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Congress and Community: Coresidence and Social Influence in the U.S. House of Representatives, 1801–1861

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Legislators often rely on cues from colleagues to inform their actions. Several studies identify the boardinghouse effect, *cue-taking among U.S. legislators who lived together in the nineteenth century. Nevertheless, there remains reason for skepticism, as legislators likely selected residences for reasons including political similarity. We analyze U.S. House members' residences from 1801 to 1861, decades more than previously studied, and show not only that legislators tended to live with similar colleagues but also that coresidents with divergent politics were more likely to move apart. Therefore, we deploy improved identification strategies. First, using weighting, we estimate that coresidence increased voting agreement, but at only half of previously reported levels. Consistent with theoretical expectations, we find larger effects for weaker ties and those involving new members. Second, we study legislators who died in office, estimating that deaths increased ideological distance between survivors and deceased coresidents.*

Legislatures are clearly social places. Members not only collaborate purposively—on legislation (Fowler 2006; Kirkland 2011), in committees (Ringe, Victor, and Gross 2013), through caucuses (Ringe, Victor, and Carman 2013)—but they also socialize informally, dining together (Steinhauer 2013), playing golf together (Booker 2015), even living together (Bash 2013). Elite socializing has deep roots in American political development and consequences for today's politics. Young's (1966) classic *The Washington Community* identifies the social fabric created by shared residences as foundational in the early Republic. And contemporary critics partially attribute elite polarization to declines in informal socialization (Mann and Ornstein 2006, 232).

Social ties can affect legislative behavior incidentally, for example via cues about roll-call votes passed between legislators who are merely physically proximate (Liu and Srivistava 2015; Masket 2008). In fact, Young (1966) bases his claims about the importance of shared residences on high levels of voting agreement among coresidents, a phenomenon known as the "boardinghouse effect" (Bogue and Marlaire 1975; Kirkland and Gross 2014; Parigi and Bergemann 2016). The implications of this research echo critics who link elite polarization to declines in socializing. For example, Parigi and Bergemann (2016) conclude that "a possible cause of the current political polarization among political elites in Washington [is] the lack of time congressmen spend together informally" (527).

Yet, there remains reason for skepticism. The boardinghouse effect is a social influence process: one legislator's actions change another's. Opportunities for influence depend on existing relationships, which in turn depend on latent tendencies, including how members typically vote. Homophily—the propensity for people with similar traits to form ties (McPherson, Smith-Lovin, and Cook 2001)—complicates efforts to infer influence from behavioral similarity (Fowler et al. 2011). Indeed, many boardinghouses developed regional, partisan, and ideological reputations. Existing studies attempt to address this issue using fixed effects or focusing explicitly on legislators who move midterm (Parigi and Bergemann 2016). But such designs are vulnerable to dynamic interrelationships between cause and effect (Imai and Kim 2019), such as when legislators form or dissolve ties for political reasons (Noel and Nyhan 2011).

More compelling research designs focus on moments of genuine randomization, though these are rare. Vexingly, such studies yield varying results, and none have focused on the boardinghouse effect. For example, Rogowski and Sinclair (2012) exploit the House's randomized office lottery to identify the effects of office proximity on voting agreement; they find null effects. Likewise, Coppock (2016) detects little evidence of spillovers among ideologically similar pairs. In contrast, Zelizer (2019) finds that state legislators responded to a randomized treatment that was exposed only to their officemates. As always, there are trade-offs for strong identification strategies, including conceptual slippage. Even though office proximity and ideological similarity increase the potential for cues, they may generate fewer opportunities for influence than living, dining, and socializing together.

We therefore return to the setting Young (1966) made famous, the boardinghouses of early Washington. First, we argue that cue-taking due to socializing will be primarily incidental, and thus more likely with sustained contact and few competing sources of influence.

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Consistent with the "strength of weak ties" (Granovetter 1973), legislators who have not lived together previously should be more likely to influence each other, especially when they share easily audited information. Further, new members may purposively select into residences with the intent of being influenced, essentially "choosing to be changed" (Santoro 2017), so influence should be larger for ties including newcomers.

We marshal evidence to probe these claims using a variety of methods. First, we use qualitative evidence from the historical record and secondary literature to argue that these conditions were satisfied in the decades before the Civil War. Living together during this period involved sustained contact and few competing sources of influence.

Next, we analyze the universe of extant evidence on congressional residences from 1801 to 1861, decades more evidence than in previous studies. We report strong evidence of political homophily in residence selection, a finding that is expected, yet surprisingly missing from the literature. Furthermore, we show that coresidents were more likely to move apart when their voting patterns diverged, an example of the "unfriending" problem that complicates inference about influence (Noel and Nyhan 2011). These findings cast doubt on previous attempts to measure the boardinghouse effect.

In light of this evidence, we take multiple quantitative approaches to measure influence. First, we analyze longitudinal data on coresidence using inverse probability weighting. Conditional on identifying assumptions, we show that coresidence increased voting agreement but at rates below previous estimates. We also find evidence for hypotheses consistent with both "the strength of weak ties" and "choosing to be changed." These findings illuminate the role of informal socialization in American political development. Effects peak in the late 1820s and early 1830s, before the consolidation of the second party system. Intriguingly, we find that informal socializing—in terms of social influence and boardinghouse culture itself—declined to its nadir coincident with the rising elite polarization of the 1850s and the advent of the Civil War.

Finally, to burnish our causal claims, we present what is, to our knowledge, the strongest identification strategy yet deployed to study the boardinghouse effect. We study legislators who died in office, identifying the effects of deaths on survivors' behavior, and find that survivors drifted away from their deceased colleagues in ideological space. Taken together, our results strengthen the case that declines in informal socialization exacerbate elite polarization.

SOCIAL INFLUENCE AND LEGISLATIVE BEHAVIOR

Legislators have a difficult job. They deal with uncertainty and limited resources, yet participate in hundreds or thousands of votes per term. One coping strategy is to rely on cues (Kingdon 1973; Matthews and Stimson 1975). Cue-taking[1] is a diffusion process that subsumes an array of mechanisms, such as imitation, coercion, and learning (Lindstädt, Vander Wielen, and Green 2016). Cues may be purposively disseminated by parties (Hershberger, Minozzi, and Volden 2018; Minozzi and Volden 2013), interest groups (Box-Steffensmeier, Christenson, and Craig 2019), senior colleagues (Box-Steffensmeier, Ryan, and Sokhey 2015), member organizations (Ringe, Victor, and Carman 2013), or peers (Zelizer 2019).

Cues may also be passed informally between legislators who socialize together. Such cue-taking is more likely to be incidental than purposive. Elites share many behaviors with members of the mass public (e.g., Sheffer et al. 2018), among whom informal political discussion is often an incidental byproduct of opportunity (Minozzi et al. 2020). Contact provides opportunity for cues via informal discussion—even among elites. Indeed, physical proximity fosters social ties among legislators (Caldeira and Patterson 1987), and there is evidence of cue-taking between legislators who are merely proximate. Masket (2008) shows that California state legislators who were desk mates agreed on more votes, and Liu and Srivastava (2015) find that copartisan U.S. senators with proximate desks were more ideologically similar. Thus, informal, incidental cue-taking should be more likely given sustained contact.

Contact alone is not sufficient for proximity-based cue-taking, however. The ambient political environment includes an array competing sources of cues (parties, interest groups, etc.). Competition limits the chances that a single source has an effect. Regardless of the mechanism, a multiplicity of cues makes it less likely that any single vector carries much signal. Not only does competition pose a theoretical challenge to any particular sort of cue-taking, it complicates the inferential problem, since weaker signals are more difficult to detect. Consequently, cue-taking should be more likely to occur and be observable in environments with less competition.

Although the theoretical case for social influence among legislators is compelling, uncovering empirical evidence of influence is complicated by its causal complement: homophily. Homophily refers to tie formation between similar individuals, the tendency for "birds of feather to flock together" (McPherson, Smith-Lovin, and Cook 2001). Influence and homophily are confounded (Fowler et al. 2011). The most inferentially threatening sorts of homophily occurs if legislators *establish* relationships because of their tendency to vote together or *terminate* relationships because of their tendency to vote differently, as in the "unfriending" problem (Noel and Nyhan 2011). Not all relationships form because of homophily, but the primary challenge to measuring social influence is specifying conditions under which the distribution of ties is ignorable.

[1] For brevity, we refer to both sending and receiving cues as "cue-taking."

Yet, there is also a third theoretical possibility that combines homophily and anticipated influence. Individuals may form ties with the explicit purpose of changing their own future behavior, in effect "choosing to be changed" (Santoro 2017). For example, a new member may seek out a senior colleague as a role model because of both perceived homogeneity and the intent to learn and thus change future behavior. This phenomenon blends elements of homophily and influence, implying that individuals not only select into relationships for nonrandom reasons but also subsequently change because of those relationships. Insofar as individuals bear the seeds of the change that follows, the process resembles homophily. Insofar as they were unlikely to change in the absence of the relationship, it is also unmistakably influential.

Cue-taking depends on qualities of social ties more generally. Ties vary in terms of strength—whether the relationship entails close, frequent, and durable interactions rather than casual, novel contact. Granovetter (1973) famously argues that the latter, "weaker" ties are actually the source of a network's "strength." While strong ties often develop in tightly knit groups, weak ties are more likely to bridge structural holes between different groups (Burt 2009). Within tightly knit groups, information is shared quickly and exhaustively. In contrast, weak ties transmit more novel information. Scholars have documented the strength of weak ties between legislators, with consequences for legislative success (Kirkland 2011), legislative effectiveness (Battaglini, Sciabolazza, and Patacchini 2020), and information exchange (Ringe, Victor, and Gross 2013) in a variety of contexts (Wojcik 2018). In the case of legislative cues, these weak ties should be better at transmitting easily audited information, such as party calls (Minozzi and Volden 2013) or the positions of allies and opponents, than technical information that would fall prey to cheap talk-like impediments to information flow.

We identify both weak ties and "choosing to be changed" with certain relationships. First, ties between legislators should be weaker when the ties are newer and stronger when they have been repeated often. Second, when members enter a legislature for the first time, they may be more likely to form relationships with peers or senior colleagues whose traits they seek to emulate. During the boardinghouse era, the first decision made by many new members will have been where—and with whom—to live. The resulting relationships will be nascent and thus both relatively weak and chosen with the intent for future change.

To summarize, informal cue-taking should be more prominent under certain conditions. First, actors must have sustained contact. Second, actors must have limited capacity to curate relationships. Third, there must be relatively few competing sources of influence. Fourth, weaker ties should convey more information than stronger ties, and cue-taking should be more common for ties including new members who "choose to be changed." In the next section, we use the historical record and secondary sources to argue that conditions for social influence were satisfied by the boardinghouse culture of Washington DC before the Civil War.

ELITE SOCIAL LIFE IN EARLY WASHINGTON

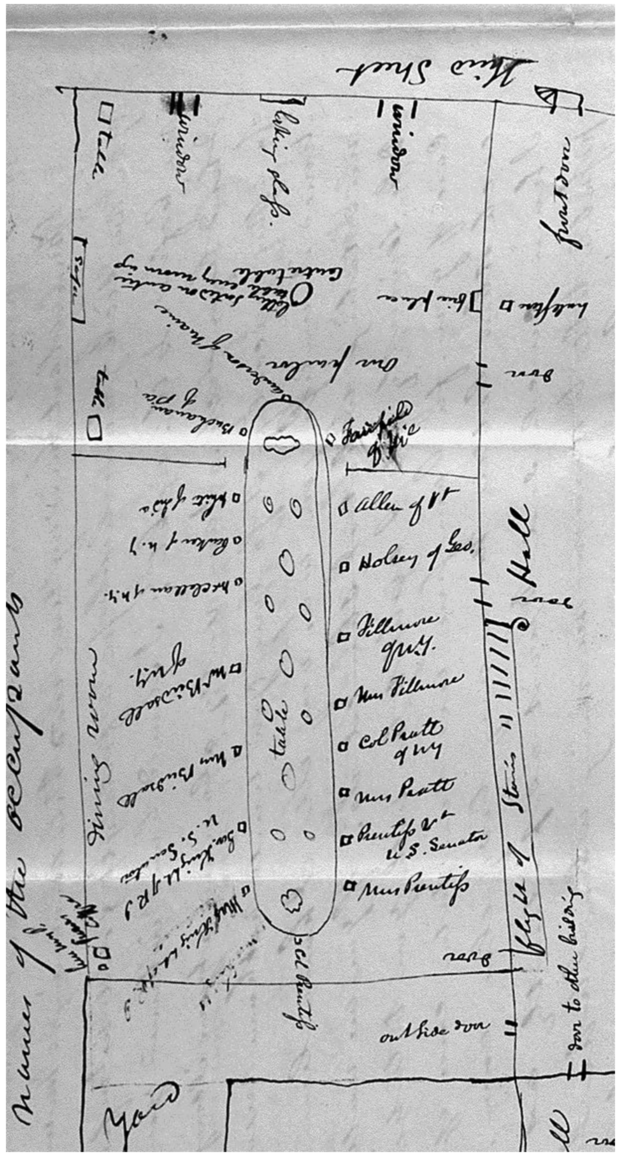
Before the Civil War, elite social life in Washington was marked by intense, sustained contact. Members were socially concentrated yet physically isolated from the rest of the country. In 1801, serving in Congress meant isolation from everything familiar and comforting: families, friends, and associates (Riley 2014; Zagarri 2013). Postal service was slow; a letter would take a week to travel from Washington to New England and even longer to the westernmost part of the country (Young 1966). Washington itself was exceptionally limited (Earman 1992). At the dawn of the century, there were only about 400 buildings (Green 1962, 4ff.). Yet the city grew quickly. A complicated system of etiquette emerged (Cooley 1829), including the pivotal role of women in establishing informal social ties with new arrivals (Allgor 2002; Earman 2000).

Practically speaking, joining Congress meant finding somewhere to live and eat. Limited choices meant that most members lacked fine control over the selection of housemates. Options included boardinghouses, hotels, and private residences, in order of increasing cost (Earman 1992; Shelden 2013, 102ff.; Young 1966). Boardinghouses were the most common selection, with many located near Capitol Hill (Earman 2000). They varied in size, expense, quality, and social composition. Coresidents typically shared meals with each other. In some, dining companions included military officers, professionals, and travelers; in others, legislators had separate dining rooms (Earman 1992). All told, boardinghouse life featured sustained contact and frequent opportunities to interact with coresidents but incomplete control over selection.

These opportunities were literally illustrated by Rep. Amasa Parker (D-NY), who included a drawing of his boardinghouse, Mrs. Pittman's, in a letter to his wife (December 31, 1837; see Figure 1). Seated at the dining table were representatives and senators, including future president Millard Fillmore. The dining room and parlor for legislators and their spouses were separate from those for other boarders, permitting members to talk politics over meals in privacy. The oval table encouraged conviviality, and the company and environment were conducive to informal socialization. This example highlights the limits—or disinclination—of at least some legislators to select coresidents. Parker's messmates included members of both chambers and major parties, Northerners, and Southerners. Few such illustrations remain, but evidence suggests that Parker's experience was not atypical (see, e.g., Shelden 2013).

Outside the boardinghouse, the ambient political environment included relatively few competing sources of information, especially early in the period. Parties were not solidly entrenched as institutions, leaders, and sources of influence until the 1830s, and partisan labels and attachments were not as meaningful as they later

FIGURE 1. A Map of Seating at Mrs. Pittman's from a Letter from Rep. Amasa Parker to His Wife, December 31, 1837, 25th Congress



Note: Clockwise from the top, they are Rep. Hugh Anderson (D-ME), Rep. John Fairfield (D-ME), Rep. Heman Allen (W-VT), Rep. Hopkins Holsey (D-GA), Rep. Millard Fillmore (W-NY), Mrs. Fillmore, Rep. Zadock Pratt (D-NY), Mrs. Pratt, Mrs. Prentiss, Sen. Samuel Prentiss (W-VT), Rep. John H. Prentiss (D-NY), Mrs. Knight, Sen. Nehemiah Knight (W-RI), Mrs. Birdsall, Rep. Samuel Birdsall (D-NY), Rep. Robert McClelland (D-NY), Rep. Amasa Parker (D-NY), Rep. Albert White (W-IN), and Rep. Andrew Buchanan (D-PA). Finding aid retrieved from http://hdl.loc.gov/loc.mss/eadmss.ms008132.

became (Aldrich 2011). Sectional conflict often counteracted partisanship (Poole and Rosenthal 1997), and standing committees did not fully emerge until the 1820s (Gamm and Shepsle 1989). To the extent that presidents lobbied, they did so informally, over dinners and on social occasions.[2] Outside interests were similarly undeveloped. Organized interests did not exist even in vestigial state until after Reconstruction (Jacob 2010; Thompson 1985; but see Peart 2018), and newspapers were controlled by party entrepreneurs rather than independent interests (Carson and Hood 2014; Pasley 2002).

Some boardinghouses developed distinct political reputations. For illustration, consider Carroll Row, sometimes called "Six Buildings" or "Duff Green's Row," on First Street between East Capitol and A Streets SE. Daniel Carroll built these row houses on speculation before the government relocated (Bryan 1904). Over the decades, tenants included boardinghouses, hotels, and

[2] Jefferson was the most ambitious in his pursuit of organized congressional action, holding regular dinners several times a week throughout his tenure (Scofield 2006; Young 1966).

businesses, including an apothecary and a pub. In 1834, the easternmost house of Carrol Row became Mrs. Sprigg's boardinghouse, which quickly emerged as a haven for abolitionists, earning the name "Abolitionist House" (Winkle 2013, Chap. 2). Residents included ardent antislavery advocate Joshua Giddings of Ohio, legislative allies William Slade of Vermont and Seth Gates of New York, and journalists Theodore Dwight Weld and Joshua Leavitt, who were publicists for abolition. Although Mrs. Sprigg had once been a slaveholder, by then she was a quiet abettor of the Underground Railroad.

Mrs. Sprigg's most famous resident was Abraham Lincoln, who lived there during his single term in Congress in 1847–8. This choice of lodging placed the future president in the midst of abolitionists and may have shaped his views on slavery (Paullin 1921). In 1847, Lincoln was one of three first-term representatives who chose to live at Mrs. Sprigg's. The other two were Elisha Embree (W-IN), and P.W. Tompkins (W-MS), the lone Southerner in the house. All five other coresidents, including Giddings, were Whigs who had previously lived in the boardinghouse.[3] But not all would remain. By the second session, Embree and Tompkins had departed for different accommodations, Embree to a different boardinghouse, and Tompkins to Brown's Hotel. The others—including Lincoln—all returned to Mrs. Sprigg's. Given Lincoln's later actions, this example is consistent with the possibility that he selected, chose to remain among, and may even have been influenced by his senior coresidents.

CONGRESSIONAL RESIDENCES, 1801–1861

To systematically analyze influence based on coresidence, we collected data from the 58 extant editions of the *Congressional Directory* from 1801 to 1861,[4] recording each member's residence and where they "messed," or took their meals, when available.[5] In all, we have residential and/or mess data for 11,775 legislator-residence-sessions.[6]

For each member, we coded whether they lived in a ***Boardinghouse***, ***Hotel***, or ***Private*** residence based on contextual information. Many editions of the *Directory* specifically labeled private residences, hotels included the word "hotel" in their names, and most boardinghouses were named after their proprietors. In our initial pass, we categorized 88% of 3,589 residences. All but 65 of the remaining locations included only one resident, suggesting they were private residences; at least, they will not affect our analysis of coresidence. We resolved remaining ambiguities on a case-by-case basis, using other editions of the *Directory*.[7]

We matched this information to data on legislators and votes.[8] Our unit of analysis is an undirected dyad of legislators in a session ($n > 1.32$ million). Our key causal variable is an indicator for ***Coresidence*** for a dyad.[9] To match residency information to voting records, we split roll-call votes into periods corresponding to editions of the *Congressional Directory*.

Our outcome measures focus on roll-call votes. Consistent with the literature, our primary outcome measure is voting ***Agreement***, the fraction of votes on which members of a dyad voted similarly out of the number on which both voted. These scores are employed to measure similarity because they include information omitted by ideal point estimation (Masket 2008). Since ***Agreement*** scores are logically impossible when comparing a legislator with a deceased colleague, we later rely on ideal point estimation, as described below.

To model coresident selection, we augmented these data with indicators for dyadic similarity in ***Party***, ***State***, or ***Region*** (North or South)[10]; ***Occupation***; whether they were in their ***First Session*** or ***Returning*** from the most previous session[11]; service in the ***Military***; graduation from ***College***; ***Age***; and ***Seniority***. Finally, absenteeism was common, reaching more than a third of votes in the 1840's, perhaps because of the prevalence of alcohol (Shelden 2013). Therefore, we also track

[3] The other returning members were John Blanchard (W-PA), John Dickey (W-PA), A.R. McIlvaine (W-PA), James Pollock (W-PA), and John Strohm (W-PA).

[4] Directories exist from as early as the first Congress, but editions from before the 7th Congress (before March 1801) correspond to legislatures that met in Philadelphia and New York. Until 1840, private printers produced the directories. Because they were not government documents, they are fugitive; some are entirely missing to history (Colket 1953–1956). Our sourcing of documents ranged widely. Goldman and Young (1973) present evidence from many directories before 1840, although we located some that eluded them. We obtained others from Google Books, ProQuest, and various libraries and archives. We engaged a third-party vendor to transcribe them. Finally, the authors and research assistants corroborated transcriptions.

[5] Each session had only one edition of the *Directory*, with the exception of the 1st session of the 29th Congress, which had two. We split this session at the date of the second edition.

[6] Data are missing for both residence and mess in 1,094 legislator-sessions, for an overall missingness rate of 8.5%. Missingness is most likely for legislators who arrived in DC after the *Directory* was printed or who served the remainder of terms of others who left office.

[7] We could not code 42 observations, all of which are vague: "Georgetown" or "the Navy Yard." Members who both resided at one of these vague addresses were not classified as coresidents.

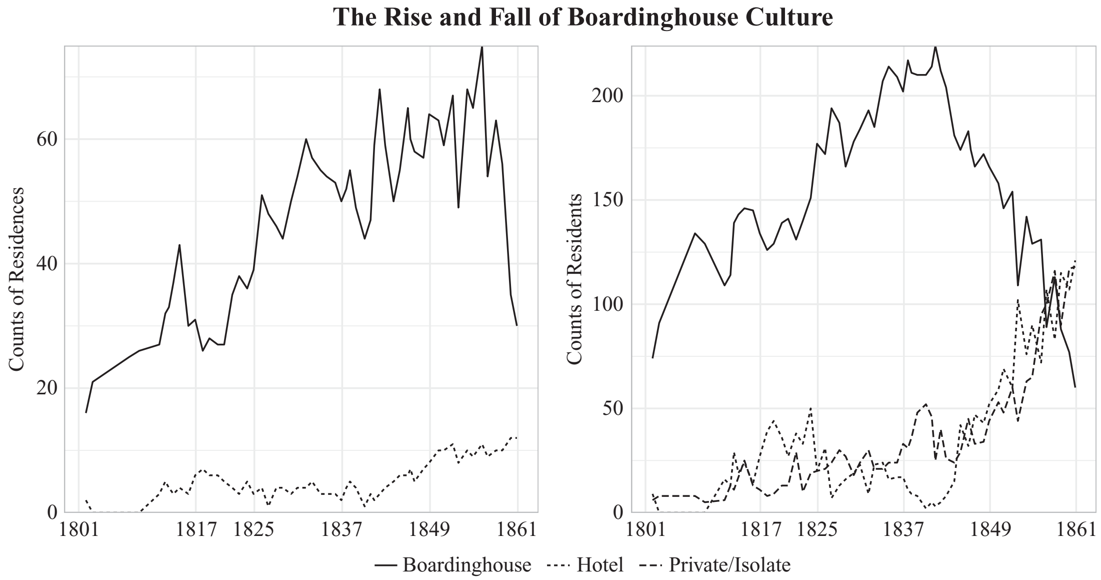
[8] Roll-call evidence comes from voteview.com. We cleaned these data for dates, party labels, and votes. During the collapse of the first party system, Poole and Rosenthal (1997) do not provide party labels, so we followed Martis, Rowles, and Pauer (1989), coding legislators by faction in the 1824 presidential election. Biographical data are from McKibbin (1992).

[9] Starting in the 27th Congress, the *Directory* reported both messes and residences, comprising about 30% of cases from those terms. In the vast majority, 97%, where both are listed, members messed where they resided. In the remaining cases, members lived and ate at different locations. It is plausible that social influence via cue-taking operated in both locales. Therefore, we combine residence and mess; by ***Coresidence***, we mean either literal coresidence or attendance at the same mess. Results are robust to excluding mess information.

[10] We identified region by secession. Results are robust to alternative definitions of the South.

[11] We use these measures rather than freshman status because the norm of rotation meant that many members had previously served but did not immediately return (see, e.g., Kernell 1977).

FIGURE 2. Counts of Residences of Members of the House of Representatives (*left panel*) and Residents Themselves (*right panel*) by Year and Type of Residence



Note: Both panels reveal that boardinghouses were the dominant residence type for members, emerging early on. Yet by the Late Jacksonian (1837–1849) and into Antebellum era, boardinghouses fell out of favor, gradually being replaced by hotels.

Coabsence, the fraction of votes for which neither voted or was present.[12] Ultimately, we estimate the following regression model:

$$\Pr(\boldsymbol{Coresidence}_{ijt} = 1) = \text{logit}^{-1}(X_{ijt}b + W_{ij}g + Z_{ij(t-1)}d + a_t),$$

where i and j index legislators, t indexes sessions, a_t refers to session-specific fixed effects, X_{ijt} to time-varying covariates, W_{ij} to time-invariant covariates, and $Z_{ij(t\text{-}1)}$ to lagged covariates.

Our data and analysis improve on previous studies in several ways. The quantitative evidence Young (1966) presents is remarkable for its time, but limited to 116 roll-call votes from 1800 to 1821, largely serving to corroborate qualitative evidence. Bogue and Marlaire (1975) use regression to analyze the 17th, 22nd, and 27th Congresses (1821–2, 1831–2, 1841–2), but they find no association between voting and coresidence. Parigi and Bergemann (2016) analyze data from 1825 to 1841 using fixed-effects analysis, reporting a positive relationship between agreement and coresidence. In a second study, they focus on members who moved midterm, finding that they voted less often with their erstwhile colleagues. Each design relies on assumptions to identify effects, assuming that the forces that bring legislators together are independent of potential outcomes, no time-varying unobservables confound inference, or no dynamic relationships exist between coresidence and voting. As we shall see, all these assumptions are incorrect.

THE RISE AND FALL OF BOARDINGHOUSE CULTURE

During the six decades before the Civil War, boardinghouse culture emerged, quickly became the dominant feature of social life, and then slowly declined (Figure 2). To anchor inferences, we split the period into five eras: the *Jeffersonian* (1801–17), Era of *Good Feelings* (1817–1825), *Early Jacksonian* before the Whig party emerged (1825–37), *Late Jacksonian* (1837–49), and *Antebellum* (1849–61). These breakpoints are useful but to some extent arbitrary, but all inferences are robust to small changes in definitions of eras.

When looking for shelter, legislators had many options (*left panel*, Figure 2). Boardinghouses were most common. At the peak in 1855, members collectively resided in 75 different boardinghouses. Boardinghouses were also the dominant selection (*right panel*). Of our nearly 12,000 cases, more than 77% chose a boardinghouse. In 12 sessions—about 20% of cases—more than 90% of legislators lived in one of these buildings. But by the end of this period, boardinghouse culture was moribund. As late as 1855, 57% of legislators still lived in boardinghouses, but that number declined to 26% by 1861. Simultaneously, the number of colleagues one could expect to live with had also dwindled. Between 1801 and 1846, a boardinghouse resident could expect to live with an average of 4 to 7 other legislators. By 1861, that number fell to 1.5. These trends reveal how the social role of the boardinghouse waned with the advent of the Civil War.[13]

[12] Descriptive statistics are presented in Appendix Table A1.

[13] A change in data quality is very unlikely to have driven this change. Data quality is poorer in the earlier years of our dataset. After about

TABLE 1. Logistic Regression Models of *Coresidence*

Era	Jeffersonian	Good Feelings	Early Jacksonian	Late Jacksonian	Antebellum
Congresses	7–14	15–18	19–24	25–30	31–36
Years	(1801–17)	(1817–25)	(1825–37)	(1837–49)	(1849–61)
Lagged coresidence	3.00***	3.17***	3.48***	3.12***	2.79***
	(0.27)	(0.21)	(0.14)	(0.19)	(0.06)
Lagged agreement	0.33***	0.11*	0.35***	0.28**	0.22***
	(0.09)	(0.05)	(0.07)	(0.10)	(0.04)
Lagged coabsence	−0.02	0.04	−0.01	0.03	0.06
	(0.04)	(0.05)	(0.03)	(0.02)	(0.03)
Same party	1.89***	0.52***	0.76***	1.36***	0.26*
	(0.16)	(0.16)	(0.11)	(0.19)	(0.11)
Same state	0.78***	1.04***	1.11***	1.10***	0.79***
	(0.09)	(0.16)	(0.12)	(0.13)	(0.10)
Same region	0.53***	0.27***	0.33**	0.55***	0.49***
	(0.07)	(0.07)	(0.10)	(0.06)	(0.08)
Same occupation	0.13*	0.05	0.05	0.02	0.01
	(0.07)	(0.04)	(0.03)	(0.03)	(0.06)
Age difference	−0.04	−0.07***	−0.09**	−0.03**	−0.06
	(0.02)	(0.02)	(0.03)	(0.01)	(0.03)
Seniority difference	−0.01	−0.10**	0.03	0.02	−0.14***
	(0.02)	(0.05)	(0.02)	(0.02)	(0.04)
Both first session	0.05	0.12	0.36***	0.22***	0.08
	(0.10)	(0.12)	(0.10)	(0.07)	(0.35)
Both in previous session	−0.14	−0.06	−0.16	−0.05	−0.07
	(0.10)	(0.16)	(0.11)	(0.11)	(0.12)
Neither in previous session	0.30*	−0.06	−0.16*	0.10	0.00
	(0.11)	(0.15)	(0.08)	(0.05)	(0.14)
Both college	0.16*	0.14***	0.05	0.02	0.09
	(0.07)	(0.03)	(0.05)	(0.04)	(0.05)
Neither college	0.09	−0.04	0.31***	0.16***	−0.04
	(0.08)	(0.03)	(0.05)	(0.04)	(0.07)
Both military	0.01	0.13	−0.06	−0.07	0.32
	(0.10)	(0.09)	(0.08)	(0.09)	(0.14)
Neither military	0.09*	−0.00	0.08	−0.05	−0.06
	(0.04)	(0.09)	(0.04)	(0.04)	(0.06)
n Dyads	111,000	134,522	274,565	384,410	317,086
n Legislators	521	485	728	898	890
n Sessions	11	8	12	15	12

Note: The table presents the results of logistic regression models for each era. Standard errors are based on 1,000 bootstrap resamples over congressional sessions. Models also include indicators for each congressional session and for availability of covariates.
*$p < 0.05$, **$p < 0.01$, ***$p < 0.001$.

WHO LIVED WITH WHOM, AND WHY?

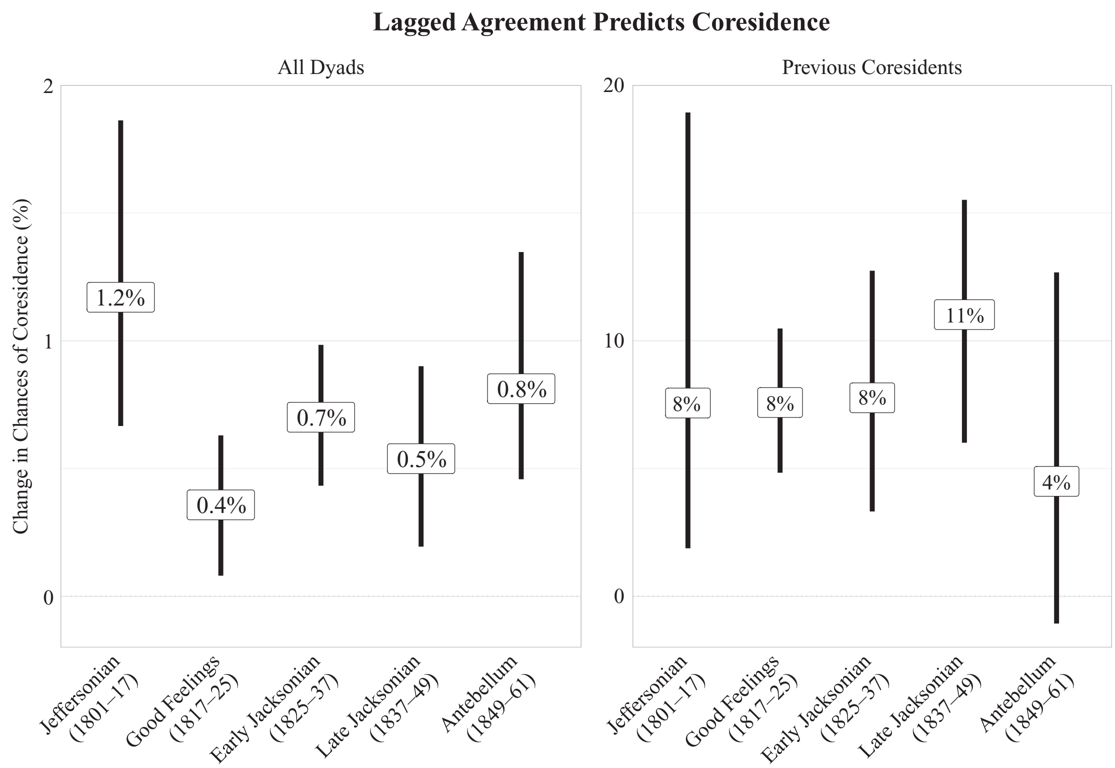
With whom did legislators choose to live? To answer this question, we estimated logistic regressions of ***Coresidence***. Given the over-time variation we show above, we fit separate models by era, with fixed effects at the period (i.e., congressional session) level. For inference, we bootstrap at the congressional session level, which is a common procedure used in networks, as dyads themselves are not exchangeable (see, e.g., Leifeld, Cranmer, and Desmarais 2018).

Based on the logic of homophily, we hypothesized that similarity would explain who lived with whom. Indeed, ***Same Party***, ***State***, or ***Region*** all reliably predict who lived with whom (see Table 1).[14] These estimates are substantial and persistent. The estimates for ***Party***, in particular, cohere with conventional wisdom. The strongest relationships appear during the first and second party systems, the *Jeffersonian* and *Late Jacksonian*. In the *Jeffersonian* era, legislators from the ***Same Party*** had a predicted probability of ***Coresidence*** that was 4.4 percentage points larger than that of

1825, quality is much improved because the government formalized the printing of the *Congressional Directory*.

[14] Our results are robust to many alternative specifications. Results are similar if we use dyadic cluster-robust standard errors (Aronow, Samii, and Assenova 2015; see Appendix, A2–A3). Similar results also emerge when adjusting for legislator-level fixed effects (Appendix, A4–A5). One might also object that legislators did not choose coresidents, but residences, in which case the data constitute a bipartite network. We therefore estimated bipartite exponential random graph models (Wang, Pattison, and Robins 2013), which again yielded similar inferences. See Appendix (A6–A7).

FIGURE 3. Average Marginal Effects of Lagged Agreement on the Probability of Coresidence in Percentage Points



Note: Bars depict 95% intervals based on bootstrapping over congressional sessions.

noncopartisans (95% interval [3.8%, 5.6%]).[15] This difference is larger than it may seem. The overall rate of ***Coresidence*** in this era was only 3.9%, meaning copartisanship almost doubles the chances of living together. Other similarity measures also predicted ***Coresidence***, albeit more sporadically. Overall, the models strongly suggest homophily in coresident selection.

More troubling for inference about influence, homophily also appears between ***Coresidence*** and ***Lagged Agreement***. Through all eras, ***Lagged Agreement*** strongly predicts whether members lived together. In substantive terms, the estimates are smaller than those for ***Same Party***. For example, in the *Jeffersonian* era, the average marginal difference in predicted probabilities for ***Lagged Agreement*** is 1.2% (see Figure 3, left panel). Despite this small size, the estimates are precisely estimated; all 95% intervals exclude zero. Thus, as with party and geography, there is clear evidence of homophily in voting and coresidence.

Whatever the reason for cohabitation, members who lived together were likely to repeat the arrangement. The strongest predictor of ***Coresidence*** is ***Lagged Coresidence***. About 40% of cases of ***Lagged Coresidence*** were repeated, ranging from 38% to 44% across eras, revealing a potential problem. Legislators who usually disagreed may have sought alternative lodging, while those that agreed continued living together. Longitudinal designs can mistake the dissolution of ties for different levels of influence, conflating influence with the underlying causes of such "unfriending" (Noel and Nyhan 2011). If dissimilarity explains moves, then comparing coresident dyads to noncoresident ones will overestimate influence. Fixed effects models assume away such dynamic relationships between treatment and outcome (Imai and Kim 2019), so a complete picture requires analysis of repeated ***Coresidence***.

Therefore, we analyzed dyads who lived together in the previous session, focusing on repeated ***Coresidence***.[16] The results tell a different story from the full sample (see Table 2). The roles of ***State***, ***Party***, and ***Region*** are weaker, though intermittently predictive. In contrast, homophily based on similar voting not only remains important, but the relationship between agreement and coresidence is also more pronounced. The coefficients on ***Lagged Agreement*** are substantial and significant in the first four eras. Differences in predicted probabilities are about 10 times larger (Figure 3, right

[15] Differences in predicted probabilities are calculated with numerical derivatives for continuous covariates and differences for dichotomous ones, using coefficients from bootstrap resamples.

[16] We exclude two sessions for which we lack evidence from the previous session.

TABLE 2. Logistic Regression Models of Repeated *Coresidence*

Era	Jeffersonian	Good Feelings	Early Jacksonian	Late Jacksonian	Antebellum
Congresses	7–14	15–18	19–24	25–30	31–36
Years	(1801–17)	(1817–25)	(1825–37)	(1837–49)	(1849–61)
Lagged agreement	0.30***	0.33***	0.35***	0.52***	0.19
	(0.23)	(0.08)	(0.11)	(0.12)	(0.17)
Lagged coabsence	–0.08	0.10	–0.28***	–0.06	–0.09
	(0.07)	(0.10)	(0.07)	(0.05)	(0.07)
Same party	0.51**	0.15	–0.14	0.62***	0.15
	(0.23)	(0.16)	(0.20)	(0.20)	(0.41)
Same state	–0.03	0.14	0.14*	0.08*	0.02
	(0.07)	(0.09)	(0.06)	(0.04)	(0.10)
Same region	0.35*	0.19	0.49***	0.23	0.40*
	(0.14)	(0.16)	(0.14)	(0.13)	(0.14)
Same occupation	0.18	0.34**	–0.03	–0.14	–0.17
	(0.51)	(0.23)	(0.12)	(0.25)	(0.23)
Age difference	0.23**	0.02	–0.04	0.01	–0.07
	(0.09)	(0.07)	(0.07)	(0.04)	(0.06)
Seniority difference	–0.15	–0.18*	–0.09	–0.31***	–0.01
	(0.13)	(0.06)	(0.06)	(0.06)	(0.10)
Both college	0.08	–0.03	–0.22*	–0.07	–0.08
	(0.14)	(0.11)	(0.10)	(0.08)	(0.14)
Neither college	–0.07	0.15	0.38***	0.13	0.09
	(0.15)	(0.12)	(0.09)	(0.10)	(0.09)
Both military	0.15	0.35	–0.06	–0.40***	0.18
	(0.17)	(0.20)	(0.17)	(0.11)	(0.41)
Neither military	0.08	–0.10	0.09	0.16	–0.04
	(0.23)	(0.15)	(0.11)	(0.14)	(0.17)
n Dyads	1,881	2,543	3,857	5,175	4,706
n Legislators	341	422	646	791	675
n Sessions	7	8	12	15	11

Note: The table presents the results of logistic regression models for each era on the subsamples of dyads who were coresidents in the previous period. Sessions are excluded when no evidence exists for the relevant prior session. Standard errors are based on 1,000 bootstrap resamples over congressional sessions. Models also include indicators for each congressional session and for availability of covariates. *p<0.05, **p<0.01, ***p<0.001.

panel) than in the overall model, with an average increase of 7 percentage points. We conclude that legislators were likely to rely on political views not only to choose coresidents but also to choose whether to dissolve social relationships. As a consequence, fixed effects are inappropriate for identifying the effect of ***Coresidence*** on ***Agreement***.

DYNAMIC IDENTIFICATION OF *CORESIDENCE* EFFECTS

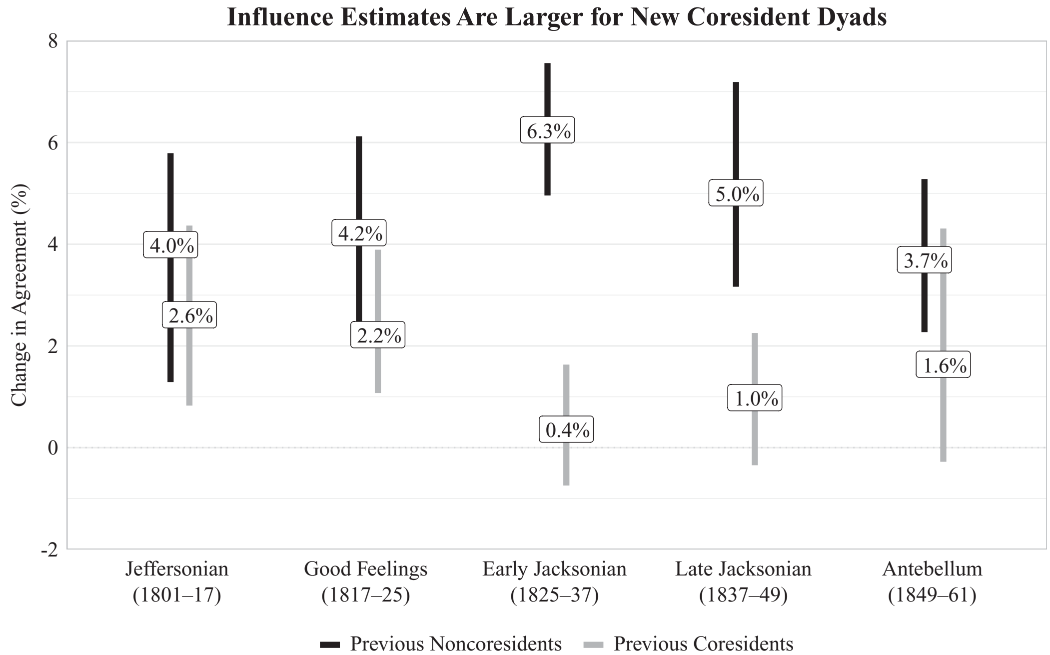
Given that ***Coresidence*** is predicted by previous voting ***Agreement***, research designs such as fixed effects do not plausibly identify the effects of the former on the latter. An alternative identification strategy is to use inverse probability weighting (Blackwell and Glynn 2018) to explicitly account for dynamic relationships between treatment and outcome. This strategy relies on strong identifying assumptions, most notably sequential ignorability conditional on previous ***Coresidence*** and covariates, including ***Lagged Agreement***. An important cost is that we no longer adjust for unobservable, time-invariant covariates, as with fixed effects. Although the cost is steep, our strategy is to offset it by pairing our aggregate analysis with a better-identified subsequent study based on legislator deaths, described below.

We used models with similar forms to those from above to estimate the probability of ***Coresidence***, including fixed effects at the congressional session.[17] Because the selection processes for previously coresident and noncoresident dyads were different, we analyze these groups separately. For weights, we took predicted probabilities of ***Coresidence*** and assigned the inverse probability to coresident dyads and the inverse of its complement to noncoresident dyads.[18] Doing so substantially improved balance (see Appendix, A8–A9). Finally, we estimated weighted linear models of ***Agreement*** on ***Coresidence***. For statistical inference, we bootstrapped at the congressional session level,

[17] Balance was considerably improved by adding second-order interactions to the models from the previous section. In the case of the Antebellum era, we also included higher-order interaction terms with ***Same Party***. Use of these complicated models is warranted because our goal is to yield weights that eliminate imbalance rather than to make inferences about specific covariates.

[18] Following recommended practice, we stabilized weights by multiplying by average ***Coresidence*** for treated dyads and its complement for the untreated.

FIGURE 4. Estimated Effects of Coresidence on Agreement in Percentage Points



Note: Bars depict 95% intervals. Estimates are calculated using inverse probability of treatment weighting, with the block bootstrap over sessions.

resampling 1,000 times over sessions, testing the null hypothesis of zero average effect.

The results of our analysis appear in Figure 4. The black bars depict estimates of the effect of ***Coresidence*** on ***Agreement*** for dyads who did not live together in the previous session; the gray bars display the same for previous coresidents. In both cases, results are shown by era. Since ***Agreement*** ranges from 0 to 100%, results can be understood in terms of the percentage point increase in roll-call voting agreement due to ***Coresidence***.[19]

In all cases, our estimates of the effects of ***Coresidence*** on ***Agreement*** are positive and, in the case of previous noncoresidents, statistically significant.[20] The appendix presents several robustness checks, including a specification that adjusts for whether both members of a dyad chose to live in a boardinghouse and another in which we include fixed effects at the level of the individual legislator (see Appendix, A14–A15). In both cases, inferences are similar to those reported in the main text.

On balance, these findings suggest that social influence and cue-taking occurred regularly throughout the decades before the Civil War. On average, effect estimates were about 4.6 percentage points for dyads who lived apart in the previous session and about a third of that among pairs who previously lived together.[21]

These results depart from previous work. Parigi and Bergemann (2016) analyze the period from 1825–41, roughly equivalent to the *Early Jacksonian* era, using fixed effects to find estimates of 10 to 13 percentage points. For previous noncoresidents, we do see our largest estimated effect in that era—but our estimate is only half as large as is theirs. Given the evidence of homophily, this difference may be due to the improved research design.

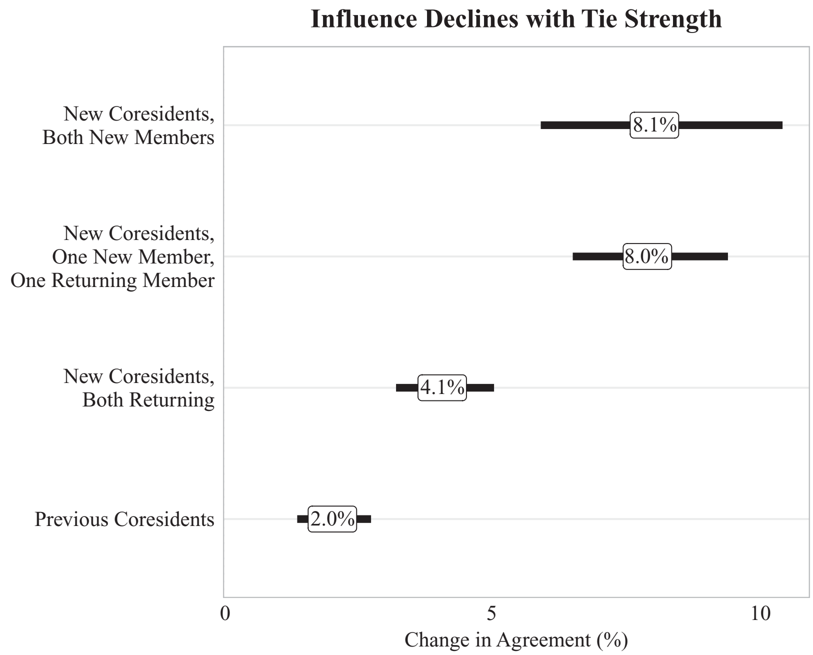
We go beyond previous studies to test hypotheses about different sets of dyads. Based on the logic of "choosing to be changed" (Santoro 2017), we expected new members to have chosen coresidents with influence in mind and thus to have larger effects. Further, tie

[19] At the legislator-dyad level, we are missing a small fraction of ***Agreement*** scores (about 0.4%). Further, a substantial number of legislators did not case votes on specific roll calls, meaning there is attrition in this study. In the Appendix (A10–A13), we report details on analyses of worst-case scenarios at both levels to bound the point estimates of the effect of ***Coresidence***. At the legislator-level, worst case bounds very tightly bound the results presented in the paper. At the roll-call vote-level, worst-case bounds remain positive in most cases.

[20] We also estimated the effects on ***Agreement (with Coabsence)***, which includes all votes for which both legislators in a dyad are eligible, counting mutual absences as agreement, and on ***Agreement (Imputed)***, which uses ideal point estimation to impute missing roll-call votes, and then recalculates ***Agreement***. In both cases, results are similar to those in the text. Moreover, we estimated the effects on ***Coresidence*** on the rate of ***Coabsence***, which were small in magnitude in all cases (<1%), insignificant in all cases for previous coresidents, and insignificant in three of five cases for previous noncoresidents. These significant effects may be due to many factors, including, e.g., coresidents suffering from communicable diseases, though it could also be due to strategic nonvoting. See the Appendix (A14–A15).

[21] We also tested the sharp null hypothesis of no effect by permuting the ***Coresidence*** vector. For previous coresidents in the Early Jacksonian era, the p-value is 0.530 and we fail to reject the sharp null. In all other cases, p-values are 0.022 or less.

FIGURE 5. Estimated Effects of *Coresidence* on *Agreement* in Percentage Points



Note: Bars depict 95% intervals. The subgroups of dyads differ in terms of both tie strength and presence of a new member.

strength should increase with shared history. Based on the "strength of weak ties" (Granovetter 1973), we expected effects to be small for previous noncoresidents who had lived together in the more distant past; smaller still for previous coresidents who lived together only once, in the most previous session; and smallest for previous coresidents who lived together more than once.

Figure 5 displays the conditional effects of ***Coresidence*** for each of these subgroups. In general, the results support both that new members may select into residences that are likely to influence them and that influence declines with tie strength. There is a clear, statistically significant difference between dyads with at least one new member and dyads with two returning members who had never lived together.[22] Moreover, there is a two-percentage-point difference between the estimate for those returning members who had never lived together and those for the strongest ties, legislators who had previously lived together (two-tailed $p < 0.001$).[23]

Once we adjust for the dynamic relationship between residence selection and voting behavior, there is robust evidence of social influence among legislators throughout the six decades prior to the Civil War. Our results further suggest that existing studies, which have focused on subsets of this period, have also overestimated these effects. Finally, our estimated effects are smallest in the years just before the Civil War, coincident with the decline in boardinghouse culture we documented above.

But our research design still relies on strong identifying assumptions, including that ***Coresidence*** is sequentially ignorable, conditional on covariates. This assumption has varying plausibility. For example, it is less plausible when we lack lagged ***Agreement*** scores, as with new members. We are reassured by the evidence of declining influence with increasing tie strength. Yet, despite the clear improvement in identification, this research design does not rival those that exploit randomization, such as Rogowski and Sinclair's (2012) study of the office lottery. Next, we offer one last study to better approximate randomization.

IDENTIFYING *CORESIDENCE* EFFECTS WITH LEGISLATOR DEATHS

We leverage occasions when a legislator died in office to identify the effect of his sudden absence on surviving coresidents.[24,25] Using the *Biographical Directory of the United States Congress*, we identified legislators

[22] For example, comparing "New Coresidents, Both New Members" and "New Coresidents, Both Returning Members" yields a two-sided bootstrapped $p < 0.001$.

[23] We also estimated subgroup effects for boardinghouse residents and for hotel residents, expecting larger effects for the former group. We observe statistically significant differences in that direction for the *Early* and *Late Jacksonian* Eras, but not in other cases. See Appendix Table A8 (A15).

[24] Azoulay, Zivin, and Wang (2010) use a similar research design to study the effect of academic authors' deaths on their coauthors' productivity.

[25] The estimated effects we report are local to boardinghouse and hotel residents, as private residents did not (typically) have coresidents.

who died during congressional terms between 1801 to 1861. We include such legislators when they had at least one coresident. Further, to permit reliable measurement, we included only deceased legislators who cast at least 20 roll-call votes before their deaths and survivors who cast at least 20 votes before and 20 votes after. Ultimately, we identified 60 deceased legislators.[26]

These deceased legislators were representative of their population. Their deaths occurred throughout the period. The earliest was Rep. Charles Johnson (R-NC), who died on July 23, 1802; the latest, Rep. Silas Burroughs (R-NY), who died on August 3, 1860. The average deceased legislator resided in a boardinghouse, served two terms, and lived with about six coresidents. Twenty hailed from the South. More Virginians are included in the sample than denizens of any other state, but members of 20 delegations are included. The set includes Democrats, Federalists, Republicans (in both party systems), Whigs, and members of several third parties. The surviving coresidents are similarly representative.

Because legislators do not vote after their deaths, we cannot rely on ***Agreement*** scores. Therefore, we estimate changes in ideological distances between survivors and their deceased colleague before and after his death. If any legislator exerts influence on his coresidents, he would have had an attractive effect on the ideal points of his coresidents. After death, we should see those survivors move away from their erstwhile coresident's ideal point, perhaps now more influenced by their own preferences, constituencies, parties, state delegations, or remaining or even new coresidents. The result should be that the distance between deceased legislator's and survivors' ideal points increases after death.

To estimate changes that occurred when a legislator died, we need separate measures for each survivor before and after his colleague's death. Therefore, we used bridging (Poole 2005, Chap. 6). First, for each deceased legislator, we built a dataset of votes and legislators. For legislators who died between sessions, we included the sessions immediately before and after the death; for those who died within a session, we restricted attention to that session. In both cases, we included the deceased legislator and all noncoresidents who served in the House both before and after the death date. We also included each survivor twice: once for votes before his colleague's death and once for after. Noncoresidents are the bridges that identify distances between ideal points before and after death.[27]

We observed both before- and after-death ideal points for all surviving coresidents, so the attrition rate in this study is zero.

For each deceased legislator, we simulated two-dimensional ideal points using item response theory (Clinton, Jackman, and Rivers 2004).[28] While multidimensional ideal point models suffer from nonidentification of several sorts, we are interested in distances between points rather than nominal values. Therefore, our measure is already invariant to rotation and translation. For nonidentification due to the scale of the policy space, we standardized each set of ideal points with respect to the bridge observations. Nevertheless, combining measures from different models means comparing sessions with different scales and thus the assumption that the policy space does not expand or contract. It is plausible, however, to make comparisons between different sets of ideal point distances in proportional terms using logs—effectively gauging the ratio by which the distances between legislators grew or shrank relative to the average distance between pairs of (bridge) legislators. Our outcome is therefore ***log Ideal Point Distance*** between a deceased legislator and surviving coresident, measured both before and after death.

To provide an effective contrast for residences in which legislators died, we used a similar strategy to estimate ideal point drift in residences that experienced no deaths.[29] That is, for each of the remaining residences that housed more than one resident, we used the same technique as described above on each resident—essentially treating all residents from these residences as control cases—that is, nondecedents. We calculated the (log) distances before and after that legislator's "death," which we set at the break between sessions, to yield a comparable outcome variable. The result is a set of 28,362 (directed) dyads over 967 residence-terms.

Our analysis plan focuses on the changes in ***log Ideal Point Distance*** aggregated to the level of the residence.[30] Thus, we regard each residence as either having been treated to a resident's death or not. First, we calculate the change in ***log Ideal Point Distance*** after each legislator death or session break, based on treatment status. Second, we calculate the average of those changes at the level of the residence (so n = 60 residences with deaths + 967 residences without deaths = 1,027). Finally, we estimate a weighted least squares model of the mean change in ***log Ideal Point Distance***, weighted by the number of dyads in each

[26] One boardinghouse experienced the deaths of two legislators: Jonathan Cilley (D-ME), who famously died in a duel, and Timothy Carter (D-ME). Both died while residing at Mr. Birth's. Our research design assumes no changes other than (a single) death, so we exclude these cases.

[27] Bridging strategies are criticized for using incommensurate votes from different contexts, small numbers of bridging legislators, or placing legislators on a common scale over large timescales. Our application suffers from none of these issues. We do not use votes from different contexts, our number of bridges is large, and we never bridge more than two sessions from one term.

[28] For each, we use the pscl package in R (Jackman 2017) to simulate ideal points, with 5,000 burn-in iterations and 1,000 posterior samples. We rely on the typical assumption that absences are missing at random.

[29] In previous versions of this paper, our analysis plan focused only on residences in which a death occurred. However, a placebo test revealed that that design suffered from bias. A potential source is that different sorts of votes are scheduled earlier vs. later in a term, creating correlation with before and after death. We thank an anonymous reviewer for recommending the placebo test.

[30] See the Appendix for a detailed description of our analysis plan (A16).

residence to track the amount of information provided by each observation. The main model specification is

mean change in ***log Ideal Point Distance***$_{rt}$ = ***Resident Death***$_{rt} + a_t$,

where r indexes residences; t indexes congressional terms; ***Resident Death*** is the treatment variable, which is 1 if and only if a resident of residence r died within term t; and a_t represents fixed effects for era. This model emphasizes the focal role of the residence, though other designs are reasonable and yield similar results.[31] We also report on specifications including an indicator for ***Within Session***, which indicates whether death occurred midsession (1) or between sessions (0); an indicator for ***Boardinghouse***, which indicates whether a residence was a boardinghouse (1) or not (0); and a count of the ***Number of Legislator Residents*** in that residence-term.

Identification relies on one primary assumption. We assume that deaths occurred as though at random, so that each boardinghouse was equally likely to have experienced a legislator's death.[32] While legislators died of many different causes (Maltzman, Sigelman, and Binder 1996), we regard this assumption as at least facially plausible. For statistical inference, we use bootstrapped standard errors, resampling over residence-terms, with two-tailed p-values.[33]

SURVIVORS DRIFT AWAY FROM DECEASED COLLEAGUES

As we hypothesized, surviving legislators drifted away from the ideological positions of their deceased colleagues. Overall, the estimated effect of ***Resident Death*** on mean change in ***log Ideal Point Distance*** was 0.072, with 95% interval [0.002, 0.140] and $p = 0.04$. Effects are interpretable in proportional terms, so this result means that a colleague's death causes the distance between his ideal point and that of his deceased coresident to grow by about 7%, similar to the magnitude reported in the IPW study above.[34]

To vet the efficacy of this design, we deploy a placebo study, focusing exclusively on the 967 residences that did not experience a death, effectively treating residents of such residences as, alternately, "placebo decedents" and "placebo survivors," with "placebo death" occurring at the break between sessions. Here, we randomly sample 60 residences—none of which experienced an actual death—and recode ***Resident Death*** to be equal to 1 only in those cases. We then use the weighted least squares procedure described above, bootstrapping for statistical inference.

We repeat this placebo test procedure 1,000 times to yield a distribution of placebo estimates and p-values.[35] Given that none of the residences in this test experienced death, if the design is unbiased, we would expect to see (1) a flat, uniform distribution of placebo p-values and (2) that our estimated effect appears as an outlier in the distribution of placebo effects. Figure 6 confirms both of these expectations. Exactly 5% of placebo p-values were less than or equal to 0.05, and only 1.6% of placebo estimates were greater than or equal to the actual estimated effect of 0.072. We conclude that this design does not suffer from overrejection of the null or bias.

We also examined several alternative models. First, we used the same design to estimate effects on mean change in (unlogged) ***Ideal Point Distance***, which yields an estimated effect of ***Resident Death*** of 0.042 [–0.029, 0.109], in a similar direction to the effect for logged distance but not statistically significant. The result is therefore sensitive to transformation of the outcome variable. However, as we argued above, logging is more appropriate in this case, given that ***Ideal Point Distance*** is positive-valued, and that the ideal points are measured separately by congressional term. Second, we estimated a model including the multiplicative interaction of ***Resident Death*** and ***Boardinghouse***, along with the constituent terms, expecting that deaths in boardinghouses would have larger effects due to the more close-knit ties such residences fostered. The interaction term was positive though imprecisely estimated, 0.068 [–0.072, 0.214]. However, the effect for boardinghouse residents is represented by the sum the coefficients on the interaction term and the main effect of ***Resident Death***, and this quantity remains significant (0.970 [0.007, 0.186], $p = 0.032$). Similarly, we estimated two more interaction models, one with ***Within Session*** and a second with ***Number of Legislator Residents***. In each case, the interaction terms were small and imprecisely estimated.

For robustness, we also estimated a version of this model at the dyad-level, rather than at the residence-level. Specifically, the model specification in this case is

log Ideal Point Distance$_{dt}$ = ***After Death***$_{dt}$ + ***Resident Death***$_{dt}$ × ***After Death***$_{dt}$ $+ g_d$,

where d indexes dyads, t indexes congressional term, ***After Death*** indicates whether the outcome is measured before (0) or after (1) death/session break, and g_d is a dyad fixed effect. The main coefficient for ***Resident Death*** is subsumed by the fixed effects, and the quantity of interest is now the coefficient on the interaction term. The main model specification only differs by including fixed effects for era, which would also have been subsumed by the dyad fixed effects. If we omit era fixed effects from the main specification, the two models are identical. The estimates from the dyadic model are similar to those from the residence-level

[31] For example, this design is similar to a dyad-level fixed effects model; see below.

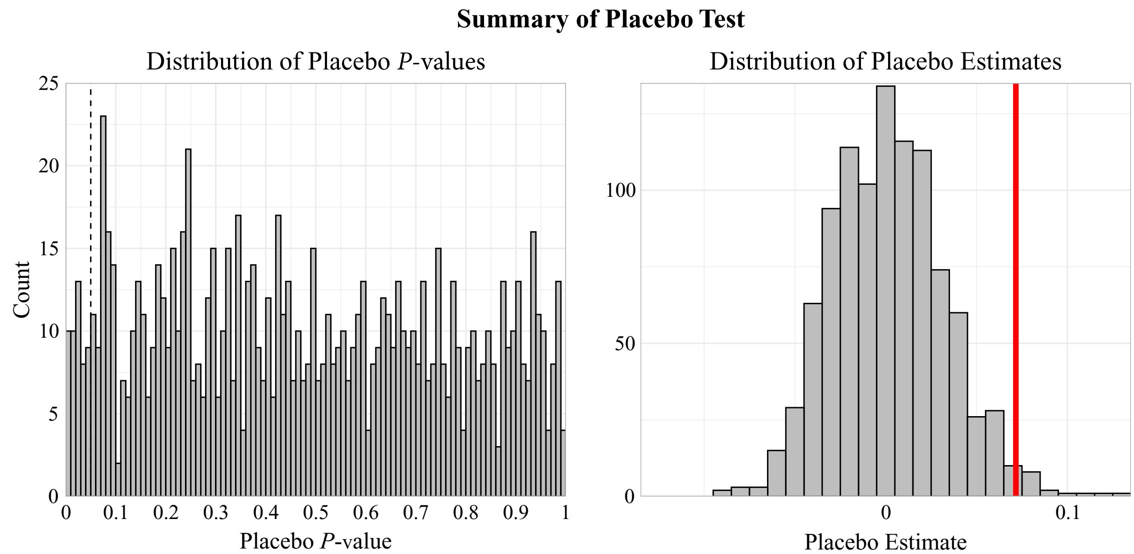
[32] Another assumption that would identify this design is that legislators do not anticipate their coresidents' deaths and change their behavior as a consequence.

[33] Results are similar when using heteroscedasticity-robust standard errors; see Appendix Table A10 (A19).

[34] See Appendix Table A9 (A17) for details on regressions.

[35] We replicated this placebo test using classical estimation and heteroscedasticity-robust standard errors; results are similar. See Appendix Figure A2 (A18).

FIGURE 6. Results of 1,000 Simulated Placebo Tests Using All Residences That Did Not Experience a Death of a Resident



Note: Residences that did experience a death are excluded. The *panel on the left* displays a histogram of the (bootstrapped) *p*-values from these tests. The *vertical dashed line* indicates $p = 0.05$. The *panel on the right* displays a histogram of the estimated placebo effects, and the *solid vertical line* indicates the observed estimate of 0.072. Consistent with the design assumptions, only 5% of placebo *p*-values are less than or equal to 0.05, and only 1.6% of placebo estimates exceed the actual estimated effect.

model, though the *p*-value increases slightly (0.060 [-0.007, 0.128], $p = 0.078$); see Appendix Table A11 (A20). Therefore, it appears that dyad-level analysis, and the concomitant omission of era-level fixed effects, decreases the point estimate, suggesting overtime variation consistent with that seen in the IPW study.

Finally, to judge whether extreme outliers are responsible for these results, we reestimated our residence-level main specification, replacing mean change in ***log Ideal Point Distance*** with median change, which is more robust to outliers. In this case, the point estimate is slightly higher, and the *p*-value decreases slightly (0.078 [0.007, 0.153], $p = 0.034$). Based on this model, it appears that, if anything, outliers may dampen the estimated effect.

We conclude from the analyses that the evidence from legislators' deaths largely confirms that from the IPW study, both in terms of sign and magnitude, while noting that this result extends only to log distances and the associated changes in proportional terms.

CONCLUSION

We reported on the most systematic collection of evidence on residences and social influence in U.S. House of Representatives before the Civil War and have made several contributions. With evidence from both secondary historical sources and primary sources, we showed the role of boardinghouse culture in the social milieu of Washington. While previous analyses have been limited in scope, never analyzing more than two decades, we examined 60 years' worth of evidence. We further documented the important and heretofore unexamined role of homophily in both selection of residences and, more troubling for inference, "unfriending" (Noel and Nyhan 2011), in which conflicting coresidents move apart. These results cast doubt on previous studies.

To cope with selection, we presented two quantitative studies. First, we used a weighting strategy designed to account for dynamic relationships between treatment (coresidence) and outcome (voting behavior). In so doing, we found that coresidence had identifiable, positive effects on voting agreement—but at only about half of the levels reported in previous studies. Nevertheless, we also found support for key hypotheses. Our estimates of effects were highest for new members, who are likely to have both the weakest social ties (Granovetter 1973) and the most interest in "choosing to be changed" (Santoro 2017), and smallest for members who had previously and repeatedly lived together.

Despite the improvement yielded by this strategy, it still requires strong identifying assumptions. Thus, in our second study, we examined legislators who died in office. Assuming such events occurred as though at random, we showed that surviving coresidents drift away from their erstwhile colleagues, increasing ideological distance by 7%. This identification strategy is stronger than that in any previous study of this era and represents the strongest evidence yet of social influence among elites before the Civil War.

That said, there remain some important limitations to our study. The primary limitation is that we lack experimental manipulation. All our analyses are based on observational data and therefore depend on strong identifying assumptions. Furthermore, we lack some key evidence, including data on the prices of lodging in different residences, as well as their capacities. Finally, at times we cannot discriminate between competing explanations. For example, we cannot say whether the estimated effect of coresidence on coabsence is due to contagious diseases spreading through residences or due to strategic abstention on key roll-call votes.

Nevertheless, on balance our analysis suggests that the six decades before the Civil War were characterized by some stable regularities and some temporal changes. Coresidence was persistently predicted by similarity in party, state, region, and voting agreement. Repeated coresidence was even more strongly predicted by such agreement. But the aggregate analysis based on dynamic identification indicates changes over time, especially for first-time coresidents. We observed the largest effects in the *Early Jacksonian* era, at the apex of boardinghouse culture, with smaller estimates of influence earlier and later.

This period is important in its own right and as a near-ideal opportunity to study cue-taking, but our analysis also offers important inferences for the modern era. Intriguingly, our results are consistent with the idea that informal socializing counteracts elite polarization. While we certainly lack direct evidence on this point, we have illustrated trends that are coherent. Boardinghouse culture—in terms of the number of residences, the number of legislators who lived in them, and the number of coresidents one could expect—sharply declined in the 1850s, although the homophily we document does not. Further, our estimates of social influence decline to their nadir during the elite polarization of the late 1850s. There is a clear similarity between the declining informal socializing and increasing polarization of that period, and similar trends in the modern era.

SUPPLEMENTARY MATERIALS

To view supplementary material for this article, please visit http://dx.doi.org/10.1017/S0003055421000630.

DATA AVAILABILITY STATEMENT

Research documentation and/or data that support the findings of this study are openly available at the American Political Science Review Dataverse: https://doi.org/10.7910/DVN/LIJSWE.

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CONFLICT OF INTEREST

The authors declare no ethical issues or conflicts of interest in this research.

ETHICAL STANDARDS

The authors affirm this research did not involve human subjects.

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