

The Evolution of Congressional Collaboration*

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Abstract

Members working together on lawmaking is central to legislative success in the United States Congress. We propose a new measure of legislative collaboration that captures the extent to which members coordinated their efforts on bill introduction from 1873 to 2010, covering both within- and across-chamber collaboration. This requires a new methodology leveraging text-as-data and large language models to identify effectively identical pairs of bills from 11 billion candidates. We provide the first empirical evidence about the extent and nature of legislative collaboration before the early 1970s. In explaining what drives collaboration, we focus on the role of shared constituency interests, leveraging variation in the proximity and similarity of members' districts alongside moderation by partisanship. Despite large changes in legislative organization and the lawmaking process, we find striking historical stability in the dominant role that shared constituency interests, particularly representing neighboring districts within the same state, play in driving legislative collaboration.

Short Title: The Evolution of Congressional Collaboration

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1 Introduction

Despite the prevailing scholarly view of the United States Congress as being characterized by partisan polarization and legislative gridlock, significant legislative achievements have been and continue to be enacted (Mayhew, 2005; Clinton and Lapinski, 2006). Further, even though very few introduced bills survive the legislative process (Krutz, 2005), Congress still passes a considerable volume of legislation in most sessions. Because of the complexity of the lawmaking process and the extensive political bargaining required to gain attention and consideration for one's bill, successful legislative outcomes are not merely the product of a single member's effort (Craig, 2023). Rather, lawmaking requires that groups of legislators are able to effectively work together, both at the inception of the formal legislative process (i.e., bill introduction) and throughout subsequent stages of legislative progression. Indeed, legislative collaboration has long been recognized as foundational to legislative success (Kessler and Krehbiel, 1996; Fowler, 2006*a,b*; Kirkland and Kroeger, 2018; Casas and Wilkerson, 2025).

Many factors contribute to why collaboration between members exists and persists, including psychological and relational variables (e.g., Craig 2023; Curry and Roberts 2023; Gaynor and Millburne 2026), district-specific characteristics (e.g., electoral vulnerability; Koger 2003), demographic homophily between members (e.g., both members being female; Bratton and Rouse 2011), and shared constituency interests (e.g., both members coming from the same state; Kirkland and Kroeger 2018; Treul 2017). Scholars have also studied these dynamics both within (e.g., Fowler 2006*b*; Craig 2023; Curry and Roberts 2023) and across chambers (Kirkland and Kroeger, 2018; Casas and Wilkerson, 2025). To study collaboration, scholars have used a variety of different measures; much of the existing work relies on cosponsorship of introduced legislation (e.g., Koger 2003; Fowler 2006*a*) or refinements, such as original cosponsorship (i.e., members who are the initial signatories to the bill at the time of its introduction; Bernhard and Sulkin 2013; Curry and Roberts 2023). Other work focuses on measures that can capture pre-introduction collaboration (e.g., Box-Steffensmeier, Christenson and Craig 2019; Craig 2023) or

include activity across all stages of the legislative process (e.g., Eatough and Preece 2025).

Despite this rich literature, surprisingly little is known systematically about legislative collaboration between members before the early 1970s, perhaps because cosponsorship was prohibited in the House before 1967. Given the centrality of collaboration to the lawmaking process, this is a major gap in our ability to understand the legislative process for most of American history. We address this by providing a new measure of collaboration that spans from 1873 to 2010. This measure can be used both descriptively (e.g., to understand the evolution and changing extent of collaboration) and inferentially (e.g., to test hypotheses about what factors correspond to more collaboration). Our paper contributes to a growing body of work that leverages new data and methods to quantitatively analyze American political development (e.g., Clinton and Lapinski 2006; Chiou and Goplerud 2024; Finocchiaro and MacKenzie 2024; Kaufman and Rogowski 2024).

Specifically, we focus theoretically on one key determinant of collaboration: shared constituency interests. Our primary contribution to the theoretical literature is a more systematic account of the role these interests play in collaboration. While many studies have noted that being from the same state considerably increases collaboration (Bratton and Rouse, 2011; Gross and Kirkland, 2019; Fowler, 2006*b*; Kirkland and Kroeger, 2018; Treul, 2017; Craig, 2023), we provide a novel and more detailed set of theoretical expectations leveraging the geographical proximity of districts and other measures of shared constituency interests. Furthermore, we hypothesize that copartisanship moderates these effects.

To test our theoretical expectations, we focus on measuring the extent to which members collaborate on bill introduction, drawing on the concept of original cosponsorship (Bernhard and Sulkin, 2013; Curry and Roberts, 2023). However, cosponsorship of any kind was prohibited for much of the period we examine. In particular, the House prohibited cosponsorship before 1967, and the practice in the Senate was not widespread until the early 1930s (Campbell, 1982; Wilson and Young, 1997). As Section 3 justifies in detail, before cosponsorship was permitted, members coordinated their legislative efforts on bill introduction by submitting identical or very similar

copies of the same legislation, hereafter referred to as “effectively identical,” in relatively quick succession to each other. Thus, by identifying temporally proximate pairs of effectively identical bills, one can quantify a behavior that we refer to as “informal” original cosponsorship. Using this in conjunction with “formal” original cosponsorship (i.e., where multiple members’ names are printed on the introduced version of the bill) allows us to study within-chamber collaboration on bill introduction from 1873 to 2010. This approach also permits the systematic study of across-chamber collaboration through the use of companion bills, as these are also characterized in the literature by the introduction of identical or effectively identical legislation (Kirkland and Kroeger, 2018; Casas and Wilkerson, 2025).

Our key methodological innovation is to develop a procedure to identify the rare set of effectively identical pairs out of approximately 11 billion (within-congress) pairwise comparisons between all introduced bills from 1873 to 2010. One critical element in this process is leveraging a large language model to create summaries of each document and using these summaries for downstream analysis (Benoit et al., 2026). Using the full-text corpus of around one million bills, we use these summaries and other textual features to build a predictive model to assess whether a pair of introduced bills is effectively identical. To create a unified measure that spans the entire period, we combine our measure of informal original cosponsorship with data on formal original cosponsorship to provide a comprehensive measure of original cosponsorship from 1873 to 2010. After extensive validation, we create a new dyadic measure of members’ collaboration on bill introduction for each congress.

We provide new descriptive evidence of the trends in collaboration. Interestingly, our measure shows a stark increase in the volume of within-chamber collaboration starting in the 1930s (Senate) and 1940s (House). Notably, this increase considerably precedes the introduction of (formal) cosponsorship in the House in the late 1960s; this corroborates our qualitative evidence that the institution of cosponsorship was mostly introduced in response to cost pressures associated with the informal system of introducing effectively identical bills that was already well-established by that time. By contrast, across-chamber collaboration—i.e., the use of com-

panion bills—has remained remarkably stable from the 1870s to 2010. Even in the late 19th century, we find that hundreds of companion bills were introduced each congress, suggesting that the incentives for bicameral coordination on lawmaking are durable and long-standing.

Turning to the factors that are associated with higher collaboration, we find clear evidence that both geographic proximity (i.e., being from physically neighboring districts or states) and state-delegation effects (i.e., being from the same state) are consistently associated with more collaboration throughout the entire period we study. The magnitude of this effect is quite large substantively, often dwarfing the effect of two members serving on the same committee. Especially after 1975, there is also consistent evidence that being from the same party increases the effect of shared constituency interests for within- and across-chamber collaboration.

2 Shared Constituency Interests and Collaboration

Collaboration between members, broadly conceived as a group of members working together towards a shared goal, is central to the lawmaking process and has been extensively theorized and studied (see Craig 2023 for a thorough discussion). For parsimony, we focus primarily on constituency interests and how they facilitate collaboration both within and across the chambers.¹ Legislative collaboration partially involves a strategic calculus in which a lawmaker determines which colleagues to align with and on what projects. This decision is fundamentally shaped by institutional and personal gains such as enhancing reelection prospects, securing career advancement, or accumulating political capital (Mayhew, 1974). Given the severe constraints on members' time and capacity (Hall, 1996), they must be selective about who they work closely with and likely seek partners who are capable of sustained effort over the lifecycle of the bill.

¹We are also focused only on dyadic factors that facilitate collaboration (e.g., factors that depend on the interaction between member characteristics—such as coming from the same state). While other work explores *which* members are more likely to collaborate (e.g., Koger 2003), we generally do not analyze this and our empirical analyses include member-congress fixed effects to control for all such factors.

2.1 Shared Constituency Interests

We focus on shared constituency interests as a primary factor for facilitating legislative collaboration because of the fundamental electoral incentives that members face. The literature suggests a number of mechanisms through which shared constituency interests matter. First, shared constituency interests can serve as a foundation for credible commitment: Because any deviation from a legislative project that benefits a partner’s district would likely trigger electoral repercussions from the member’s own constituency, both partners are incentivized to maintain a cooperative front (Gross and Kirkland, 2019). This is especially important for lawmaking, where shepherding a bill requires a sustained investment of political capital over many months. Second, collaborating with colleagues who share similar constituency interests allows legislators to more effectively identify and monitor district-level preferences by sharing information (Deckard, 1972; Treul, 2017). Finally, by reducing coordination costs, legislators who have shared interests can bargain more effectively with their party leadership or with others in the chamber (Deckard, 1972). This can be especially true when members’ districts share high-demand constituencies, such as dominant local industries.

To empirically assess the importance of shared constituency interests across time, we conceptualize and operationalize them based on geographic factors and district demographic factors. Starting with geographic factors, coming from the same state is widely recognized in the literature as a primary proxy for capturing common constituency interests (Truman, 1956; Deckard, 1972; Schiller, 2000; Treul, 2017; Gross and Kirkland, 2019; Craig, 2023). Members representing the same state are exposed to shared socioeconomic conditions and frequently encounter collective challenges. For example, the centrality of the oil industry in Texas creates common interests for the Texas delegation, incentivizing cooperation regardless of individual district lines (Treul, 2017). Senators from the same state represent completely overlapping constituencies and have shared electoral concerns.

However, a shared state delegation combines two distinct sources of shared interests: Beyond

projects that must be tackled collectively at the state level, members from the same state often represent geographically proximate districts. It is thus important to separate out the role of proximity in encouraging collaboration. As existing research has shown, neighboring districts are inherently more likely to share similar economic landscapes or develop robust regional ties, such as integrated political organizations or joint economic associations (Fowler, 2006*b*; Bratton and Rouse, 2011; Craig, 2023). In large, heterogeneous states, spatial proximity may be especially important if localized factors are salient to a member’s electoral fortunes. While Bratton and Rouse (2011) demonstrate that adjacency fosters cosponsorship within nine state legislatures, the impact of geographic proximity—beyond state delegations—remains under-explored in Congress.

Geographic proximity also suggests that members from neighboring states should have shared interests, albeit weaker, as regional externalities often generate significant spillover effects. Examples of these include factors such as shared watersheds or river systems (Treul, 2017; Craig, 2023) or shared labor markets (Ommundsen, Treul and Olivella, 2024).

The final factor we consider is shared interests defined by demographic similarity between districts that is not based on geographic proximity. Legislators representing districts with similar occupational profiles—such as high concentrations of farmers, veterans, or laborers—are more likely to focus on legislation that caters to the interests of those groups (Adler, 2000; Bratton and Rouse, 2011; Craig, 2023; Finocchiaro and MacKenzie, 2024).

From this discussion, we focus on four proxies for shared constituency interests that should be associated with more collaboration: representing the same state, physically adjacent districts, neighboring states, and districts with high demographic similarity. We use this to generate three hypotheses that we will test empirically.

Shared Geographic Interests: We leverage variation in geographic proximity of districts to test whether an increasing intensity of shared interests should further increase collaboration. Using our proxies of (i) physically adjacent districts, i.e., sharing a border, and (ii) whether members represent the same or neighboring states, we classify all pairs of districts into five mutually exclusive categories: (*A*) Same State & Physically Adjacent Districts, (*B*) Same State &

Non-Adjacent Districts, (*C*) Neighboring State & Physically Adjacent Districts, (*D*) Neighboring State & Non-Adjacent Districts, with group (*E*) containing all other dyads. All five types of dyads exist for within-House collaboration; other types of collaboration (e.g., within-Senate) contain only certain relationships (e.g., *A*, *C*, *E*). We operationalize our first hypothesis as a partial ordering of expected effects over these categories:²

Hypothesis 1: Increasing intensity of shared interests should increase collaboration, i.e., $A \succ \{B, C\} \succ D \succ E$.

The one exception to a strict ordering of the categories is whether *B* (same state but not physically adjacent) or *C* (neighboring state but physically adjacent) should see higher collaboration as they each have only one type of shared interest (same state vs. physical adjacency) for which we lack clear *ex ante* theoretical expectations. Thus, our empirical results provide new descriptive evidence on this point.

While our first hypothesis was derived from geographic indicators, our second hypothesis focuses on the effect of our non-geographic proxy, described above.

Hypothesis 2: Legislators representing districts with higher levels of demographic similarity will be more likely to collaborate.

Moderation by Party: Given that political parties are central actors in the period of the U.S. Congress that we consider (Schickler, 2001; Cox and McCubbins, 2005), we examine how they might shape or moderate the effects of shared constituency interests. There are natural reasons for members from the same party to collaborate more, separate from constituency interests (e.g., producing collective goods for their party or signaling their commitment to the party leadership; Bratton and Rouse 2011; Treul 2017; Kirkland and Kroeger 2018; Craig 2023).

²Alternatively, we could operationalize the intensity of geographic interests using the distance between districts. Appendix C.6 shows the robustness of our main results to doing so.

This hypothesis conjectures that there is an interactive effect between shared interests (in both Hypotheses 1 and 2) and members coming from the same party. Focusing on collaboration within the House or across chambers, member districts do not perfectly overlap so we expect an unambiguously positive interaction between geographic proximity categories (A through D) and whether the members are from the same party. Further, we also suspect that this will intensify as shared interests increase and thus the magnitude of the interactions will also track the (partially) ordered expectations over the geographic categories.

Hypothesis 3: For within-House and across-chamber collaboration, there will be a positive interaction between copartisanship and shared interests. The interaction effect magnitudes should follow the ordering in **Hypothesis 1**.

However, the expectations for within-Senate collaboration are more complex. We expect a positive interaction between neighboring state senators (C) and being from the same party, by the same logic to within-House or across-chamber collaboration. However, for senators representing the same state (A), there is a possibly countervailing logic insofar as the same-party senators may wish to distinguish themselves from each other and collaborate *less* (Schiller, 2000). The existing empirical evidence on this point is limited to late-20th-century congresses and is somewhat mixed,³ and thus we do not have a firm prediction as to what to expect with a same-party and same-state interaction in the Senate. We provide new evidence as to this question across a broad historical span.

2.2 Other Determinants of Collaboration

The literature has identified other important factors for collaboration besides shared constituency interests. While these are not our primary theoretical focus, we briefly enumerate the ones here that we control for in our empirical analyses.

³For example, Schiller (2000) finds evidence of differentiation when looking at introducing bills in the same issue area, but increased rates of cosponsorship with same-state/same-party senators.

Ideological Proximity: Legislators who are more ideologically proximate face lower transaction costs when bargaining or negotiating over legislation and thus likely collaborate more frequently (Harward and Moffett, 2010; Bratton and Rouse, 2011; Kirkland and Kroeger, 2018).

Shared Committee Membership: Working together on the same committee often generates norms of reciprocity that increase collaboration by leveraging shared expertise and policy interests (Fenno, 1973; Treul, 2017; Craig, 2023).

Shared Service: Members who have served together in Congress often have stronger personal connections that help collaboration (Bratton and Rouse, 2011; Kirkland and Kroeger, 2018). Unfortunately, due to data limitations, we cannot systematically test other forms of interpersonal connection explored in existing studies (e.g., Craig 2023; Curry and Roberts 2023; Gaynor and Millburne 2026).

Copartisanship: As noted above, we also control for whether the members in the dyad are from the same party (Campbell, 1982; Bratton and Rouse, 2011; Treul, 2017; Craig, 2023; Gaynor and Millburne, 2026). We also control for whether both members are in the majority party as this may also facilitate collaboration (Wilson and Young, 1997; Craig, 2023).

2.3 Temporal Dynamics

Our analysis spans the period from 1873 to 2010, an era characterized by significant institutional and political evolution.⁴ When analyzing the trends in collaboration, it is important to allow for the determinants of collaboration to evolve over time. Indeed, analyzing a similar period, Chiou and Goplerud (2024) find the determinants of legislative effectiveness vary considerably across eras with changes in legislative institutions, electoral rules, and party strengths. They

⁴We begin in 1873 for two reasons: First, as noted by Cooper and Young (1989), members of Congress gained the right to introduce bills under their own names following the end of Reconstruction. Second, many of the data sources we utilize (e.g., from ProQuest) keep systematic records by drawing from the *Congressional Record* which began in 1873. At the time of data collection, our coverage ended in 2010.

analyze the House and Senate in five and six periods, respectively, where key institutional reforms such as the Legislative Reorganization Act or the reforms in the House in the 1970s delineate each period; Appendix A.1 provides more details. We do not think it is possible to make precise, theoretically informed hypotheses about how these different periods might moderate our key hypotheses and thus we present the results separately by period to facilitate further theory generation and examine more descriptively whether our hypotheses hold in all periods we examine. Appendix A.2 provides more context on two reforms in particular (direct election of the Senate; the Legislative Reorganization Act of 1946) that were major changes to central parts of the lawmaking process and draws out some potential implications for collaboration.

3 Conceptualizing Legislative Collaboration Across Time

To test our theoretical expectations, we must develop a measure for collaboration that can be applied consistently for both within- and across-chamber collaboration as well as across the entire period from 1873 to 2010. The existing literature has used a variety of measures of collaboration; following Adcock and Collier (2001), we briefly survey them here to motivate our systematized concept of collaboration as coordinated effort at the start of the legislative process.

Starting with within-chamber collaboration, early systematic and quantitative work studying collaboration focused on the formal part of the legislative process—introduced legislation. To measure this, a key outcome was often formal cosponsorship (Schiller, 2000; Koger, 2003; Fowler, 2006*a,b*), i.e., where members formally attach their name to a piece of legislation. However, the collaboration involved in cosponsorship could be rather shallow if a member simply appends their name to a bill months after its introduction (Curry, 2025, p. 2). To address this, researchers have exploited rich datasets (e.g., Dear Colleague Letters; Box-Steffensmeier, Christenson and Craig 2019; Craig 2023) that provide signals of collaboration that might come *before* bill introduction and, indeed, might be used by the bill’s primary authors to gather cosponsors before introduction (Craig, 2023). Others have tried to refine the measure of cosponsorship to extract a clearer signal

of meaningful collaboration.

One popular alternative relies on the notion of “original cosponsorship” (e.g., Bernhard and Sulkin 2013; Curry and Roberts 2023): “Original cosponsors, in Hill parlance, are those whose names are affixed to the legislation when it is first introduced” (Curry and Roberts 2023, p. 342; see also Bernhard and Sulkin 2013, p. 466). By focusing only on those individuals who were formally involved at the time of bill introduction, the problem of counting very loose and weak connections as collaboration is partially addressed. While sometimes original cosponsors are added after the bill has been drafted but before it has been introduced (Bernhard and Sulkin, 2013; Craig, 2023), qualitative evidence suggests that it is viewed as a measure of serious collaboration (Curry and Roberts, 2023). Thus, we believe original cosponsorship within a chamber corresponds closely to a systematized concept of collaboration as coordinated effort at the start of the legislative process. The remainder of this section discusses how we operationalize this concept historically and for across-chamber collaboration.

3.1 Conceptualization across Time

A key challenge with using original cosponsorship for a longer historical period is that formal cosponsorship (both original and postintroduction) was explicitly prohibited for much of American history. In brief, the House prohibited cosponsorship until 1967, after which 25 cosponsors per bill were permitted; from 1979 (96th Congress), this was relaxed and an unlimited number of cosponsors could be attached to a bill (Wilson and Young 1997; see Appendix A.3 for details). In the Senate, the story is more complex as the practice of formal original cosponsorship evolved more informally; the consensus is that it was ruled out of order in 1913 (Riddick 1974; Schiller 2006), but its return was somewhat informal. Looking systematically at ProQuest’s corpus of digitized bills, it is clear that original cosponsorship (i.e., bills printed with multiple members’ names at the top) was systematically occurring from 1930—as evidenced by printed bills and the Congressional Record listing multiple members.

Yet, even before 1967 in the House and the 1930s in the Senate, members were, in fact, still

clearly coordinating on bills at and before the start of the formal legislative process. In what might be thought of as a clever form of political imagination or “rule bending” (Shepsle, 2017), members would introduce *identical* legislation to coordinate their efforts and signal support for a proposal. All of these bills were numbered, printed, and referred to the relevant committee—often at great expense when thousands of duplicate bills were introduced each congress (Thomas and Grofman, 1993). We present a brief case study that illustrates how this behavior was clearly seen as analogous to original cosponsorship; Appendix A.3 includes four additional case studies.

In the Senate in 1913 (*Congressional Record* 1913, 5798–5799), Senator William Kenyon (R-IA) attempted to introduce a resolution with Senator William Borah (R-ID) to stop the practice of granting the widows of deceased senators one year of their salary. A short debate ensued about whether this “joint introduction” was permitted; Kenyon offered that he and Borah had both thought of proposing a similar resolution and he “supposed we [himself and Borah] could unite in offering one resolution.” This logic seems to closely track the notion of “original cosponsorship” as a joint effort to legislate together. However, the general consensus was that this was either not permitted or would be a change in existing practice, and instead, a third senator suggested a simple solution: “each Senator introducing the identical resolution, or one practically the same.” The chair ruled that a resolution should only be introduced by a single senator, and Kenyon withdrew the resolution. This is widely agreed to have led to the prohibition of formal cosponsorship in the Senate (e.g., Riddick 1974; Schiller 2006). In 1961, echoed by other senators, Senator Everett Dirksen (R-IL) criticized the cosponsorship system and advocated for a return to duplicate bill introductions (detailed in Appendix A.3). Despite the failure of Dirksen’s “crusade,” these historical debates highlight how senators viewed these two mechanisms as functional substitutes.

In the House, Appendix A.3 provides three case studies at critical moments (prohibition of cosponsorship in 1909; creation of formal cosponsorship in 1967; extension to unlimited cosponsorship in 1979); in all cases, members are explicit that introducing duplicate bills and cosponsoring legislation served as direct functional alternatives.

Relying on an informal system of duplicate bill introductions to signal collaboration also might be repurposed by members to different ends. For example, there was a clear concern in the House in the late 1960s that certain committee chairs were viewed as using it to *steal* credit for another member’s idea, e.g., introduce an effectively identical copy of their bill subsequent to the initial version and report out the chair’s version (see Appendix A.3 for contemporaneous examples). A stated benefit of the formal system of cosponsorship was to limit the ability to steal credit insofar as the bill that would be advanced out of committee would bear the name of the original sponsor and cosponsors. Alternatively, one could imagine that a member introduces a slightly different version of a bill—in lieu of an amendment—to attempt to have *that* version taken up in the legislative process instead of the original version.

These alternative motives for duplicate bill introduction would complicate the interpretation of this behavior as directly mirroring *postintroduction* cosponsorship. However, given our theoretical focus on *original* cosponsorship, we are only looking at duplicate bills introduced in close proximity to the original version—following the definition of original cosponsorship as those added on the date of the bill’s introduction (Curry, 2025). Our measure finds that this is quite common, with around 6,500 instances where effectively identical bills are introduced in the same chamber on the *exact* same day. If we look at effectively identical introductions within one week, the number rises to nearly 14,000.

3.2 Conceptualization across Chambers

Because no formal cross-chamber cosponsorship is permitted, members again turn to informal means of coordination. The existing empirical work on across-chamber collaboration is more limited but generally analyzes “companion bills,” i.e., identical or effectively identical bills introduced in both chambers (Kirkland and Kroeger, 2018; Casas and Wilkerson, 2025).⁵ There

⁵Appendix B.1 compares different definitions of companion bills; they are all generally consistent, referring to companions as “identical” or “[substantially] similar.”

are many reasons why members might seek to introduce companion bills, some mirroring the logic of cosponsorship (e.g., signaling support for the legislation in both chambers; Kirkland and Kroeger 2018) and some specific to across-chamber collaboration (e.g., allowing consideration of both bills to proceed simultaneously in each chamber; Casas and Wilkerson 2025). In the modern era (1993-), the practice is rather common with around 25% of (non-minor, non-appropriations) bills having a companion (Casas and Wilkerson, 2025).

Precisely defining companion bills, however, remains somewhat slippery as it is not a formal designation and thus is an informal institution. The Congressional Research Service defines companion bills as “a bill introduced in one House of Congress that is similar or identical to a bill introduced in the other House of Congress” (Sullivan, 2007). Generally, the bar for similarity in empirical work is quite high, although bills need not be word-for-word identical (e.g., Casas and Wilkerson 2025). We note that the modern-day practice of companion bills closely parallels the practice of introducing effectively identical bills. The temporal requirement that characterizes the conceptualization and operationalization of within-chamber original cosponsorship is generally not used in existing work on companion bills. However, we impose some temporal constraints for across-chamber collaboration, albeit with a longer window, to distinguish collaboration that occurs around the coordinated introduction of a proposal in both chambers versus one chamber’s version originating much later.

3.3 Systematized Concept of Legislative Collaboration

Taken together, we arrive at our systematized concept and formal definition of legislative collaboration that we explore in this paper: A set of members of the House and/or Senate collaborate on a piece of legislation if they (i) formally have their name on the introduced legislation or (ii) have introduced an (effectively) identical piece of legislation within close temporal proximity to the initial introduction. The following section details how we operationalize this definition. While this does not capture all forms of collaboration, we believe it captures a meaningful form of collaboration at the start of the legislative process.

4 Measuring Collaboration

Given this definition, we must first solve two methodological challenges: First, how can one identify effectively identical bills and, second, how should this bill-level data be aggregated into member-dyad scores. We first turn to the challenge of identifying effectively identical bills from the millions of possible pairs in a given congress. Unlike existing work that explores the textual similarity of legislation in the United States Congress (e.g., Casas, Denny and Wilkerson 2020; Kroeger et al. 2024; Casas and Wilkerson 2025), we do not have access to clean, sectioned bill texts as those are only systematically available from the early 1990s. Rather, our bill texts consist of scans of bills from microfiche provided by ProQuest that contain significant OCR errors and artifacts that are irregular and difficult to resolve with a simple pattern-based approach. To address these challenges, we employ a variety of machine learning techniques—grounded in careful human validation. To produce a member-dyad score, we take the implied network of effectively identical bills, add in the formal (original) cosponsorships, and produce a weighted network that reflects the extent to which two members have collaborated in a given congress.

4.1 Identifying Effectively Identical Legislation

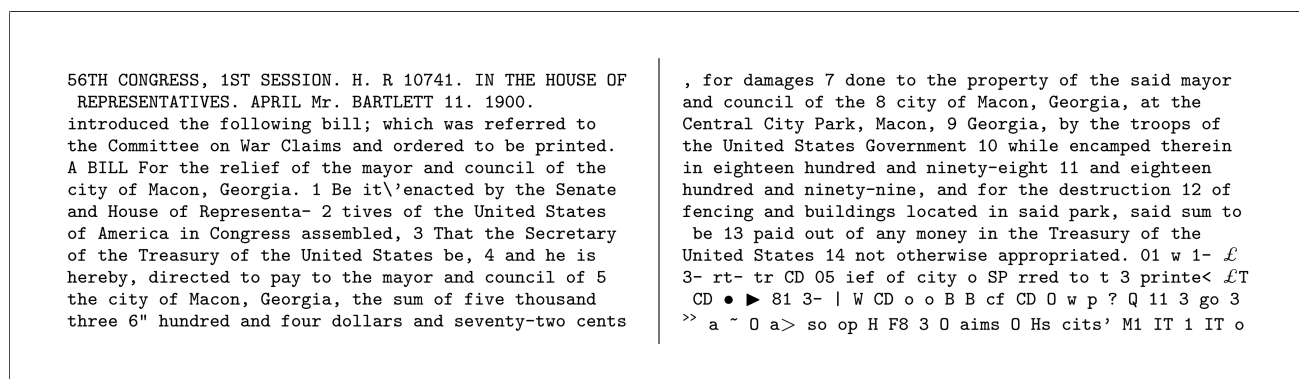
Existing work on identifying effectively identical bills in the United States Congress has focused exclusively on across-chamber collaboration, i.e., companion bills (Kirkland and Kroeger, 2018; Casas and Wilkerson, 2025).⁶ That work focuses on the post-1973 period and has sometimes used labels provided by the Congressional Research Service or other vendors as to whether two bills are effectively identical or companions (e.g., Kirkland and Kroeger 2018). Beyond being unavailable before 1973, Casas and Wilkerson (2025) also show that these labels suffer a serious

⁶A related literature has looked at finding insertions or hitchhikers, i.e., portions of legislation that appear in others (e.g., Linder et al. 2020; Casas, Denny and Wilkerson 2020; Kroeger et al. 2024). We seek to find bills that are very similar looking at the entire document; however, Appendix B.5 discusses how we use key textual features from this literature in our model.

problem with “false negatives,” i.e., many bills that are *in fact* (effectively) identical are not labeled as such. To address this, they rely on a text-based approach where bills with a sufficiently high share of five-gram phrases are classified as “companions.” Their new measure suggests that a large number of companion bills are not labeled as such by the Congressional Research Service.

For our purposes, however, it is not possible to directly adopt bag-of-words or n -gram shingle approaches used by existing work (e.g., Casas, Denny and Wilkerson 2020; Casas and Wilkerson 2025) as we lack access to a clean, sectioned version of the bill text. Rather, in the period we study, we only systematically have the following pieces of information: (a) the bill’s title and (b) a scan of the bill’s text that contains an unpredictable number of typos, extraneous characters, line numbering, and lacks any metadata as to the structure of the bill itself such as sections. One typical example is shown below; in this bill (56 H.R. 10741), Charles Bartlett (D-GA) introduced a bill in the 56th Congress (14 April 1900) for relief for the mayor and council of Macon.

Figure 1: Example of Scanned Bill (56 H.R. 10741)



While the messy scan of the text is (mostly) readable to a human, it clearly prevents any simple measure based on n -gram or shingles from being a “crisp” way of detecting effectively identical pairs of bills. Some cleaning can be done with regular expressions (e.g., to remove line numbers or stray punctuation marks), although our experience suggests many mistakes or unusual artifacts remain in the data. We thus proceed by using an automated approach, leveraging machine learning and large language models, to identify effectively identical bills in

the historical period. At a high level, Table 1 describes our procedure for identifying effectively identical pairs from a collection of N bills that generate $N(N - 1)/2$ possible pairs.

Table 1: Two-Stage Procedure for Identifying Effectively Identical Bills

- | |
|---|
| <ol style="list-style-type: none"> 1. Stage 1 (Initial Screening): Use some inexpensive procedure to eliminate pairs that are obviously <i>not effectively identical</i>, i.e., eliminate (many) pairs that have a very low risk of being effectively identical. Define this set of “candidate” pairs as $\mathcal{A}_{\text{similar}}$. 2. Stage 2 (Primary Classification): For each pair in $\mathcal{A}_{\text{similar}}$, use a more expensive and higher-quality algorithm to identify the effectively identical pairs, defined as $\mathcal{A}_{\text{identical}}$. |
|---|

The motivation behind our two-stage process is as follows: For a collection of, say, 5,000 bills, there are about 25 million pairs to evaluate. Of those, the vast majority are clearly not effectively identical (e.g., a bill on agriculture and a bill on defense). We thus need some procedure to quickly eliminate the vast majority of obviously distinct pairs—with a very limited risk of false negatives (i.e., accidentally eliminating a pair that is truly effectively identical). Then, we can train a more expensive but higher-quality model to identify the effectively identical pairs from a much smaller set.

4.1.1 Stage 1: Initial Screening

For the first stage in our classification process, we use a large language model (GPT-4o-mini) to take the raw text of the bill without modification and ask it to write a concise summary (50-100 words for short bills; 200-300 words for all others) that focuses on summarizing the policy proposed in the bill. Appendix B.2 contains the prompt and additional details about this process.⁷ We found that these summaries were quite informative and were not often tripped up by the messy nature of the underlying text. This mirrors recent work in political science (e.g., Benoit et al. 2026) that shows the advantages of using LLMs to generate summaries of complex documents for downstream tasks. For the bill above, provided *without any cleaning or*

⁷It also justifies our use of a commercial, proprietary language model (see Palmer, Smith and Spirling 2024).

pre-processing, the summary is shown below:

This bill aims to authorize the payment of \$5,304.72 to the mayor and council of Macon, Georgia, as compensation for damages to Central City Park caused by United States troops while encamped there in 1898 and 1899. The funds are to be drawn from the U.S. Treasury, ensuring relief for the city's financial losses related to the destruction of property during that time.

To perform our initial coarse procedure, we take all pairs of bills in a given congress,⁸ embed the summaries using sentence embeddings (Reimers and Gurevych, 2019) and take the cosine similarity between them.⁹ We also do this for the titles, as these are less subject to spelling errors, and take the cosine similarity between the title embeddings. If *neither* similarity score is above 0.80, we conclude that the pair is not effectively identical. If either score is above 0.80, they are included in the set of pairs considered in the second stage of our procedure.

We performed extensive validation of this threshold. For a human baseline, we trained an expert RA with whom we worked for nearly one year and who coded thousands of pairs of bills for whether they were effectively identical; Appendix B.3 provides a detailed discussion of our training protocol. For the first stage in our procedure, our validation comes from a stratified random sample of 360 pairs of bills. Appendix B.4.1 shows that *no* pairs with a similarity score below 0.90 were coded as effectively identical by the RA; 16.7% of those pairs with a similarity score in $[0.9, 1]$ were coded as effectively identical (32% after 1947; 8% before). This gives us confidence that our threshold of 0.80 is deliberately lenient. Our qualitative examination suggests that a score of around 0.80 would lead to bills that are generally on the same topic but

⁸Our analysis encompassed all bills introduced from 1873 to 2010, including private and claim bills, which play an important role in constituency-motivated legislative activities, especially prior to 1946 (Finocchiaro and MacKenzie, 2024).

⁹The text generated by the LLM is not deterministic; however, using sentence embeddings, the similarity between different summarizations of the same documents is quite high (median of 0.97; see Appendix B.2).

certainly not effectively identical. To examine our procedure on a larger scale, Appendix B.4.2 outlines our digitization of a previously unused dataset of identical and companion bills published by Commerce Clearing House’s *Congressional Index* from 1945 to 1980. This generates 400,000 pairs of bills that are implied to be effectively identical. Appendix B.4.3 shows that our initial screening also performs well on this much larger corpus.

Applying this initial screening step considerably reduces the challenge of classifying pairs. It eliminates around 94% of pairs—going from 11 billion to around 629 million, with an average of around 9 million per congress. Appendix B.4.4 shows that the number of pairs that pass this initial screening changes considerably over time, peaking in the early 1900s due to a considerable number of private claims bills.

4.1.2 Stage 2: Primary Classification

The second stage in our procedure requires a set of bill pairs that are known to be truly effectively identical in order to train a classifier. However, existing datasets do not have coverage before 1945 and, as noted in Section 4.1, have severe issues with false negatives, i.e., truly effectively identical pairs that are coded as not effectively identical (Casas and Wilkerson, 2025).

We thus again leverage a large language model (LLM), specifically GPT-4.1-mini, to generate many comparisons of whether two bills are effectively identical. Appendix B.5 contains more details on this process, the exact prompt, and our choice of model. In brief, we give it a slightly cleaned text of two bills and ask it to assess whether the bills are “effectively identical” on a scale of 0 to 100, while also writing a one-sentence justification as we found this helped performance (Benoit et al., 2026). Given our preliminary experiments, we set a threshold of 80 for defining effectively identical bills; if the LLM score is above 80, we classify a pair as “effectively identical.” However, it is not feasible to use the LLM to examine all pairs given that nearly 630 million survive the initial screening. Instead, we collect a number of features of each bill and use them to predict the LLM label after taking a random sample of pairs to give to the LLM to annotate. One additional benefit of this approach is a guard against LLM “hallucinations”; if there is

some low rate at which the LLM makes mistakes in its coding (as also would be true for any human-created source), the predictive model should not be wholly misled by this when making a prediction for an unseen bill.

Appendix B.5 provides additional details on the text features and the predictive model. We include a wide range of textual features to capture semantic similarity (e.g., the similarity scores used above in addition to an embedding of the raw text), n-gram similarity (e.g., Casas, Denny and Wilkerson 2020; Kroeger et al. 2024; Casas and Wilkerson 2025), exact text similarity (local and global alignment; e.g., Linder et al. 2020), and discrepancy in length. For each congress, we sample approximately 3,000 pairs from $\mathcal{A}_{\text{similar}}$, stratifying to ensure a reasonable number of positive examples in the training data (see Appendix B.5.1). To make predictions for a congress t , we fit a random forest holding out all pairs from that congress. This avoids any concern about information leakage given the complex network dependence between bill pairs.

Table 2 summarizes our validation of this stage. The first panel confirms that our textual features and predictive model successfully predict the LLM label in the (stratified) training data. Using the held-out predictions for each congress, Table 2 reports various measures of performance at a threshold of 0.50 (i.e., a pair is predicted to be effectively identical if its predicted probability is above 0.50) and 0.80. We use the latter threshold to ensure that if we use a rather stringent criterion to reduce the risk of mis-classifying distinct bills as effectively identical, our procedure still retains good classification accuracy.

All metrics are quite good, although the precision and F1 score must be viewed carefully as the stratified sampling, by design, ensures a higher rate of positive examples in the training data than in the true population. Appendix B.6 shows these metrics across time; performance is generally stable across the metrics considered.

More importantly, it is critical to assess whether the predicted labels align with human judgment. We first worked with our expert RA to code a large stratified random sample of 3,450 pairs (50 per congress). The predictions from our model closely track human judgment; the second panel in Table 2 shows the performance is high for both thresholds. Appendix B.6 shows

Table 2: Performance of Predictive Model

	Held-Out LLM Labels				
	Specificity	Sensitivity	Precision	F1	Bal. Acc.
Threshold (0.50)	0.95	0.89	0.89	0.89	0.92
Threshold (0.80)	0.98	0.73	0.95	0.83	0.86
	Human Annotations				
	Specificity	Sensitivity	Precision	F1	Bal. Acc.
Threshold (0.50)	0.95	0.88	0.89	0.88	0.91
Threshold (0.80)	0.98	0.71	0.95	0.82	0.85

Note: “Held-Out LLM Labels” compares performance against LLM labels not used to generate the predictions for that congress ($n \approx 203k$); “Human Annotations” compares against human annotation on a sample from the LLM labels ($n \approx 3,450$). “Threshold (0.5)” and “Threshold (0.8)” binarize the predicted probabilities at 0.5 and 0.8, respectively, to generate the measures of prediction accuracy.

the results across time; performance is generally stable. Appendix B.6 additionally validates our approach by comparing it to the set of 13 million pairs covered by Commerce Clearing House (1945-1980); the performance is generally comparable to the other validation checks.

Overall, therefore, our validation suggests that a predictive model based on textual features is quite good at estimating whether two bills are effectively identical. This is a cost-effective method insofar as training data for the predictive model can be generated based on a reasonable number of (randomly sampled) pairs.

4.2 Quantifying Collaboration Between Members

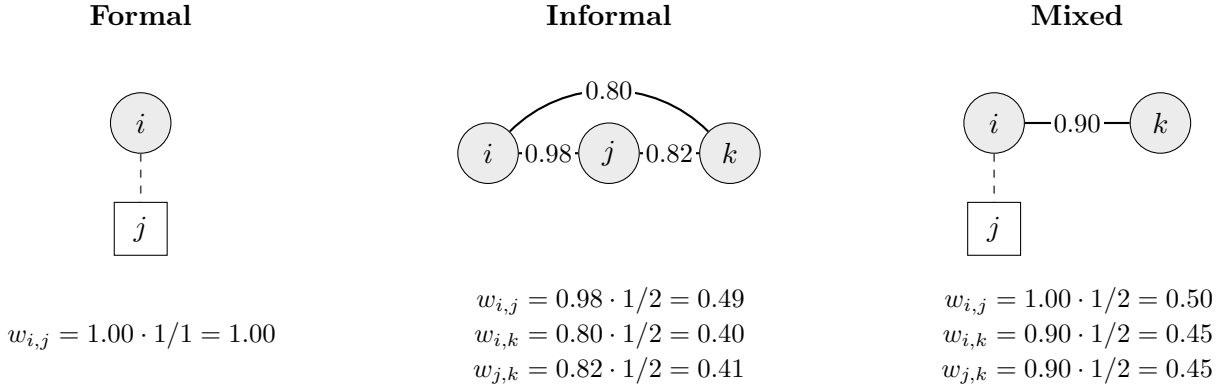
We next turn to quantifying collaboration between members in a given congress. To do this, our measure includes both formal original cosponsorship (when permitted by the rules) and informal original cosponsorship (i.e., introducing effectively identical bills).¹⁰ To ensure this captures original cosponsorship (vs. “post-introduction” cosponsorship), we require that (i) within-chamber bills be introduced within seven days of each other, (ii) across-chamber bills be introduced within 30 days, and (iii) formal original cosponsors be added on the date of

¹⁰Appendix C.3 shows that our empirical results are robust to separately analyzing formal vs. informal original cosponsorship when both could occur.

introduction (following Curry and Roberts 2023) or printed on the earliest introduced version. Appendix C.7 shows the robustness of our empirical results to different temporal thresholds.

Our measure is built around the notion of a “cluster” c of bills that are connected together. There are three types of clusters that might exist: “Formal” clusters are ones where members are connected through formal original cosponsorship and there are no bills that are identified as being effectively identical to the introduced bill. “Informal” clusters are ones where no formal cosponsorship is present, but bills are identified as being effectively identical and were introduced within close temporal proximity. “Mixed” clusters contain both types of relationships. Figure 2 illustrates these schematically where dashed lines indicate formal cosponsorship and solid lines (with a number) indicate bills that are estimated to be effectively identical, i.e., informal original cosponsorship.

Figure 2: Types of Collaboration Clusters



Note: Circles are introduced bills labeled by their sponsor; squares indicate formal original cosponsors, attached by a dashed line. Solid lines connect bills that the model predicts to be effectively identical, labeled with the predicted probability. A member’s collaboration weight $w_{i,j}$ for a cluster c is the probability that their bills are effectively identical (set to one for formal original cosponsorship) times $1/(|\mathcal{S}_c| - 1)$ where $|\mathcal{S}_c|$ is the number of unique members in the cluster. The types of clusters are described in the main text.

To turn this into a dyad-level score, we generalize Fowler (2006a)’s approach to measuring the connections between members’ formal cosponsorship activity to our more complex setting where some connections arise informally and are estimated probabilistically. Mathematically, consider a single cluster c that has a set of members $i \in \mathcal{S}_c$ who participate in it. Existing work

(e.g., Fowler 2006a) notes that it is important to down-weight the connection between members in a cluster c that has many members, as the strength of the collaboration likely declines as more members are involved.¹¹ For a formal cluster c , we exactly follow Fowler (2006a) and weight by $1/(|\mathcal{S}_c| - 1)$, where $|\mathcal{S}_c|$ is the number of members in set $\mathcal{S}_c$. For an informal cluster, we additionally weight by the probability $p_{i,j}^c$ that members i and j are working on a bill in cluster c that is effectively identical. In the case of formal original cosponsorship, $p_{i,j}^c = 1$. In the case of informal original cosponsorship, it is the probability estimated in Section 4.1.2 for the bills accompanying i and j 's collaboration; we set $p_{i,j}^c = 0$ if the relevant bills do not pass the initial screening. Figure 2 provides three simple numerical examples of how the weights are computed. To create a dyad-level score, we sum across all clusters in a given congress and add together the scores for members who are involved in that cluster.¹² Formally, the collaboration score between members i and j is defined as follows:

$$y_{i,j} = \sum_c \mathbf{1}\{i \in \mathcal{S}_c, j \in \mathcal{S}_c\} \left(\frac{1}{|\mathcal{S}_c| - 1} \right) p_{i,j}^c. \quad (1)$$

While somewhat elaborate to construct, this score can be benchmarked straightforwardly: If i and j collaborated together on a single bill in a given congress and did so exclusively, i.e., without others, $y_{i,j} = 1$. While it is possible to obtain a score of $y_{i,j} = 1$ in other ways, one can interpret our measure as the number of “effective” exclusive collaborations, accounting for uncertainty in our predictions and cases of multiple members introducing the same bill.

¹¹In the full data from 1873 to 2010, 50.3% of clusters have only two members, although the median size has increased over time.

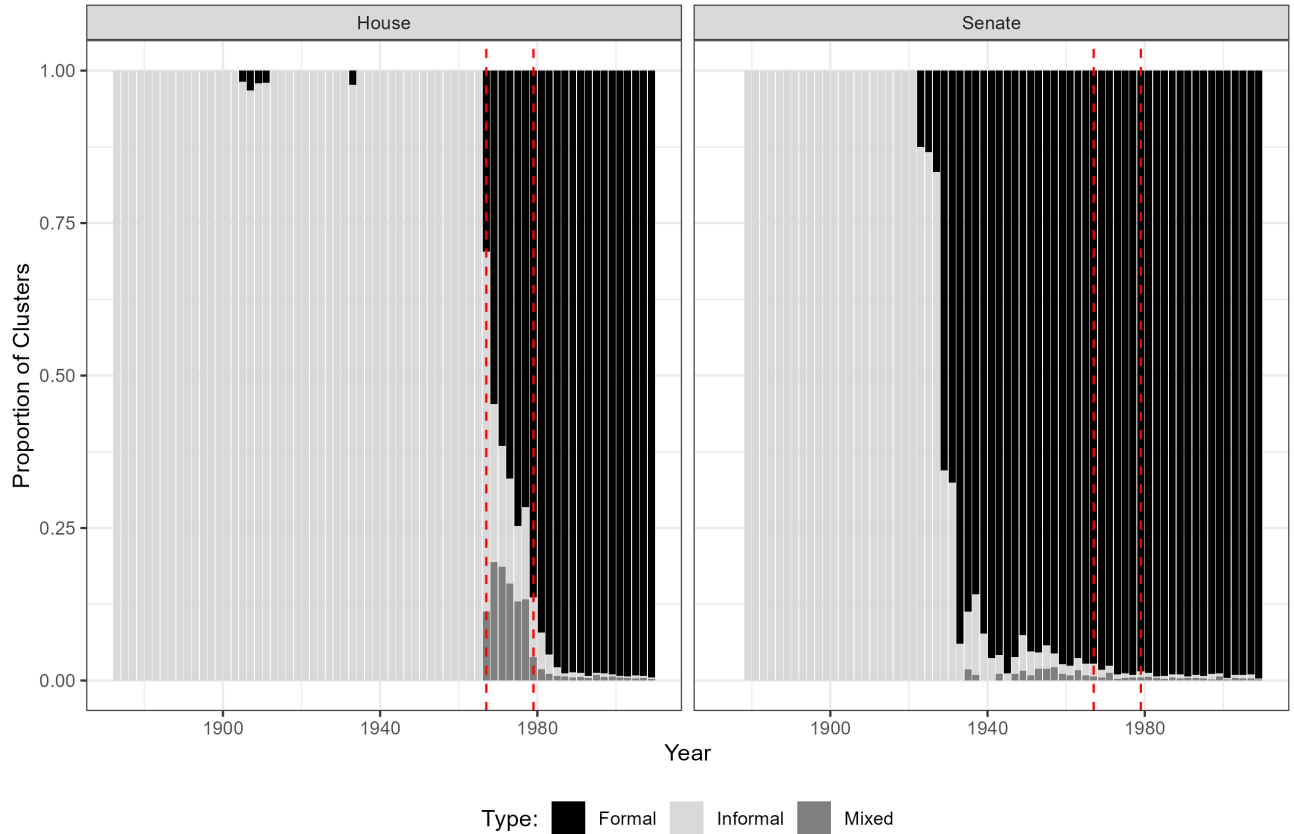
¹²The set of clusters is constructed by building a network connecting all introduced bills with $p_{l,l'} > 0.80$ (to avoid false positives) and then splitting it into connected components that are used as clusters; Appendix B.7 provides further details.

4.3 Descriptive Results and Validation

Before testing our main hypotheses, we show some descriptive results about our measure of collaboration and perform some additional validation. First, in the period when cosponsorship was permitted, members still had the option of introducing duplicate legislation. The incentives for this behavior should have changed considerably over time, especially if one thinks that formal original cosponsorship is less costly (e.g., in terms of solving coordination problems) and sidesteps some of the potential pitfalls of duplicate bill introduction discussed in Section 3.1. Drawing on Adcock and Collier (2001)’s idea of construct or nomological validation, we examine whether our measure agrees with existing well-established hypotheses in the literature. In the House, formal original cosponsorship should rapidly displace introduction of effectively identical bills following the introduction of unlimited cosponsorship in 1979 (e.g., Thomas and Grofman 1993). In the Senate, existing literature suggests a more gradual change as formal original cosponsorship becomes more widely adopted in the early 1930s (e.g., Campbell 1982). We can see if our measure is sensitive to this by looking at the distribution of types of clusters (see Figure 2) where “formal” clusters (i.e., collaborations consisting of formal original cosponsorship) displace “informal” clusters (i.e., collaborations consisting of informal original cosponsorship) and mixed (i.e., having both types). Figure 3 shows the relative proportion of each of these categories across time and for within-chamber collaboration.

Focusing on the House, the dashed lines indicate the introduction of limited cosponsorship in 1967 and unlimited cosponsorship in 1979, respectively. As expected, we see a surge in formal original cosponsorship and in “mixed” patterns—reflecting the fact that with cosponsors limited to 25, duplicate bill introduction was still sometimes necessary. After the introduction of unlimited cosponsorship, the mixed type dramatically decreases in frequency. In the Senate, we see a fairly rapid adoption of formal original cosponsorship starting in the 1930s, aligning with the more informal evolution of that practice in the Senate. Overall, this provides good validation of our measure capturing how members coordinated their effort on introducing legislation and

Figure 3: Trends in Method of Original Cosponsorship



Note: This figure shows the proportion of clusters of bills (Section 4.2) of each type of original cosponsorship (“Formal,” “Informal,” and “Mixed”) for within-chamber collaboration across the period we examine. The two dashed lines mark the 90th (1967-1968) and 96th Congresses (1979-1980), corresponding to the introduction of formal cosponsorship in the House and the removal of the cap on the number of formal cosponsors, respectively.

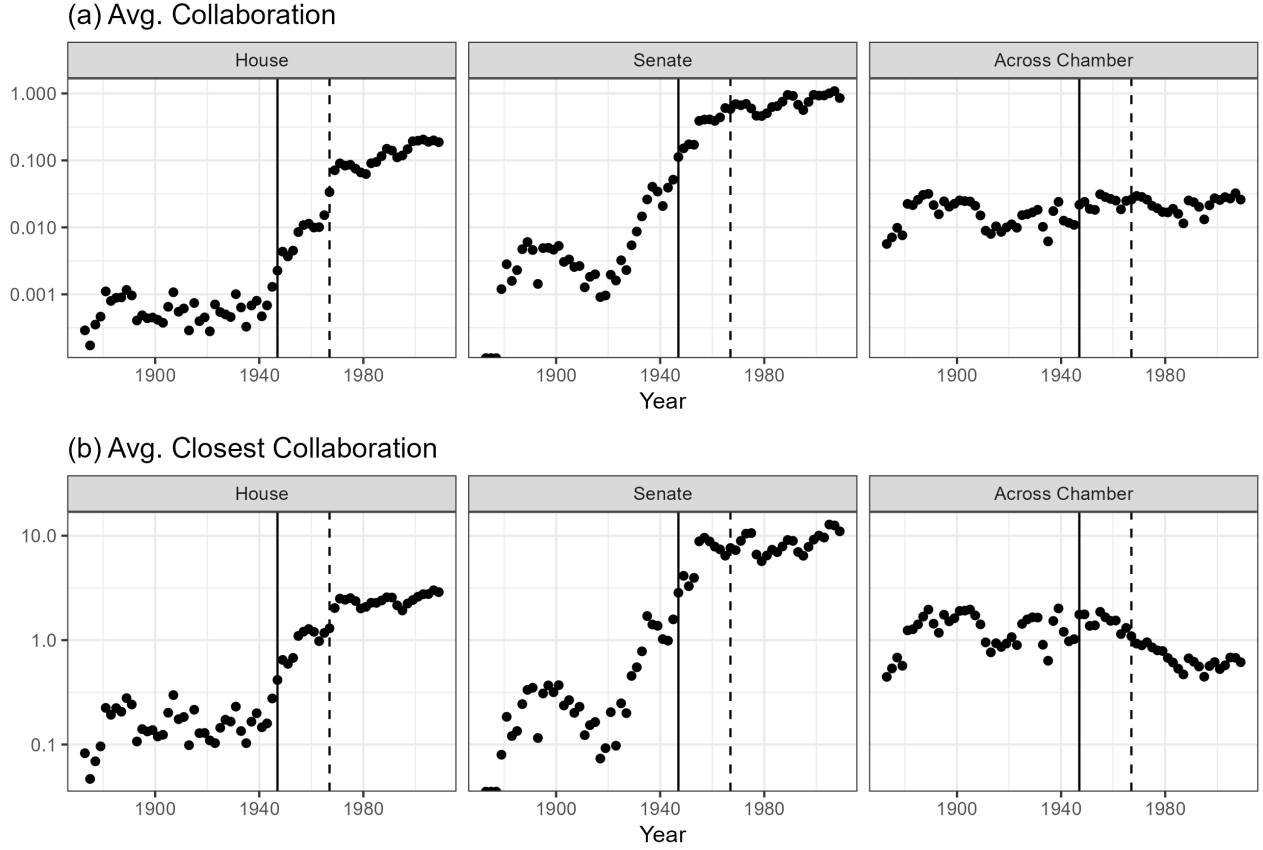
how it responded to changing institutional incentives. This is especially true given that our operationalization requires within-chamber duplicate bills to be introduced within seven days to be counted in our measure.

Turning to our member-level data, we consider the average level of collaboration between members, i.e., the average of all dyad-level scores in a congress. We expect this to be relatively low insofar as most pairs of members do not meaningfully collaborate with each other. To show the extent of meaningful collaboration more clearly, we estimate the strength of a member's closest collaboration: For each member i , what is the value of their *strongest* bond with another member j in that congress? We expect this to be considerably higher insofar as we expect that most members collaborate with *someone* in a given congress. Figure 4 shows the results where the 80th Congress (1947-1948) is labeled with a solid black line as this corresponds to the major change in legislative organization associated with the Legislative Reorganization Act discussed in Appendix A.2. Because of the large trend, we show the y -axis on the log scale.

Starting with across-chamber collaboration, the results are striking. Even in the oldest period, there is clear evidence of across-chamber collaboration. Thus, our measure provides the first evidence that the practice of companion bills is not new but has existed systematically from at least the 1870s onwards. When we look at within-chamber collaboration, we see a surge in the Senate in the 1930s, corresponding to the rise of formal original cosponsorship. Both chambers see a jump following the reforms of the Legislative Reorganization Act (i.e., in the 80th Congress; solid black line) with a surge that continues through the subsequent decade. While the House does see a jump following the introduction of formal cosponsorship in 1967 (dashed black line), the major increase in the House before then shows that the change in collaboration, as measured through original cosponsorship, is not purely a mechanical artifact of changes in the rules around cosponsorship.

Validating our member-level measure with convergent validation (Adcock and Collier, 2001) is challenging as no systematic list of which members are close collaborators exists. However, Appendix B.8 explores which pairs of members collaborate the most. The ten highest scoring

Figure 4: Trends in Collaboration



Note: The solid black line corresponds to the 80th Congress, i.e., following the Legislative Reorganization Act of 1946. The dashed black line refers to 1967, i.e., when cosponsorship was permitted in the House.

dyads often consist of chairs and ranking members on the same committee as well as chairs of mirroring committees in the House and Senate. This seems reasonable and suggests further face validity to our measure.

5 Constituency Interests and Legislative Collaboration

We now turn to systematically testing our hypotheses from Section 2 by examining how well dyadic covariates (e.g., both members being from the same state) predict the level of collaboration between members. We estimate dyadic regressions of the following form

$$y_{i,j,t} = \mathbf{x}_{i,j,t}^T \boldsymbol{\beta} + \alpha_{i,t} + \alpha_{j,t} + \epsilon_{i,j,t}, \quad (2)$$

where $y_{i,j,t}$ represents the level of collaboration between member i and member j in congress t . To adjust for unobserved confounding, we include member-congress fixed effects ($\alpha_{i,t}$); because of the dyadic structure, the same parameter is used for member i and member j . This means that, unlike other analyses of collaboration (e.g., Kirkland and Kroeger 2018), we explicitly control for all variables that are constant within a member-congress as we are mostly interested in the *dyadic* covariates that explain collaboration. To adjust for correlation across dyads (e.g., $y_{i,j,t}$ and $y_{i,k,t}$ share member i), we use dyad-robust standard errors (Aronow, Samii and Assenova, 2015).¹³ All models include controls for additional factors noted in Section 2 (i.e., whether two members are from the same party, their ideological distance using the first dimension of DW-NOMINATE, whether both are in the majority, the number of prior terms they had both served in Congress together); Appendix C.2 contains the full empirical results including those controls.

As Figure 4 shows, our outcome variable has changed considerably over time, especially for

¹³Our theoretical focus on dyadic predictors and desire to control for unobserved member(-congress) confounding motivates our use of dyadic regression. Appendix C.1 discusses other methods of analyzing network data.

within-chamber collaboration. For this reason, and to understand if the effects we find persist and are stable across time, it is important to estimate different models in different time periods as well as to adjust the scale of the outcome to be interpretable across periods. To do this, we split our data into periods following those in Chiou and Goplerud (2024); Appendix A.1 provides brief details on the key events defining each period. In each period and for each type of collaboration (House, Senate, Across Chamber), we standardize the outcome variable to be mean zero and variance one. Thus, the effects can be interpreted in relative terms using z -scores, i.e., a coefficient of one corresponds to a one standard deviation increase in the expected outcome.

Table 3 begins by showing the effects of geographic proximity, ordered following the expected magnitudes from Hypothesis 1. To benchmark the magnitude of the effects, we also include the estimated coefficient for members serving on the same committee. We only include this for the House and Senate as we are not able to harmonize jurisdictions across the two chambers for the entire time period.

There is clear evidence supporting Hypothesis 1 within and across chambers. In all periods and for all types of collaboration, the largest effect is from being from the same state (Across Chamber and Senate) or from being from the same state and physically adjacent (House). The effect is substantively large, often increasing the predicted level of collaboration by more than one standard deviation. The effect is often many times larger than serving on the same committee—a key variable that also facilitates collaboration (e.g., Craig 2023). We also see smaller, but consistently significant effects of being geographically proximate but *not* from the same state delegation (e.g., “Nb. State”) and these are larger if there is physical proximity (e.g., “Adj. & Nb. State”).

Both within and across chambers, Table 3 also shows the (standardized) effect sizes have generally increased over time. In the House, where we have the most variation in geographic proximity to examine, we see that the relative importance of being from the same state has grown over time. With the exception of the pre-1888 period, “Adj. & Nb. State” (i.e., physically neighboring but not in the same state) was often statistically significantly larger or indistinguishable

Table 3: Standardized Effects of Geographic Proximity

	House					
	Early (1873-1888)	EPG (1889-1910)	Inter-War (1911-1946)	Textbook (1947-1974)	Reform (1975-1994)	Modern (1995-2010)
Same Committee	0.01* (0.01)	0.02*** (0.00)	0.03*** (0.00)	0.36*** (0.02)	0.51*** (0.01)	0.34*** (0.01)
Not Adj. & Nb. State	0.02*** (0.01)	0.02*** (0.00)	0.02*** (0.00)	0.08*** (0.01)	0.13*** (0.01)	0.11*** (0.01)
Adj. & Nb. State	0.04 (0.02)	0.14*** (0.05)	0.17*** (0.03)	0.43*** (0.03)	0.57*** (0.03)	0.68*** (0.04)
Not Adj. & Same State	0.13*** (0.03)	0.11*** (0.02)	0.10*** (0.01)	0.42*** (0.02)	0.68*** (0.02)	1.07*** (0.03)
Adj. & Same State	0.29*** (0.05)	0.50*** (0.07)	0.37*** (0.03)	1.12*** (0.05)	1.76*** (0.05)	2.33*** (0.06)
	Senate					
		Pre-Direct (1873-1914)	Post-Direct (1915-1946)	Textbook (1947-1974)	Reform (1975-1994)	Modern (1995-2010)
Same Committee		-0.01 (0.01)	0.04*** (0.01)	0.15*** (0.01)	0.26*** (0.02)	0.19*** (0.01)
Adj. & Nb. State		0.04*** (0.02)	0.09*** (0.01)	0.23*** (0.01)	0.31*** (0.02)	0.31*** (0.02)
Adj. & Same State		1.17*** (0.16)	1.80*** (0.23)	4.44*** (0.22)	4.62*** (0.20)	5.12*** (0.25)
	Across Chamber					
		Pre-Direct (1873-1914)	Post-Direct (1915-1946)	Textbook (1947-1974)	Reform (1975-1994)	Modern (1995-2010)
Not Adj. & Nb. State		0.04*** (0.00)	0.02*** (0.00)	0.04*** (0.00)	0.08*** (0.01)	0.11*** (0.01)
Adj. & Nb. State		0.05*** (0.01)	0.04*** (0.01)	0.09*** (0.01)	0.17*** (0.01)	0.22*** (0.01)
Adj. & Same State		1.96*** (0.07)	1.33*** (0.05)	1.38*** (0.05)	1.85*** (0.08)	2.49*** (0.11)

Note: *: $p < 0.10$; **: $p < 0.05$; ***: $p < 0.01$. The outcome is the (standardized) collaboration score between members in a given congress. “Adj.” stands for physically adjacent district; “Nb. State” is short for neighboring states. All models include member-congress fixed effects and the additional controls noted in the main text. Dyad-robust clustered standard errors are shown in parentheses. “EPG” stands for “Early Party Government.” Note that within the Senate, “Adj. & Same State” and “Adj. & Nb. State” reduce to both senators being from the same state or neighboring states, respectively. For across-chamber collaboration, “Adj. & Same State” reduces to both members being from the same state.

from “Not Adj. & Same State” (i.e., not physically neighboring but in the same state). However, for the periods covering 1975 to 2010, the effect on collaboration is statistically significantly larger for “Not Adj. & Same State.” Within the Senate, the (standardized) effect of “Same State” has increased dramatically across time. Before 1947, senators from the same state collaborated about 1-2 standard deviations more than senators with no geographic proximity. After 1947, however, the effect increases to around 4-5 standard deviations. For collaboration across chambers, the effects of “Adj. & Nb. State” and “Same State” increase from 1975 onwards, although the increase is more modest.

Appendix C.3 runs this analysis separately for formal and informal original cosponsorship. Hypothesis 1 is supported for both types of cosponsorship.

5.1 District Demographic Similarity

As noted in Section 2, it is challenging to know *a priori* which constituency demographics matter for member cooperation. To provide an initial test, we leverage data on constituency characteristics that predict member committee assignments and preferences (Adler and Lapinski 1997; Adler 2002); this data ranges from 1943 to 1998, fully covering the Textbook and Reform periods. Given that these are salient enough to affect the important choice of committee membership, it is reasonable to suspect they also matter for collaboration. However, it is not obvious which variables should matter for collaboration; instead, we compute a measure of overall similarity of the districts for each pair of members using the Mahalanobis distance calculated on a variety of variables measured in each congress (see Appendix C.4) and then multiply by -1 such that positive values indicate more similarity. We also standardize this variable to be a z -score, so a one unit change represents a one standard deviation increase in district similarity. Table 4 includes this as an additional covariate in the model, keeping the measure of geographic proximity from Table 3, its dyadic controls, and member-congress fixed effects.

The effects of both demographic homophily and geographic proximity are generally statistically significant for within-House collaboration and across-chamber collaboration. The effect of

Table 4: Standardized Effects of Demographic Similarity

	House		Senate		Across Chamber	
	Textbook (1947-1974)	Reform (1975-1994)	Textbook (1947-1974)	Reform (1975-1994)	Textbook (1947-1974)	Reform (1975-1994)
Same Committee	0.36*** (0.02)	0.51*** (0.01)	0.15*** (0.01)	0.23*** (0.03)		
Not Adj. & Nb. State	0.06*** (0.01)	0.11*** (0.01)			0.03*** (0.00)	0.07*** (0.01)
Adj. & Nb. State	0.39*** (0.03)	0.50*** (0.03)	0.20*** (0.02)	0.25*** (0.04)	0.05*** (0.01)	0.11*** (0.03)
Not Adj. & Same State	0.37*** (0.02)	0.62*** (0.02)				
Adj. & Same State	1.04*** (0.04)	1.63*** (0.05)	4.18*** (0.24)	3.61*** (0.35)	1.30*** (0.04)	1.56*** (0.10)
Demographic Similarity	0.08*** (0.01)	0.13*** (0.01)	0.08* (0.05)	0.10* (0.05)	0.38*** (0.04)	0.59*** (0.12)

Note: *: $p < 0.10$; **: $p < 0.05$; ***: $p < 0.01$. The outcome is the (standardized) collaboration score between members in a given congress. All models include member-congress fixed effects and the additional controls noted in the main text. Dyad-robust clustered standard errors are shown in parentheses. See Table 3 for information on variable abbreviations.

demographic homophily is only marginally significant (p -value below 0.10) in the Senate.¹⁴ Overall, the evidence supports Hypothesis 2, noting that demographic shared interests are associated with more collaboration even once geographic factors are taken into account.

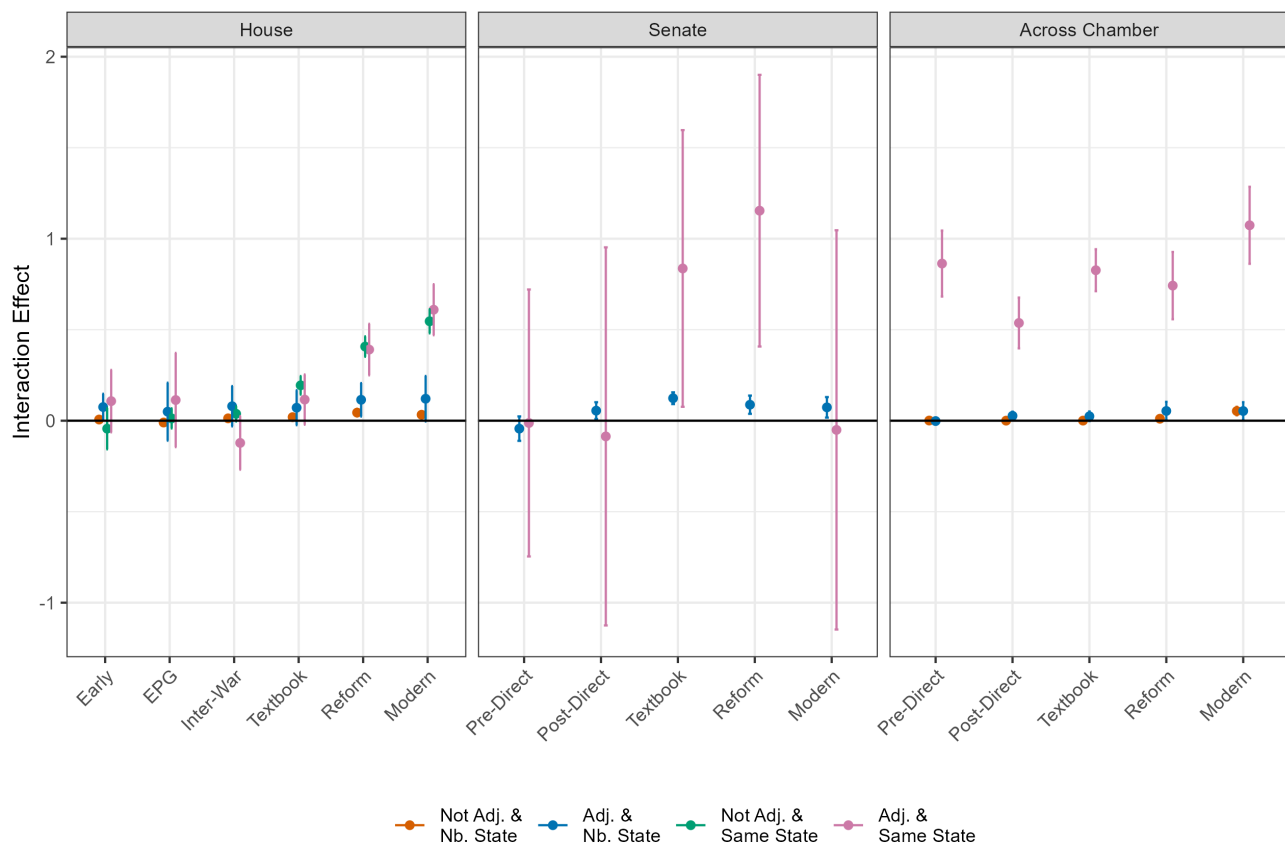
5.2 Moderation by Party

We finally examine how the above results are moderated by members being from the same party. To do this, we re-run the above regressions but interact the variable for constituency interests with whether the two members are from the same party. For simplicity, we focus on re-analyzing Table 3 with this interaction. To present the results compactly, Figure 5 shows the interaction effect, i.e., the increase in the estimated effect of geographic proximity by whether two members

¹⁴If the variables for geographic proximity are removed, demographic similarity is positive and statistically significant (p -value below 0.01) across all time periods and types of collaboration.

are from the same party versus the effect when they are not copartisans. Appendix C.5 reports the full table.

Figure 5: Standardized Interaction Effect Between Copartisanship and Geographic Proximity



The moderating effects of copartisanship have changed considerably across time. In the House, before the Reform period (1873-1974), there is limited support for Hypothesis 3; most of the interaction terms are insignificant and their estimated effects do not generally follow the ordering hypothesized. After 1975 (i.e., Reform and Modern periods), however, there is clear support for an interaction of all types of geographic proximity with copartisanship. The interactive effects for being in the same state (both adjacent and non-adjacent) are considerably larger than the effects for neighboring states but are not distinguishable from each other.

Turning to the Senate, we again only see systematic evidence for interactions with copartisanship for the Textbook and Reform periods, although the point estimates are generally larger

for same-state and same-party interactions. The lack of a significant effect in the Modern period, versus the significant positive effects found in the House at this time, provides some evidence for the countervailing effects discussed in Section 2.

Finally, looking at across-chamber collaboration, there are positive and statistically significant interactions between members being from the same state and same party throughout the entire period we observe. The magnitude is also substantively large in all periods, e.g., an interaction effect of around 0.5-1 standard deviations.

Appendix C.5 further shows that members from different parties with shared constituency interests continue to collaborate at statistically significantly higher rates than those without, generally in the order predicted by Hypothesis 1. Appendix C.5 interacts copartisanship with both measures of shared interests in Table 4; the results do not generally support the hypothesis that copartisanship intensifies the effect of demographic similarity.

5.3 Additional Analyses

First, to examine the role of distance between districts directly, Appendix C.6 operationalizes the intensity of shared interests with the physical geographic distance between districts, interacted with whether members are from the same, neighboring, or non-neighboring states. It finds results consistent with Table 3, demonstrating that members from the same state are much more likely to collaborate within or across chambers over historical periods. Additionally, closer districts are generally associated with more collaboration, especially from 1947 onwards. Appendix C.7 conducts an additional robustness test of the main results using different thresholds for collaboration in terms of temporal distance between bills. The results are robust to this choice.

6 Conclusion

Collaboration between members, both within and across chambers, is an essential part of the lawmaking process. We have provided a novel approach to quantify the extent to which members coordinated their efforts at the bill introduction stage from 1873 to 2010 by extending the measurement of original cosponsorship to a much longer historical period and to across-chamber collaboration by leveraging the fact that members can engage in informal original cosponsorship by introducing effectively identical bills.

A key finding from our empirical analysis is the remarkable historical stability in the determinants of which members collaborate and the relative importance of various shared constituency interests. Across all periods and types of collaboration, representing neighboring districts within the same state emerged as the strongest predictor of legislative cooperation, exhibiting a magnitude far exceeding that of any other variable. Furthermore, we show that while within-chamber collaboration appears to have increased substantially starting around the 1940s, across-chamber collaboration, i.e., by introducing companion bills, has existed at a remarkably stable level for the entire period we examine. Given the relatively limited empirical work on bicameral cooperation, this suggests a need for more detailed theoretical and empirical exploration.

Our paper suggests several other open areas for future research. First, while we focused on original cosponsorship, more work is needed to understand the rich system of informal cosponsorship that existed in the House before 1967 and that co-existed with formal cosponsorship in the Senate from the 1930s. Second, our work suggests that using more detailed measures of shared constituency interests might improve our understanding of legislative collaboration. In particular, refining our somewhat crude measures of shared district demographic similarity and physical proximity might lead to greater insights.

An important additional contribution of our paper is the creation of original cosponsorship networks from 1873 to 2010. Beyond our use of them to understand the determinants of dyadic relationships between members, the networks can be analyzed in their own right or used

to understand which members collaborate more overall, how such collaboration differentially affects bill success over time, and how collaboration shapes legislative effectiveness. Further, our methodological approach opens up the ability to understand other important questions in legislative studies, such as identifying the re-introduction of bills across subsequent congresses. More broadly, our paper provides the tools and data for scholars of American politics to examine legislative collaboration over the past 150 years.

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Supporting Information for “The Evolution of Congressional Collaboration”

A Additional Historical Information

This section contains additional historical information including a discussion of the periodization used in our empirical analysis, two major institutional reforms as well as four additional case studies on the introduction or elimination of formal cosponsorship.

A.1 Additional Details on Historical Periods

We rely on the periodization from 1873 to 2010 used by Chiou and Goplerud (2024). Table A.1 defines the periods and notes the key event that defines their start. For across-chamber collaboration, we use their Senate periodization.

Table A.1: Periodization of 1873 to 2010

<i>House</i>					
Early (1873-1888) (43rd-50th)	EPG (1889-1910) (51st-61st)	Inter-War (1911-1946) (62nd-79th)	Textbook (1947-1974) (80th-93rd)	Reform (1975-1994) (94th-103rd)	Modern (1995-2010) (104th-111th)
Early institutions	Reed Rules	Revolt against Cannon	Legislative Reorganization Act of 1946	Committee and party reforms	Republican Revolution
<i>Senate</i>					
Pre-Direct (1873-1914) (43rd-63rd)	Post-Direct (1915-1946) (64th-79th)	Textbook (1947-1974) (80th-93rd)	Reform (1975-1994) (94th-103rd)	Modern (1995-2010) (104th-111th)	
Early institutions	Direct election of senators	See above	See above	See above	

Note: “EPG” stands for Early Party Government.

A.2 Additional Details on Legislative Reforms

We briefly outline some additional detail on two major legislative reforms (direct election of the Senate; the Legislative Reorganization Act of 1946) that occur during the period we study. While the implications for collaboration are somewhat unclear, we draw out some possible implications from existing research.

Direct Election of the Senate: Prior to 1913, senators were elected by state legislatures rather than by popular vote. Despite this manner of election, senators used similar tools to those used by their colleagues in the House (bill sponsorship; committee work; etc.) to advance their careers and reputations (Schiller and Stewart, 2014). Indeed, Schiller and Stewart (2014) find that senators frequently introduced private claims or pension bills to cultivate electoral

support even before direct election, and that the primary drivers of sponsorship—including state population and committee assignments—remained remarkably stable before and after the adoption of the 17th Amendment. However, other work (e.g., Meinke 2008) has found that member behavior on bill sponsorship was affected by the introduction of direct election, and thus it is important to explore whether the determinants of collaboration change.

Legislative Reorganization Act of 1946 (LRA): This reform was a major change to the legislative organization of Congress, strengthening the committee system by consolidating standing committees and more clearly defining their jurisdictions (Davidson, 1990). In doing so, the average number of members on each committee increased, likely facilitating more frequent collaboration. Other features of the act, especially eliminating certain types of popular private bills (see, e.g., Adler 2002, ch. 5) and increasing staffing (Bolton and Thrower, 2016), may also have implications for collaboration by changing what lawmakers were incentivized to focus on and their capacity to do so.

A.3 Additional Case Studies

Because a core part of our argument is that identical or effectively identical bill introduction was seen as the analogous behavior to cosponsorship before it was formally permitted, we include four additional case studies on this point. We structure the case studies into three sections: First, we provide a comprehensive look at cosponsorship in the House from its formal prohibition in 1909, to its introduction in 1967, and its extension to unlimited cosponsorship in 1979. In all cases, members clearly and explicitly viewed duplicate bill introduction as either a replacement for cosponsorship (1909) or behavior cosponsorship was intended to replace (1967; 1978). Next, we provide an additional case study for the Senate—to complement the main one in the text around its prohibition. We explore Senator Dirksen’s self-described complaints *against* cosponsorship and his explicit suggestion of a return to a system of duplicate bill introduction. Finally, we provide more discussion on the idea of “stealing credit” discussed briefly in the main text; while this might complicate using effectively identical bill introduction as a measure of *cosponsorship* or legislative effectiveness, we argue it is unlikely to affect our measure of *original* cosponsorship.

A.3.1 Cosponsorship in the House

Banning Cosponsorship Formally in the House

In 1909, a small select committee was formed by passing a resolution (60 H.Res. 553) led by John Fitzgerald (D-NY) to investigate whether multiple members sponsoring a bill or resolution was permitted under the House rules. The inciting incident seems to have been the introduction of a resolution (60 H.Res. 551) with 29 cosponsors, although the committee referenced a second resolution with multiple sponsors. The committee’s report noted that a small number of cases of this had been observed in the current and prior congresses (“at least 10 bills and resolutions have been presented with the name of more than one Member attached thereto”; H.R. Rep. No. 60-2322). ProQuest also records a few instances of formal original cosponsorship around this period.¹⁵ The judgment of the committee was that this practice was inconsistent with the

¹⁵It lists 59 H.R. 25884, 60 H.R. 14413, 60 H.R. 21357, 60 H.R. 21539 and 60 H.R. 27522.

existing rules, and it also noted an incident under Speaker Reed in which a multiply sponsored memorial was adjusted by the Speaker to ensure that only one sponsor remained.

When the resolution was debated (*Congressional Record* 1909, 3808–3811), the main interesting implication—besides Fitzgerald denying that the ruling was singling out H.Res. 551 in particular—was an exchange between Fitzgerald and William Hepburn (R-IA) in which Hepburn pressed Fitzgerald as to whether the 29 sponsors introducing identical resolutions would be prohibited under the rules. Fitzgerald noted that nothing in the rules would prohibit this, and then Hepburn made the following, rather prescient remark:

And that course might have been pursued; and the gentleman [Fitzgerald] reports now, as I understand, that it would be better practice that 29 gentlemen each for himself should have introduced this bill and required the printing of it, and then left the record to be made with regard to each one of them, and that that would be the better practice than the 29 uniting in one in order to save the extraordinary printing of 28 of these bills?

Fitzgerald demurred and the committee’s report was agreed to by voice vote, creating a binding interpretation banning cosponsorship. Hepburn’s quote again is a formal acknowledgment of the equivalence between cosponsorship and introducing duplicate legislation, nearly 60 years before cosponsorship was formally permitted in the House, as well as acknowledging the issue (the cost of printing many duplicate bills) that was a key factor in the decision to formally allow cosponsorship.

Creation of the Cosponsorship System in the House

Over time, the system of duplicate bill introduction evolved to not only encompass duplicate bills introduced at the same time (e.g., mirroring original cosponsorship), but also a rich system where members could signal support for legislation after its introduction by introducing a duplicate copy or an effectively identical bill. The sheer number of duplicate bills eventually rendered this system untenable in the House as illustrated by the debate in 1967 around permitting cosponsorship (*Congressional Record* 1967, 10708–10712). The opening remarks of the sponsor of the resolution (William Colmer, D-MS), chair of the Rules Committee, clearly illustrate that formal cosponsorship was *explicitly* designed to replace duplicate bill introduction: “Under the present system, there may be 25, 30, 50 or even 100 Members of the House who want to introduce a similar bill. As a result of this desire to cosponsor a bill or a resolution, each Member introduces one.” The main reason for a change, argued Colmer, was reducing the cost of typesetting and printing so many duplicate bills.

The bulk of the debate consisted of clarifications about the particular implementation of the rule as well as a number of members extolling the cost-saving advantages of the proposed change in terms of reducing printing costs associated with duplicate legislation. For the purposes of understanding how duplicate bill introduction was viewed, there was one particularly useful exchange. When asked about the ability to add additional cosponsors after the bill was introduced, Colmer was clear that this was not permitted and said, instead, that the member would have to introduce a subsequent (identical) bill in order to do so. This clearly suggests that, in the era of limited cosponsorship, duplicate bill introduction served as a clear precursor to the modern system of cosponsorship—both original and post-introduction. After a relatively short discussion, the resolution passed without objection. Formal cosponsorship was not wholly

uncontroversial (see, e.g., Senator Dirksen’s self-described “one-man crusade” to eliminate it in the Senate, discussed below), but it quickly became widely adopted in the House.

Permitting Unlimited Cosponsorship in the House

After the introduction of limited cosponsorship in 1967, the number of duplicate or effectively identical bills being introduced did not considerably decrease (see, e.g., H.R. Rep. No. 95-1612, 1978; Thomas and Grofman 1993). This was a key factor in further efforts to reform the system. In 1978, the House considered a resolution to allow unlimited cosponsorship, with additional changes around permitting cosponsors to be added after introduction and a procedure for removing cosponsors. The resolution’s changes would go into effect in the following congress (96th Congress; 1979-1980). We note, in passing, that this resolution (95 H.Res. 86) itself illustrates the complex patterns of duplicate bill introduction and cosponsorship that existed during this period: The sponsor (Burton; D-CA) said on the floor that he thought there were 27 versions of the resolution introduced (*Congressional Record* 1978, 34930), but the legislative calendar (H. Rules Comm., Legis. Cal. No. 2, 1978) lists 10. These 10 versions were *all* introduced by one of two people (Burton; D-CA; Ketchum; R-CA), seemingly to signal additional support for the resolution.

The committee report and debate mostly stressed previously discussed factors, e.g., the cost of introducing duplicate legislation, confusion for constituents from having identical bills with different numbers, etc. As in the case of the debate to permit cosponsorship, the opening speech (Gillis Long; D-LA) was explicit about the equivalence between cosponsorship and duplicate bill introduction: “The result [of the cap on the number of cosponsors] is that a measure with many cosponsors may be repeatedly introduced in order that all the cosponsors may have their names on the legislation” (*Congressional Record* 1978, 34929).

Interestingly, it was stressed in both the report and the debate that the resolution “does not prohibit the introduction of measures identical to cosponsored ones, but it makes them unnecessary” (*Congressional Record* 1978, 34930; see an almost identical phrasing in H.R. Rep. No. 95-1612, 1978, p. 4). In fact, the historical trajectory shows that as soon as this resolution was passed, formal cosponsorship almost immediately replaced the practice of duplicate bill introduction in the House (see, e.g., Thomas and Grofman 1993 and Figure 3 in this manuscript).

Looking at the committee report that accompanied this resolution (H.R. Rep. No. 95-1612, 1978), a letter from the Clerk of the House notes a practice where, after cosponsoring an original bill, members introduce another duplicate version with only their name on it, saying that “this seems to be done because Members wish to provide their constituency with the indicator of interest in and support for a particular bill, especially when the bill becomes a national or ‘popular’ issue.” During our reading of many sets of effectively identical bills, we also noticed this similar practice of the original (co)sponsor introducing the same bill repeatedly weeks or months later with either additional (new) cosponsors or, seemingly, to attempt to gather support for the legislation.

This confirms that, beyond our focus on the use of effectively identical bills to signal *original* cosponsorship (i.e., duplicate introduction around the time of the initial bill), there is a rich and complex system of more general cosponsorship that future work might explore.

A.3.2 Complaints around Cosponsorship in the Senate

In the Senate, the practice of cosponsorship was sometimes controversial with some senators seeking to eliminate the practice. We focus here on one figure, Everett Dirksen (R-IL), who described himself in 1961 as having been on a “one-man crusade” against the practice (*Congressional Record* 1961, 1104–1105).

Dirksen’s complaints were echoed by other members (including in the House) and primarily revolved around (i) being pressured by a colleague to cosponsor a bill; (ii) cosponsoring bills that one had not fully read; (iii) the awkwardness of voting against a measure that one had cosponsored, perhaps after it had been amended. It is worth analyzing the debate in 1967 around Dirksen’s amendment to the proposed Legislative Reorganization Act of 1967 that would have eliminated cosponsorship, with an exception for “bills and resolutions of local interest (affecting only one state or providing for interstate compacts) and for private relief” (*Congressional Record* 1967, 3725–3732).

This debate is particularly interesting because Dirksen *explicitly* suggested going back to a system of duplicate introduction to replace cosponsorship. For example, in response to a question about whether eliminating cosponsorship would hurt bipartisanship, Dirksen said “a Democrat can introduce a bill and a Republican can introduce the same bill, and we would still have bipartisanship without dual sponsorship.” Others also clearly viewed a return to duplicate bill introduction as the inevitable consequence of eliminating cosponsorship.

The amendment was defeated by a wide margin (18-48), although for some time afterwards Dirksen or a colleague objected to unanimous consent requests for bills to be held at the desk in the Senate to solicit additional cosponsors.

A.3.3 “Stealing” Credit Using Duplicate Versions

As noted in the main text, contemporaneous sources in the 1960s report a concern about the system of duplicate introductions where someone might “steal” credit from another member. Specifically, imagine that fifty duplicate versions of a bill were introduced. There is nothing requiring that the committee report out the first version; rather, any version could become the vehicle for the legislative project.

Various debates around cosponsorship noted this point. For example, in a debate in 1967 in the Senate around eliminating cosponsorship, various members noted that some chairs in the House would effectively introduce an identical version after the original one and then report that version, leaving the author behind and claiming credit for it (*Congressional Record* 1967, 3725–3732). Clapp (1963)’s contemporaneous discussion of cosponsorship also notes this concern as well as one where majority members might take credit for the work of a minority-party member (pp. 140-144), although he noted that some chairs were better about doling out legislation to rank-and-file members than others.

Exploring this in detail is outside of the scope of this paper; however, it would not affect our measure of original cosponsorship as we look at who introduces the *earliest* copy of the bill and define collaboration as introducing a bill in close proximity to that version (see Appendix B.7). It suggests, however, that if one sought to update legislative effectiveness from being based on a sponsor (e.g., Volden and Wiseman 2014; Chiou and Goplerud 2024) to being sensitive to insertions into enacted legislation (e.g., Kroeger et al. 2024) in this period, one would need to

take account of this dynamic.

B Identifying Effectively Identical Bills

This appendix describes in detail each step of our process for identifying effectively identical bills as well as providing more information about our validation of each step.

B.1 Definitions of “Companion Bill”

As noted in the main text, there is no precise formal definition of a “companion bill.” We include several definitions below to show they are all rather similar and tap into the notion of nearly or effectively identical legislation.

Congressional Research Service (Sullivan, 2007): “a bill introduced in one House of Congress that is similar or identical to a bill introduced in the other House of Congress.”

congress.gov: “a measure that is identical or substantially similar to a measure introduced in the other chamber.”

Commerce Clearing House’s *Congressional Index*: Discussed in detail in Appendix B.4.2, this is a contemporaneous guide to Congress published starting in 1945. The definition (see Figure A.1) used says “Many times, identical or similar measures are introduced simultaneously in both chambers. These are called companion bills and are introduced to permit Committees in both houses to work on them at the same time.”

Existing Academic Studies: The two studies of companion bills in Congress of which we are aware (Kirkland and Kroeger, 2018; Casas and Wilkerson, 2025) both rely on the Congressional Research Service definition above, though Kirkland and Kroeger (2018) perhaps place more emphasis on the bills being identical. Casas and Wilkerson (2025) identify companion bills using a threshold of 95% shared unigrams and 80% shared bigrams.¹⁶

Taken as a whole, the definitions share the idea of a bill being quite similar—not merely in topic but in specific legislative language—to be classified as a companion. However, none of the definitions seem to require the bills to be word-for-word identical. We thus interpret this as requiring companions to be quite similar in their specific legislative language but some deviation is acceptable. We return to this in Appendix B.5 for our LLM prompt to detect effectively identical bills and Appendix B.3 where we discuss the training protocol for our RA who hand labeled thousands of pairs of bills.

B.2 Summarization Prompts

As noted in the main text, a key innovation of our methodological approach is clean text summaries of each bill from the raw, messy scans provided by ProQuest. This is done using a large language model (GPT-4o-mini), using batch processing through Microsoft Azure OpenAI API, under an enterprise agreement through an author’s university which is approved for use with non-public university data. The prompts used to generate the summaries are shown below. For

¹⁶Looking at the case of insertions or “hitchhikers,” Kroeger et al. (2024) use a ratio of 60-70% of the 5-gram shingles from the introduced bill must be found in the enacted law.

bills that were detected to be short (i.e., below 300 tokens after some cleaning), the first prompt was used. For bills that were detected to be long (i.e., above 300 tokens), the second prompt was used. They differ in the text highlighted in red.

- Short Prompt

You are a helpful assistant who recalls information about the United States Congress from sources such as the Congressional Record, Federal Register, Congressional Research Service, and academic articles on the US Congress.

I will give you the text of a bill that was introduced into the US Congress. Your job is to create an informative concise summary of the bill of around 50-100 words that conveys what the bill is about. As the bill you will read is rather short, your summary may also be very concise.

This summary should not mention the bill number or the Congress of introduction and should always start with "This bill aims".

Please note that the bill text comes from an OCR of a PDF document so there may be typos and messy characters that you should address when creating the summary. You should ignore information about the sponsor, when the bill was introduced, and the passage of the bill. Rather, you should focus *only* on summarizing the policy proposed in the bill.

In rare instances, the bill will be illegible and not contain any information that can be summarized. In this case *only*, your summary should be "ILLEGIBLE BILL" in all capital letters.

- Standard Prompt:

You are a helpful assistant who recalls information about the United States Congress from sources such as the Congressional Record, Federal Register, Congressional Research Service, and academic articles on the US Congress.

I will give you the text of a bill that was introduced into the US Congress. Your job is to create an informative concise summary of the bill of around 200-300 words that conveys what the bill is about. This summary should not mention the bill number or the Congress of introduction and should always start with "This bill aims".

Please note that the bill text comes from an OCR of a PDF document so there may be typos and messy characters that you should address when creating the summary. You should ignore information about the sponsor, when the bill was introduced, and the passage of the bill. Rather, you should focus *only* on summarizing the policy proposed in the bill.

In rare instances, the bill will be illegible and not contain any information that can be summarized. In this case *only*, your summary should be "ILLEGIBLE BILL" in all capital letters.

The cost of the summarization was around \$200-300 when it was applied to 1.6 million documents. We summarized all versions provided by ProQuest but focused on the primary

version, i.e., the one corresponding as closely as possible to an introduced version—or, in the absence of this, the oldest available version, in our subsequent analyses.¹⁷

The exact wording of the summaries vary when they are recomputed given the non-deterministic nature of LLMs. To check how this affects our similarity scores, we took a random sample of 20 bills per year from 1873 to 2010 and re-summarized them with the same prompts as described above. Using the sentence embeddings (Reimers and Gurevych, 2019), the median similarity score is 0.975. The lower 10th percentile is 0.944. Thus, despite some differences in wording across repeated summarizations, the summaries are highly semantically similar.

Palmer, Smith and Spirling (2024) suggest that papers that use proprietary large language models justify this choice versus using an open-source model. There are two major reasons for doing so in this project. First, our bills range considerably in their tokenized length with some being quite long. Thus, we need a model that has a large context window. GPT-4o-mini’s context window (128,000 tokens) covered the vast majority of bills; to ensure that all bills can be processed alongside the prompt, we pass the first 115,000 tokens of every document; only 0.07% of documents hit this cap. Second, we have an enormous number of bills to summarize (1.6 million). It was rather straightforward to use batch processing to summarize the bills; generally, an entire year of data could fit into a single batch. This could be done on a normal desktop as the computational demands were offloaded to Microsoft Azure and the main bottleneck was waiting a few hours for the summaries to be provided.

B.3 Human Annotator

In many of the following validations, we use a single RA who was carefully trained to compare two bills and annotate them as being effectively identical. To do this, we shared the prompt used for the LLM to identify effectively identical bills (Appendix B.5) and went over numerous examples with the RA. We found, as with the LLM, having the RA provide a continuous measure of confidence (0 to 100) and writing a one-sentence justification of the score was useful to help clarify when the RA was initially making mistakes and quickly understand why a particular score was given. After reviewing various examples with the RA, some general themes were developed for whether bills are effectively identical and for what score to give:

- If the bills are narrowly scoped, localized or personalized, bills (e.g., building a single bridge or renaming a post office), if they do not refer to the same person or location, then they are certainly not effectively identical and should receive a very low score.
- If the bills are quite similar but differ in a specific numerical amount (e.g., allocating \$400 vs. \$500), they should be given a score of 85 or 90.

¹⁷Specifically, we filter to versions of the bills that have titles. Then, we attempt to take a version that corresponds to the bill as introduced or without amendments (i.e., IS [Introduced Senate], IH [Introduced House], RTS [Referred to Committee Senate], RTH [Referred to Committee House], OPS [Ordered to be Printed Senate], OPH [Ordered to be Printed House]), taking the earliest version. 99.4% of bills have a version of this type that is extracted. If none of the versions have that designation, we take the earliest version (with title) of the remaining versions, with the exception that some versions that typically correspond to amended versions are only used if no other version exists.

- If the bills are quite similar but one adds or removes a section, they should be given a score of 85 or 90. Exactly *how* much change is permitted before they are no longer effectively identical is challenging and requires (often) detailed qualitative knowledge, and reasonable observers could differ. If the changes are quite extensive, give the pair a lower score such as 80.
- The highest scores of 95 or 100 should only be used for bills that are *obviously* identical, differing in only small OCR or typographical errors.

When annotating bills, we provided the RA with two pieces of information. First, they were given the titles of the respective bills from ProQuest as this is a useful quick heuristic for determining if they are effectively identical. Second, using `diffchecker`, we provided the RA with an HTML of the text of two bills. This displays them side-by-side and attempts to align the text, noting visually whether text appears to have been inserted in one that does not appear in the other. We found this was extremely useful in reducing the cognitive burden to the RA of coding thousands of pairs of bills.

On the question of inter-coder reliability, we believed it was better to use our resources to train one RA to an expert standard and ensure that they were able to make high-quality annotations. We have worked with this RA for nearly one year in labeling thousands of pairs of bills, and thus we are confident that they provide generally high-quality annotations beyond what we could do if using a series of shorter-term contracts for a larger pool of RAs. Further, we never use the RA codings in our predictive model and thus while we view them as close to a human “gold standard” (with, of course, some idiosyncratic errors to be expected), if there is systematic bias in the RA’s coding, it would not affect our empirical results.

B.4 Initial Screening and Validation

As noted in the main text, our initial screening step selected only pairs whose similarity score is above 0.80. This similarity score is computed by the following procedure: For each pair, compute a similarity score $s_{i,j}^{\text{title}}$ based on the sentence embedding (Reimers and Gurevych, 2019) of their title as provided by ProQuest. If it is missing, code it as NA. Using the summaries from Appendix B.2, similarly create $s_{i,j}^{\text{summary}}$. Define $s_{i,j}^{\text{max}}$ as the maximum of the two values, ignoring missing values. If $s_{i,j}^{\text{max}}$ is over 0.80, the pair passes the initial screening.¹⁸

As noted in the main text, we systematically validate this initial screening in two ways. We provide more details on the human validation and then describe a new corpus of data on identical bills from Commerce Clearing House’s *Congressional Index* (described below) that is used in the evaluation of the Initial Screening and the Primary Classification.

B.4.1 Human Validation of Initial Screening

As noted in the main text, we used our expert RA to hand label a stratified random sample of 360 pairs. We stratified the sample on $s_{i,j}^{\text{max}}$ ([0-0.2, 0.2-0.4, 0.4-0.6, 0.6-0.8, 0.8-0.9, 0.9-1]) and

¹⁸Of all pairs, 1% are missing the title similarity score ($s_{i,j}^{\text{title}}$) and 0.25% are missing summary similarity scores ($s_{i,j}^{\text{summary}}$). Only 0.002% of pairs are missing both. Some bills (3%) are missing from ProQuest entirely and are excluded from our analysis.

congress ([43rd-57th, 58th-71st, 72nd-85th, 86th-101st]) and drew approximately 15 pairs from each stratum. None of the 300 pairs in the [0-0.2, 0.2-0.4, 0.4-0.6, 0.6-0.8, 0.8-0.9] strata were coded as effectively identical by the RA. 10 of the 60 pairs (i.e., 16.7%) in the 0.9-1 stratum were coded as effectively identical. Before 1947 (i.e., 43rd-79th Congresses), 8% were coded as effectively identical; after 1947 (i.e., 80th-111th Congresses), 32% were coded as effectively identical. This provides further evidence that the 0.8 threshold is unlikely to eliminate any truly (effectively) identical pairs. We thus dedicated most of our RA’s labor to validating the second stage of our process (primary classification).

B.4.2 Commerce Clearing House

In the process of looking for sponsors of legislation for a previous project, we discovered that Commerce Clearing House’s *Congressional Index*—a contemporaneously published guide to the activities of Congress—systematically reported in each congress a set of (a) identical bills in the House and (b) companion bills [i.e., across chamber]. Figure A.1a shows a preface to this section from the 91st Congress and Figure A.1b shows an example of the partial page from each section.

Figure A.1: Example Data from CCH

(a) Description of Section from CCH

(b) Example of Companion and Identical Bills

COMPANION BILLS—IDENTICAL HOUSE BILLS

Table of Contents	
Companion Bills	4803
Identical House Bills	4831
Identical Bills Not Printed	4871

Many times, identical or similar measures are introduced simultaneously in both chambers. These are called companion bills and are introduced to permit Committees in both houses to work on them at the same time. House bills with Senate companions are listed in this Division. For Senate bills with House companions, see the Division on "Companion Bills."

An identical bill is introduced when one member introduces a bill which is a word-for-word reproduction of another. Some of these are introduced because a member wants to associate himself with a measure already printed; an identical bill is introduced with his name as sponsor.

The rules of the House also give rise to some identical bills. Only 25 sponsors are permitted for a measure, and, if more want to be added, they must introduce a separate bill.

Sometimes a bill will not be printed by the Government Printing Office; in this case, the Joint Committee on Printing issues a slip mentioning the fact.

Companion Bills

H 2—S 2298	H 451—S 2627	H 1385—S 998
H 3—S 1164	H 453—S 103	H 2055—S 1539
H 7—S 1684	H 466—S 2741	H 2062—S 43
H 8—S 88	H 492—S 745	H 2067—S 998
H 16—S 1480	H 497—S 279	H 2071—S 2790
H 17—S 1480	H 511—S 998	H 2086—S 364
H 26—S 2663	H 525—S 1717	H 2129—S 278
H 35—S 1563	H 539—S 998	H 2145—S 2663
H 50—S 1717	H 551—S 1164	H 2146—S 2663
H 56—S 2663	H 552—S 998	H 2176—S 2301
H 81—S 1684	H 561—S 43	H 2183—S 369
H 83—S 328	H 568—S 1717	H 2190—S 2150
H 85—S 437	H 579—S 364	H 2193—S 654
H 86—S 436	H 599—S 524	H 2369—S 1867

Identical House Bills

Bills listed below are word-for-word reproductions of measures already introduced. All identical bills will be listed after the number of the measure first introduced. Each identical bill will also be mentioned, referring the reader to the bill summarized in "House Bills."

House Bills	26—2145; 2146; 6036; 8643; 9238; 13,605	80—2649; 2689; 4197; 4784; 6202; 11,027; 14,286
2—8445	30—12,129	81—7
3—180; 551; 995; 1184; 2734; 3288; 3332; 3801; 4629; 5245; 5495; 5574; 5814; 5869; 6027; 6243; 6548; 6555; 6757; 7475; 7501; 7969; 8152; 8406; 9713; 9853; 10,454; 11,993; 12,133; 12,721; 13,698	33—6628 34—6627 35—4847; 5533; 6182 40—5985 45—537; 1202; 2141; 5998; 8320 50—223; 232; 329; 525; 568; 673; 938; 2371; 2647; 9322; 9998; 14,607	83—340; 341; 876; 983; 1095; 3784; 3855; 3856; 3857; 3858; 3859; 5500; 6542 84—1093; 6476; 9515 85—5231; 6789; 7343; 7511; 7763; 7773; 8269; 9123; 9357; 9967; 11,251; 11,327;

The coverage of these sections varies with companion bills being reported from 1945 to 1980 (79th–96th Congresses) and identical bills being reported from 1969 to 1980 (91st–96th Congresses). Interestingly, the end date of CCH’s coverage closely follows the introduction of unlimited cosponsorship in the House (see Appendix A.3), and the sharp decrease in the number of identical bills (Thomas and Grofman, 1993), although we have not found evidence as to whether this was the reason for the section being discontinued. Further, its coverage of identical bills also nearly exactly coincides with the introduction of (limited) cosponsorship.

To turn this data into a set of identical bills, we digitized the pages and then performed some

light cleaning (i.e., ensuring that the numbers are in increasing order—see Figure A.1b—and if not, manually correcting them). From this, we extracted the implied network of identical bills. We must be careful to distinguish three issues here that might cause pairs to be inaccurately listed as identical. First, CCH did not publish a list of identical bills in the Senate. We infer such connections using logical implications (e.g., H.R. 1 and S.1 and H.R. 1 and S.3 being listed as companion implies that S.1 and S.3 are identical), although this clearly misses identical within-Senate pairs without a House companion. Second, if there is a typo in CCH—either by an OCR error in our digitization or by a printing error—this may introduce many mistakenly identical pairs. For example, imagine that a cluster of bills (H.R. 1 through 10) are detected as being identical as well as a second cluster with H.R. 100 through H.R. 115. If a mistaken link is drawn between H.R. 10 and H.R. 100, then using the logical implications will mistakenly connect many pairs together. As we note below, this appears to be the cause of most cases where a pair fails the initial screening but CCH codes it as an identical pair.

Third, as Casas and Wilkerson (2025) showed for the CRS list of companion bills, there are clearly cases where (exactly) identical bills are not recorded by CCH in their list. This is, perhaps, due to the enormous cost of doing this task manually. Thus, while we believe that the CCH dataset contains a large collection of (truly) effectively identical bills, we do believe it is likely missing a considerable number.

B.4.3 CCH Validation of Initial Screening

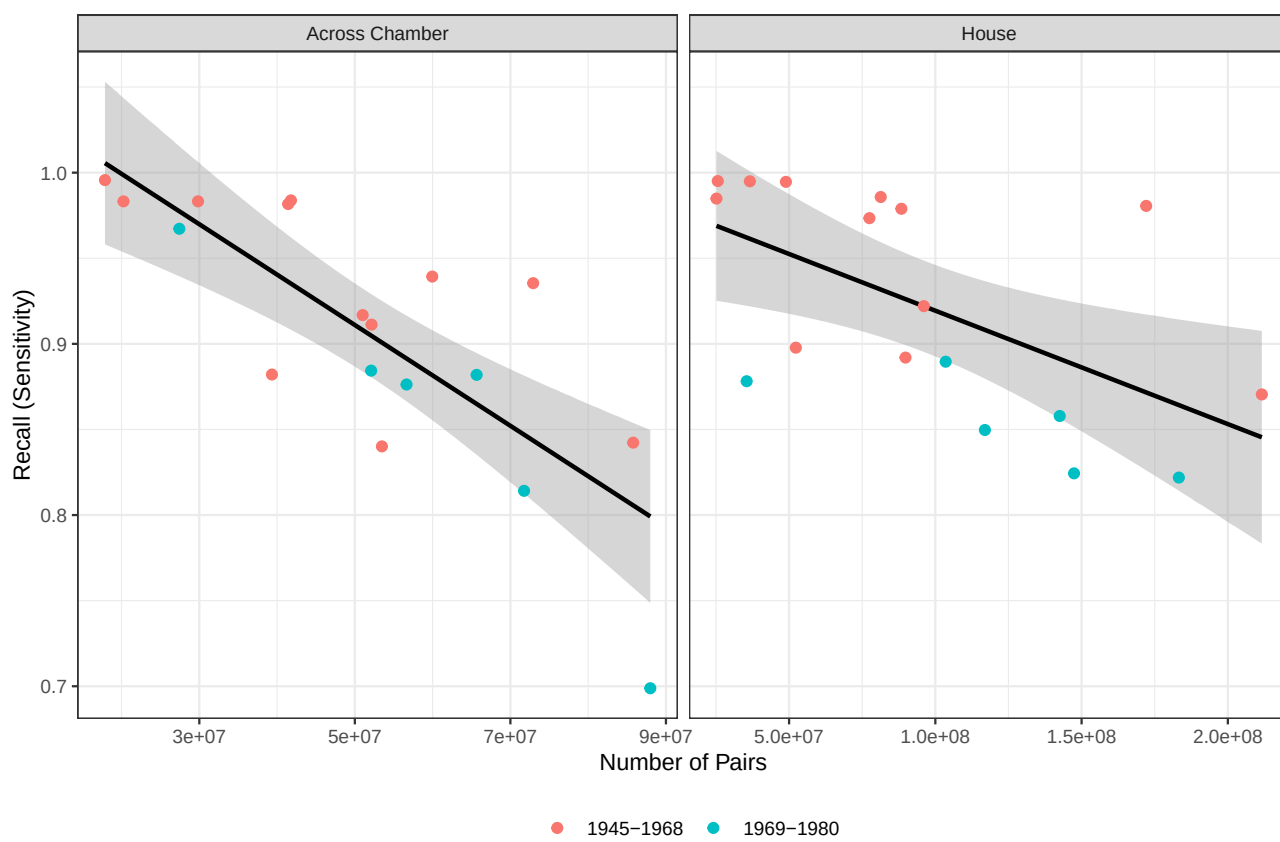
Using the CCH data as validation for the initial screening, we first focus only on within-House and across-chamber pairs, as CCH does not directly provide a list of identical bills in the Senate. Pooling across all congresses, the specificity of the measure (i.e., the proportion of pairs *not* in CCH that do not pass the initial screening) is 0.989. This confirms that the vast majority of pairs eliminated using the Initial Screening are, in fact, not identical using CCH’s judgment.

Our procedure also has a recall of 0.878 with CCH; this implies that about 12% of pairs that CCH codes as identical fail the screening. Given that effectively identical pairs are rare events, it would be worrying if we were losing 12% of them by using our initial screening. We carefully examined this to see if it was an issue with the Initial Screening or with the CCH data itself. We first note that the quality of recall for CCH degrades systematically after (a) cosponsorship was permitted in the House (1967-1980) and (b) as the number of pairs that must be examined grows. Figure A.2 plots the recall against the number of pairs of bills considered. In some sense, CCH was attempting a super-human task: If there are hundreds of millions of pairs of bills to compare, it is perhaps too much to ask that all of the (rare) effectively identical pairs are correctly identified!

Next, we had our expert RA code a stratified sample of 443 pairs of bills that were coded by CCH as identical but failed this initial screening (see Appendix B.3 for a discussion of the RA’s training and coding protocol). *None* were viewed by the RA as effectively identical. This is consistent with the results in the previous subsection (Appendix B.4.1) on a simple random sample of all pairs.

Together, this gives us confidence that the 0.88 recall of the initial screening against CCH is an overly conservative estimate and thus our method does not incorrectly eliminate truly (effectively) identical pairs of bills.

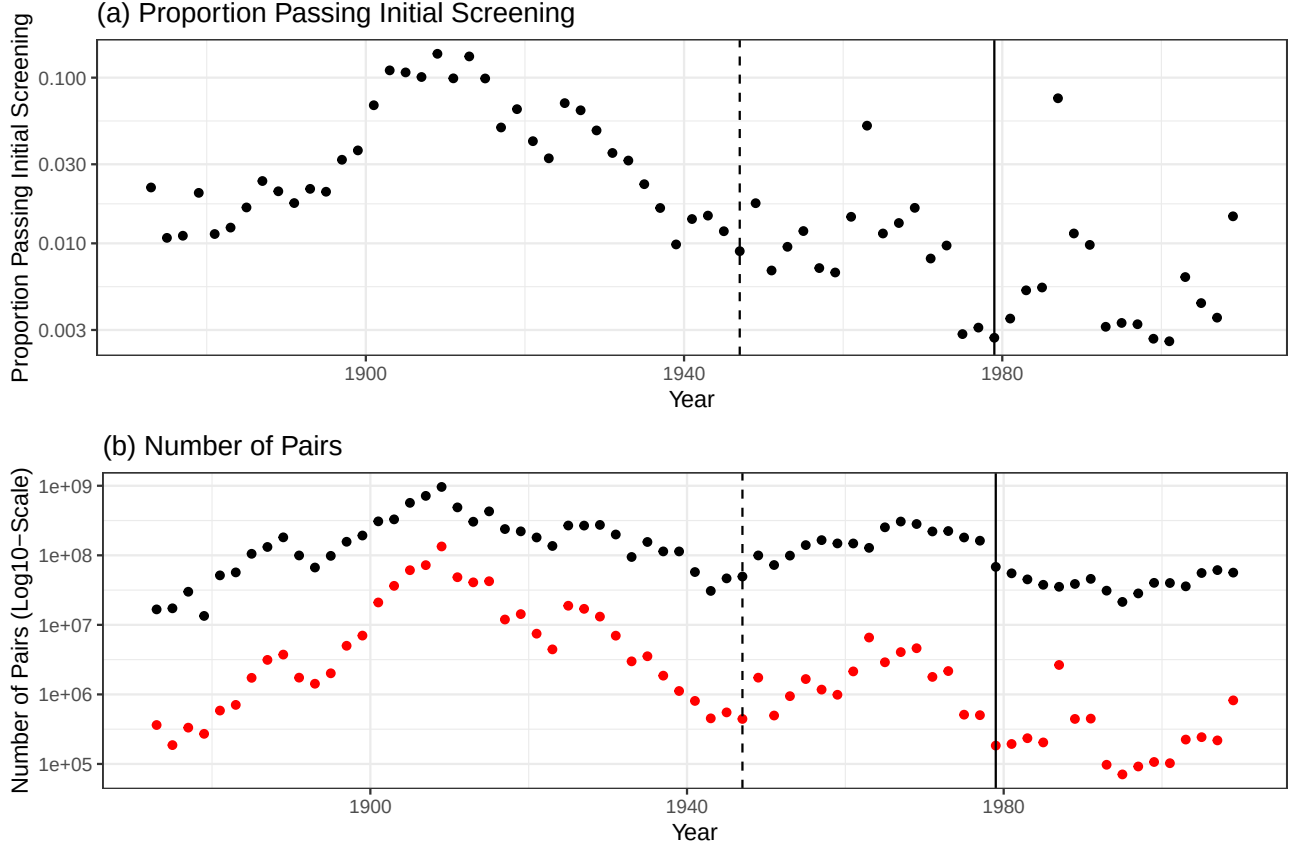
Figure A.2: Recall vs. Number of Pairs Considered



B.4.4 Impact of Initial Screening across Time

Figure A.3 shows how the initial screening reduces the number of pairs dramatically; across all of our congresses, there are 11 billion pairs to be examined. 94% do not pass this initial screening, resulting in around 629 million pairs to be analyzed. The number in each congress varies considerably over time—with a high period from around 1900 to 1920, where many private and claims bills (e.g., granting a pension for Mr. Smith and granting a pension for Mr. Jones) pass the threshold. This corresponds to the period with the most introduced bills.

Figure A.3: Number of Pairs Across Time



Note: The dashed line marks the 80th Congress (1947-1948), corresponding to the first congress after the Legislative Reorganization Act of 1946; the solid line marks the 96th Congress (1979-1980), corresponding to the introduction of unlimited cosponsorship in the House.

B.5 Primary Classification

For our primary classification, we proceeded in two steps. First, we used an LLM (GPT-4.1-mini) to compare the lightly cleaned texts of two bills to generate a judgment as to whether they were effectively identical. The light cleaning used simple regular expressions to mostly address the line numbers inserted between words. The prompt is shown below.

You will be provided with two bills introduced into the U.S. Congress that have been digitized from PDFs. Your task is to assess whether the bills are effectively identical in their legislative language.

You **must not** consider information about the sponsors, bill number, date of introduction, committee of referral or other "metadata" about the bill. Rather, you should focus **exclusively** on the text of the bill itself.

Further, the documents may have numerous OCR errors, artifacts or minor typographical errors. Do not let these influence your judgment as to whether the bills are effectively identical.

Your response should consist of two parts, formatted as follows:

" Score: [XX]

Justification: [one sentence] "

If the legislative text is effectively identical, assign a high score—with 100 indicating certainty that they are identical. A score of 0 should indicate bills that you believe are obviously **not** effectively identical. Use the "justification" to provide a one-sentence justification of your score.

The input is structured as

BILL 1:

[Text of Bill 1]

BILL 2:

[Text of Bill 2]

We experimented with different prompts and more precise definitions of "effectively identical" but our initial experiments found they did not perform as well as this more nebulous definition. We also found that asking the LLM to provide a justification helped generate more sensible scores. Our justification for using this LLM (e.g., Palmer, Smith and Spirling 2024) is similar to the one above, but especially noting that the task here is challenging so a state-of-the-art model is useful. We found that the cheaper GPT-4.1-nano performed much less well in our initial qualitative experiments.

Unfortunately, it was too financially expensive to use the LLM to label all approximately 630 million pairs that survive the initial screening. Thus, we rely on a sampling-based strategy. The next sub-section describes our sampling procedure and the following one describes how the predictive model was built.

B.5.1 Stratified Sampling for Training Data

It is clear that even in $\mathcal{A}_{\text{similar}}$, most pairs are not effectively identical. We can obtain a rough estimate of the prevalence of effectively identical bills by using the human coding of a stratified random sample (see Appendix B.4.1). From that analysis, if we roughly estimate that no pairs with $s_{i,j}^{\max} < 0.9$ are effectively identical and 32% of pairs in the $[0.9, 1]$ bin are effectively identical

(from 1947 onwards) or 8% of pairs (before 1947), we can roughly estimate the prevalence of effectively identical pairs using the proportion of all pairs with $s_{i,j}^{\max} \in [0.9, 1]$ in each period. This gives a rough estimate of the prevalence of effectively identical pairs at 0.05% before 1947 and around 4% after 1947.

Thus, a stratified sample is clearly needed to ensure that a reasonable number of positive examples are collected for training the predictive model. To do this, we use a different measure of similarity: $s_{i,j}^{\min}$, i.e., the minimum of $s_{i,j}^{\text{title}}$ and $s_{i,j}^{\text{summary}}$. This is only high if *both* the title and summary are similar and thus is a good way to select pairs to label. We broke this into four strata, $[0-0.8, 0.8-0.9, 0.9-0.95, 0.95-1]$. From 1945 to 1980, we instead stratified on whether the pair was implied to be identical by CCH.

We also stratified on whether the pair contained a bill that was likely to be a private claim (e.g., granting relief or a pension to a particular individual). These were extremely common before 1947 and their similarity (e.g., differing only in the name of the individual) means they dominate the set of pairs that survive the initial screening $\mathcal{A}_{\text{similar}}$. While we are interested in learning how to distinguish pairs involving these bills, drawing a sample at random would heavily overweight these pairs. Thus, we create a crude indicator for whether a bill is a claims bill if it was referred to one of four committees (“Invalid Pensions”, “Pensions”, “Claims”, and “War Claims”). After the Legislative Reorganization Act of 1946, these bills mostly disappear and thus we only stratify on the $s_{i,j}^{\min}$ for this period.

Overall, we wanted to draw 3,000 samples from each congress. Before 1945, we drew 2,000 pairs with no claims bills (evenly stratified by the categories of $s_{i,j}^{\min}$ discussed above) and 1,000 pairs where at least one bill has a claim (again evenly stratified by the categories of $s_{i,j}^{\min}$). From 1945 to 1980, we drew 1,500 pairs coded as identical by CCH and 1,500 that were not coded by CCH as identical. From 1981 to 2010, we drew 750 pairs from each of the four strata of $s_{i,j}^{\min}$ discussed above. If in any case the strata contained fewer than the required number of pairs, we sampled all of them and thus some congresses contain fewer than 3,000 samples—the 44th Congress has the minimum with around 2,200 samples.

Labeling all of these pairs with the LLM cost a few hundred dollars. The stratification was effective in that around 20-30% of the sampled pairs were positive in the period without CCH and around 50% when stratifying on whether the pair was listed as identical by CCH.

B.5.2 Generating Predictions at Scale

Next, we trained a predictive model to predict the LLM labels based on a variety of textual features drawn from existing research and our own experimentation. We reference existing papers in political science that have used the corresponding measure for more details. The features are described below.

- **Semantic Similarity:** As discussed above, compute the similarity score based on sentence embeddings (Reimers and Gurevych, 2019) of the title, define this as $s_{i,j}^{\text{title}}$. Similarly, compute the sentence embedding of the LLM-generated summaries discussed in Appendix B.2. While, in theory, a predictive model could learn non-linear features of these variables, they are occasionally missing and thus we provide the maximum and minimum of the two as additional inputs, i.e., $s_{i,j}^{\max}$ and $s_{i,j}^{\min}$.

- **Shingled n -grams (Text):** Taking the text of the bill with some light cleaning using regular expressions, compute the shingled n -grams as in Casas, Denny and Wilkerson (2020). For 1, 2, 5, and 10 n -grams, what is the minimum of the share of n -grams of bill A found in bill B and bill B found in bill A . Existing studies have used this in an asymmetric way to identify hitchhikers or legislation that has been inserted into another (e.g., Kroeger et al. 2024; Casas and Wilkerson 2025). However, as we want to identify effectively identical bills, we use the symmetric version, i.e., a pair must be high in *both* shares to be effectively identical.
- **Shingled n -grams (Title):** Taking the title of the bill, compute the shingled n -grams of the *characters*. This would allow us to detect overlapping sequences in the titles and address concerns about small typos or variants in similarly titled bills. For 4, 10, and 20 character n -grams, compute the minimum of the share of bill A found in bill B and vice versa, as above.
- **Exact Text Alignment:** Using Smith-Waterman (local) alignment or Needleman-Wunsch (global) alignment, find the longest common sequence. Of that sequence, we compute the share of words that match exactly to define a “local” or “global” ratio. Linder et al. (2020) provide a good discussion of local and global alignment for identifying portions of bills that have been inserted into another.
- **Length-Based Features:** We believe that the way to classify pairs might differ systematically based on their length, e.g., short bills may have higher or lower thresholds of other predictors. Thus, we include the (logged) maximum length of the bills in the pair. We also include the negative of the absolute distance in length, logged, between the bills as effectively identical bills should not be extremely divergent in length.
- **Temporal Heterogeneity:** The predictors of what makes a pair effectively identical may change over time; thus, we include the number of the congress as a continuous predictor. We do not expect this to have a strong direct effect but it may moderate the other predictors.

Generating these features was relatively computationally expensive but can be done on a modern desktop with 120GB of RAM over a few days. Code to do so is provided in our replication archive. Using these features, we fit a probability random forest with 5,000 trees to predict the labels. To prevent leakage across pairs, to make a prediction for congress t , we train on a set of congresses $\mathcal{C}$, excluding t . For any congress before the Legislative Reorganization Act of 1946 (i.e., 43rd-79th Congresses; 1873 to 1946), we train on data from 1873 to 1946. For any congress after that (i.e., 80th-111th Congresses; 1947-2010), we trained on data from 1947 to 2010. This ensures that the major structural break in the types of bills introduced induced by eliminating many types of private claims bills is not contaminating our predictive model.

B.6 Validating the Primary Classification

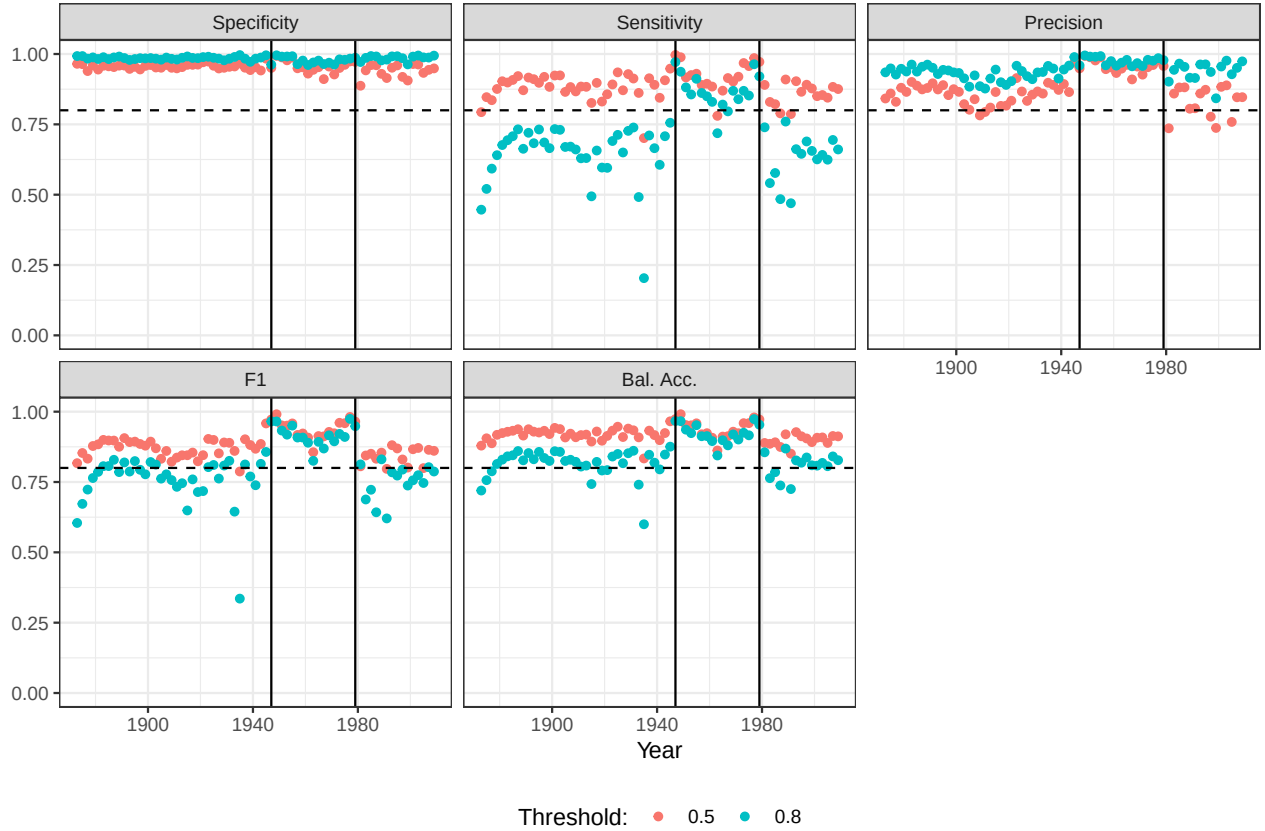
In the main text, Table 2 outlines two validations: (i) how do the predictions from the random forest recover the LLM labels; (ii) how do they compare against human judgment. This appendix

provides more information on those tasks as well as using the CCH data (see Appendix B.4.2) for an additional, large-scale validation.

B.6.1 Comparing Against LLM Labels

The main text reports the specificity, sensitivity (i.e., recall), precision, F1-score and balanced accuracy of our out-of-sample predictions against the LLM labels. To ensure the performance is stable over time, Figure A.4 shows each measure at the two thresholds (0.50 and 0.80) over time. The two vertical lines mark the 80th Congress (1947-1948) and the 96th Congress (1979-1980). The performance is slightly higher in that period; this corresponds to two things. First, the training data in that period is stratified based on CCH (versus the similarity score) so the rate of positive examples is higher. While this could affect the F1 and precision, it should not affect the sensitivity. Second, this period corresponds to a surge in the rise of introducing duplicate bills in the House that was, eventually, ended in the 96th Congress by the introduction of unlimited cosponsorship (see Appendix A.3).

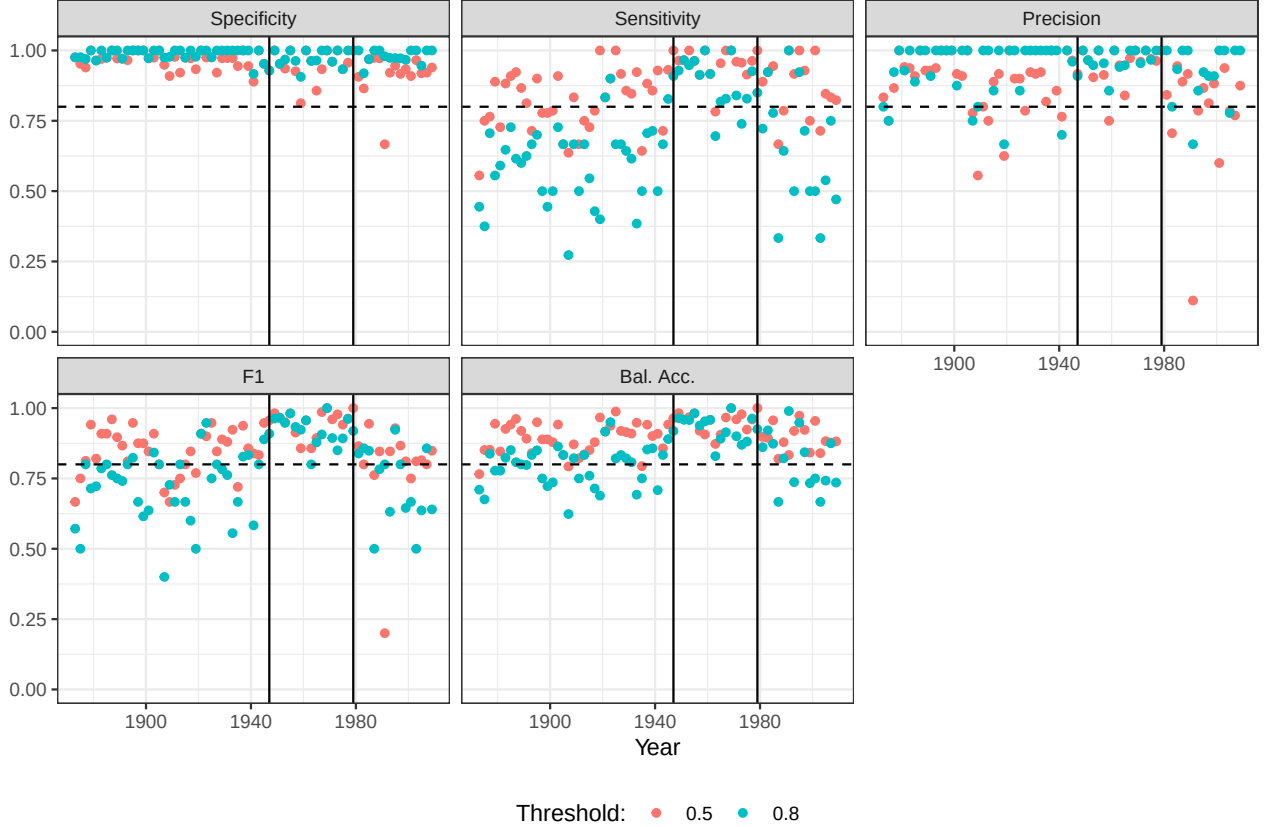
Figure A.4: Trends in Performance for Held-Out LLM Labels



B.6.2 Comparing Against Human Annotation

As above, we examine whether the performance against human annotation is stable across time. We note there are only 50 observations in each congress so the measures are considerably noisier. However, we see a similar pattern to the performance against the held-out LLM labels—a slight increase in performance in the 1947-1979 period, versus the surrounding periods.

Figure A.5: Trends in Performance for Human Annotation



B.6.3 Comparing Against CCH

Our final validation replicates the analysis from Table 2 where data from CCH is used as the ground truth. This compares our predictive model against the implied within-House and across-chamber pairs ($n \approx 13.3$ million; 1945 to 1980). As noted in Appendix B.4.2, we believe there is a systematic issue with CCH where many truly (effectively) identical bills are *not* recorded as such, leading to systematic downward bias in the precision and F1. With this in mind, the final panel of Table A.2 shows a similarly strong performance across specificity, sensitivity, and balanced accuracy to the other panels.

Table A.2: Performance of Predictive Model

	Specificity	Sensitivity	Precision	F1	Bal. Acc.
Threshold (0.50)	0.96	0.95	0.38	0.54	0.96
Threshold (0.80)	0.97	0.87	0.44	0.58	0.92

Note: “Threshold (0.5)” and “Threshold (0.8)” binarize the predicted probabilities at 0.5 and 0.8, respectively, to generate the measures of prediction accuracy.

B.7 Quantifying Collaboration from Bill Pair Predictions

This sub-section provides additional information on how we generate our member-dyad level collaboration score. As noted in the main text, we need two elements to construct this: (a) a predicted probability as to whether a pair of bills (i, j) are effectively identical and (b) the universe of (formal) original cosponsors. For the latter, we rely on ProQuest’s digitization of this data before the 101st Congress; this has coverage for the entire period, with the first few occurrences of formal original cosponsorship appearing in the early 1920s in the Senate before becoming systematic by the early 1930s. From the 101st Congress onwards, we use data from `congress.gov` as ProQuest appears to start recording *all* cosponsors. When using data from `congress.gov`, we use Curry and Roberts (2023)’s operationalization as cosponsors listed on the day of the bill’s introduction. For ProQuest, the data reported comes from the names of members who are printed on the bill at the introduction of the earliest recorded version.

For the former, we assume a probability of being effectively identical of zero if the pair did not pass the initial screening; if they did, we use the predicted probability from the model discussed in Appendix B.5. Thus, for each bill pair (i, j) , we have a predicted probability $p_{i,j}$. We implicitly assume for original cosponsors there is a “shadow bill,” i.e., i' , that is connected to bill i with probability 1, i.e., $p_{i,i'} = 1$. This generates a network of connections between bills.

Our first step is to generate “clusters” of bills that are likely to all be mutually effectively identical, i.e., bills i , j , and k are all effectively identical. To do this, we threshold the edges of the network at 0.80—a threshold that still maintains good predictive power (see Appendix B.6). We then obtain all connected components $\mathcal{C}$ to define the clusters $c \in \mathcal{C}$ of bills that we treat as effectively identical. The median probability of being effectively identical, $p_{l,l'}$, between bills in the same cluster c is 0.975, with a lower 25th percentile of 0.906. However, it is logically possible for an edge to be below 0.80 inside a cluster, i.e., $p_{A,B} = 0.80$, $p_{B,C} = 0.80$, but the weight assigned by the predictive model to that edge $p_{A,C} = 0.72$. This is addressed by the downweighting of the edge by $p_{l,l'}$ detailed in the main text. Around 11% of edges are below 0.80, but only 1.4% are below 0.50. In terms of the number of bills in each cluster—note this is distinct from the number of members in each cluster due to (formal) original cosponsorship—the median cluster contains two distinct bills and the 75th percentile of the number of distinct bills is also two. This suggests that, when effectively identical bills are introduced, there is usually only one copy introduced.

As discussed in the main text, we thus need to generate a collaboration weight score for each member involved in the component c . To do this, we use the simple formula noted in the main text: First, we exclude all links that do not have the required temporal proximity—

see Appendix C.7 for robustness to different choices. In our main analysis, we require that (a) within-chamber duplicate bills are introduced within seven days of each other; (b) across-chamber duplicate bills are introduced within 30 days of each other; (c) original cosponsors are listed on the day of the bill’s introduction or on the earliest printed version digitized by ProQuest. We implement this by looking at the date associated with the *earliest* introduced bill in the cluster c and exclude all bills that are not within the threshold of the date.¹⁹

Second, we compute the number of unique members $n_c = |\mathcal{S}_c|$ associated with the bills in the component. This prevents a member who appears twice from inflating the size of the set of collaborations. Then, for each pair of bills in the component, we assign a weight equal to its probability of being effectively identical $p_{i,j}$. We multiply these weights $1/(n_c - 1) \cdot p_{i,j}$ to get the edge weight. This ensures that if two bills are in the component c but are not likely to be effectively identical, this is downweighted; further, if many people are involved in the collaboration, the weight is also reduced using the formula $1/(n_c - 1)$ used to analyze cosponsorship (Fowler, 2006a,b).

There are certain occasions where member i appears multiple times in a cluster c (i.e., they introduce the same bill twice in a given congress in the temporal window). In this case, we use only the number of unique members to compute n_c but take the maximum edge weight between member i and member j of the multiple bills associated with member i .

This has a clean interpretation: If two members introduce an effectively identical bill (or are the original cosponsors) and no other members do so, the weight those members get for that collaboration is $p_{i,j}$. Thus, if the pair of bills is predicted very strongly to be effectively identical, they get a weight close to one. A bill with no effectively identical copies but one original cosponsor gets the maximal weight of one.

To get the member dyad score $y_{i,j}$, we sum across all clusters c in a given congress. If two members do not appear in any cluster in the congress, they are given a score of 0.

B.8 Validating Member-Level Measure

It is challenging to validate this member-level measure directly as no existing systematic dataset exists for the historical period we examine. To provide some simple evidence for face validity, Table A.3 shows the ten highest scoring collaborations between members in our data. Of the top ten, five are pairs of committee chairs and ranking members and two are chairs of mirroring committees in the House and Senate. There are clear institutional incentives for these members to work together. The other three are all pairs of senators from the same state delegation.

C Additional Empirical Results

This section contains additional information on our empirical results in Section 5.

¹⁹For within-chamber collaboration, we do this with respect to the earliest introduced House or Senate bill, respectively.

Table A.3: Highest Scoring Dyads

Member i	Member j	Congress	Score	Notes
ANDREWS (H-R-NY)	GURNEY (S-R-SD)	80	121.41	Chairs (Armed Services)
JACKSON (S-D-WA)	FANNIN (S-R-AZ)	94	84.12	Chair/RM (Interior)
VOINOVICH (S-R-OH)	BROWN (S-D-OH)	111	73.51	Same State
NEUBERGER (S-D-OR)	MORSE (S-D-OR)	85	70.41	Same State
DEVINE (H-R-OH)	STAGGERS (H-D-WV)	93	62.33	Chair/RM (Commerce)
SOLOMON (H-R-NY)	MONTGOMERY (H-D-MS)	100	60.36	Chair/RM (Veterans)
SCHUMER (S-D-NY)	CLINTON (S-D-NY)	107	57.14	Same State
BABCOCK (H-R-WI)	GALLINGER (S-R-NH)	59	56.18	Chairs (D.C.)
JACKSON (S-D-WA)	FANNIN (S-R-AZ)	93	55.87	Chair/RM (Interior)
SPRINGER (H-R-IL)	STAGGERS (H-D-WV)	92	54.70	Chair/RM (Commerce)

Note: For each member, their name is formatted as “SURNAME (CHAMBER-PARTY-STATE)” where “H” indicates House, “S” indicates Senate, “D” indicates Democrat, “R” indicates Republican. “Chairs” indicates that both members were chairs of the corresponding committees in their chamber. “Interior” refers to Interior and Insular Affairs; “Commerce” refers to Interstate and Foreign Commerce; “Veterans” refers to Veterans’ Affairs.

C.1 Alternative Models for Network Data

Our main analysis uses a dyadic regression with member-congress-specific fixed effects. One reason for this is computational; across the periods and types of collaboration we examine, the average size of the regressions in the first analysis (i.e., Table 3) has around 4,400 nodes (treating members as distinct in each congress) with around 560,000 dyads. This is computationally challenging for two popular alternatives in political science: ERGM (Exponential Random Graph Model; e.g., Cranmer and Desmarais 2011) and AME (Additive and Multiplicative Effects; e.g., Minhas, Hoff and Ward 2019; Hoff 2021). Second, because of our focus on dyadic predictors, as discussed in the main text, we are less interested in specific features of the network (e.g., effects such as triadic closure that can be specified in an ERGM or the latent space from an AME model) but are especially interested in adjusting for unobserved node (member-congress) level confounding.

However, there is a close connection between our model and the AME approach. In an undirected setting, an AME-type model can be written as follows (Hoff 2021, p. 40):

$$y_{ij} = \mathbf{x}_{ij}^T \boldsymbol{\beta} + \alpha_i + \alpha_j + \epsilon_{ij} + \mathbf{u}_i^T \text{diag}(\{d_k\}) \mathbf{u}_j \quad (3)$$

$$\alpha_i \sim N(0, \sigma_\alpha^2); \quad \epsilon_{ij} \sim N(0, \sigma_\epsilon^2) \quad (4)$$

The model is quite similar to ours, with the main differences being AME models (i) use a random effect for α_i instead of a fixed effect and (ii) use a latent space model to capture third-order dependencies (Minhas, Hoff and Ward, 2019). In our approach, the dyad-robust standard errors (Aronow, Samii and Assenova, 2015) are designed to be robust to that dependence. Thus, one could view the AME model as the “random effects” version of our approach, where the distinction between the two is related to that between accounting for (one-way) clustering using either a cluster-specific random effect or fixed effects and cluster-robust standard errors. The fixed

effect approach controls for all variables that are constant within a member-congress that affect their level of collaboration; controlling for these helps us isolate the effect of dyadic predictors.

C.2 Main Results

We include the full regression tables, including all controls, for the results shown in Table 3. The first finding is that the effects of geographic proximity are substantively large in magnitude compared to other controls. One interesting finding is that when both ideological distance and copartisanship are controlled for, there is a consistently negative effect of ideological distance from the early 1910s in the House and Senate and from 1947 for across-chamber collaboration. Interestingly, there is often a negative and significant effect of being copartisans; this must be carefully interpreted, however, as it is highly correlated with ideological distance. Thus, its interpretation is the effect of being copartisan (vs. not) holding ideological distance constant.

C.3 Disaggregating Formal and Informal Original Cosponsorship

Our member-level measure includes both formal and informal original cosponsorship. Both types of activity occur within each chamber, although Figure 3 shows that formal original cosponsorship rapidly supplanted informal original cosponsorship in both the House and Senate. Figure A.6 shows the robustness of our empirical results to looking at formal and informal original cosponsorship separately. We treat mixed clusters as informal for this analysis. The standardized coefficients, i.e., expected change measured in terms of period-chamber-type (formal vs. informal) standard deviations, are reported.

The results are consistent with the main text; for both informal and formal original cosponsorship, the largest coefficient across all periods and chambers is for members from the same state who are physically adjacent. In the Senate, there is a small but statistically significant positive effect from being from a neighboring state for both formal and informal cosponsorship. In the House, the only major difference between formal and informal cosponsorship is the ordering of the two intermediate categories (Adj. & Nb. State; Not Adj. & Same State) for which no clear expectations were hypothesized. For formal original cosponsorship, members from the same state but not adjacent districts collaborate more than those from physically adjacent districts in different states. For informal original cosponsorship, these effects are often not statistically distinguishable, although occasionally the order is the opposite of that found in formal original cosponsorship.

C.4 District Demographic Similarity

Our data for district demographic similarity comes from replication data from Adler and Lapinski (1997) and Adler (2002). Detailed information on the variables can be found in those sources and in the replication data, but it includes 24 to 41 variables in each congress that correspond to variables that likely induce demand for serving on policy-focused standing committees. They include things such as the number of military installations in the district, the number of people employed in various sectors such as finance, construction, etc. When computing the distance

Table A.4: Full Results for Geographic Proximity (House and Senate)

	House					
	Early (1873-1888)	EPG (1889-1910)	Inter-War (1911-1946)	Textbook (1947-1974)	Reform (1975-1994)	Modern (1995-2010)
Same Party	0.01 (0.01)	0.00 (0.01)	0.00 (0.02)	0.15 (0.18)	−0.18 (0.14)	−0.68*** (0.16)
Ideological Dist. [Dim 1]	0.00 (0.01)	−0.01 (0.01)	−0.04*** (0.01)	−0.39*** (0.03)	−0.86*** (0.03)	−1.80*** (0.06)
Both Majority	−0.01 (0.02)	−0.01 (0.02)	−0.03 (0.03)	−0.44 (0.36)	0.07 (0.27)	0.19 (0.31)
Same Committee	0.01* (0.01)	0.02*** (0.00)	0.03*** (0.00)	0.36*** (0.02)	0.51*** (0.01)	0.34*** (0.01)
Shared Service	0.02*** (0.01)	0.01*** (0.00)	0.00*** (0.00)	0.02*** (0.00)	0.01*** (0.00)	0.04*** (0.00)
Not Adj. & Nb. State	0.02*** (0.01)	0.02*** (0.00)	0.02*** (0.00)	0.08*** (0.01)	0.13*** (0.01)	0.11*** (0.01)
Adj. & Nb. State	0.04 (0.02)	0.14*** (0.05)	0.17*** (0.03)	0.43*** (0.03)	0.57*** (0.03)	0.68*** (0.04)
Not Adj. & Same State	0.13*** (0.03)	0.11*** (0.02)	0.10*** (0.01)	0.42*** (0.02)	0.68*** (0.02)	1.07*** (0.03)
Adj. & Same State	0.29*** (0.05)	0.50*** (0.07)	0.37*** (0.03)	1.12*** (0.05)	1.76*** (0.05)	2.33*** (0.06)

	Senate					
	Pre-Direct (1873-1914)	Post-Direct (1915-1946)	Textbook (1947-1974)	Reform (1975-1994)	Modern (1995-2010)	
Same Party	0.11** (0.04)	−0.00 (0.04)	0.11 (0.11)	0.08 (0.12)	−0.43** (0.19)	
Ideological Dist. [Dim 1]	0.04 (0.03)	−0.05 (0.03)	−0.73*** (0.06)	−0.92*** (0.05)	−1.26*** (0.08)	
Both Majority	−0.14* (0.08)	−0.04 (0.07)	−0.40* (0.23)	−0.54** (0.24)	0.07 (0.37)	
Same Committee	−0.01 (0.01)	0.04*** (0.01)	0.15*** (0.01)	0.26*** (0.02)	0.19*** (0.01)	
Shared Service	0.01** (0.00)	0.00** (0.00)	0.01*** (0.00)	0.01*** (0.00)	0.01** (0.01)	
Adj. & Nb. State	0.04*** (0.02)	0.09*** (0.01)	0.23*** (0.01)	0.31*** (0.02)	0.31*** (0.02)	
Adj. & Same State	1.17*** (0.16)	1.80*** (0.23)	4.44*** (0.22)	4.62*** (0.20)	5.12*** (0.25)	

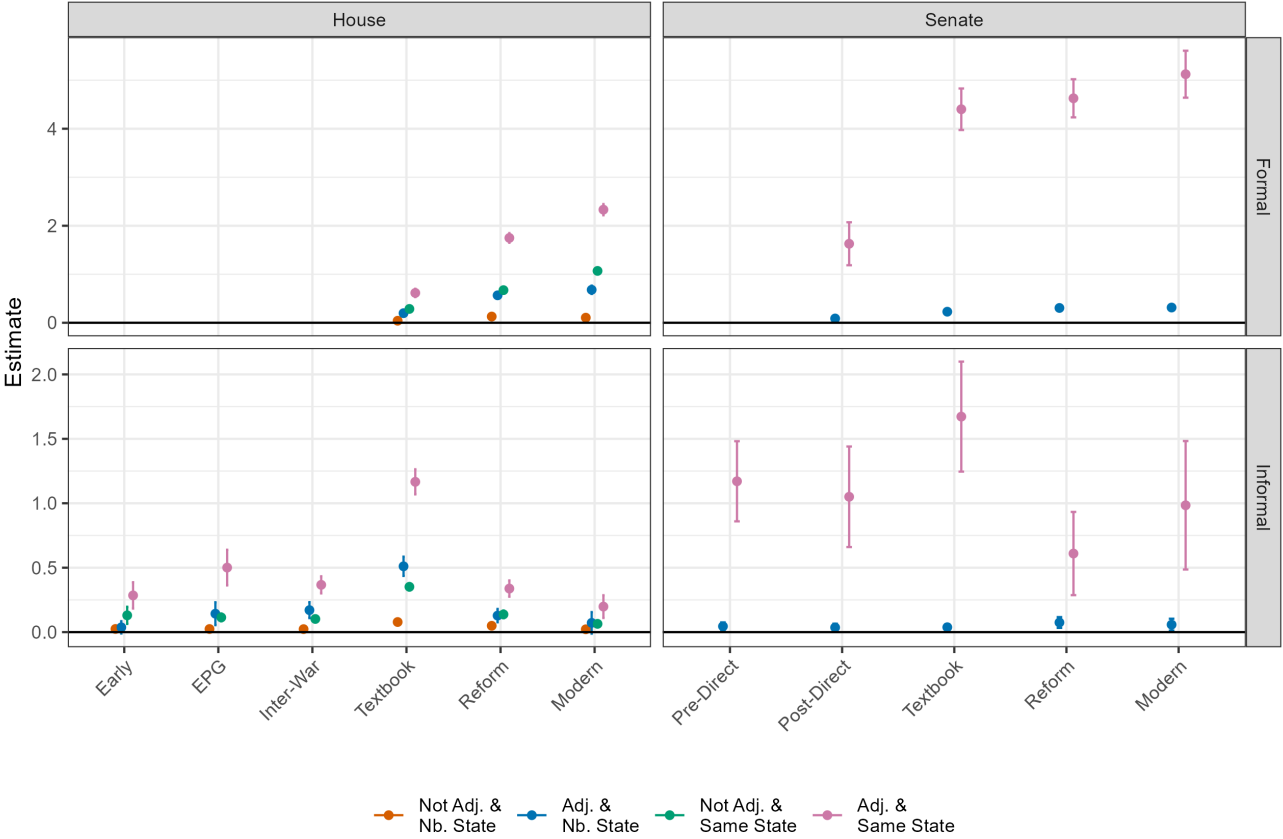
Note: *: $p < 0.10$; **: $p < 0.05$; ***: $p < 0.01$. The outcome is the (standardized) collaboration score between members in a given congress. All models include member-congress fixed effects. Dyad-robust clustered errors are shown in parentheses. See Table 3 for information on variable abbreviations.

Table A.5: Full Results for Geographic Proximity (Across Chamber)

	Across Chamber				
	Pre-Direct (1873-1914)	Post-Direct (1915-1946)	Textbook (1947-1974)	Reform (1975-1994)	Modern (1995-2010)
Same Party	0.02*** (0.01)	0.03*** (0.01)	0.04*** (0.01)	-0.03*** (0.01)	-0.17*** (0.02)
Ideological Dist. [Dim 1]	0.00 (0.01)	0.01 (0.01)	-0.12*** (0.01)	-0.27*** (0.02)	-0.62*** (0.03)
Both Majority	0.03*** (0.01)	0.01 (0.01)	-0.04* (0.02)	0.03*** (0.01)	0.00 (0.02)
Shared Service	0.01*** (0.00)	0.01*** (0.00)	0.01*** (0.00)	0.01*** (0.00)	0.01*** (0.00)
Not Adj. & Nb. State	0.04*** (0.00)	0.02*** (0.00)	0.04*** (0.00)	0.08*** (0.01)	0.11*** (0.01)
Adj. & Nb. State	0.05*** (0.01)	0.04*** (0.01)	0.09*** (0.01)	0.17*** (0.01)	0.22*** (0.01)
Adj. & Same State	1.96*** (0.07)	1.33*** (0.05)	1.38*** (0.05)	1.85*** (0.08)	2.49*** (0.11)

Note: *: $p < 0.10$; **: $p < 0.05$; ***: $p < 0.01$. The outcome is the (standardized) collaboration score between members in a given congress. All models include member-congress fixed effects. Dyad-robust clustered standard errors are shown in parentheses. See Table 3 for information on variable abbreviations.

Figure A.6: Disaggregating Formal and Informal Original Cosponsorship



between districts, we first use multiple imputation to fill in missing data on certain variables in some districts.

The full regression tables are shown below to complement the results in the main text.

Table A.6: Full Results for Demographic Similarity

	House		Senate		Across Chamber	
	Textbook (1947-1974)	Reform (1975-1994)	Textbook (1947-1974)	Reform (1975-1994)	Textbook (1947-1974)	Reform (1975-1994)
Same Party	0.16 (0.18)	-0.16 (0.14)	0.11 (0.11)	0.04 (0.08)	0.03** (0.01)	-0.02** (0.01)
Ideological Dist. [Dim 1]	-0.39*** (0.03)	-0.85*** (0.03)	-0.73*** (0.06)	-0.61*** (0.06)	-0.11*** (0.01)	-0.21*** (0.02)
Both Majority	-0.44 (0.36)	0.04 (0.28)	-0.41* (0.24)	-0.36** (0.16)	-0.03 (0.03)	0.02* (0.01)
Same Committee	0.36*** (0.02)	0.51*** (0.01)	0.15*** (0.01)	0.23*** (0.03)		
Shared Service	0.02*** (0.00)	0.01*** (0.00)	0.01*** (0.00)	0.01*** (0.00)	0.01*** (0.00)	0.01*** (0.00)
Not Adj. & Nb. State	0.06*** (0.01)	0.11*** (0.01)			0.03*** (0.00)	0.07*** (0.01)
Adj. & Nb. State	0.39*** (0.03)	0.50*** (0.03)	0.20*** (0.02)	0.25*** (0.04)	0.05*** (0.01)	0.11*** (0.03)
Not Adj. & Same State	0.37*** (0.02)	0.62*** (0.02)				
Adj. & Same State	1.04*** (0.04)	1.63*** (0.05)	4.18*** (0.24)	3.61*** (0.35)	1.30*** (0.04)	1.56*** (0.10)
Demographic Similarity	0.08*** (0.01)	0.13*** (0.01)	0.08* (0.05)	0.10* (0.05)	0.38*** (0.04)	0.59*** (0.12)

Note: *: $p < 0.10$; **: $p < 0.05$; ***: $p < 0.01$. The outcome is the (standardized) collaboration score between members in a given congress. All models include member-congress fixed effects and the additional controls noted in the main text. Dyad-robust clustered standard errors are shown in parentheses. See Table 3 for information on variable abbreviations.

C.5 Interaction

To complement the plot of the interaction effects in the main text, the full tables are provided here. The negative main effect of “same party”, i.e., that members who have no geographic proximity are likely to collaborate less if they are from the same party, observed in the models without interaction (Appendix C.2) is also observed here.

To illustrate this more concisely, Figure A.7 shows the effect of geographic proximity by copartisanship. It shows the effect of geographic proximity (e.g., being from the same state)

Table A.7: Full Effects of Moderation of Copartisanship (House)

	House					
	Early (1873-1888)	EPG (1889-1910)	Inter-War (1911-1946)	Textbook (1947-1974)	Reform (1975-1994)	Modern (1995-2010)
Same Party	0.01 (0.01)	0.00 (0.01)	0.00 (0.02)	0.15 (0.18)	-0.18 (0.14)	-0.71*** (0.16)
Ideological Dist. [Dim 1]	0.00 (0.01)	-0.02 (0.01)	-0.04*** (0.01)	-0.38*** (0.03)	-0.84*** (0.03)	-1.78*** (0.06)
Both Majority	-0.01 (0.02)	-0.01 (0.02)	-0.03 (0.03)	-0.44 (0.36)	0.04 (0.27)	0.23 (0.32)
Same Committee	0.01* (0.01)	0.02*** (0.00)	0.03*** (0.00)	0.36*** (0.02)	0.51*** (0.01)	0.34*** (0.01)
Shared Service	0.02*** (0.01)	0.01*** (0.00)	0.00*** (0.00)	0.02*** (0.00)	0.01*** (0.00)	0.04*** (0.00)
Not Adj. & Nb. State	0.02*** (0.01)	0.03*** (0.01)	0.02*** (0.00)	0.06*** (0.01)	0.11*** (0.01)	0.09*** (0.01)
Adj. & Nb. State	-0.01*** (0.00)	0.11* (0.06)	0.11*** (0.04)	0.39*** (0.04)	0.51*** (0.03)	0.61*** (0.06)
Not Adj. & Same State	0.15*** (0.06)	0.11*** (0.02)	0.08*** (0.02)	0.31*** (0.02)	0.47*** (0.02)	0.79*** (0.03)
Adj. & Same State	0.21*** (0.07)	0.41*** (0.10)	0.46*** (0.07)	1.04*** (0.07)	1.53*** (0.06)	1.98*** (0.07)
Same × Not Adj. & Party Nb. State	0.01 (0.01)	-0.01 (0.01)	0.01** (0.01)	0.02** (0.01)	0.04*** (0.01)	0.03*** (0.01)
Same × Adj. & Party Nb. State	0.07** (0.04)	0.05 (0.08)	0.08 (0.06)	0.07 (0.05)	0.11** (0.05)	0.12* (0.06)
Same × Not Adj. & Party Same State	-0.04 (0.06)	0.01 (0.03)	0.04* (0.02)	0.19*** (0.03)	0.41*** (0.03)	0.55*** (0.03)
Same × Adj. & Party Same State	0.11 (0.09)	0.11 (0.13)	-0.12 (0.08)	0.12* (0.07)	0.39*** (0.07)	0.61*** (0.07)

Note: *: $p < 0.10$; **: $p < 0.05$; ***: $p < 0.01$. The outcome is the (standardized) collaboration score between members in a given congress. All models include member-congress fixed effects. Dyad-robust clustered standard errors are shown in parentheses. See Table 3 for information on variable abbreviations.

Table A.8: Full Effects of Moderation of Copartisanship (Senate)

	Senate				
	Pre-Direct (1873-1914)	Post-Direct (1915-1946)	Textbook (1947-1974)	Reform (1975-1994)	Modern (1995-2010)
Same Party	0.11** (0.05)	-0.00 (0.04)	0.09 (0.11)	0.05 (0.11)	-0.43** (0.19)
Ideological Dist. [Dim 1]	0.04 (0.03)	-0.05 (0.03)	-0.72*** (0.06)	-0.91*** (0.05)	-1.26*** (0.08)
Both Majority	-0.14* (0.08)	-0.05 (0.07)	-0.38 (0.24)	-0.51** (0.22)	0.07 (0.36)
Same Committee	-0.01* (0.01)	0.04*** (0.01)	0.15*** (0.01)	0.27*** (0.02)	0.19*** (0.01)
Shared Service	0.01** (0.00)	0.00** (0.00)	0.01*** (0.00)	0.01*** (0.00)	0.01** (0.01)
Adj. & Nb. State	0.07*** (0.03)	0.06*** (0.02)	0.16*** (0.01)	0.26*** (0.02)	0.27*** (0.02)
Adj. & Same State	1.18*** (0.33)	1.86*** (0.47)	3.88*** (0.27)	4.00*** (0.23)	5.15*** (0.50)
Same $\times$ Adj. & Party Nb. State	-0.04 (0.03)	0.06** (0.02)	0.12*** (0.02)	0.09*** (0.03)	0.07** (0.03)
Same $\times$ Adj. & Party Same State	-0.01 (0.37)	-0.09 (0.53)	0.84** (0.39)	1.15*** (0.38)	-0.05 (0.56)

Note: *: $p < 0.10$; **: $p < 0.05$; ***: $p < 0.01$. The outcome is the (standardized) collaboration score between members in a given congress. All models include member-congress fixed effects. Dyad-robust clustered standard errors are shown in parentheses. See Table 3 for information on variable abbreviations.

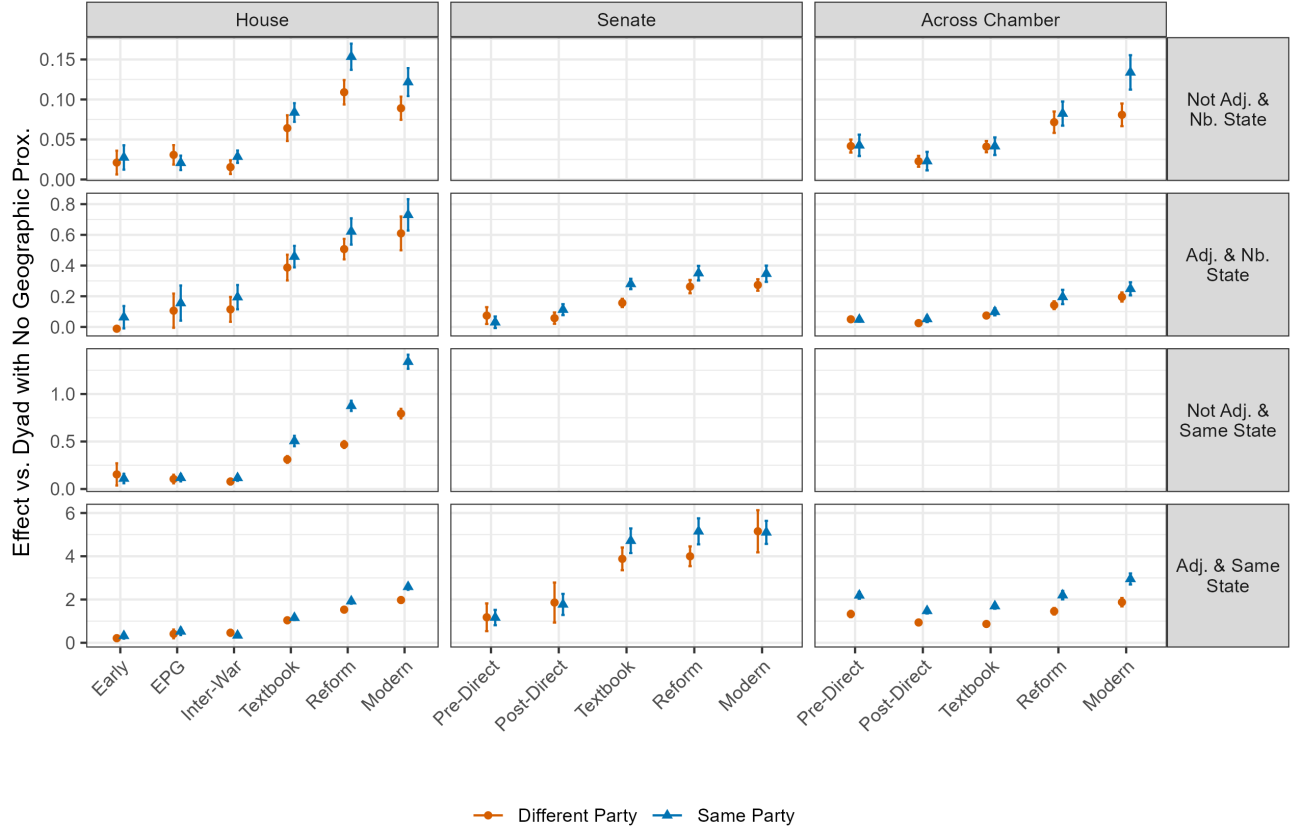
Table A.9: Full Effects of Moderation of Copartisanship (Across Chamber)

	Across Chamber				
	Pre-Direct (1873-1914)	Post-Direct (1915-1946)	Textbook (1947-1974)	Reform (1975-1994)	Modern (1995-2010)
Same Party	0.01 (0.01)	0.02*** (0.01)	0.02* (0.01)	-0.04*** (0.01)	-0.19*** (0.02)
Ideological Dist. [Dim 1]	0.00 (0.01)	0.02* (0.01)	-0.11*** (0.01)	-0.26*** (0.02)	-0.61*** (0.03)
Both Majority	0.03*** (0.01)	0.01 (0.01)	-0.03 (0.02)	0.03*** (0.01)	0.00 (0.02)
Shared Service	0.01*** (0.00)	0.01*** (0.00)	0.01*** (0.00)	0.01*** (0.00)	0.01*** (0.00)
Not Adj. & Nb. State	0.04*** (0.00)	0.02*** (0.00)	0.04*** (0.00)	0.07*** (0.01)	0.08*** (0.01)
Adj. & Nb. State	0.05*** (0.01)	0.03*** (0.01)	0.07*** (0.01)	0.14*** (0.01)	0.20*** (0.02)
Adj. & Same State	1.33*** (0.07)	0.94*** (0.06)	0.87*** (0.04)	1.46*** (0.08)	1.88*** (0.10)
Same × Not Adj. & Party Nb. State	0.00 (0.01)	0.00 (0.01)	0.00 (0.01)	0.01 (0.01)	0.05*** (0.01)
Same × Adj. & Party Nb. State	-0.00 (0.01)	0.03** (0.01)	0.02* (0.01)	0.05** (0.03)	0.05** (0.02)
Same × Adj. & Party Same State	0.86*** (0.09)	0.54*** (0.07)	0.83*** (0.06)	0.74*** (0.09)	1.07*** (0.11)

Note: *: $p < 0.10$; **: $p < 0.05$; ***: $p < 0.01$. The outcome is the (standardized) collaboration score between members in a given congress. All models include member-congress fixed effects. Dyad-robust clustered standard errors are shown in parentheses. See Table 3 for information on variable abbreviations.

versus a dyad with the same copartisanship status but no geographic proximity. It shows that the positive interactions seen in Figure 5 are not due to a lack of an effect of geographic proximity for non-copartisan dyads but rather a larger effect for copartisan dyads. Indeed, the ordering proposed in Hypothesis 1 generally holds for both copartisan and non-copartisan dyads.

Figure A.7: (Standardized) Effects by Copartisanship



Finally, we include the interaction terms estimated from the models where demographic similarity is included (i.e., extending Table 4). Table A.10 shows the full model with interactions between copartisanship and both geographic proximity and demographic similarity. The positive interactions between geographic proximity and copartisanship seen in Figure 5 generally remain. However, the interaction effects with demographic similarity are not in the expected direction; they are negative and significant in the House, in both periods, and in the Senate (Textbook). For across-chamber collaboration, the interaction is positive and significant in the Textbook period, although not significant in the Reform period. Interestingly, for non-copartisan members, more demographic similarity corresponds to statistically significantly increased collaboration.

C.6 Analysis using Physical Distance

Our main text uses physical adjacency between districts as a key proxy. We compute this using the physical distance between districts leveraging Lewis et al. (2013)'s data on district

Table A.10: (Standardized) Interaction Effects with Demographic Similarity

	House		Senate		Across Chamber	
	Textbook (1947-1974)	Reform (1975-1994)	Textbook (1947-1974)	Reform (1975-1994)	Textbook (1947-1974)	Reform (1975-1994)
Same Party	0.15 (0.18)	-0.15 (0.14)	0.08 (0.12)	0.02 (0.08)	0.02 (0.01)	-0.04*** (0.01)
Ideological Dist. [Dim 1]	-0.38*** (0.03)	-0.82*** (0.03)	-0.74*** (0.06)	-0.60*** (0.06)	-0.09*** (0.01)	-0.20*** (0.02)
Both Majority	-0.44 (0.37)	-0.00 (0.29)	-0.40* (0.24)	-0.34** (0.16)	-0.01 (0.03)	0.02** (0.01)
Same Committee	0.36*** (0.02)	0.51*** (0.01)	0.15*** (0.01)	0.23*** (0.03)		
Shared Service	0.02*** (0.00)	0.01*** (0.00)	0.01** (0.00)	0.01*** (0.00)	0.01*** (0.00)	0.01*** (0.00)
Not Adj. & Nb. State	0.05*** (0.01)	0.09*** (0.01)			0.03*** (0.00)	0.06*** (0.01)
Adj. & Nb. State	0.34*** (0.04)	0.43*** (0.03)	0.11*** (0.02)	0.23*** (0.05)	0.04*** (0.01)	0.08*** (0.02)
Not Adj. & Same State	0.27*** (0.02)	0.41*** (0.02)				
Adj. & Same State	0.95*** (0.07)	1.40*** (0.06)	3.50*** (0.31)	3.15*** (0.35)	0.80*** (0.04)	1.21*** (0.12)
Demographic Similarity	0.10*** (0.01)	0.14*** (0.01)	0.12** (0.05)	0.12** (0.05)	0.36*** (0.04)	0.58*** (0.12)
Same × Not Adj. & Party Nb. State	0.02*** (0.01)	0.05*** (0.01)			0.00 (0.01)	0.01 (0.02)
Same × Adj. & Party Nb. State	0.08* (0.05)	0.12*** (0.05)	0.14*** (0.02)	0.05 (0.04)	0.02* (0.01)	0.08 (0.05)
Same × Not Adj. & Party Same State	0.20*** (0.03)	0.41*** (0.03)				
Same × Adj. & Party Same State	0.14* (0.07)	0.41*** (0.07)	1.01** (0.39)	0.82 (0.58)	0.81*** (0.06)	0.66*** (0.14)
Same × Demographic Party Similarity	-0.03*** (0.00)	-0.03*** (0.01)	-0.06*** (0.01)	-0.01 (0.01)	0.02*** (0.00)	0.00 (0.01)

Note: *: $p < 0.10$; **: $p < 0.05$; ***: $p < 0.01$. The outcome is the (standardized) collaboration score between members in a given congress. All models include member-congress fixed effects and the additional controls noted in the main text. Dyad-robust clustered standard errors are shown in parentheses. See Table 3 for information on variable abbreviations.

boundaries. We define the distance between districts as the smallest distance between the corresponding district boundaries. Initial inspections noted that occasionally a trivially small gap between districts existed (e.g., because of a river); thus, we define two districts as physically adjacent if their closest distance is below 0.62 miles (1 km).

In this section, we test whether our primary results (Hypothesis 1) are robust to using physical distance itself. Modeling the effect of distance requires care for two reasons: First, it is important to assess non-linear effects of distance and account for skew in this variable. Second, the distribution of distance within the categories of same state, neighboring state, and non-neighboring state differs dramatically. For example, it is rare for House members in the same state to have districts that are more than 300 miles apart but common for dyads in non-neighboring states.

To address this, for each type of collaboration (House, Senate, Across Chamber) and state-pairing (Same State, Neighboring State, Neither), we cut the distance variable into a bin capturing physically adjacent districts and then cut the remaining (non-physically adjacent) distances into ten quantiles. This ensures there is a reasonable amount of data in each bin and allows for non-linear effects to be assessed. These quantiles are done *separately* for each type of state-pairing to account for the different distributions of distance. These (interacted) bins are included as categorical predictors in the main regression (Table 3), replacing the existing measures of geographic proximity.

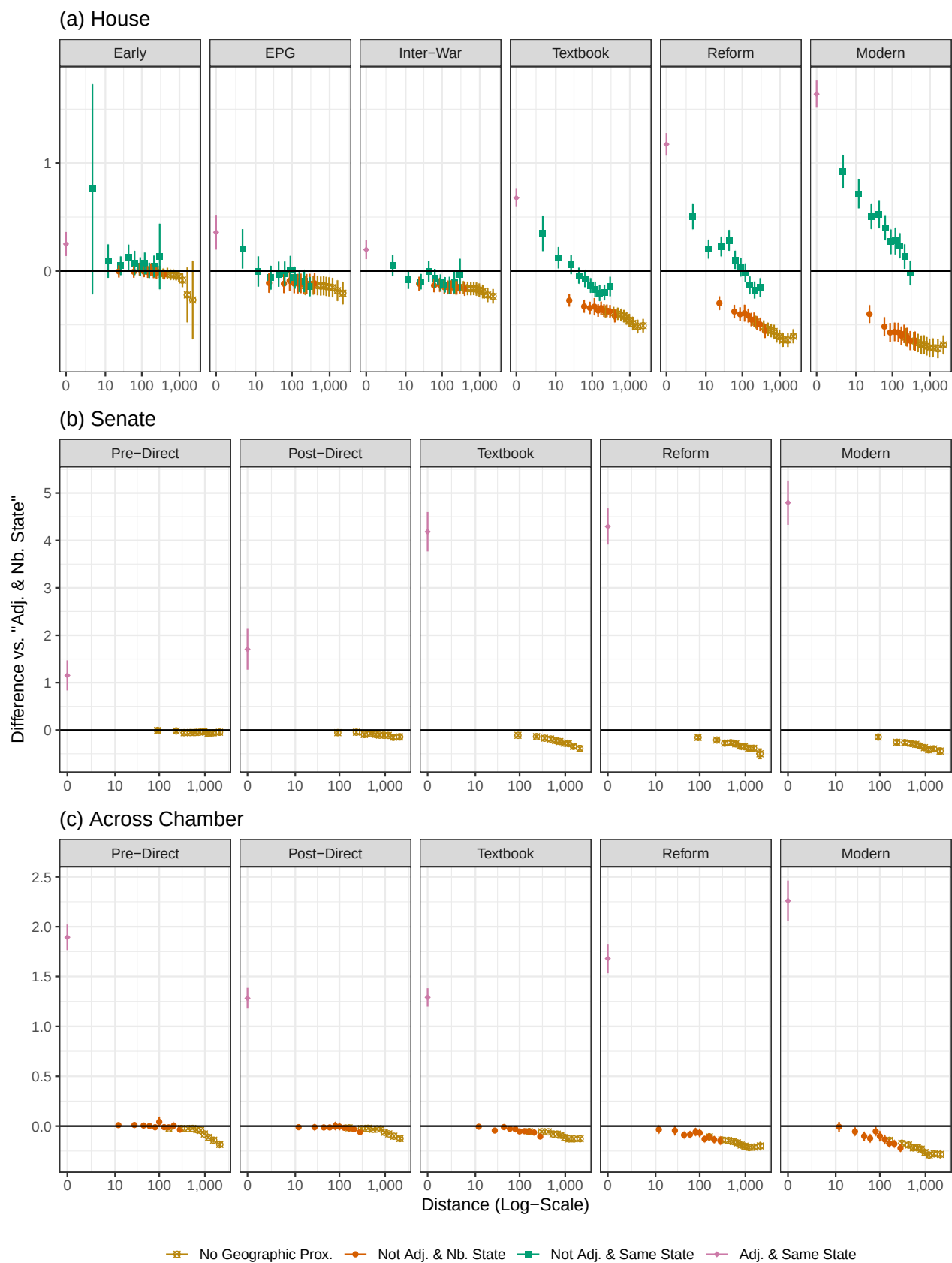
Figure A.8 treats physically adjacent districts in neighboring states (“Adj. & Nb. State”) as the reference category and shows effects relative to this group. While distance is always included as a categorical binned variable, as noted above, Figure A.8 plots each bin at the median distance observed in that bin, on the log-scale, to visually display the results.

The results strongly support Hypothesis 1. Starting with the House, we see that, in almost all periods, members from more geographically distant districts are less likely to collaborate. The magnitude of the effect increases as distance grows, despite our not assuming linearity or monotonicity, as the farthest right golden points are statistically significantly lower than the leftmost burnt orange points from the Inter-War period onwards. There are no differences between burnt orange and golden points at similar levels of distance. This suggests that positive effect of “Not Adj. & Nb. State” vs. the reference category of no proximity in Table 3 occurs because those districts (Not Adj. & Nb. State) are generally closer together than those in the reference category in Table 3 (“No Geographic Prox.” in Figure A.8).

Looking at the green points (Not Adj. & Same State), we see that these also show the effect of distance from the Textbook period onwards: Within the same state, members who are farther apart have lower expected collaboration. Interestingly, at similar distances, the green points are systematically higher than the burnt orange points from the Textbook period onwards. This shows that, holding the level of distance constant, members from the same state are more likely to collaborate than those from different states. As expected from Table 3, the largest effects are from those members in the same state who are physically adjacent (“Adj. & Same State”).

The results from within-Senate and across-chamber collaboration are rather similar. For most periods in the Senate (Post-Direct onwards) and all periods in across-chamber collaboration, members who are from more physically distant districts are less likely to collaborate and the largest effects are for members from the same state. A similar interpretation of “Not Adj. & Nb. State” applies to across-chamber collaboration as to the House, i.e., burnt orange points at

Figure A.8: (Standardized) Effects of Distance Regression



similar distance to golden points do not differ much in the magnitude of the estimated effects.

C.7 Temporal Robustness

This section considers the robustness of the results to different temporal thresholds for whether bills should be considered as original cosponsorship or companion bills. We consider the following four regimes where “Main” corresponds to the results in the main text. In all cases, we keep the definition of formal original cosponsors as those listed on the original bill or who signed on the day of introduction (see Appendix B.7).

- “Very Permissive”: Any temporal distance between effectively identical bills in the same chamber; any temporal distance between effectively identical bills in different chambers
- “Permissive”: within 30 days between effectively identical bills in the same chamber; within 180 days between effectively identical bills in different chambers
- “Main”: within 7 days between effectively identical bills in the same chamber; within 30 days between effectively identical bills in different chambers
- “Very Strict”: within 7 days between effectively identical bills in the same chamber; within 7 days between effectively identical bills in different chambers

The following figures show the coefficients from Tables 3 (effects of geographic proximity) and 4 (including demographic similarity) across those thresholds and the coefficients from Figure 5 (i.e., the interaction effects on geographic proximity and copartisanship). The results are quite stable in terms of magnitude and statistical significance across the different choices.

Figure A.9: Robustness of Main Results (Table 3)

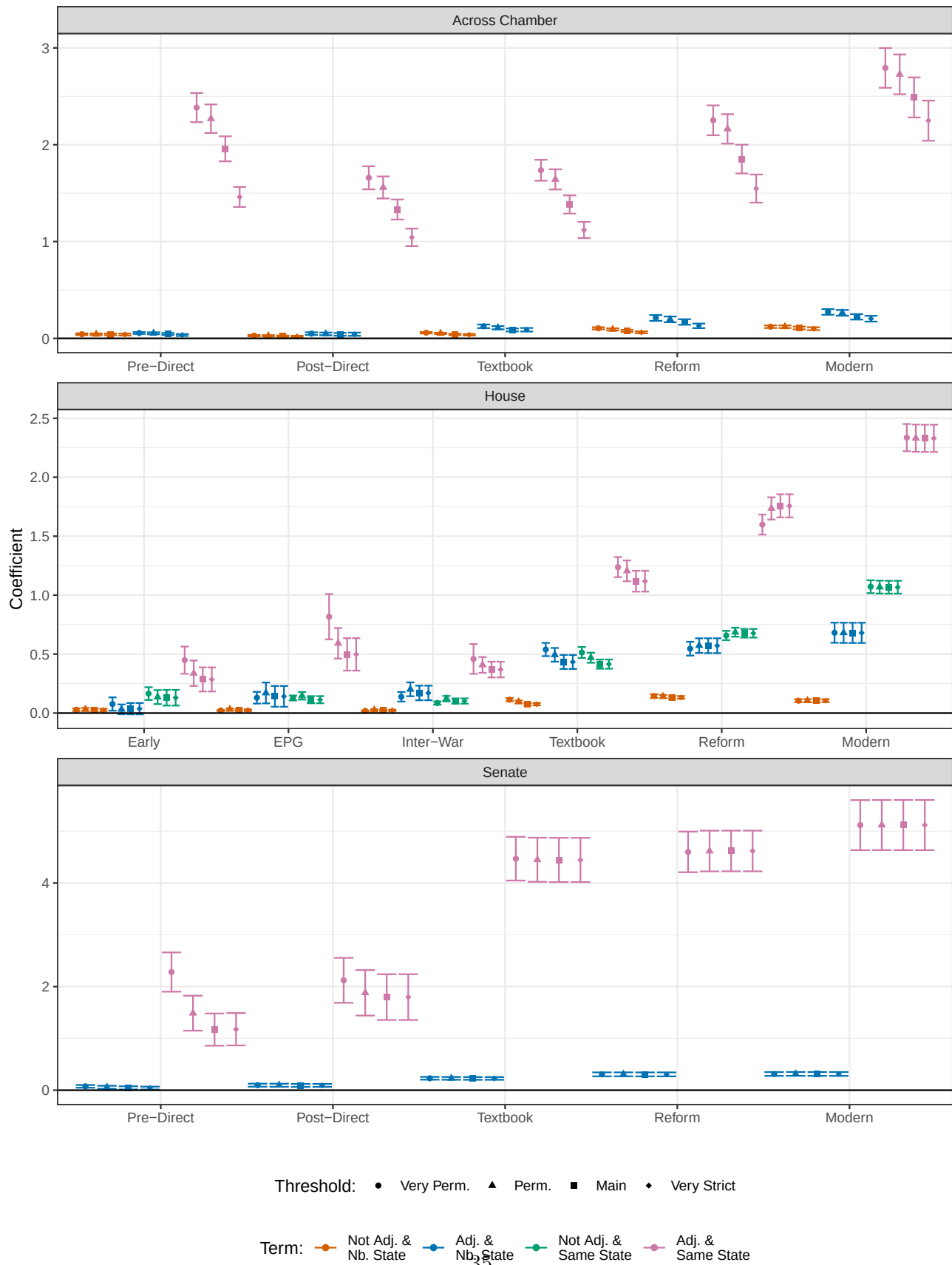


Figure A.10: Robustness of Interaction Effects (Figure 5)

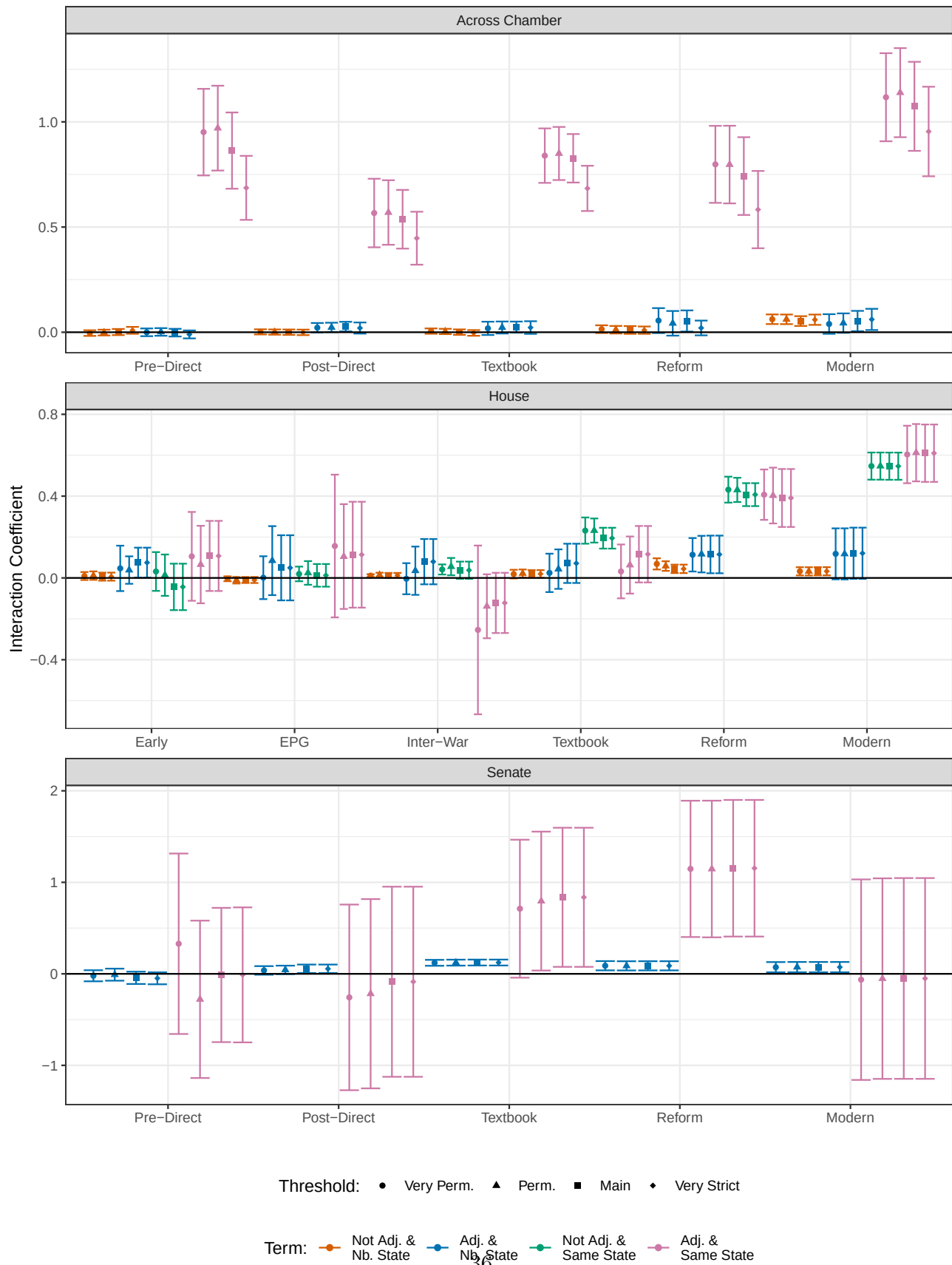
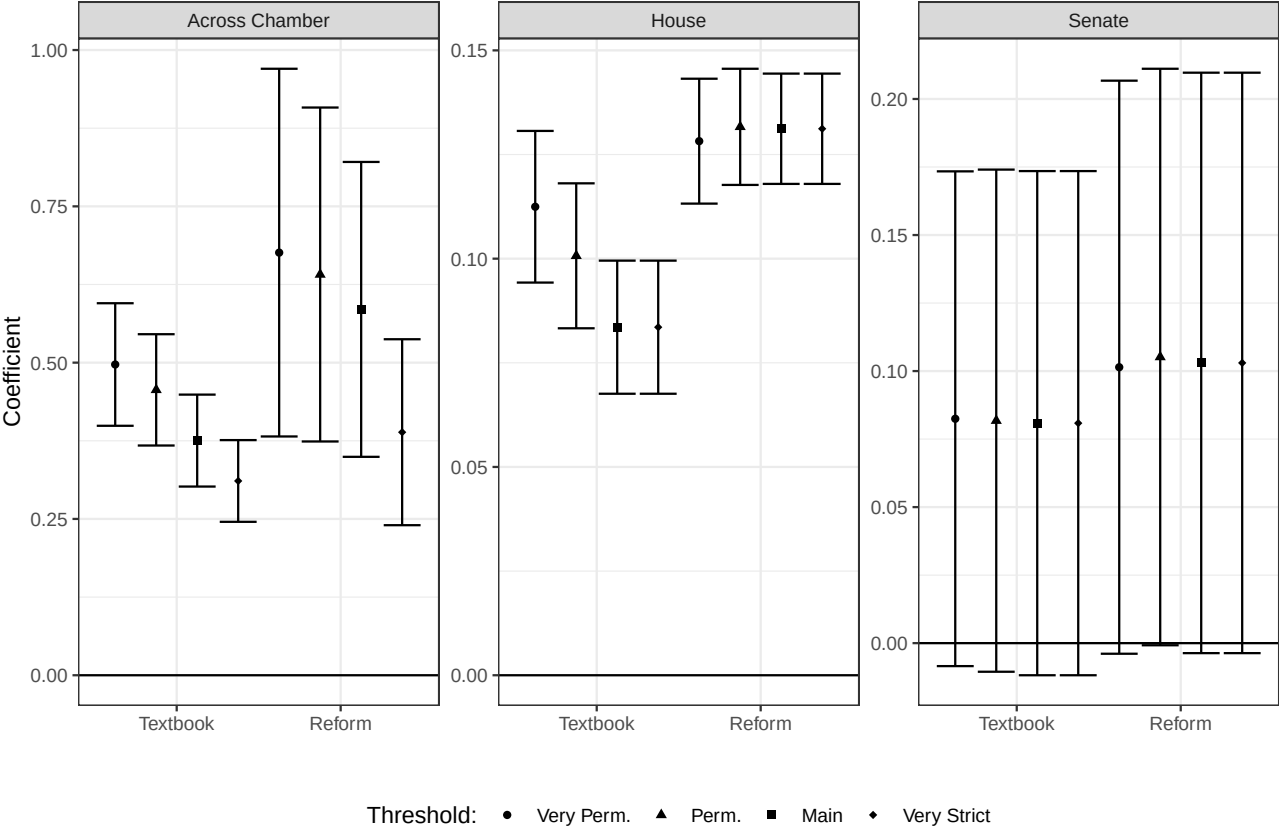


Figure A.11: Robustness of Demographic Similarity (Table 4)



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