

Congressional Investigations and the Electoral Connection*

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We demonstrate that a direct “electoral connection” with voters motivates members of Congress to more vigorously investigate the executive branch during divided government. Our strategy for estimating the effect of the electoral connection is to leverage the enactment of 17th Amendment—which influenced the electoral mechanism for senators but not for members of the House of Representatives. This plausibly exogenous institutional variation allows us to isolate the effect of the electoral connection from other possible historical influences—such as the growth of the administrative state or the rise of political progressivism. We find that the 17th Amendment dramatically increased the Senate’s propensity to investigate during divided party control. Importantly, we also find little evidence of such an increase in the House. Our findings support the contemporary claim that congressional investigations are political tool motivated by the desire to discredit the opposition and reap individual electoral gains.

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1 Introduction

On April 19, 1922, the Harding Administration made public its decision to lease federal oil reserves in California and Wyoming.¹ Officials within the Department of Interior—the administrative agency responsible for negotiating the deal—characterized it as a boon for both the Navy and the federal government. Some legislators were not convinced. Just over one week after the announcement, Senator Robert La Follette (R-WI) blasted the deal and condemned the the Department of Interior as the “sluiceway for 90 percent of the corruption in this country.”² La Follette went on to call for a full congressional investigation of how the contracts were awarded. In 1924, Senator Thomas J. Walsh (D-MT) led a Senate investigation which uncovered bribery and corruption in executive departments run by Republican appointees. When the extent of wrongdoing became clear, *The New York Times* ran a full page story lauding Walsh’s efforts.³ That same year, Walsh handily defeated his Republican opponent to win re-election. The revelations uncovered by his committee comprise what is now remembered as the “Teapot Dome” scandal.

This well-known case helps illustrate existing explanations for why Congress investigates the executive branch. Though investigations are not among the enumerated powers of the Constitution, existing studies argue they serve the individual electoral goals of legislators (Parker and Dull, 2009; Kriner and Schwartz, 2008), allow Congress to check the authority of the executive branch (Kriner and Schickler, 2014, 2016), and provide legislators with an opportunity to collect and synthesize policy-relevant information (McGrath, 2013). While these studies find consistent evidence that Congress investigates more often during divided government, they have one common limitation. Namely, it is difficult to adjudicate between the theoretical mechanisms underlying this empirical relationship, because both political opportunism and policy disagreement could plausibly drive members of Congress to investigate more frequently during divided government.

We address this limitation by demonstrating that individual electoral incentives have an independent influence on Congress’ decision to investigate. Our strategy for estimating the impact of the electoral connection on investigations is to compare the propensity of the House and Senate investigations of the executive branch before and after enactment of the 17th Amendment in 1913. Before enactment, senators were elected *indirectly*—they were chosen by state legislators. After enactment, they were elected “*directly*,” by “the people” themselves. Members of the House of

Representatives, on the other hand, have always been constitutionally responsible to citizen voters. This difference is critical because it allows us to compare over time differences in the behavior of directly and indirectly electorally responsible senators with those in the House. This comparison allows us to isolate the effect of direct election from the myriad other factors in this historical period that might plausibly drive changes in investigation patterns.⁴

We argue that the 17th amendment generated electoral incentives that motivate senators to more frequently investigate the executive branch during periods of divided government. Before enactment, senators served a small subset of well-informed political elites. Reelection hinged on a senator's ability to provide goods and services to powerful local constituencies and to represent the preferences of state politicians. After enactment, senators had to acquire the support of less educated, less motivated citizen voters with limited ability to properly monitor senators' behavior. Thus, by changing a senator's electoral constituency, the 17th amendment enhanced the political appeal of legislative tactics designed specifically to publicize the incompetence and malfeasance of a presidential administration. For opposition senators, investigations became a tool for creating and capitalizing on public dissatisfaction with the president's party.

To substantiate the central claim of this paper, we demonstrate that the 17th Amendment dramatically increased the Senate's propensity to investigate the executive during divided government, but that no comparable increase occurred in the House of Representatives. These findings support a body of research illustrating how the 17th Amendment directly influenced legislative behavior (Crook and Hibbing, 1997; Bernhard and Sala, 2006; Meinke, 2008; Patty, 2008; Gailmard and Jenkins, 2009).⁵ Moreover, our results imply the electoral connection generates incentives to politicize investigations—a finding which has implications for understanding the efficacy of legislative oversight of the executive branch.

2 Investigations and Electoral Constituencies

Benchmark models of political agency identify two key theoretical components of the relationship between public officials and their constituencies. The voters' ability to sanction the official, and the voters' capacity to observe (or monitor) the official's behavior (e.g. Ferejohn, 1986; Besley, 2006). In the case of the 17th Amendment, we argue that the latter component was dramatically

altered after enactment. By shifting to a principal with limited attentiveness—and thus, monitoring capacity—the shift to direct popular election of senators created incentives for members to send overt signals to improve their re-election chances. Congressional investigations are one such signal, given they often garner media attention and portray the opposition party as corrupt or incompetent. Moreover, investigations also allow individual members of Congress to cultivate favorable individual reputations. This behavior is especially attractive under periods of divided government—since the target administration is a political opponent, and an investigation can be launched “unilaterally” by the House or Senate.

Indirect election offered senators comparatively fewer electoral incentives for investigating the executive branch. Prior to 1913, a senator’s electoral constituency included business interests, active party members, and state-level elected officials. Senate candidates frequently personified political compromises reached by intra-party factions, each supporting a different financial sector. An individual senator’s electoral constituency, therefore, expected her to work on behalf of these elite interests. Additionally, because the two major party organizations within the states would often factionalize around particular business interests, they viewed senatorial contests as “opportunities to exert control over statewide politics more broadly” (Schiller, Stewart, and Xiong, 2013). With clear material and power interests at stake, local political elites reliably monitored a senator’s behavior. Recognizing that state legislators frequently “sold their vote to the highest bidder,” senators also understood that reelection hinged on the satisfaction of clearly identifiable elite interests. As long as the local party and financial elites were satisfied, a senator could assume that her electoral prospects were not at risk. In short, congressional investigations were not necessary to communicate the message that senators served the interests of their principals.

Direct election made members of the upper chamber constitutionally responsible to a public that is more diverse and less informed than state-level elites. As a consequence, overt, easily digestible political actions—like congressional investigations—became an attractive way to signal voters with fewer incentives to monitor and reward the work of individual senators. Gailmard and Jenkins (2009) find empirical support for this basic notion in Senate voting records. Following enactment of the 17th amendment, voting patterns in the Senate shifted such that individual legislators reflected the electoral outcomes of the presidential candidates within their respective states. Accountability to “political novices,” in other words, led to a discrete change in political strategy—with senators

turning roll-call votes into a signal about their attitudes toward the party’s national candidate.

Investigations provide a similar tool for winning the support of low-information voters. More specifically, investigations are electorally useful because they allow legislators who are not in the president’s party to publicize corruption or incompetence. As Kriner and Schickler (2014) show, publicity of this kind has the potential to erode popular support for the president and his party. Moreover, the mass public attributes the actions of bureaucratic agencies to the president in power (Ruder, 2014).⁶ These notions appear to play out among high-profile investigations in the 19th and earlier 20th century. In 1872, for example, members of Congress began an investigation into bribery and corruption charges related to construction of the transcontinental railroad. The “Credit Mobilier” scandal of 1872-1873, publicized by the *New York Sun*, did significant damage to the Republican party and cost Vice President Schuyler Colfax his position as President Ulysses Grant’s running mate.⁷ The accusations hurled at President Grant by some members of Congress even led the *New York Times* to write an editorial defending his tenure. “General Grant has not been, it is well known, an ideal statesman,” argued the editors, “but [...] our regard for the character and services of the President is stirred into enthusiasm by our keen sense of the scandalous injustice of the aspersions which he has too much self-respect and reticence to deny.”⁸

Investigations also provide individual legislators with the opportunity to cultivate favorable reputations and personal political appeal. Senator Thomas Walsh, discussed above, won himself national headlines for his role in the Teapot Dome Investigation. Similarly, the so-called “Pujo Committee” of 1912-1913 earned Representative Arsene Pujo (D-LA) widespread public acclaim for his investigation of the “Money Trust”—a group of Wall Street bankers who wielded significant control over the nation’s financial system. Louis Brandeis publicized Pujo’s role in a series of articles published in *Harper’s Weekly* (Brandeis, 1914). Harry Truman used a series of investigations into war time expenditures by the Roosevelt Administration to cultivate a national reputation and win himself a spot on the national ballot (Schickler, 2007; Kriner and Schickler, 2016). In short, by developing an individual reputation for “honest dealing,” or a commitment to “good government,” a legislator who plays a public role in investigations can build political support among voters in her home state.

Our analysis focuses in particular on investigations of alleged wrong-doing within executive branch departments because they offer an easily understood, overt signal to voters.⁹ In particular,

investigations of this kind allow senators an opportunity to signal their opposition to, or even embarrass an opposition party president and his party (Cox and McCubbins, 1993; Parker and Dull, 2009). Additionally, because congress can launch an investigation of this kind “unilaterally”—without consent of the president—this tool affords senators a clear opportunity to charge a sitting president with corruption or incompetence. Thus, once they are directly accountable to voters, senators pursuit of politically damaging investigations becomes more attractive when a member of the opposition party controls the White House. Stated differently, we test the following hypothesis:

Electoral Hypothesis: When MCs are directly electorally accountable to voters, divided government will increase a chamber’s propensity to investigate. When no direct electoral connection exists, there should be no discernible difference between divided and unified government.

In other words, if it is correct that investigations are (in part) a political tool wielded by the opposition, then that tool should only be valuable when senators are directly responsible to voters.¹⁰ Thus, the empirical relationship uncovered by past research on investigations is conditional on the electoral connection—as understood in the postwar era (Mayhew, 1974).¹¹

In sum, the central claim of this paper is that enactment of the 17th amendment provided senators with electoral incentives to investigate the executive branch under divided government. Prior to enactment of the 17th amendment, a senator’s electoral constituency was chiefly concerned with her ability to provide goods and services to the state, and to represent the political preferences of state politicians. After enactment, however, a senator’s electoral success came to also hinge on her ability to cultivate support from a low-information voting public. By changing a senator’s electoral constituency from a small subset of local elites to the entire voting public of a given state, this constitutional reform incentivized legislative tactics designed specifically to publicize the incompetence and malfeasance of the executive branch bureaucracy. These new incentives are a direct consequence of the change from indirect to direct election. Moreover, given that no contemporary change occurred in the House, this historical moment provides a rare opportunity in terms of research design—which we take advantage of in the following sections.

3 Data

Our empirical focus calls for a dataset of congressional investigations conducted by the House and Senate from 1789-1948. We confine our analysis to investigations of alleged wrongdoing, misconduct, or gross inefficiency. To be included, the investigations must be initiated by Congress and target some individual, department, or agency within the executive branch.¹² We conducted keyword searches of published Congressional hearings and reports in the American State Papers and *CIS Index* from 1789-1948.¹³ To be sure that we did not overlook any investigations due to a change in the terminology associated with this form of congressional activity, we also supplemented our analysis with a handful of retrospective raters. Specifically, we collected secondary accounts which dealt exclusively with congressional investigatory activity during the time in question: Weber (1919); Eberling (1928); Dimock (1929); Taylor (1955); Schlesinger and Bruns (1975). A more detailed description of this procedure appears in Appendix A. Collectively, systematic searches of congressional publications, along with these retrospective raters, yielded 500 congressional investigations from 1789-1948.¹⁴

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The quantity of investigations is far lower in the historical sweep considered, compared with existing studies of the post-war era.¹⁵ Parker and Dull (2009), for example, find 1,015 investigations during the post-World War II period. We offer two explanations for the difference. First and foremost, our analysis includes the time period during which the instrument itself was developing. Eberling's (1928) history, for instance, notes that though the investigatory power was prevalent at the time of the Founding in American state legislatures, it is not among Congress's enumerated powers. As a result, whether Congress could compel testimony and punish offenders remained a contestable institutional question into the 1830s. Second, the scope and scale of administrative activity on the part the federal government is meager compared with the post-war period.¹⁶ Thus, the *supply* of potential wrongdoing may have increased over time.

4 Empirical Analyses

In order to test the Electoral hypothesis, we perform two sets of analyses. First, we analyze the total number of investigations in each Chamber, per Congress. Most existing studies adopt this approach (Mayhew, 1991; Kriner and Schwartz, 2008; Parker and Dull, 2009). Second, we disaggregate the investigations by major department or agency. Here, we look into what influences the probability a given department will be investigated in any given year.¹⁷ The advantage of disaggregating the investigations by department is that it allows us to account for unit heterogeneity that may otherwise bias the total count of investigations. This is particularly important for our historical period and theoretical focus. Merit systems were adopted in different departments, at different rates, and the investigation of some departments may be more electorally beneficial than others during divided government—based upon unobserved variance in public association with the President.

To address an inherent limitation of the first and second approaches, we supplement these analyses with Bayesian change point detection. As we discuss in Section 2, we expect that the adoption of the 17th Amendment resulted in a change in Senate behavior. This requires an imperfect (but theoretically driven) specification of a “breakpoint”—a change in the relationship between divided government and investigations—at the 64th Congress. The method developed by Park (2010) and implemented by Martin, Quinn, and Park (2015) allows us to estimate the probability of a given “regime” at each Congress. Modeling partitions in the time series provides a descriptive compliment to our initial analyses. In effect, it provides an assessment of when the expected shift in investigatory activity is likely to have occurred. It also allows us to answer substantively interesting questions, like whether the initiation of direct primaries or the general anticipation of institutional change influenced the behavior of senators. We present these results as supplemental material in Appendix B, given that they largely corroborate the findings in the following sections.

COMP: Place Table 1 about here

As Figure 1 implies, mere presentation of empirical models of Senate behavior cannot properly substantiate the effect of the 17th Amendment. The historical sweep of our analysis is significant, and the political environment prevailing in 1918 looked much different from the political

environment of 1868 or 1804. National administrative expansion likely increased opportunities for congressional investigations. Moreover, the Progressive movement explicitly called for legislators to facilitate accountability in the executive branch. Absent a plausible comparative case, either of these developments might provide a potential alternative explanation for any observed change.

Table 1 summarizes our strategy to address this potential problem. Here, the key institutional change is the adoption of the 17th Amendment, which resulted in a direct electoral connection between voters and Senators. As summarized in Table 1, our expectation is that the adoption of the 17th Amendment should increase the Senate’s incentive to investigate during divided government. The House’s propensity to investigate during divided government, however, should be present both before and after the introduction. Moreover, we should see no substantial increase in the House’s propensity to investigate during divided government after the amendment. In this way, the House acts as a useful placebo test—it will be influenced by other well known developments in American politics in the early 19th century, without being directly subject to the amendment.

4.1 Investigations by Congress

Our initial empirical strategy is to estimate a series of time-series negative binomial regressions—relying on a comparison of patterns in the House and the Senate.¹⁸ The unit of analysis is an individual Congress. While our key assumption is that the Senate and House provide a useful comparison, the critical challenge for estimation is that the “assignment” of our key independent variable forces us to compare dissimilar time periods within chambers. In order to render Congresses within chambers plausibly comparable, we estimate the following model

$$y^* = \beta_0 + \beta_1 \textit{DividedGov} + \beta_2 \textit{17thAmend} + \beta_3 \textit{DividedGov} * \textit{17thAmend} + \delta \mathbf{X} + \alpha t \quad (1)$$

where αt is a cubic polynomial designed to account for problematic trends in the number of investigations over time, and $\delta \mathbf{X}$ represents a vector of control variables discussed later in this section. Though there are a variety of possible approaches for accounting for the explosive trends apparent in Figure 1, we include a cubic polynomial in y^* for two reasons. First, work on structural means of modeling latent time-series processes in count variables is ongoing, such that computational implementation of such models has yet to show regression parameters can be estimated consistently

(e.g. Fried et al., 2015; Liboschik, Fokianos, and Fried, 2015). Second, incorporation of a time series “correction” within y^* means that readily interpretable point estimates can be estimated directly.¹⁹

The dependent variables are the total number of investigations by chamber and Congress. Our key independent variable is an indicator for divided government, coded “1” if different parties control the chamber and the presidency, and “0” otherwise (*DividedGov*). One apparent concern is variation in the process that generated divided government in the 19th century. As Engstrom and Kernell (2005) have shown, the frequency of divided government in the 19th century was held “artificially” low due to the impact of state electoral laws. While this does not alter our expectations regarding the process that generates investigations, it does mean that our key statistical enterprise—comparing periods of divided and unified government—is more difficult. There are simply fewer observations of divided government to use a comparison group, as shown in Table 2. The table also highlights another potential concern—namely, that the control (1st-63rd) and treatment (63rd-80th) periods are unbalanced in terms of divided government and Congresses, more generally. To address this, we also include results that begin with the 39th Congress (1865). In general, we have found the results are not sensitive to arbitrarily dropping earlier Congresses.

One additional concern is that the Senate divided government Congresses after the 17th Amendment occur following the First and Second World War. This raises the possibility that any increase in investigations could be attributable to the war effort. After probing this possibility further, however, we do not believe this to be the case. Importantly, the House divided government congresses overlap with the Senate’s during this period. If the First and Second World Wars drove an increase in investigations, we would likely observe this shift in both the House and the Senate. As we go on to show, there is little evidence of such an increase in the House. We also examined the list of investigations that took place in both Congresses for evidence they were predominantly war-related. Of the 20 investigations in the 66th Congress, only 4 appear to be war-related. The House conducted investigations of the War Risk Insurance Bureau²⁰, the U.S. Shipping Board²¹, and a general investigation of war expenditures²²; whereas the Senate undertook an investigation of the Navy.²³ Of 44 investigations in the 80th Congress, 9 appear to be directly related to the war effort. For example, the Senate investigated the Navy for its administration of acquired islands²⁴, as well as the War Department for its disposal of surplus property.²⁵ It is worth noting that a majority (9 of 13) of these war-related investigations were initiated by the House. Finally, we also

re-estimated the models of Senate investigations of agencies presented in Section 4.2.1 after excluding the War Department—since these investigations were most likely to be related to the war effort. The results are robust to this specification. Thus, after careful examination of these Congresses, we are confident that the results we present in Sections 4.1.1 and 4.2.1 are not be driven by post-world war investigatory activity.

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To estimate the effect of the 17th amendment, we include an indicator which takes a value of “1” beginning in the 64th Congress (*17thAmend*). Specification of this point poses a challenge for the analysis. The first Senators to be elected (or re-elected) under the new system campaigned in 1914. We code the first year of this system as 1915, but incumbents may have adjusted their behavior prior to the first election. As discussed earlier, in the Appendix we allow the data to speak for itself, in order to verify this breakpoint and examine the implications of possible alternatives. The data suggest the predicted transition in the Senate may have occurred slightly earlier—suggesting that the advent of direct primaries may have contributed to a gradual transition (rather than a clean breakpoint).²⁶ However, in general, the results of this strategy strongly support those in Sections 5.1 and 5.2.

We also include a vector of control variables for two reasons: (1) to account for changes in political and economic circumstances faced by any given Congress, (2) to be sure none of these variables correlated with divided government or the 17th Amendment.²⁷ We include the total annual expenditures (in billions) of the federal government in order to control for the size of bureaucracy, as well as a measure of bureaucratic professionalization—the proportion of federal employees covered by civil service protections.²⁸ Since these variables both exhibit trends over time, both were differenced until stationarity was achieved (Box, Jenkins, and Reinsel, 1994). As we discussed earlier, to account for the fact that congressional investigations may be the direct result of wartime inquiries into the conduct of the military, we include a dummy variable coded “1” during the War of 1812, the Mexican American War, the Civil War, the Spanish American War, World War I, and World War II. We also include an indicator variable for the Reconstruction period.

4.1.1 Results: Total Investigations

Overall, the results strongly support the Electoral Hypothesis. Prior to the enactment of the 17th Amendment, divided government is not associated with an increase in investigatory activity. However, after enactment, the Senate engaged in, on average, 2.4 additional investigations of the Executive Branch during divided government. In the House, divided government is associated with more investigations across the complete time series. This initial evidence suggests that the change in electoral institutions resulted in the behavioral change theories of investigations would predict.

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We present the estimation results in Tables 3 (House) and 4 (Senate). The primary results are robust to a variety of different specifications that include various control variables. Moreover, as models 4 and 5 show, the results remain when nearly half the dataset is dropped. In the House, as expected, the coefficient for *DividedGov* is positive and statistically distinguishable from zero, whereas the key interaction with the *17thAmend*, is not. As we report in Table 5, this results in a substantive effect in the two periods that is not distinguishable. When the presidency is controlled by the opposite party, the House tends to engage in an additional 2.5 (pre-17th Amend.) or 3.3 (post-17th Amend.) investigations. Again, the difference is not statistically distinguishable. In contrast, in the Senate, the coefficient for *DividedGov* is close to zero, and the interaction is positive and significant. Whereas the pre-17th Amendment period saw no consistent change in investigatory activity under divided government, in the post-17th period, the Senate tended to carry out an additional 2.4 investigations. Importantly, the increase in the magnitude of the effect of divided government only appears in the Senate.²⁹ Thus, other potential “treatments” that have no plausible heterogeneous effect among the chambers—such as the rise of progressivism or the growth of the administrative state—cannot explain the observed difference.

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4.2 Investigations by Agency

The unit of analysis for our second set of results is Agency-Congress. Given that executive departments and agencies are nested within each other, they must be analyzed at a level that provides stable and meaningful units. This means that investigations specific to subordinate agencies are coded as investigations of the larger department. For instance, investigations of the Freedman’s Bureau and Isthmian Canal Commission were coded as an investigations of the War Department, since the Secretary of War was directly responsible for their administration. Investigations of the United States Reclamation Service and Bureau of Pensions, on the other hand, are coded as investigations of the Interior Department. Direct investigations of the President include investigations of officials who are directly appointed by and responsible to the President and are not part of a regular cabinet department (e.g. territory secretaries). Though most of our units are cabinet departments (or eventually became cabinet departments), we also include independent agencies and commissions. Though we argue it is appropriate to include them as comparable units, they account for a sufficiently small number of observations that omitting them does not substantively alter the results we present later.

Finally, some investigations do not explicitly target a single department. Instead, they are described as general investigations of the executive branch. For example, in 1822, the House led an investigation into the expenditures and accounting procedures of all executive departments. These instances pose a challenge because these “general” investigations may have had an intended target department. Thus, though these investigations are included in Section 5.1, they are omitted from the following analysis. We present a summary of our agency units, the Congresses that were included for our analyses, and the number of investigating Congresses by chamber in Table 6. Overall, not surprisingly, the Department of War is the most frequently investigated in the dataset—it is one of the oldest (second only to State), the most expensive, and has been treated with general suspicion by political leaders since the Founding.³⁰ Moreover, wartime actions of the department tend to invite Congressional inquiry. Given its vintage, the State Department was investigated with the least regularity. Those with knowledge of administrative history will not be surprised by the high frequency of investigations of the Interior Department, given numerous scandals involving corruption and fraud of pensions, public lands, and Native American accounts

(Carpenter, 2001).

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The dependent variable takes a value of “1” when a chamber initiates an investigation against a given agency. Accordingly, we estimate panel logistic regression models, which take the following general form

$$y^* = \beta_0 + \beta_1 \text{DividedGov}_t + \beta_2 \text{17thAmend}_t + \beta_3 \text{DividedGov}_t * \text{17thAmend}_t + \delta \mathbf{X} + \alpha(t - t_{Y=1}) + \phi_i \tag{2}$$

where $(t - t_{Y=1})$ is the number of Congresses since the agency was last investigated and ϕ_i denotes agency fixed-effects. Accordingly, we model variation within agencies—accounting for time-invariant differences among agencies that may explain variation in investigations. Note also, to remain unbiased, this model assumes that selection out of sample is unrelated to investigatory activity (Wooldridge, 2002). Since all but one agency remains operative at the end of the time series, we do not believe our unbalanced panels pose an inferential problem.³¹ We account for time since last initiated investigation to account for the fact that investigations may be carried out across multiple Congresses, and that—all else equal—there may be less utility in investigating the same agency, Congress after Congress.³²

As in Section 5.1, \mathbf{X} denotes a vector control variables. These controls are identical to the previous models. Ideally, we would include expenditures and rate of civil service coverage in each agency. However, existing data sources for expenditures only cover a few major departments. In the case of the civil service, the best historical resource—the annual report of the Civil Service Commission includes inconsistent information over the time period in question, which would result in a non-trivial amount of missing data. In lieu of these options, the individual data points reside in agency archival records. The control variables are included for the sake of probing the sensitivity of our results to alternative specifications. Thus, we have forgone the extensive data collection that would be required for agency-level variables. As in the previous analysis, in expectation, $\beta_1 > 0$ for House investigations, whereas $\beta_1 \approx 0$ and $\beta_3 > 0$ for Senate.

4.2.1 Results: Probability of Investigation

We present our second set of results in Tables 7 and 8. As in the previous analyses, the estimates support our basic expectations. In the House, divided government increases the probability of an investigation across the complete time series. In the Senate, on the other hand, this empirical regularity only appears after the enactment of the 17th Amendment. Importantly, structuring the data to account for variation in the “target” of investigations supports our initial findings in Section 5.1. That is, whether we look at the raw count of investigations or investigations targeted at specific agencies, we still find evidence that the 17th Amendment altered the Senate’s propensity to investigate under divided government.

As anticipated, *DividedGov* is only positive and significant for House investigations. In addition, the key interaction between *DividedGov* and the *17thAmend.* is positive and significant in the Senate. As Table 9 indicates, this means that prior to the 17th Amendment, the probability of a House investigation for a given agency increased by 10 percentage points under divided government. The marginal change in probability is near zero for Senate investigations. After the 17th Amendment, divided government is associated with a 20 percentage point increase in the probability of a Senate investigation. Unlike the previous analysis, there is some evidence of a corresponding increase in the magnitude of the effect of divided government in the House. In the post-amendment period, the House was 16 percentage points more likely to investigate when the President was a member of the minority party in the House. Though this raises the possibility that the difference in the Senate may be the result of other developments that influenced both chambers—the magnitude of the effect in the Senate is far greater. Moreover, when we subset the model to Congresses after the Civil War in Table 9, the significant difference in the House is weaker in magnitude—and in some cases, is entirely absent. Thus, those there is some evidence of other possible explanations, these cannot adequately account for the dramatic change in the Senate.

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5 Conclusion

Congressional investigations have the potential to generate long-lasting political change. The Watergate investigation forced President Richard Nixon from office and contributed to a significant legislative reform agenda; Senator Harry Truman’s investigation of the Roosevelt Administration helped secure him the vice-presidential nomination; the Teapot Dome investigation encouraged the Supreme Court to ensure that Congress had the authority to compel testimony from witnesses. The historical significance of congressional investigations compels political scientists to explain when they are more or less likely to be initiated, as well as how they allow entrepreneurial legislators to pursue individual ambitions.

On this front, we have made two primary contributions. First, we have demonstrated that the contemporary theories of investigations find empirical support in the 1st through 80th Congresses—which were hitherto unexamined by previous work. Second, by considering this extended time series, we were able to leverage an important institutional change in elections to test an underlying mechanism of existing theory—namely, that Congressional investigations are (in part) motivated by the desire for electoral gain. Specifically, we find that the Senate’s probability of investigating the executive branch during divided government increases significantly in the years following enactment of the 17th Amendment. These results are robust to alternative model specifications, data structures, and to dropping nearly half of the observations in any given analysis.

The analysis demonstrates that constitutional reform led to a meaningful change in legislator behavior. This finding is particularly striking, given that past research has found that the Senate exhibits less investigatory activity during divided government, compared with the House. Moreover, it demonstrates that a direct connection with voters—beyond policy disagreement or informational concerns—is an important component of legislators’ decision-making. Put differently, oversight of the Executive branch—a critical function of Congress—is directly tied to partisanship and electoral incentives.

These findings raise a variety of avenues for future research. While existing work on investigations has focused on explaining their *frequency*—it seems readily apparent that a shift to considering their relative *quality* or benefit to the public is warranted.³³ Suppose, for instance, that all investigations are equally valuable—in that they uncover information that can be put to use to serve

constituent's interests. In that case, then the finding that divided government increases investigatory activity would suggest that divided government is beneficial, all else equal. But if the quality of investigations under divided government is systematically different—such that the concerns about frivolous hearings and political grandstanding are well warranted—then divided government may fundamentally undermine a critical function of Congress. Our findings suggests this might be the case—in that, the electoral connection appears to be a necessary condition for this pattern. But absent a direct consideration of the quality of investigations, we cannot begin to determine whether these incentives undermine effective governance.

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Appendix: Collecting Investigations

To reiterate, we searched for investigations of alleged wrongdoing, misconduct, or gross inefficiency that were initiated by Congress and target some individual, department, or agency within the executive branch. This entailed two steps. As we note in Section 3, we conducted keyword searches of published Congressional hearings and reports in the American State Papers and *CIS Index* from 1789-1948 accessed via Proquest Congressional. In selecting keywords, we sought to be as inclusive as possible to avoid missing observations that might result in changes in language use over time. Thus, our search for congressional publications included the terms: “investigation” OR “report”, AND “administration” OR “executive” OR “agency” OR “department.” The first term references the subject—hearings of an investigation, or a report produced by an investigation. The second references the target. This yielded 71,056 search results. The vast majority of search results were either redundant (e.g. both the hearings and report produced by the same investigation) or did not meet our standard for inclusion. Given the number of search results, we are fairly confident

that our keyword search did not exclude potentially relevant documents. In total, this yielded 373 investigations.

We also consulted additional sources for investigations (Weber, 1919; Eberling, 1928; Dimock, 1929; Taylor, 1955; Schlesinger and Bruns, 1975). The purpose of this second “sweep” was to check the validity of our document search and address potential limitations with ProQuest’s database. More specifically, the completeness of ProQuest’s coverage of the American State Papers, hearings, reports and the Congressional Serial Set is a proprietary “black box” for researchers. To supplement its documents, we read and logged investigations from topic-specific publications using the same standard for inclusion. This yielded 169 investigations. Many cases (42) appeared in both the document search and the secondary sources, resulting in 500 unique investigations.

Appendix B: Structural Change in the Senate

Though the previous empirical strategies largely confirm our expectations about investigatory activity, they share one inherent limitation. Each requires *ex ante* specification of when the electoral connection became operative in the Senate. We chose the 64th Congress because it included first full cohort of Senators elected directly by voters. However, there are plausible arguments for both earlier and later breakpoints. For instance, the predicted relationship might not appear until the entire Senate was elected by popular vote. Alternatively, since the 17th Amendment gained serious political support in the mid-1890s, and the first popular elections took place in 1908, Senators may have rationally anticipated the change in principal. To determine whether these scenarios have any support in our data, we implement a series of change point models of investigatory activity in the Senate. Change point detection has seen a variety of applications across natural and social sciences as well as in political science (Spirling, 2007; Park, 2010, 2011, 2012; Freeman and Jackson, 2012). More generally, as Wawro and Katznelson (2014) emphasize, privileging over-time flexibility in parameter estimates allows researchers to model periodicity—often a key component of qualitative, historical approaches.

We implement a Poisson regression with multiple changepoints (Park, 2010; Martin, Quinn, and Park, 2015).³⁴ As in Section 5.1, the dependent variable is the total number of Senate investigations in a given Congress. Again, our key regressor is *DividedGov*. In Table B1 and Figure B0, the results

include control variables for wartime Congresses and the Reconstruction Era, but the results do not substantively change if they are excluded. Note, the model requires *a priori* specification of the number of structural breaks.³⁵ Following Park (2010), we select the optimal number of changepoints by comparing the marginal likelihood of each model. Note that we are largely agnostic to the precise number of regimes. Theoretically, there should be at least two, but we have no expectations associated with the earlier time series. More breaks might be present at other watershed moments in American politics—like the Civil War, the rise of Jacksonian Democracy, or the introduction of the “Australian” ballot. Each of these events occur prior to the particular moment we are interested in—and so, each falls outside our substantive focus.

To recap, we have two basic predictions. First, we expect that a parameter “regime change” should occur at or around the introduction of the 17th Amendment. Put simply, we should see behavioral changes in the way the Senate investigates under divided government over time—and, in particular, this change should be associated with the 17th Amendment. Second, we expect that the change in regime to follow the basic predictions outlined in Table 1, and borne out in every analysis thus far: before the shift, there should be no statistical association between divided government and investigations; after the shift, they should be positively associated.

Investigation Regime Change

The estimation results provide support for earlier models, while raising a variety of possibilities about political development in the Senate. As expected, across all models, the “terminal” regime includes a *DividedGov* coefficient that is positive and likely different from zero. We report these estimates in Table B1. Regardless of the number of breaks, the 95 percent credible region is positive. A comparison of Bayes factors (presented in Table B2 suggests that a two-break (three regime) model provides the best fit of the data. Moreover, each of the three and four break models fit the data better than a single break model. Again, we were largely agnostic as to the precise number of breaks in the earlier Congresses. But each of our multiple break models provides a similar substantive story. Prior to the last regime in the data, the relationship between divided government and investigations is near zero, afterwards the relationship is positive. This supports our findings in Section 5.1 and 5.2.

The models also reveal interesting results about the timing of this structural change. Specifically,

the posterior probability of the divided government regime suggests that the change may have occurred earlier than the 64th Congress. The 50 percent threshold is exceeded following the 55th Congress in all but one of the models. After the 60th Congress, the probability of this regime is over 90 percent; after the 64th, the probability is one. Thus, the model suggests that the divided government regime fits the data best after the 64th Congress, but there is a reasonably high probability it operated prior to the enactment of the 17th Amendment. In other words, there is evidence that the senatorial behavior change occurred in advance of the constitutional change.

COMP: Place Table B1 about here

COMP: Place Table B2 about here

Substantively, this suggests that as early as William McKinley’s first term as president, the Senate began investigating the Executive branch in a manner consistent with what a direct electoral link would predict. There are a variety of historical explanations for why the shift might have occurred earlier. As Schiller and Stewart (2014) note, the House began passing versions of the 17th Amendment in 1894 (53rd Congress), amid turmoil in the state selection processes that left some seats vacant. Moreover, the first direct primaries were enacted in 1901 (during the 57th Congress). By the time of the enactment of the 17th Amendment, these primaries were widespread. Lapinski (2000), for example, leverages the 29 states who had put a direct primary in place by 1912. We argue that—rather than posing a challenge to our earlier results—this finding illustrates matters of historical record about the transition to popular elections. In the course of this paper, we have called the 17th Amendment “plausibly exogenous” for the purposes of our analysis. But, as these results reminds us, political developments do not “drop from the sky.” Institutional change is endogenous to the preferences of legislators, and deft politicians may anticipate these changes and adjust accordingly.

COMP: Place Figure B0 about here

Notes

¹“Oil Contracts Let in Naval Reserves,” *The New York Times*, April 19, 1922: 36.

²“Naval Oil Leases By Secretary Fall To Be Investigated,” *The New York Times*, April 29, 1922: 1.

³“Walsh Arraigns Teapot Dome Looters,” *The New York Times*, February 10, 1924: 20.

⁴Previous studies that use a chambers as comparable cases have described this research design as a “control series regression discontinuity” (Cox and McCubbins, 2005) and a “natural experiment” (Gailmard and Jenkins, 2009). We stop short of labeling our analysis as the latter, because we believe chamber comparison does not strictly adhere to the assumptions of the Rubin Causal Model (Holland, 1986). For example, though we believe the House and Senate constitute relevant comparison units (e.g. Sekhon and Titiunik, 2012) and that the 17th amendment is plausibly exogenous, the presence of national political parties, media publications of investigations, and the frequent interaction of legislators suggests a possible violation of the non-interference assumption (Dunning, 2008). Moreover, as we go on to show, the data suggest that the “treatment” of interest (direct electoral connection) may have been anticipated by forward-looking senators. For those reasons, we simply argue the House provides a useful comparison that allows us to rule out many alternative explanations for any observed change in the Senate, which provides support for the electoral connection as the theoretical mechanism for investigatory activity.

⁵See also Wawro and Schickler (2006), who find no effect for the 17th Amendment.

⁶Note, however, Ruder (2014) finds that *how* media sources choose to cover these agencies influences public attribution.

⁷“The King Of Frauds: How the Credit Mobilier Bought Its Way Through Congress,” *The New York Sun*, September 4, 1872: 1.

⁸“The Swarming of Falsehoods,” *The New York Times*, September 19, 1872: 5.

⁹A description of how we went about identifying these investigations in particular follows below.

¹⁰It is also worth noting that contemporary accounts noticed this change. Eberling (1928), for example, wrote that “whereas the House was formerly the ‘grand inquest of the Nation and conducted most of the more important investigations, the tables have turned and now the Senate is the grand inquisitor” (272).

¹¹Though Mayhew’s analysis focuses on the behavior of post-war MCs, a body of research (Carson and Jenkins, 2011; Kernell and McDonald, 1999; Katz and Sala, 1996; Theriault, 2003) suggests that this electoral incentive helps to explain legislator behavior during the 19th century as well.

¹²Some of the histories consulted recount internal investigations of representatives accused of taking bribes—or the alleged misconduct of private corporations. Investigations such as these have been excluded. Moreover, our analysis turned up a non-trivial number of investigations into the administration of the District of Columbia, which were also excluded.

¹³Studies that look at the post-war era collect congressional investigations one of two ways: (1) by surveying contemporary accounts (Mayhew, 1991; Kriner and Schwartz, 2008), or (2) by culling the *CIS Index* of congressional hearings (Parker and Dull, 2009; McGrath, 2013). Our temporal sweep presents a set of unique challenges. First, the contemporaneous rater used by past work, the front page of the *New York Times*, has only existed since 1851.

In addition, we are concerned, as Parker and Dull (2009) note, that variation in the “criteria determining when a single investigation will achieve” enough prominence to be consistently mentioned on the front page could produce a biased sample (321). Given its limited scope, and since several major developments in newspaper reporting occurred between 1789-1948, we sought alternate means of gathering historical investigations.

¹⁴Though many observations were mentioned by more than one source, we are reluctant to model a latent variable (“importance” or “significance”) as past work has among lawmaking (Clinton and Lapinski, 2006) and presidential directives (Chiou and Rothenberg, 2014). The theoretical perspective outlined earlier should be sufficiently general to underlie the universe of congressional investigatory activity—as opposed to those which are recounted in histories and reported in newspapers.

¹⁵Note, we have no reason to suspect that our data collection process systematically omits many investigations. In his historical account, Dimock (1929) counted 335 total investigations from 1789-1928, whereas our procedure resulted in 354 during that same period.

¹⁶Recent historical work has challenged the idea that 19th century governance was merely clerical and minor in scope (e.g. Balogh, 2009), but we take the notion that this period is *comparatively* minor to be a stylized fact.

¹⁷This empirical strategy is most akin to McGrath (2013), who analyzes investigations by committee.

¹⁸The total number of investigations exhibits over-dispersion. Additionally, post-estimation tests in the House strongly imply that the conditional means are not equal to the conditional variances. We find some evidence of the same in the Senate, where a likelihood ratio test suggests the nested models are marginally different ($p < 0.1$). Not surprisingly, a poisson regression for the Senate does not substantively differ (aside from a slight *reduction* in standard errors) from the negative binomial estimates we present.

¹⁹Note, the results presented later are not sensitive to this design choice. The results remain substantively unchanged if a quadratic time trend is included, or if a somewhat inappropriate (given the limited dependent variable) ARIMA model is implemented.

²⁰“War Risk Insurance Bureau: Hearings Before the Committee on Expenditures in the Treasury Department,” House of Representatives, 66th Congress, 2nd Session, 1920.

²¹“Inquiry in the Operations of the United States Shipping Board: Hearings Before the Committee on Merchant Marine and Fisheries,” House of Representatives, 66th Congress, 1st Session, 1919.

²²“War Expenditures: Hearings Before the Select Committee on Expenditures in the War Department,” House of Representatives, 66th Congress, 3rd Session, 1921.

²³“Naval Investigations: Hearings Before the Subcommittee of the Committee on Naval Affairs, United States Senate,” 66th Congress, 2nd Session, 1921.

²⁴“Allegations of Navy’s Maladministration of Islands: Hearings Before the Committee on Armed Services,” United States Senate, November 26, 1947.

²⁵“Investigation of Surplus Property Disposal: Hearings Before the Committee on Expenditures in the Executive Departments,” United States Senate, September 27, 1947.

²⁶Thus, our later specification biases *against* rejecting the null.

²⁷Note, only (2) poses a problem for inference. Past work has sought to develop more complete models of what produces investigations—so either (1) or (2) provides sufficient reason for their inclusion.

²⁸These data come from the Census Bureau report, *Historical Statistics of the United States, 1789-1945*.

²⁹Note, the coefficients for each model are not directly comparable. To address this issue, we re-estimated the House and Senate models as a seemingly unrelated regression. A Wald test of the difference between the key House and Senate interactions reveals that the Senate×Divided Gov. interaction is statistically significant ($p < 0.01$) and larger in magnitude, as expected.

³⁰See, for example, the well-known dialogue between Robert Yates and Alexander Hamilton in *Brutus 10* and *Federalist 24*.

³¹The National Recovery Administration remained operative until the 76th Congress—omitting it does not change our results.

³²See also Beck and Katz (1995), who recommend using a smoothed function of time since last event in panel data.

³³See Light (2014) for a thorough evaluation of the quality of dozens of postwar investigations.

³⁴As noted earlier, there is weak evidence that a negative binomial regression is more appropriate for the distribution of Senate investigations. Poisson models of the main results returned very similar results.

³⁵Though other Bayesian changepoint techniques provide a probability distribution of possible changepoints (e.g. Barry and Hartigan, 1993; Wang, Erdman, and Emerson, 2015), Park’s (2010) method is particularly suited to handling limited dependent variables (such as a count of investigations).

6 Figures

Figure 1 – Investigations in the House (red) and Senate (black)

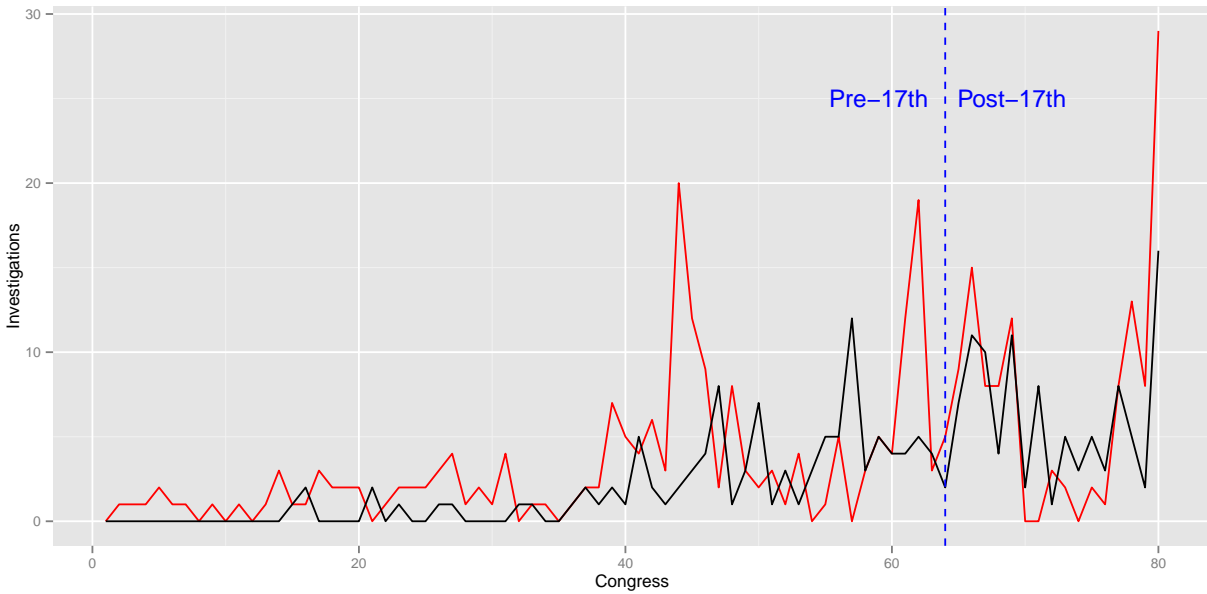
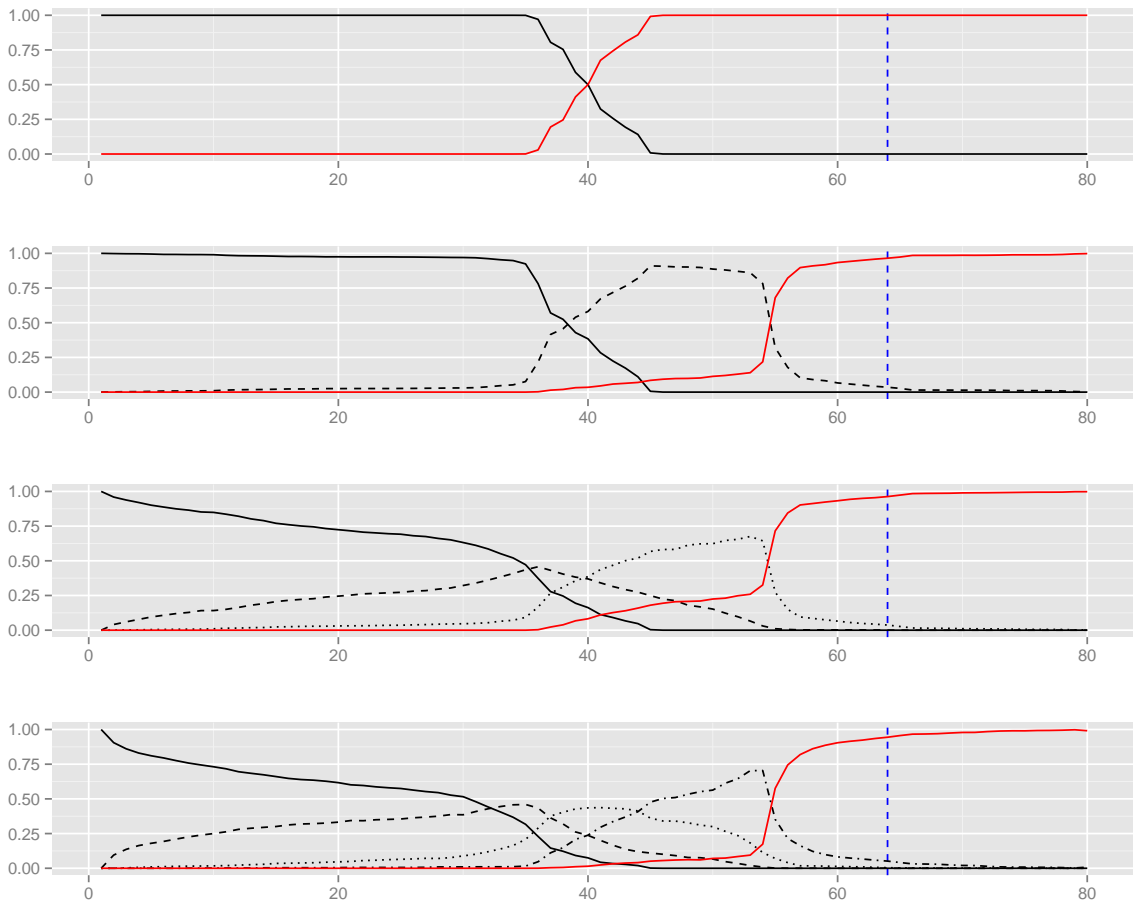


Figure B0 – Changept Detection in the Senate



Note: Plots the posterior probability of a given regime at each Congress for 1-4 pre-specified “breaks”, with the dashed-blue vertical line indicating the first full cohort of directly elected Senators; regimes in which the effect of divided government is likely positive are in red; for precise parameter estimates, see Table B1.

7 Tables

Table 1 – Expected Effect of Divided Gov. on Investigations

	Pre-17th	Post-17th
Senate	≈ 0	+
House	+	+

Table 2 – Divided Government Congresses

	Pre-17th	Post-17th
Senate	19, 23, 31, 32, 39, 40, 46, 49, 50, 54	66, 80
House	3, 4, 28, 30, 31, 32, 34, 36, 39, 40, 44, 45, 46, 48, 52, 54	65, 66, 72, 80

Table 3 – Investigations in the House of Representatives

	1789-1948			1865-1948	
	(1)	(2)	(3)	(4)	(5)
Divided Gov.	0.63** (0.25)	0.59** (0.25)	0.47** (0.23)	0.89** (0.37)	0.95*** (0.34)
17th Amend.	-0.36 (0.52)	-0.24 (0.52)	-0.23 (0.48)	-0.21 (0.69)	0.15 (0.63)
Divided Gov. × 17th Amend.	0.36 (0.50)	0.47 (0.48)	0.62 (0.45)	0.10 (0.58)	0.09 (0.54)
Civil Service Rate _{SD}		-1.92 (2.82)	-1.43 (2.73)		-0.98 (2.90)
Total Expenditures _{FD}		-0.05** (0.02)	-0.06*** (0.02)		-0.00** (0.00)
War			-0.57* (0.31)		-0.77* (0.44)
Reconstruction			0.69** (0.83)		0.89 (0.78)
t	0.06 (0.05)	0.04 (0.06)	0.02 (0.06)	-0.26 (1.16)	1.01 (1.78)
t^2	-0.001 (0.001)	-0.00001 (0.001)	0.0003 (0.001)	0.004 (0.02)	-0.02 (0.03)
t^3	0.00 (0.00)	-0.00 (0.00)	-0.00 (0.00)	-0.00 (0.00)	0.00 (0.00)
Constant	-0.57 (0.56)	-0.41 (0.62)	-0.18 (0.59)	6.62 (22.03)	-20.04 (29.43)
N	80	78	78	42	42
Log Likelihood	-178.21	-173.85	-169.54	-116.79	-112.45
θ	2.22*** (0.60)	2.50*** (0.72)	3.17*** (1.01)	1.98*** (0.64)	2.79*** (1.03)
AIC	370.42	365.71	361.08	247.57	246.90

Negative binomial coefficients with standard errors in parentheses; *p<0.1; **p<0.05; ***p<0.01

Table 4 – Investigations in the Senate

	1789-1948			1865-1948	
	(1)	(2)	(3)	(4)	(5)
Divided Gov.	0.14 (0.28)	0.15 (0.28)	0.16 (0.29)	0.05 (0.31)	-0.003 (0.34)
17th Amend.	-0.07 (0.34)	-0.08 (0.34)	-0.06 (0.35)	0.11 (0.44)	0.03 (0.47)
Divided Gov. \times 17th Amend.	0.92** (0.43)	0.91** (0.43)	0.85* (0.45)	0.96** (0.45)	1.03** (0.52)
Civil Service Rate _{SD}		-0.68 (1.92)	-0.44 (1.96)		-0.60 (1.96)
Total Expenditures _{FD}		-0.002 (0.01)	-0.01 (0.02)		-0.00 (0.00)
War			-0.17 (0.29)		-0.07 (0.34)
Reconstruction			0.05 (0.36)		-0.54 (0.75)
t	0.11 (0.10)	0.10 (0.10)	0.10 (0.11)	0.41 (0.89)	-0.25 (1.39)
t^2	-0.00 (0.002)	0.0002 (0.002)	0.0002 (0.002)	-0.01 (0.02)	0.005 (0.02)
t^3	-0.00 (0.00)	-0.00 (0.00)	-0.00 (0.00)	0.00 (0.00)	-0.00 (0.00)
Constant	-3.45** (1.43)	-3.29** (1.48)	-3.27** (1.54)	-8.42 (16.90)	5.52 (27.84)
N	80	78	78	42	42
Log Likelihood	-123.65	-123.49	-123.27	-95.94	-95.53
θ	10.34 (7.34)	10.34 (7.32)	10.49 (7.52)	10.87 (8.09)	11.24 (8.47)
AIC	261.31	264.98	268.54	205.87	213.05

Negative binomial coefficients with standard errors in parentheses; *p<0.1; **p<0.05; ***p<0.01

Table 5 – Divided Government and Investigation Frequency

	<i>Predicted Investigations under Divided Gov.</i>		<i>Marginal Increase under Divided Gov.</i>	
	Pre-17th	Post-17th	Pre-17th	Post-17th
Senate	2.73	6.86	0.36 [-1.09,1.82]	2.43** [0.09,4.77]
House	5.60	8.03	2.45** [0.38,4.52]	3.33** [0.43,6.23]

Predictions calculated (at means) from Table 3, model 1 and Table 4, model 1; 95% conf. interval in brackets, **p<0.05

Table 6 – Investigating Congresses by Agency and Chamber

Department	Congresses	House	Senate
President	1 st -80 th	8	4
War	1 st -80 th	56	29
State	1 st -80 th	12	7
Treasury	1 st -80 th	30	19
Post Office	2 nd -80 th	29	19
Navy	5 th -80 th	25	16
Printing*	13 th -80 th	7	11
Interior	31 st -80 th	32	37
Agriculture	37 th -80 th	19	9
Justice	41 st -80 th	17	8
Civil Service Commission	48 th -80 th	5	6
Commerce	57 th -80 th	7	12
Labor	63 rd -80 th	3	4
Federal Reserve	63 rd -80 th	2	1
Veterans' Administration	71 st -80 th	5	0
Reconstruction Finance Corporation	72 nd -80 th	1	2
National Recovery Administration	73 rd -76 th	1	0
Tennessee Valley Authority	73 rd -80 th	1	1
Federal Communications Commission	73 rd -80 th	3	0
Federal Housing Administration	73 rd -80 th	4	1
National Labor Relations Board	74 th -80 th	1	1
U.S. Maritime Commission	74 th -80 th	1	0
Civil Aeronautics Authority	75 th -80 th	0	1

*Note: The GPO was not formally established until the 37th Congress.

Table 7 – Investigations by Agency, 1789-1948

	House		Senate	
	(1)	(2)	(3)	(4)
Divided Gov.	0.61*** (0.23)	0.45* (0.23)	0.26 (0.34)	0.31 (0.34)
17th Amend.	-0.12 (0.25)	-0.04 (0.26)	0.46* (0.24)	0.42* (0.25)
Divided Gov. \times 17th Amend.	1.00** (0.40)	1.20*** (0.42)	1.61*** (0.55)	1.54*** (0.57)
$t - t_{Y=1}$	-0.05** (0.02)	-0.05* (0.02)	-0.05*** (0.02)	-0.05*** (0.02)
Civil Service Rates _{SD}		-1.46 (2.75)		-2.69 (2.55)
Total Expenditures _{FD}		-0.00*** (0.00)		0.00 (0.00)
War		-0.67** (0.31)		-0.37 (0.36)
Reconstruction		0.88*** (0.31)		-0.53 (0.52)
Constant	-0.73** (0.34)	-0.78** (0.36)	-1.57*** (0.44)	-1.45*** (0.45)
N	842	837	842	837
Log Likelihood	-405.87	-389.27	-316.97	-313.95
AIC	865.74	840.54	687.94	689.90

Logit coefficients with standard errors in parentheses;
Dept. FE omitted for readability; *p<0.1; **p<0.05; ***p<0.01

Table 8 – Investigations by Department, 1865-1948

	House		Senate	
	(1)	(2)	(3)	(4)
Divided Government	0.98*** (0.29)	0.84*** (0.30)	-0.12 (0.38)	-0.02 (0.39)
17th Amend.	-0.28 (0.28)	-0.11 (0.30)	-0.09 (0.25)	-0.24 (0.26)
Divided Government × 17th Amend.	0.70 (0.44)	0.86* (0.46)	1.97*** (0.57)	1.92*** (0.61)
$t - t_{Y=1}$	-0.04 (0.03)	-0.04 (0.03)	-0.03** (0.02)	-0.02 (0.02)
Civil Service Rate _{SD}		-1.08 (2.75)		-2.70 (2.36)
Total Expenditures _{FD}		-0.00*** (0.00)		0.00 (0.00)
War		-0.86** (0.40)		-0.03 (0.40)
Reconstruction		0.62* (0.34)		-1.25** (0.53)
Constant	-0.70* (0.37)	-0.75** (0.38)	-1.31*** (0.44)	-1.24*** (0.44)
N	587	587	587	587
Log Likelihood	-282.84	-270.85	-260.46	-256.17
AIC	619.67	603.71	574.93	574.34

Logit coefficients with standard errors in parentheses;
Dept. FE omitted for readability; *p<0.1; **p<0.05; ***p<0.01

Table 9 – Divided Government and Investigation Probability

	<i>Predicted under Divided Gov.</i>		<i>Marginal Increase under Divided Gov.</i>	
	Pre-17th	Post-17th	Pre-17th	Post-17th
Senate	0.17	0.47	0.01 [-0.05,0.11]	0.20*** [0.04,0.48]
House	0.30	0.50	0.10*** [0.01,0.16]	0.16*** [0.04,0.36]

Predictions calculated (at means) from Table 7, model 1 and Table 7, model 3; 95% conf. interval in brackets, ***p<0.01

Table B1 – The Effect of Divided Government Across Structural Breaks

	One Break	Two Breaks	Three Breaks	Four Breaks
Regime 1	0.37 (-1.13, 1.22)	0.09 (-1.15, 1.27)	0.06 (-1.33, 1.40)	-0.01 (-1.64, 1.69)
Regime 2	0.41 (0.048, 0.76)	0.22 (-1.01, 1.07)	0.06 (-1.57, 1.74)	0.04 (-1.64, 1.60)
Regime 3		0.74 (0.15, 1.30)	0.20 (-1.10, 1.33)	0.02 (-1.57, 1.75)
Regime 4			0.76 (0.21, 1.27)	0.15 (-1.21, 1.19)
Regime 5				0.77 (0.15, 1.32)
<i>Terminal Breakpoint</i>	41st Congress	55th Congress	55th Congress	55th Congress
Poisson coefficients with 95 percent credible intervals in parentheses.				

Table B2 – Bayes Factors for Structural Break Models

	One Break	Two Breaks	Three Breaks	Four Breaks
One Break	1.00	0.06	0.10	0.44
Two Breaks	17.85	1.00	1.75	7.85
Three Breaks	10.19	0.57	1.00	4.48
Four breaks	2.27	0.13	0.22	1.00