circuits. Neither sets of results indicate that ideological conflict between the president and the judiciary leads presidents to scale back the use of unilateral power.\textsuperscript{27}

We estimated additional models to explore the robustness of the relationship between Distance from legislature and unilateral action. To do so, we re-estimated model (5) from Table 2 across the range of significance values used to determine the threshold that distinguishes significant unilateral actions. Specifically, we estimated the model by distinguishing unilateral actions as those whose significance scores were greater than zero, greater than 0.01, greater than 0.02, and so forth until we estimated the model only when predicting the number of unilateral actions whose scores were 0.99 or larger. Each time we increased the threshold by 0.01. For each iteration, we calculated the predicted increase in the number of significant unilateral actions for a one standard deviation increase in the value of Distance from legislature. Figure 5 displays the results of this exercise. The x-axis indicates the threshold used to identify significant unilateral actions and the y-axis shows the predicted increase in the number of unilateral actions for each threshold. The points indicate the predicted increases and the vertical lines show the 95% confidence intervals. The dashed horizontal line at zero indicates the null hypothesis of no relationship between interbranch conflict and unilateral action.

The patterns shown in Figure 5 reinforce the results from Table 2 and provide strong evidence against claims that presidents issue fewer unilateral actions during periods of interbranch conflict vis-à-vis Congress. Virtually every model provides a positive coefficient for Distance from legislature and a large percentage of them are statistically distinguishable from zero. While at

\textsuperscript{26}We note, however, that the judicial common space scores for circuit court judges are estimated based on the party of the nominating president, which may present potential endogeneity and/or measurement error. Therefore, we interpret these supplementary findings with caution.

\textsuperscript{27}See Tables C.5 and C.6.
very high thresholds we find no association between divided party control of government and presidential unilateralism. We note, however, that unilateral actions with large values of significance are exceedingly rare; for instance, only 3.4%, 2.1%, and 1.0% of directives had significance estimates greater than 0.70, 0.80, and 0.90, respectively.

Figure 5: Legislative Conflict and Unilateral Action across Significance Levels

![Graph showing predicted increase in number of unilateral acts across significance levels.](image)

Estimates based on model (5) of Table 2. The points show the predicted increase in significant unilateral action associated with Distance from legislature. The horizontal lines show the 95% confidence intervals associated with each estimate. Values along the x-axis indicate the threshold used to distinguish significant unilateral acts. Positive values along the y-axis indicate that Distance from legislature is associated with more unilateral action while negative values indicate that Distance from legislature is associated with fewer unilateral actions. The horizontal dashed line indicates the null hypothesis of no association between interbranch conflict and unilateral action.

Figure 6 presents analogous results for Distance from judiciary. Across the range of signifi-
cance thresholds, we find no systematic evidence that presidents scale back the use of unilateral power as disagreement with the Courts increases. Across many of the values, moreover, the predicted values are positive, though none are statistically distinguishable from zero. Conservatively, the results in Figure 6 allow us to conclude that ideological opposition from the judiciary does not appear to affect volumes of unilateral policy output from the president.

**Figure 6: Judicial Conflict and Unilateral Action across Significance Levels**

Estimates based on model (5) of Table 2. The points show the predicted increase in significant unilateral action associated with Distance from judiciary. The horizontal lines show the 95% confidence intervals associated with each estimate. Values along the x-axis indicate the threshold used to distinguish significant unilateral acts. Positive values along the y-axis indicate that Distance from judiciary is associated with more unilateral action while negative values indicate that Distance from judiciary is associated with fewer unilateral actions. The horizontal dashed line indicates the null hypothesis of no association between interbranch and unilateral action.
Variation across Tools

Finally, we consider whether the nature of institutional constraints varies across the tools of unilateral power. Table 3 distinguishes the effects of interbranch conflict among Executive Orders, Memoranda, and Proclamations. The table reports the results of models (4) and (5) from Table 3 but where the dependent variable is the number of significant executive orders, memoranda, and proclamations, respectively. The coefficient estimate for Distance from legislature is positive in five of the six models, providing little evidence that the nature of congressional constraints on the president vary across unilateral tools. Similarly, the coefficients for Distance from judiciary are mostly indistinguishable from zero. On the whole, Table 3 provides little reason to believe that institutional constraints on unilateral power meaningfully vary across the tools of direct presidential action. For the most part, the broad similarities suggest that they are each part of presidents’ larger unilateral strategies as their use responds to similar constraints and incentives.

Table 3: Institutional Conflict and Unilateral Action: Variation across Tools

<table>
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<tr>
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<th>Proclamations</th>
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<td>Distance from legislature</td>
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Robust standard errors clustered on president are in parentheses. * p < 0.05.
Dependent variable is the number of significant executive actions in year t.
Control variables include indicators for administration change and war.
President fixed effects included where indicated.

Our results provide new evidence about the institutional and political factors that shape presidents’ calculations about the feasibility for and necessity for unilateral action. We find that ide-
ological conflict with Congress and the courts operates in competing fashion as constraints on unilateral power. Increased disagreement with Congress creates incentives for presidents to execute their policy objectives in alternate ways, including through the use of unilateral power. Moreover, our results indicate that the threat of judicial review may not be a meaningful deterrent for contemporary presidents. to strike down their actions, as unilateral action decreases as presidents and the Courts exhibit greater ideological disagreement. Presidents use unilateral powers at greater rates when legislative solutions appear increasingly unworkable and, at minimum, do not scale back their use of unilateral power when the threat of judicial disagreement increases.

Conclusion

Unilateral action is one of the most distinctive features of the modern presidency and is perhaps more publicly salient than it ever has been. Its use by recent presidents has prompted legal scholars and political observers to express concern that unilateral powers erode the Madisonian system of separation of powers. To date, however, political scientists have been relatively more measured about the importance of unilateral authority for the balance of power among American political institutions. Across the dozens of studies on presidents’ use of unilateral action, few if any have shown that presidents are more likely to issue them during periods of institutional conflict between presidents and Congress—precisely the context in which presidents might be most tempted to use them.

Our paper breaks new substantive and methodological ground in studying interbranch relations in general and unilateral action in particular. Theoretically, we posit that unilateral powers are important means for presidents to achieve their programmatic commitments and advance

their legacies. As such, we provide an integrated account of how presidents’ unilateral calculations reflect both institutional constraints and political factors. Moreover, we argue that existing studies substantially mischaracterize the use of unilateral powers by failing to account for the variety of unilateral tools that presidents wield. Methodologically, we introduce a new text-based approach to estimating the policy significance of a vast repository of presidential documents that describe unilateral actions. So doing, we provide new evidence about the nature of interinstitutional interactions and presidential behavior.

Our findings suggest that presidents are institutionally stronger than many theoretical accounts of unilateral action imply. For instance, the theoretical perspectives of Howell (2003) and Chiou and Rothenberg (2014, 2017), who argue that presidents can use unilateral powers to achieve incremental policy gains or only with the implicit approval of congressional leaders, respectively, suggest that unilateral action is a rather limited tool of presidential power. Moreover, given the longstanding findings in the literature that divided government is associated with fewer executive orders, unilateral authority would appear to be a relatively marginal contributor to presidential power. Instead, on the basis of our results presidents can—and do—issue many more unilateral actions when Congress is ideologically distant from the president, indicating that presidents appear to appreciate the myriad ways they can achieve their objectives when they cannot secure agreement from Congress.

From a research design perspective, we note the limitations of our study with respect to identifying clear causal evidence of constraints on unilateral action. Though our approach is consistent with those employed in related scholarship, we lack a true counterfactual. In a more ideal world, we would have access to a comprehensive list of each president’s policy goals which we could then compare against the list of unilateral actions presidents have issued. This approach would allow us to identify the conditions under which presidents with identical policy goals chose to exercise unilateral powers to achieve them. As such, we are reluctant to make strong inferences about the strength or weakness of various constraints on unilateral action because we cannot
dispositively rule out the possibility that presidents may have varying numbers of policy goals that correlate with the ideological composition of government. Addressing this challenge is a key opportunity for studies of presidential power. These limitations notwithstanding, our study makes important headway into understanding the range of ways through which presidents wield power. In addition, our data and text-based estimates of the significance of presidential action can be used to study a number of other important questions about lawmaking, the presidency, and political institutions more generally.
References


# ONLINE APPENDIX

Robustness Checks and Supplementary Analyses for *Interbranch Conflict and Executive Action*

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### A Supplementary Appendix: Source Record Groups

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### B Supplementary Appendix: Model Validity Robustness Checks

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### C Robustness Checks to Regression Specifications

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A Supplementary Appendix: Source Record Groups

In this appendix we indicate which unilateral action “Source Record Groups” we group into each larger category of unilateral action.

A.1 Executive Orders

This category contains documents which are numbered and unnumbered executive orders.

EO - Numbered Executive Orders 1862-present
03 - Public Land Orders 1942-present
06 - Secretary of Interior Orders 1920-1950
22 - Executive Orders Relating to the Panama Canal 1902-1934
33 - Executive Orders Relating to Public Lands 1841-1935
56 - Presidential Policy Directives & National Security Decision Memoranda*

A.2 Memoranda

This category contains Executive Memoranda or other such memoranda from collections of presidential documents.

04 - Presidential Documents 1936-present
21 - Public Papers of the Presidents 1789-present
52 - Miscellaneous Printed Sources 1789-1936
53 - Weekly Compilation of Presidential Documents 1965-present
59 - Presidential Security Directives*
A.3 **Proclamations**

This category includes only documents clearly noted as proclamations.

PR - Numbered Proclamations 1789-present
29 - Treaty Proclamations 1789-present

A.4 **Before Current Data**

These Source Record Groups contain documents prior to our window of study.

05 - White House Records 1869-present
08 - Manuscript collections 1790-1929
12 - Treasury and Justice Dept Records 1789-1908
15 - Printed Annual Agency Reports 1910-1914
17 - Navy and War Dept Records 1789-1884
20 - Messages and Papers of the President 1789-1899
25 - Official Bulletin 1917-1919
34 - Proclamations Relating to Public Lands 1813-1892
35 - Proclamations Relating to Public Lands 1834-1907
36 - Proclamations of Land Sales 1807-1886
37 - Abandoned Military Lands 1826-1905
38 - Executive Orders Relating to Indian Reservations 1850-1892
39 - Lighthouse Land Reservations 1837-1888
41 - Executive Orders Relating to Public Lands 1820-1913
43 - Abandoned Non-Military Land Reservations 1839-1901
A.5  Removed

We remove two categories of documents: those which do not have effectual policy significance, and those which are internal administrative actions (“Records”).

26 - Press Releases 1953-1955
44 - Presidential Pardons 1793-1935
48 - Pardon Attorney Records 1919-1924
51 - Codes of Fair Competition 1933-1935
57 - Statements of Administration Policy*
58 - Weekly Compilation of Presidential Documents 1965-present*

A.5.1  Records

The Records category consists of internal executive agency documents. We do not include them in our analysis.

13 - Independent Agencies Records 1917-1954
14 - Indian Agencies Records 1794-1937
24 - Treasury Dept Circulars 1859-1940
46 - Interior Department Records 1849 - 1938
47 - Water and Power Site Land Reservations 1909-1944
54 - War Department General Orders and Bulletins 1826 - 1954
Note: SRGs noted with * come from the same source as the other documents in our data. However, they lack full documentation as to their contents and origins. We have reviewed the documents contained in each category and have characterized them as reflects our substantive understanding of their contents. We note, however, that there are relatively few documents in these SRGs for the period under study and all our substantive results are robust to their exclusion.
A.6 Distribution of significance estimates

![Graph showing distribution of significance estimates across documents.](image)

**Figure A.1:** Distribution of significance estimates across documents.
B Supplementary Appendix: Model Validity Robustness Checks

In interpreting the results of our model, we note that it all documents are scored on the same scale: an estimated significance of 1 means the same thing for executive orders, proclamations, and memoranda. However, we expect that our measurement strategy induces heterogeneous measurement error. Some of our documents have their significance scores hand-coded or assumed; these have minimal measurement error if any. Some documents, on the other hand, have measurement error derived from any number of challenges: poor document transcription, language heterogeneity, the rarity of highly significant documents, and other problems generally associated with transfer learning.

We assess accuracy through two means. The first is through cross-validated AUC, or the area under the precision-recall curve; the second is through comparisons to human coders. Cross-validated AUC measures how well a model measures the relationship between covariates and outcomes in the training data. This is a difficult task: text-as-data methods are best suited to measuring concrete and measurement error-free concepts, while unilateral action significance is anything but concrete. Regardless, we achieve notable success in cross-validation accuracy.

When examining binary outcomes, as is the case in our modeling approach, the most common accuracy measures are precision and recall (Ling et al. 2003; Huang and Ling 2005). Precision is the number of correct positive identifications divided by the total number of identifications; recall is the number of correct positive identifications divided by the total number of true positive cases. Taken together, these measures produce a Receiver operating characteristic (ROC).\(^\text{29}\) The area

\(\text{29}\)Consider a model which produces probabilities that an observation is either a 1 or a 0. To measure the accuracy of this model, we must first specify a predictive cutoff. Perhaps we determine that any observation predicted to be 1 with \(p > 0.5\) is a 1, and otherwise a 0, then we can measure precision and recall. However, as we vary our predictive cutoff, precision and recall change. The ROC curve captures precision and recall for all values of the predictive cutoff from
under the ROC curve, called AUC, is the gold standard standard measure of predictive accuracy for binary classification tasks. In Figure B.1, we present the ROC and AUC for our model with a significance threshold of 0.5. The AUC is 0.99, on par with many of the best results in the field of machine learning.

This result is difficult to interpret without a relevant benchmark. Ideally, that benchmark would be the best alternative to using a machine learning model. To establish that benchmark, we trained three undergraduate research assistants to manually code unilateral actions as significant (1) or ceremonial (0) and compared those human coders’ accuracy to that of our model.\textsuperscript{30} We presented the research assistants with 100 executive orders that have significance scores from Chiou and Rothenberg (2014), as well as 100 other unilateral actions from our data set that did not have Chiou and Rothenberg (2014) estimates, and asked the students to code the significance of those documents. We then performed two analyses on these hand-coded significance scores. The first measures inter-coder reliability. An important advantage of machine learning models for coding documents is consistency: the model will yield a similar or identical result every time it is queried. Human coders, however, are often inconsistent. The research assistants’ hand-coded executive order significance scores were not highly correlated with each other. Taking the undergraduates in pairs and measuring their percent agreement at coding unilateral actions as significant or ceremonial, the three undergraduates agree with each others at rates of 65\%, 71\%, and 63\%.

In the second analysis, we calculate AUC scores for the three sets of hand-coded documents compared to with Chiou and Rothenberg’s scores for the same documents, thresholded at 0.5. If the research assistants’ AUC scores, individually or aggregated, are lower than the machine

\textsuperscript{30}The undergraduate coders were asked to research the unilateral actions and assess their policy significance using their own best judgment and knowledge of the relevant historical/political context.
learning model, then we can be confident that the machine learning model is an improvement over the current state of the art. We find that the research assistants’ codings produce AUCs of 0.68, 0.67, and 0.65, each of which is substantially lower than the AUC of 0.99 produced by the machine learning model. In practice, when using research assistants to hand code noisy data, it is common to average hand codes to produce a more reliable measure. We take the elementwise average of the three hand-coded significance codes and calculate that the AUC for that aggregated coding is 0.71. These exercises suggest that our machine learning model is substantially more accurate than trained undergraduate research assistants, and provides a dramatic improvement as a consistent and scalable approach for measuring document significance.

B.1 Document Transcription

Many of the documents we analyze are simply scanned images of printed pages in PDF format. We extracted text from these PDFs using Google’s Tesseract 4 optical character recognition (OCR) system. For documents with typed text, this OCR procedure produces high quality text. However, for many earlier and hand-written documents, the OCR-derived text is of poor quality. To improve the data quality in these cases, as well as in cases where more than 10% of the words are not found in a dictionary, we transcribed these documents by hand. Together, these two samples account for 5% of our total corpus. As a validity check, we transcribed 20% of this sample twice; concordance between the doubly-transcribed documents ensures us that our transcriptions are satisfactory.

B.2 Language Heterogeneity & Unrepresentative Training Setss

First, a critical assumption for our analysis is that the language and word choice indicative of significant executive orders is sufficiently similar to that of other types of significant unilateral action. For example, the tone and style of significant executive orders and proclamations may be
very legalistic, while important memoranda may be more rhetorical; if this is the case, then many of the textual features which contribute to a document’s significance may be legalistic, biasing downward the significance of documents other than Executive Orders and proclamations. This problem may be especially severe in cases where the temporal distribution of the training set diverges from that of the test set.

To fortify our model against this weakness, we must expand our training set to include more representative documents. However, since we do not have significance scores for documents other than executive orders, we infer them using a manual matching procedure. We first select a random 500 executive orders from our training set. Then, using the ProQuest Executive Actions database, we manually search for documents which reference one and only one executive order in our random sample. If we find a document which is substantively related to a single executive order, we assign that document the same significance, either 0 or 1, as the executive order it mentions. By assigning equal significance to those two documents, we teach our model to recognize the significance of a wider variety of rhetorical styles. We find matches for 86 of the 500 executive orders in our random sample. Many of those executive orders have multiple matching documents; as a result, our matching procedure adds 287 observations to our training data.

B.3 Heteroskedastic Predictive Accuracy

A third challenge is ensuring that the model’s predictions, which we aggregate into dependent variables for regressions, are not systematically biased. If the predictions are unbiased by measured with error, that measurement error will force our regression coefficients toward zero. If, however, the predictions are biased, then the regression coefficients may be artificially extreme. Biased predictions may be observable in the cross-validation accuracy as heteroskedasticity.

Importantly, there is little observed heteroskedasticity: our model’s residuals are only weakly correlated with the true significance labels. However, insofar as there is heteroskedasticity, it is
among the low-significance documents. Documents which Chiou and Rothenberg estimate to be of very low significance our model often overestimate as being moderately significant. This is critical for performing additional analysis, as any systematic bias in our model’s accuracy would subsequently bias any regression results for which we use our model’s predictions.

### B.4 Temporal Variation in Modeling Accuracy

A potential criticism of this approach is that our model may underestimate the significance of documents whose text is unlike the text of numbered Executive Orders or proclamations in our training set. Consider, for example, a model trained only on data from the 1940s, used to evaluate the significance of documents from the 2010s. Due to changes in language over time, that model is unlikely to perform well. The same result holds, though, if there are more documents from the 1940s than there are from the 2010s.

To test this, we perform a similar cross-validation procedure as in our main results, except instead of each fold consisting of random subsamples, each subsample is a decade of text. This allows us to test whether our model fails to accurately estimate the significance of documents from time periods outside the training set. As Figure B.1 shows, the temporal cross-validation accuracy is not substantially lower than the randomly partitioned cross-validation accuracy, providing evidence that the lexical cues indicating document significance do change over time. This gives us confidence that our model is robust to relatively mild changes in language usage, though we still acknowledge that much earlier documents pose a significant estimation challenge. However, as we discuss in the Results section, any measurement error induced by this estimation challenge should serve only to reduce the absolute magnitude of our regression coefficients.
Figure B.1: The cross-validated AUC for the randomly partitioned analysis is not substantially lower than the temporally partitioned analysis, indicating that the model has significant capacity to estimate document significance outside its temporal support.
B.5 Feature Importance

As a face validity check on our model, we perform a descriptive feature importance analysis. Since random forest models are largely black boxes where features enter and predictions are returned, determining which covariates contribute most to the model’s success can be difficult. One commonly used method to extract feature importances from tree-based models involves “feature depth” (Archer and Kimes 2008). Since random forests consist of decision trees that are ordered variable splits, features that systematically appear earlier in the decision tree are more important to the model. A covariate’s feature importance, then, is proportional to the average number of times that feature appears in the decision tree, weighted by how early in the tree it appears; more simply, higher values indicate more strongly predictive features.

Note that this is purely a descriptive exercise; if our model performs as we intend, we expect that the most important features it identifies will be ones which intuitively discriminate significant orders, which discuss tariffs, military conflict, and industry, from ceremonial ones, which memorialize the dead or declare National Ice Cream Day. We calculate feature importance for random forests model and present the 20 most important terms below in columns 1 and 2 of Table B.1, then 20 largely insignificant terms in columns 3 and 4. Among the most significant words are section, persuant, provision, necessary, and schedule, which generally indicate appeals to either constitutional or statutory authority. Insignificant terms include bank, south, sponsor, and tent. A feature importance table derived only from executive orders follows in Table B.2.

An initial inspection of our approach to coding presidential documents, therefore, suggests that we have uncovered a meaningful dimension that distinguishes actions based on whether they address consequential policy issues or more are ceremonial in nature.

As well, we examine the feature importances of a model trained on only on executive orders. Among the most significant words, verbs predominate: direct, threat, take, report, enforce, permit, establish, include, create, act, engage, amend. These verbs all relate to the positive powers of the President. The remaining significant terms are policy, law, necessary, agency, nation, act,
Table B.1: Feature Importance

<table>
<thead>
<tr>
<th>Important Term</th>
<th>Importance</th>
<th>Unimportant Term</th>
<th>Importance</th>
</tr>
</thead>
<tbody>
<tr>
<td>section</td>
<td>20.536</td>
<td>harm</td>
<td>-0.056</td>
</tr>
<tr>
<td>shall</td>
<td>17.84</td>
<td>acrland</td>
<td>-0.057</td>
</tr>
<tr>
<td>pursuant</td>
<td>16.278</td>
<td>regu</td>
<td>-0.057</td>
</tr>
<tr>
<td>warehous</td>
<td>12.408</td>
<td>signal</td>
<td>-0.057</td>
</tr>
<tr>
<td>tariff</td>
<td>12.111</td>
<td>principl</td>
<td>-0.057</td>
</tr>
<tr>
<td>proclamshall</td>
<td>11.352</td>
<td>elsewher</td>
<td>-0.058</td>
</tr>
<tr>
<td>provis</td>
<td>11.334</td>
<td>task</td>
<td>-0.058</td>
</tr>
<tr>
<td>forth</td>
<td>10.654</td>
<td>despit</td>
<td>-0.058</td>
</tr>
<tr>
<td>consumpt</td>
<td>10.501</td>
<td>tribut</td>
<td>-0.058</td>
</tr>
<tr>
<td>modifi</td>
<td>10.023</td>
<td>might</td>
<td>-0.058</td>
</tr>
<tr>
<td>enter</td>
<td>9.877</td>
<td>encunit</td>
<td>-0.059</td>
</tr>
<tr>
<td>necessari</td>
<td>9.552</td>
<td>arid</td>
<td>-0.059</td>
</tr>
<tr>
<td>schedul</td>
<td>9.292</td>
<td>korea</td>
<td>-0.059</td>
</tr>
<tr>
<td>withdrawn</td>
<td>9.207</td>
<td>sfmt4790</td>
<td>-0.06</td>
</tr>
<tr>
<td>vest</td>
<td>8.997</td>
<td>recreat</td>
<td>-0.06</td>
</tr>
<tr>
<td>act</td>
<td>8.6</td>
<td>prospect</td>
<td>-0.061</td>
</tr>
<tr>
<td>modif</td>
<td>8.592</td>
<td>sector</td>
<td>-0.061</td>
</tr>
<tr>
<td>extent</td>
<td>8.024</td>
<td>polic</td>
<td>-0.061</td>
</tr>
<tr>
<td>american</td>
<td>8.021</td>
<td>mental</td>
<td>-0.061</td>
</tr>
<tr>
<td>pursuant</td>
<td>7.958</td>
<td>telephon</td>
<td>-0.061</td>
</tr>
</tbody>
</table>

and person. The insignificant terms include verbs as well (count, resign, counter, repair, hope, roll, undertake, led, recite, hope), but these are verbs are gaeneric and are not associated with the unilateral powers of the presidency. The remaining terms are red, supreme, receipt, northeast, room, 1934, ever, single, Feb(uary), which, while plausibly related to policy, would not appear differentially in significant actions relative to ceremonial ones.

B.6 Robustness to Threshold Choices

In two places in our measurement strategy we identify thresholds for indicating significant documents. First, we select a threshold of 0.50 in coarsening Chiou & Rothenberg’s significance scores into a dichotomous indicator. Second, after generating predictions from our random forest model, we identify documents with a probability of $\geq 0.355$ as being significant.
Table B.2: Feature Importance in Presidential Documents

<table>
<thead>
<tr>
<th>Important Term</th>
<th>Importance</th>
<th>Unimportant Term</th>
<th>Importance</th>
</tr>
</thead>
<tbody>
<tr>
<td>polici</td>
<td>53.642</td>
<td>red</td>
<td>0.032</td>
</tr>
<tr>
<td>shall</td>
<td>40.587</td>
<td>suprem</td>
<td>0.032</td>
</tr>
<tr>
<td>direct</td>
<td>30.221</td>
<td>receipt</td>
<td>0.032</td>
</tr>
<tr>
<td>agenc</td>
<td>20.072</td>
<td>northeast</td>
<td>0.032</td>
</tr>
<tr>
<td>law</td>
<td>18.772</td>
<td>room</td>
<td>0.032</td>
</tr>
<tr>
<td>necessari</td>
<td>18.732</td>
<td>1934</td>
<td>0.031</td>
</tr>
<tr>
<td>threat</td>
<td>16.659</td>
<td>ever</td>
<td>0.031</td>
</tr>
<tr>
<td>take</td>
<td>15.549</td>
<td>hope</td>
<td>0.031</td>
</tr>
<tr>
<td>report</td>
<td>15.096</td>
<td>began</td>
<td>0.031</td>
</tr>
<tr>
<td>enforc</td>
<td>13.919</td>
<td>count</td>
<td>0.031</td>
</tr>
<tr>
<td>permit</td>
<td>12.636</td>
<td>singl</td>
<td>0.031</td>
</tr>
<tr>
<td>establish</td>
<td>12.474</td>
<td>feb</td>
<td>0.030</td>
</tr>
<tr>
<td>nation</td>
<td>10.761</td>
<td>recit</td>
<td>0.030</td>
</tr>
<tr>
<td>board</td>
<td>10.575</td>
<td>resign</td>
<td>0.030</td>
</tr>
<tr>
<td>includ</td>
<td>10.451</td>
<td>counter</td>
<td>0.030</td>
</tr>
<tr>
<td>creat</td>
<td>10.343</td>
<td>undertak</td>
<td>0.030</td>
</tr>
<tr>
<td>act</td>
<td>9.741</td>
<td>led</td>
<td>0.029</td>
</tr>
<tr>
<td>person</td>
<td>9.489</td>
<td>roll</td>
<td>0.029</td>
</tr>
<tr>
<td>engag</td>
<td>9.184</td>
<td>repair</td>
<td>0.029</td>
</tr>
<tr>
<td>amend</td>
<td>8.810</td>
<td>ous</td>
<td>0.029</td>
</tr>
</tbody>
</table>

The first threshold of 0.50 is the result of our qualitative reading of a large number of these documents. As we argue in Section, our decision to threshold here reduces measurement error vis-à-vis Chiou and Rothenberg (2014, 2017), but also turns our modeling problem from a regression problem on a scale from -1 to 3 with difficult interpretability to a much easier and more reliable classification problem. The outcome of our model can be interpreted as the probability that a document would receive a Chiou and Rothenberg score of greater or less than our threshold of 0.50. This interpretation likewise allows us to bootstrap standard errors. The choice of the threshold itself at 0.50, rather than at 0 or at 1, is the result of extensive qualitative readings of these documents.

The second threshold is of the modeling results. Our model produces probabilities that a document would receive a Chiou and Rothenberg score of greater or less than 0.50. To convert
these probabilities into a prediction of whether a document is significant, we identify a second threshold.\textsuperscript{31} We identify 0.355 as the estimated probability at which we declare a document “significant.” While we could reasonably choose 0.50, or 0.95, or many other thresholds, we select 0.355 since it is the value which equalizes the false-positive rate and the false-negative rate. This is a desirable property since it mitigates bias and heteroskedasticity in our regression analyses. Figures 5 and 6 show robustness checks to this threshold as well.

While we perform robustness checks to show that our substantive results are not sensitive to our choices of these thresholds, we acknowledge that these choices induce “researcher degrees of freedom” (Simmons, Nelson, and Simonsohn 2011). In the interests of transparency and reproducibility, we make public our complete replication file on the Harvard Dataverse. Included in our replication code are clearly demarcated points where we perform our coarsening allowing interested researchers to experiment with alternative thresholding decisions.

\textsuperscript{31}Note that measuring AUC as above does not require selecting a threshold here.
C Robustness Checks to Regression Specifications

C.1 Accounting for Divided Government

Table C.1: Divided Government and Unilateral Action

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Divided government</td>
<td>0.129</td>
<td>0.267</td>
<td>0.084</td>
<td>0.101</td>
</tr>
<tr>
<td></td>
<td>(0.197)</td>
<td>(0.181)</td>
<td>(0.072)</td>
<td>(0.080)</td>
</tr>
<tr>
<td>Distance from judiciary</td>
<td>1.366*</td>
<td>0.213</td>
<td>0.106</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.332)</td>
<td>(0.197)</td>
<td>(0.181)</td>
<td></td>
</tr>
<tr>
<td>President Fixed Effects</td>
<td>✓</td>
<td>✓</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Controls</td>
<td>✓</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>64</td>
<td>64</td>
<td>64</td>
<td>64</td>
</tr>
</tbody>
</table>

Robust standard errors clustered on president are in parentheses. * p < 0.05.
Dependent variable is the number of significant executive actions in year $t$.
Control variables include indicators for administration change and war.
C.2 Distinguishing Significant Unilateral Action Based on the Median Significance Estimate

**Table C.2: Institutional Conflict and Unilateral Action**

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Distance from legislature</td>
<td>1.479*</td>
<td>1.588*</td>
<td>0.243</td>
<td>0.348</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.372)</td>
<td>(0.318)</td>
<td>(0.164)</td>
<td>(0.194)</td>
<td></td>
</tr>
<tr>
<td>Distance from judiciary</td>
<td>0.281</td>
<td>0.487</td>
<td>0.074</td>
<td>-0.059</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.438)</td>
<td>(0.259)</td>
<td>(0.255)</td>
<td>(0.226)</td>
<td></td>
</tr>
<tr>
<td>President Fixed Effects</td>
<td>✓</td>
<td>✓</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Controls</td>
<td></td>
<td>✓</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>64</td>
<td>64</td>
<td>64</td>
<td>64</td>
<td>64</td>
</tr>
</tbody>
</table>

Robust standard errors clustered on president are in parentheses. * p < 0.05.

Dependent variable is the number of significant executive actions in year t.

Control variables include indicators for administration change and war.
## C.3 Alternative Specifications for Legislative-Presidential Conflict

### Table C.3: House Conflict and Unilateral Action

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Distance from House</td>
<td>0.603</td>
<td>0.702</td>
<td>0.312</td>
<td>0.441</td>
</tr>
<tr>
<td></td>
<td>(0.667)</td>
<td>(0.459)</td>
<td>(0.244)</td>
<td>(0.250)</td>
</tr>
<tr>
<td>Distance from judiciary</td>
<td>1.300*</td>
<td>0.239</td>
<td>0.124</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.345)</td>
<td>(0.215)</td>
<td>(0.177)</td>
<td></td>
</tr>
<tr>
<td>President Fixed Effects</td>
<td>✓</td>
<td>✓</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Controls</td>
<td>✓</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>64</td>
<td>64</td>
<td>64</td>
<td>64</td>
</tr>
</tbody>
</table>

Robust standard errors clustered on president are in parentheses. * p < 0.05. Dependent variable is the number of significant executive actions in year $t$. Control variables include indicators for administration change and war.

### Table C.4: Senate Conflict and Unilateral Action

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Distance from Senate</td>
<td>0.524</td>
<td>0.852*</td>
<td>0.493*</td>
<td>0.498*</td>
</tr>
<tr>
<td></td>
<td>(0.713)</td>
<td>(0.345)</td>
<td>(0.140)</td>
<td>(0.156)</td>
</tr>
<tr>
<td>Distance from judiciary</td>
<td>1.390*</td>
<td>0.332</td>
<td>0.230</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.314)</td>
<td>(0.215)</td>
<td>(0.197)</td>
<td></td>
</tr>
<tr>
<td>President Fixed Effects</td>
<td>✓</td>
<td>✓</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Controls</td>
<td>✓</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Observations</td>
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<td>64</td>
<td>64</td>
<td>64</td>
</tr>
</tbody>
</table>

Robust standard errors clustered on president are in parentheses. * p < 0.05. Dependent variable is the number of significant executive actions in year $t$. Control variables include indicators for administration change and war.
### C.4 Alternative Specifications for Judicial-Presidential Conflict

#### Table C.5: Judicial Conflict and Unilateral Action

<table>
<thead>
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<th>(1)</th>
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</tr>
</thead>
<tbody>
<tr>
<td>Distance from DC circuit</td>
<td>0.270</td>
<td>0.297</td>
<td>-0.030</td>
<td>-0.039</td>
</tr>
<tr>
<td></td>
<td>(0.402)</td>
<td>(0.395)</td>
<td>(0.091)</td>
<td>(0.097)</td>
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<tr>
<td>Distance from legislature</td>
<td>0.871</td>
<td>0.344</td>
<td>0.515*</td>
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<tr>
<td></td>
<td>(0.647)</td>
<td>(0.187)</td>
<td>(0.255)</td>
<td></td>
</tr>
<tr>
<td>President Fixed Effects</td>
<td>✓ ✓</td>
<td>✓ ✓</td>
<td></td>
<td></td>
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<tr>
<td>Controls</td>
<td>✓</td>
<td>✓</td>
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</tbody>
</table>

Robust standard errors clustered on president are in parentheses. *p < 0.05. Dependent variable is the number of significant executive actions in year $t$. Control variables include indicators for administration change and war.

#### Table C.6: Judicial Conflict and Unilateral Action

<table>
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<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Distance from median circuit</td>
<td>0.070</td>
<td>-0.103</td>
<td>-0.059</td>
<td>-0.292</td>
</tr>
<tr>
<td></td>
<td>(0.921)</td>
<td>(0.880)</td>
<td>(0.317)</td>
<td>(0.219)</td>
</tr>
<tr>
<td>Distance from legislature</td>
<td>-0.962</td>
<td>0.439*</td>
<td>0.579*</td>
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</tr>
<tr>
<td></td>
<td>(1.023)</td>
<td>(0.151)</td>
<td>(0.286)</td>
<td></td>
</tr>
<tr>
<td>President Fixed Effects</td>
<td>✓ ✓</td>
<td>✓ ✓</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Controls</td>
<td>✓</td>
<td>✓</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>36</td>
<td>36</td>
<td>36</td>
<td>36</td>
</tr>
</tbody>
</table>

Robust standard errors clustered on president are in parentheses. *p < 0.05. Dependent variable is the number of significant executive actions in year $t$. Control variables include indicators for administration change and war.