How Crisis Reshapes Government Talent *

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Abstract

How do public servants respond to institutional crises? Existing theories emphasize bureaucratic resilience, but shocks that generate workplace dysfunction and threaten professional identity may affect the stability of bureaucratic labor. To address this question, we study the aftermath of the storming of the U.S. capitol on January 6, 2021. Using comprehensive data on congressional staffers from 2008-2022, we find increased turnover in Democratic offices relative to Republican offices. Subsequent staffing patterns also shifted: Democratic offices experienced declines in observable human capital (legislative experience and educational attainment) and increased reliance on temporary staff. These changes were further accompanied by reductions in the racial and gender diversity of congressional staff. Taken together, these findings suggest that workplace disruptions can degrade institutional capacity by reshaping the composition and continuity of the congressional workforce. More broadly, we demonstrate how political unrest reverberates through the personnel infrastructure of governance.

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"I have friends doing really valuable work, doing good for the world, and they have pretty regular hours. And they don't think about dying at work, or what they would do if something happened."
— *A House aide*[1]

"Every day was like attending a wake. The joy was gone, the celebration of service was killed."
— *A former Democratic congressman talking about his staff*[2]

Government institutions depend on the competence and continuity of the people who staff them. When experienced individuals leave government service, they take with them years of institutional expertise that make their organizations function (Salisbury and Shepsle, 1981; LaPira, Drutman and Kosar, 2020; Fong, Lowande and Rauh, 2025).[3] These departures can fundamentally reshape the capacity and character of the state, determining not just whether institutions can function effectively, but who controls them, what values they embody, and how the public perceives them (for example, see Furnas and LaPira, 2020; but also Ferrer and Thompson, Forth. for a more circumspect account on the effect of turnover on institutional capacity). Yet our understanding of when and why government workers leave their posts remains primarily focused on material incentives like salary and career advancement (e.g., Jensen, 2011; Shepherd and You, 2020; Wirsching, 2025) This economic lens may miss deeper motivations rooted in professional identity attachments and beliefs about the meaning of public service (Perry and Wise, 1990). These motivations should be especially salient when workplace disruptions call that meaning into question. How do government workers respond to crises that shock these foundations?

In this paper, we investigate this question by studying how the events of January 6, 2021 influenced congressional staffers' decisions to remain in or leave government service. Beyond immediate security concerns, January 6 created a crisis of meaning for government workers. Opinion pieces and testimony from Hill employees show that the event fundamentally altered staffers' perceptions of the non-instrumental value of their work: accounts emphasize the loss of meaning and the emo-

[1] Quoted in: Tully-McManus (2021*a*)

[2] Quoted in Elliott (2023)

[3] While modest levels of turnover may yield benefits by creating opportunities for new talent to enter Congress with new policy ideas (Meier and Hicklin, 2008), the prevailing rate of congressional staff departures is sufficiently high to be of concern (Furnas and LaPira, 2020).

tional toll of working in a space that no longer felt civic or public, but instead increasingly hostile and alienating. (e.g., Korten, 2022; Elliott, 2023; Edwards, 2022; Tully-McManus, 2022; Davidson, 2024). This is particularly important for these types of political appointees who stake their jobs on particular visions of what government should be and do (Montgomery and Nyhan, 2017), making them especially sensitive to events that threaten not just their partisan interests, but the symbolic foundations of democratic governance.

We argue that January 6 was a distinct kind of organizational shock. It combined a threat to staffers' professional identities, understood as the values and meaning they attach to their work (Van Maanen and Barley, 1984; Ibarra, 1999), with severe and prolonged workplace dysfunction that disrupted normal operations. Shocks that simultaneously undermine the meaning of the work and the conditions for performing it are especially likely to push workers out of their jobs.[4] Importantly, we expect January 6 to have been experienced unevenly across partisan lines. Given partisan differences in how the event was interpreted, January 6 plausibly posed a greater identity threat for Democratic staffers than for Republican staffers, even as both groups operated within the same disrupted workplace (e.g., Craig and Albertson, 2025). Moreover, we argue that the effects of January 6 were not limited to those already employed on the Hill. By altering how potential entrants perceived the nature of congressional work, the shock likely shaped subsequent recruitment, with Democratic offices facing greater constraints in attracting candidates and, in turn, systematic differences in the profiles of new hires.

Our analysis draws on comprehensive data combining administrative and social media sources covering congressional offices and staff career trajectories from 2008 to 2022. Motivated by our theoretical framework, we analyze congressional staff outcomes around January 6 using complementary empirical strategies: descriptive timeseries approaches, a two-way fixed effects design, and a synthetic difference-in-differences design. Our main specification treats Democratic offices' staffers — the group most targeted and affected by the attack — as the "treated" population and Republican staffers as the comparison group. We then establish three sets of results: total turnover in response to January 6, relative partisan differences in turnover, and subsequent hiring and replacement dynamics following the attack.

[4]For a related formal theoretical account, see Gailmard and Gailmard (2025).

In line with our theoretical expectations, we find that January 6 was associated with a generalized increase in staff turnover across members' offices. Two-way fixed effects and synthetic difference-in-differences analyses further indicate that this increase is concentrated among Democrats: Democratic monthly turnover rises by 1.45 percentage points relative to Republican offices in the post-January 6 period. This increase reflects staffers exiting Congress altogether rather than reallocating to other offices. A series of robustness checks supports this interpretation. Placebo tests using the 2016-2017 Republican presidential transition show substantially smaller effects, suggesting that our estimates exceed those associated with routine changes in party control. Directly controlling for member- and party-specific labor-market demand using Washington, D.C.-area job postings does not attenuate the results, indicating they are unlikely to be driven by shifts in outside employment opportunities. Finally, an analysis of the effects of COVID-19 on congressional staff turnover suggests that our findings are not a result of differential partisan responses to the pandemic.

After establishing the effect of January 6 on Democratic staffer turnover, we consider the downstream consequences of the crisis on office composition and recruitment. In Democratic offices, both incumbent and replacement staff exhibited lower observable human capital. Relative to Republican offices, Democratic offices experienced an approximately 12 percent decline in average prior Hill experience after January 6, as well as a 1.51 percentage point reduction in the share of staff holding an advanced degree. Democratic offices also increased the share of temporary staffers (interns and part-time staff) by 2.59 percentage points relative to Republican offices. Finally, we document generalized post-January 6 declines in the racial and gender diversity of staff. Moreover, consistent with our hypothesis that an event like January 6 would cause the pool of prospective staffers to constrict, we also observe declines in assortative hiring — that is, members hiring individuals of their same social group. These results are especially concerning amid mounting warnings from experts and members of Congress about congressional "brain drain." Recent work by Bolton, Hassell and McCrain (2025) shows that Congress tends to retain staff with prior Hill experience and advanced educational training — tempering baseline fears about expertise loss. Our results point to a different risk: institutional shocks that undermine confidence in democratic governance are associated with a decline in human capital in the most affected offices.

Our paper makes three key contributions. First, we develop and test a general theory of when

workplace crises induce exit among workers. By showing how identity threat and workplace dysfunction interact to produce turnover and alter recruitment dynamics, our framework extends existing accounts of congressional staff that emphasize material incentives or career opportunities alone (e.g., Blanes I Vidal, Draca and Fons-Rosen, 2012; Shepherd and You, 2020; Wirsching, 2025). Although we study Congress, the theory applies more broadly to public institutions facing symbolic attack and operational disruption. Moreover, our findings highlight how changes in who stays and who enters public institutions can have lasting consequences for how those institutions function (Fox and Hammond, 1977; Wilson, 1989; Gailmard and Gailmard, 2025).

Second, we identify a new organizational channel through which political polarization undermines institutional capacity. Existing research on "elite-driven" polarization largely treats the phenomenon as operating through elected elites' preferences, behaviors, and legislative outputs (Fiorina and Abrams, 2008). We show that polarized conflict can impose asymmetric human capital costs by differentially threatening the professional identities of workers across partisan lines. In our case, partisan interpretations of January 6 produced uneven exit and selection dynamics across Democratic and Republican offices, leading to divergent trajectories in experience and workforce composition. Polarization therefore operates both as a political cleavage and a force that reshapes the human capital of governance.

Third, we extend our understanding of congressional staff and their career decisions and reposition them as a central but understudied mechanism of institutional change. While prior work on January 6 documents differences in elite attitudes (Craig and Albertson, 2025), legislative cooperation (Curry and Roberts, 2025), and campaign finance (Cohen, 2025; Li and Disalvo, 2023), it largely overlooks how the crisis affected Congress's "invisible force" sustaining day-to-day legislative and representational capacity (Fox and Hammond, 1977). By tracing how January 6 altered both staff retention and recruitment, we show its potential to degrade institutional performance in the future by disrupting professional continuity and hollowing out staff experience and quality.

The Case: January 6, 2021 and Congressional Staffers

January 6, 2021

We study the effect of the January 6, 2021 U.S. Capitol attack on congressional office-level personnel outcomes. On the date, approximately 2,000 to 2,500 individuals entered the U.S. Capitol during the certification of the 2020 presidential election results, forcing members of Congress and their staff to take cover in their offices, barricading doors and hiding under desks (Elliott, 2022).

In the months following January 6, government employees described significant effects on their mental health and workplace morale. Enhanced security measures, including the presence of the National Guard, led some to describe the congressional workplace as "toxic" and "frustrating" (Korten, 2022; Elliott, 2023). The psychological impact extended beyond those physically present — many staffers working remotely described feelings of guilt and anxiety.[5] Individual offices responded differently to these challenges, with some addressing staffers' safety concerns while others continued operations as normal (Tully-McManus, 2021*b*). Anecdotal reports suggest that January 6 catalyzed departures among staffers in both legislative and administrative positions, as well as among staffers of color (e.g., Korten, 2022; Davidson, 2024).

Congressional Staffers

Congressional staffers represent an ideal population for examining how symbolic threats to institutions affect government human capital. However, the literature has traditionally understood staffer career decisions through an economic lens, focusing on the "revolving door" to lucrative lobbying positions (e.g., Blanes I Vidal, Draca and Fons-Rosen, 2012). Future career concerns affect current behavior (Shepherd and You, 2020; Wirsching, 2025), leading to the conclusion that Hill work represents "a stepping stone from which to launch a career elsewhere" (Salisbury and Shepsle, 1981). This framework, while important, leaves open the question of how non-purely pecuniary dimensions shape career decisions. By examining departure patterns following an event with both

[5]Because of the ongoing COVID-19 pandemic, many staffers worked from home on January 6, 2021 (Korten, 2022). Since stay-at-home decisions —- like most personnel decisions — were up to individual member-managers, however, we are unable to determine precisely *which* staff were on site during January 6.

symbolic and operational dimensions, we can assess mechanisms that complement the economic factors emphasized in prior work.

Theory & Conceptual Framework

Disruption is part of the congressional workplace. Unexpectedly high workloads, political turnovers, shutdowns, and even direct threats against members of Congress (MCs) are relatively routine events that staffers can expect to endure. We may therefore wonder precisely *why* an event like January 6 would alter congressional staff turnover over and above these more everyday occurrences. Ultimately, we argue that unlike these less remarkable events, events that simultaneously threaten staffers' professional identity (*identity threat*) and undermine their capacity to practice that identity (*workplace dysfunction*) are much more likely to drive staffers out of their positions.[6]

Professional Identity & Exit Decisions

Individuals develop strong attachments to their occupation and the specific organization in which they practice it through both relational and individual processes, including on-the-job socialization and personal sense-making about their daily work (Fine, 1996; Pratt, Rockmann and Kaufmann, 2006). Through professional identity — the internalized values, standards, and role expectations people use to define themselves in specialized, skill- and education-based occupations — workers come to see their daily job tasks as expressions of who they are (Van Maanen and Barley, 1984; Ibarra, 1999).[7] When workplaces function normally, this identity-environment alignment allows individuals to enact and reaffirm their sense of self through work (for a review, see Ashforth and Schinoff, 2016).

Crises complicate this picture. Drawing on Hirschman's (1970) framework, an individual responding to a workplace crisis can exit; attempt to improve conditions through voice; or remain

[6]Of course, January 6 was a relatively unprecedented attack. The last time Capitol Hill was stormed in this fashion was during the War of 1812. We view January 6 as an opportunity to theorize about a broader class of events.

[7]While work on professional identity stems from the broader study of *personal* identity, a topic with a strong presence in political science research, the importance of professional and occupational identity has been raised in recent research, for example in the study of medical doctors' political activism (Han, 2016) and judges' sentencing decisions (Harris and Sen, 2026).

loyal and continue working despite disruption. Exit, the most drastic response, occurs when the costs of remaining in an organization exceed any benefits. These costs are not merely material (e.g., Withey and Cooper, 1989; Whitford and Lee, 2015): Workers may exit when continued employment undermines their sense that their work aligns with their professional self-concept (Zacka, 2017; Schabram and Maitlis, 2017; DiBenigno, 2022; White, Hurst and Kagan, 2025).

Exit becomes more likely when voice and loyalty are unviable. Both responses presuppose a functional organizational environment in which workers can repair disrupted routines and reaffirm professional identity through work. Because professional identity is enacted on the job (Ibarra, 1999; Pratt, Rockmann and Kaufmann, 2006), workers require stable environments to maintain alignment between who they are and what they do. When dysfunction prevents this enactment of professional self or limits avenues for voice, identity threat can intensify, potentially leading workers to question whether their organization remains aligned with their values (Turco, 2016).

Critically, neither *identity threat* nor *workplace dysfunction* alone is sufficient to drive widespread exit. Dysfunction without identity threat may frustrate workers, but strong professional identities can buffer against departures. Animal shelter workers, for instance, respond to challenges like poor facilities and lack of funding by deepening their commitment to work (Schabram and Maitlis, 2017), and physicians with a strong sense of calling exhibited lower turnover during COVID-19 than those with a weaker sense despite feelings of burnout (Dalla Rosa, Vianello and Mauno, 2024). Conversely, identity threat in otherwise functional workplaces can be remedied through routine work: librarians responded to the spread of Internet search by proving their mastery of search with patrons (Nelson and Irwin, 2014), and nurses whose caregiving identities go unaffirmed by some patients shift their attentions to others (DiBenigno, 2022). When both conditions coincide, however, workers face an untenable situation: their professional identity is threatened, yet the workplace no longer allows them to restore it. Events high on both dimensions should therefore produce the strongest exit responses.

Figure 1 illustrates this framework in the context of congressional employment. Like other government workers, staffers are motivated by public service commitments (Romzek and Utter, 1996; Jensen, 2011) that tie professional identity to institutional values and democratic norms (Perry and Wise, 1990). Routine turnover occupies the lower-left quadrant, where identity threat

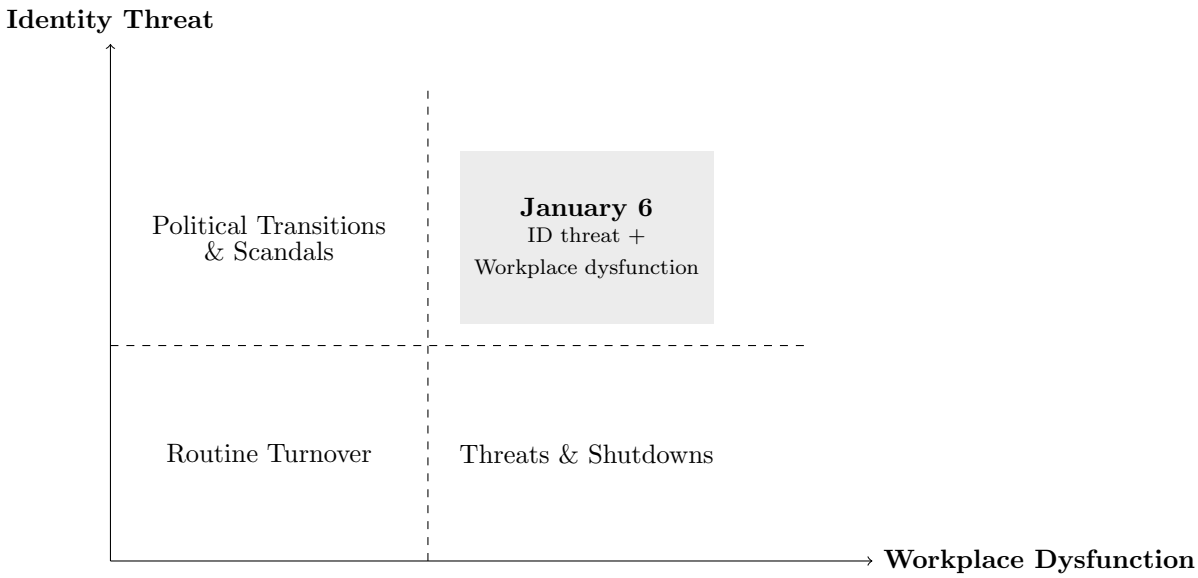

Figure 1: Theorizing Conditions for Exit

and dysfunction are low. The lower-right quadrant reflects events such as government shutdowns or operational crises that disrupt work but leaves staffers' professional identities preserved. The upper-left quadrant reflects identity-threatening events such as political transitions or institutional scandals that challenge staffers' values while preserving functional work routines.[8]

The shaded upper-right quadrant represents the highest turnover scenario. January 6 coupled a direct identity threat – an attack on the democratic institution staffers serve – with workplace dysfunction that prevented normal operations and undermined staffers' ability to practice their professional roles. This combination limited opportunities for voice or identity repair through work, making exit more likely. We therefore conceptualize January 6 as part of a broader class of shocks that simultaneously threaten professional identity and disable the workplace. We return to this framework in the Discussion to consider its broader implications.

The Role of Partisanship

How staff locate a crisis within Figure 1 is shaped by partisanship. Congressional staffers' connections to their parties (Montgomery and Nyhan, 2017), means their professional and partisan identities are tightly linked.[9] Although many government workers enter public service out of civic

[8]For example, the Congressional Black Caucus Scandal in 2010 (Lipton and Lichtblau, 2010); and the accusation of several MCs of misconduct during the #MeToo Movement (Opsahl, 2018).

[9]Between 2000 and 2024, less than 2% of staffers worked for MCs that did not share their partisan affiliations.

commitment, the content of these motivations varies systematically across partisan lines (Grossmann and Hopkins, 2016), reflecting broader disagreements over government, institutions, and democratic legitimacy (e.g., Hibbing and Theiss-Morse, 2002; Wolf, Strachan and Shea, 2012; Hetherington and Rudolph, 2015). As a result, Democrats and Republicans may interpret the same institutional shock differently.

Partisan political cultures provide unequal resources for interpreting institutional threats. Republican ideological frameworks such as "fighting the deep state" or challenging corrupt institutional or cultural elites (Smallpage, Enders and Uscinski, 2017; Enders and Smallpage, 2019) can recast attacks on institutions as validations of long-standing critiques. Democratic discourse, by contrast, emphasizes maintaining coalitional unity and procedural legitimacy (Grossmann and Hopkins, 2016), offering fewer narratives that can re-frame direct challenges to institutional authority as constructive. This asymmetry suggests that loyalty responses may seem more reasonable to Republican staffers following institutional threats.

The costs of different responses may vary systematically by partisan affiliation. Professional networks and post-Hill opportunities are structured differently for Democrat versus Republican staffers (Skocpol and Hertel-Fernandez, 2016; Furnas, Heaney and LaPira, 2019; Fagan, 2024), shaping the material consequences of exit. Social identity costs may diverge as well. After January 6, Democrats largely framed the event as a breach of democratic norms requiring an aggressive institutional response (Craig and Albertson, 2025). By contrast, many Republicans emphasized media bias in the event's coverage and portrayed subsequent inquiries as partisan overreach (Yang, 2022; Cathey, 2022).

These differences complicate the meaning of observed loyalty responses. Democratic staffers who remain after an institutional attack may face professional or social pressure for continuing to work in a symbolically damaged institution, requiring active identity reconstruction to justify staying. Republican staffers, by contrast, may remain without comparable cognitive effort if partisan narratives preserve institutional legitimacy. Prior work suggests that prolonged identity–workplace misalignment, and the cognitive burden it entails, increases the likelihood of eventual exit (Schabram and Maitlis, 2017; Zacka, 2017).

Taken together, partisan frames condition how identity threat and workplace dysfunction are

experienced. Because January 6 simultaneously challenged institutional legitimacy and disrupted normal work, we expect higher turnover in Democratic offices than in Republican ones. We do not argue that Republicans are immune to exit, only that their threshold is higher.

Long-Term Effects

Our theoretical discussion thus far explains why congressional staffers exit following identity threats and workplace dysfunction, but the consequences of such shocks extend beyond departures. Individuals' expectations about their daily work shape early career decisions (DiBenigno, 2022). Following January 6, prospective Hill staff may view congressional employment as less safe, more dysfunctional, or less affirming, making it a poorer fit with their idealized career path. As a result, first-best applicants — such as those with higher human capital or other desirable traits — may disproportionately select out of the congressional applicant pool. Given the partisan asymmetries described above, this effect may be especially pronounced among prospective Democratic staffers.

Those who select into the applicant pool may therefore differ systematically from earlier cohorts. Beyond lower average observable quality, entrants may be more ideological (e.g., Hall, 2019). Differences in partisan networks suggest that younger Democrats who continue to view congressional careers as viable may skew more moderate than their Republican counterparts (Skocpol and Hertel-Fernandez, 2016). At the same time, Democrats selecting into Hill work after a period of identity threat and dysfunction may be especially institutionally oriented, seeking to restore their pre-crisis vision of the institution (Obodaru, 2017; DiBenigno, 2022), while new Republican staffers may feel licensed to embrace more anti-institutional stances. Compounding these dynamics, the loss of veteran staff can disrupt socialization processes that transmit institutional knowledge and professional norms (Wilson, 1989; Ibarra, 1999). Together, these selection effects suggest that January 6 may have lasting consequences for the composition and character of the congressional workforce.

Hypotheses

Our theoretical framework generates the following primary testable hypotheses:

H1 Generalized Turnover: The months following January 6 will be associated with higher

turnover than routine churn, political transitions, and scandals.

H2 Partisan Asymmetries: Republican staffers will be less likely to exit than Democratic staffers after January 6.

H3 Selection Effects: Post-January 6, the profile of new hires will differ between Democratic and Republican offices, with the former being less able to make first-best hires (e.g., of higher human capital).

While January 6 was extraordinary, our framework treats it as an instance of a broader theoretical phenomenon rather than a *sui generis* event. The mechanisms we theorize and hypotheses do not depend on direct exposure to physical violence. This is in part because pandemic-related remote work meant that relatively few staffers were present in the Capitol that day. Instead, political violence directed at an institution, or at groups within it, fits within our framework because it simultaneously challenges professional identity via institutional legitimacy and disrupts the organizational conditions through which professional identity is enacted.

Our empirical focus on January 6 therefore serves a dual purpose. It allows us to test whether a highly salient institutional shock produces the predicted exit responses and to illustrate a more general theoretical claim about the conditions under which workplace crises generate turnover.

Data

To test our hypotheses, we compile a panel dataset of detailed employment histories of United States Congress staff. This dataset combines information on individual congressional staffers from 2008-2022 with data on employment before and after working in Congress. With these data we analyze patterns in office turnover and the human capital of office staff before and after January 6, 2021. We restrict our analysis to Democratic and Republican offices, so as to best compare differential partisan responses to January 6, 2021.[10] Each of our data sources, outcomes, and measurements, along with summary statistics are detailed, below.

[10]This excludes independents and other non-party affiliates. Over our study window, this removes the two Senate independents: Bernard Sanders (VT) and Angus King (ME).

Personnel data

Our personnel data comes from *LegiStorm*, a data vendor that primarily assists organizations with contacting congressional offices and managing potential conflicts of interests via detailed staffer biographies and contact information. Numerous studies in political science and economics have relied on LegiStorm's data to study questions related to congressional staffers the revolving door of government to lobbying work (e.g., Wirsching, 2025; McCrain, 2018; Blanes I Vidal, Draca and Fons-Rosen, 2012; Shepherd and You, 2020; Bolton, Hassell and McCrain, 2025). For each staffer, we observe the congressional offices for which they have worked, the dates of their employment at each office, as well as staffer attributes including position titles, education, gender, and race.[11] Additionally, for many staffers, LegiStorm records employment positions following their departure from Congress. We document the coverage and limitations of these data in Supporting Information (SI) Section A.1 and use them to assess the robustness of our findings.

The result from this data construction is a dataset of individual-staffers with information on staffer demographics and Congressional careers. With these data, we aggregate up to the congressional office, to construct a monthly panel of offices across the time-frame of our data.

Office-level characteristics

We include a variety of member-office level controls in our analyses. Members' chamber assignment, seniority, party identification, committee chair status, and party leadership status for a given session come from Volden and Wiseman (2014, 2018). Members' committee assignments come from Stewart (2021*b*,*a*). Monthly summary statistics across Democratic and Republican offices are shown in Table 1. Across all month-years in our data, Democratic and Republican offices offer similar workforce sizes and compositions, with monthly averages of approximately 22 staffers in Democratic offices and 20 staffers in Republican offices.[12] Of these, we see comparable levels of the share of staff defined as legislative versus temporary staffers, and comparable levels of monthly turnover,

[11] Education, gender, and race variables are incomplete in the dataset because of LegiStorm's reliance on social media profiles to gather staffer information. LegiStorm manually codes gender and race. For the uncoded staffers, we infer these attributes from names using the R packages `rethnicity` and `gender` (Mullen, Blevins and Schmidt, 2015; Xie, 2022).

[12] House members are limited to 18 full-time and 4 part-time staff positions, while senators have greater discretion in staff size (Congressional Research Service, 2020, 2023).

Table 1: Summary Statistics (Office-Month Unit)

	Democrat (N=13002)		Republican (N=10758)	
	Mean	Std. Dev.	Mean	Std. Dev.
Office size	22.55	13.39	20.20	11.29
Share Legislative Staff	0.26	0.09	0.25	0.09
Share Temporary Staff	0.12	0.11	0.11	0.11
Office-Level Separation Rate	0.06	0.08	0.05	0.07
Office-Level Exit Congress Rate	0.04	0.06	0.04	0.06
Avg. Congressional Experience (months)	56.15	26.14	56.36	21.33
Share with Advanced Degree	0.15	0.09	0.08	0.07
Years in office	7.02	4.53	5.40	3.71
Committee chair	0.11	0.31	0.07	0.25
House Member	0.79	0.41	0.76	0.43

and average congressional experience. The largest differences across party are observed for the number of years the member of Congress has been in office (approximately 7 years for Democratic MCs and approximately 5 years for Republicans), and the share of staffers with an advanced degree (approximately 15 percent for Democratic offices, 8 percent for Republican offices). In the following subsection, we describe our main outcomes in greater detail.

Main Outcomes

Our analysis focuses on on two types of office-level outcomes. First, we analyze the differential response to January 6, 2021 between Democratic and Republican offices in terms of staff turnover. Second, we assess the impact on the human capital composition of each office's staff, to gain insight into whether greater turnover translates to lower quality staff after January 6. In Table 2, we list each of these outcomes, along with information on how each are measured and what they capture theoretically.

Turnover

To measure turnover, for each month of our data we calculate the fraction of staffers who are leaving a given office at the end of that month. This "Office-Level Separation Rate" serves as our primary turnover outcome. As Table 1 reports, offices tend to consist of about 22 employees with an average of about 5-6 percent of employees leaving in any given month. This is close to the general public-

sector turnover rate, which averages around 3.6 percent over our 2008 to 2022 analysis period. However, it is substantively higher than the government turnover rate of 1.5 percent reported by the Federal Reserve Bank of St. Louis.[13] Figure 2 documents the cyclicality of staff turnover over the course of a Congressional session and a President's term. Because of the general increase in staff turnover around certain parts of the calendar year, we account for this in our empirical specifications.

This measure of turnover does not account for where staffers go to work after leaving a given congressional office. Staffers may opt to go work in a different congressional office, or may stop working in Congress altogether. Our theoretical expectations – that the experience of politically-targeted workplace violence on January 6 will influence staffers in Democratic office to quit their jobs at greater rates than staffers in Republican offices – suggest that overall turnover should be driven more by a desire to stop working in Congress completely than simply to work in a different office. Therefore, we further measure the "Office-Level Exit Congress Rate", which is the fraction of staffers in each office who leave the office at the end of the month and do not go to work in another congressional office. Table 1 reports these rates at approximately 4% for both Democratic and Republican offices. Most of observed monthly turnover is staffers leaving congressional work, rather than rotating within Congress.

Human Capital

To measure human capital, we look at three indicators of staffer quality: prior congressional experience, the prevalence of advanced degrees, and the share of temporary staff. Across months and offices in our data, staffers have on average 56 months of experience working in congressional offices. This includes experience working in the current office as well as previous offices. More experienced staff is indicative of higher quality staff, in that the staff on average have more knowledge of how to be effective in their congressional duties (Crosson et al., 2020). When staff exit working in Congress, they are inevitably replaced by more inexperienced staffers, particularly new hires who have not previously worked in a congressional office. Therefore, if we observed a decrease

[13]Specifically, we refer to the average non-seasonally adjusted "separation rate" for non-farm firms (`https://fred.stlouisfed.org/series/JTUTSR`) and government (`https://fred.stlouisfed.org/series/JTU9000TSR`)between January 2008 and December 2022.

Table 2: **Outcome Definitions and Measurement (Office–Month Unit)**

Turnover Outcomes		
Office-Level Separation Rate	Share of staff separating from a member office in a given month	Baseline measure of turnover; counts exits from the office regardless of whether staff remain in Congress.
Office-Level Exit Congress Rate	Share of staff leaving Congress entirely in a given month	Captures institutional exits; excludes within-Congress job changes.
Human Capital Composition		
Avg. Congressional Experience (logged)	Average tenure in months across all staff	Captures depth of institutional knowledge within the office.
Share with Advanced Degree	Fraction of staff holding an advanced (post-BA) degree	Proxy for higher human capital & educational credentialing in the office.
Share Temporary Staff	Fraction of staff with "temporary" job titles	Indicates reliance on short-term or contingent workers.

in this measure of human capital for Democratic offices relative to Republican offices after January 6, then it would indicate that this workplace disruption had downstream impacts on the quality of Democratic staffers (resp. Republican staffers).

We also analyze effects on the share of staffers in each office with an advanced degree (master's degree or higher). Employees with higher educational attainment have developed more specific domain knowledge valued by their employer, are more competitive in considering other employment opportunities, and often compensated at a higher rate than those without advanced degrees (Bureau of Labor Statistics, U.S. Department of Labor, 2024; OECD, 2025). Democratic offices in particularly have high rates of staffers with post-graduate degrees, and a decrease in the educational attainment of Democratic staffers after January 6 would further indicate a marked compositional shift in the types of employees going into congressional staff work.

Lastly, we examine fluctuations in the share of office staff who are temporary workers. Staffers are classified as temporary if their job title includes "intern" or "part-time." As such, these workers are, by design, short-tenure and limited-scope positions: typically with constrained policy responsibilities, minimal security clearances, and reduced opportunities to accumulate or transmit institutional knowledge (Connelly and Gallagher, 2004; Smithberger and Schuman, 2020). Accordingly, a rise in the share of temporary staffers can serve as a proxy for staffing instability (e.g., vacancy back-filling, onboarding bottlenecks, etc.) rather than an expansion of policy capacity. Thus, an

increase for Democratic offices relative to Republican offices after January 6 would be consistent with greater reliance on temporary labor to bridge post-shock gaps.

Trends in Congressional Office Outcomes

As a first step, we document descriptive patterns in congressional staff separations and office characteristics across months from 2008 through 2022. These trends provide context for our subsequent causal analysis. Figures 2 and 3 present our visualizations for our main office outcomes. Alongside the raw time series, we present a post-January 6 counterfactual using pre-COVID office-level personnel data as training data to estimate counterfactual paths for each outcome. We then compare the realized post-COVID and post–January 6 trajectories to these forecasts in order to assess whether staffing patterns departed from what would have been expected absent those shocks. SI Section C.1 describes our methodology in greater detail. COVID-19 constituted a major disruption to congressional operations, and simple mean reversion after that disruption could generate the appearance of a treatment effect — something we interrogate directly below in our "Additional Findings" section. By benchmarking actual trends against pre-COVID forecasts, the descriptive time-series analysis allows us to distinguish between a return to baseline and a genuine break in trend, and it highlights that January 6 was associated with a clear intercept shift in staff turnover and office composition.

Consistent with Hypothesis 1, Figure 2 shows that after January 6, office-level separations increased above pre-pandemic averages. Figure 3 shows office-level declines in average log congressional experience, in the share of advanced-degree holders,[14] and increases in the share of temporary staff after January 6. Taken together, these visualizations demonstrate clear shifts in congressional staffing patterns after January 6, with outcomes diverging sharply from the counterfactual forecasts based on pre-COVID trends. Further motivating our empirical analysis, Section C.2 presents an alternative figure where we overlay averages by party for our primary outcomes. Not only does the figure demonstrate partisan differences in office structure — for example, Democratic offices

[14]LegiStorm's education fields are populated primarily from staffers' self-reported LinkedIn profiles, and coverage improves markedly over time. Early years exhibit substantial missingness, whereas from 2017-2022, the window used in our analysis, coverage is much higher and more stable. Accordingly, while very early increases may partly reflect improving coverage, the post-January 6 dynamics occur precisely when coverage is strongest.

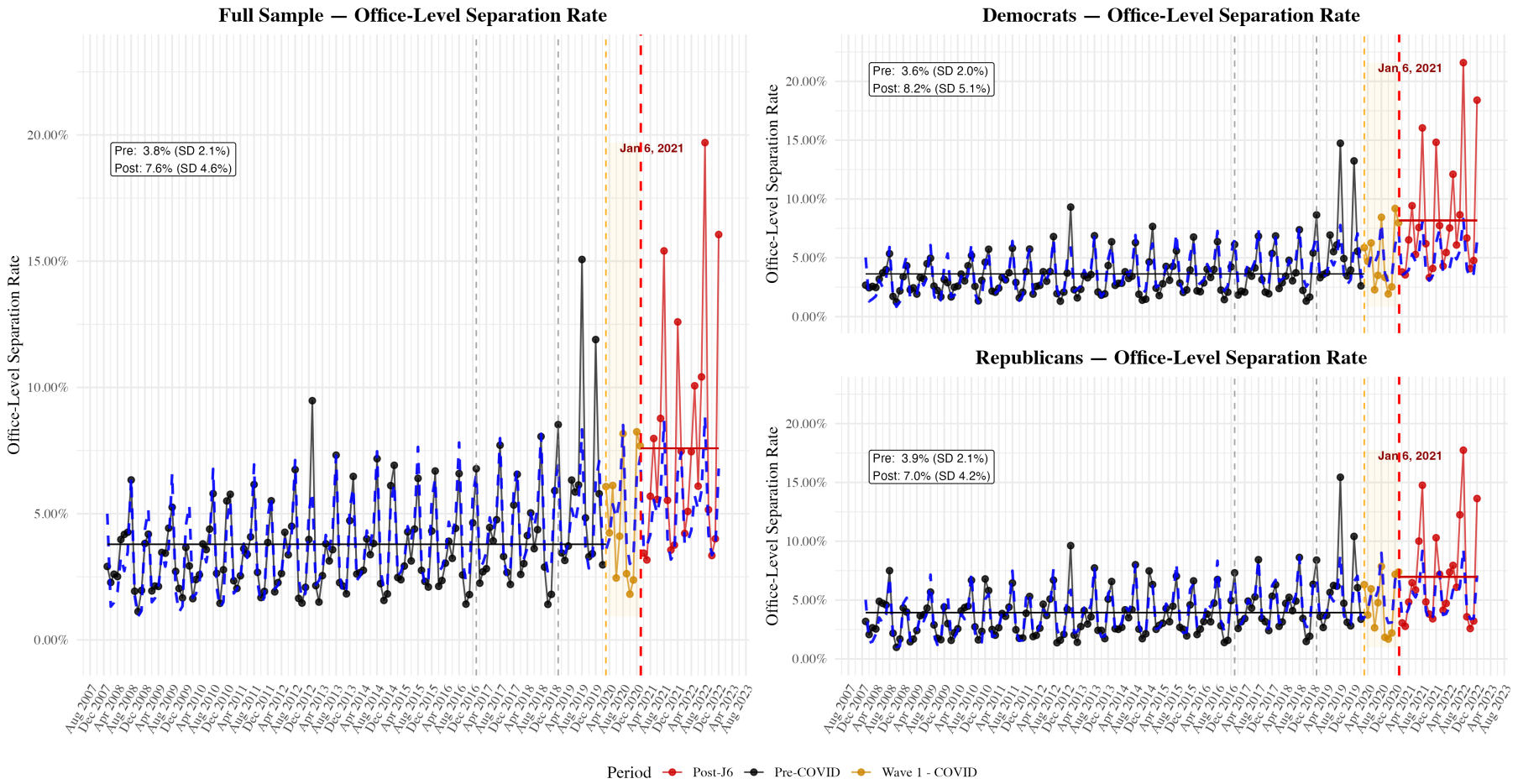

Figure 2: Average Office-level Resignations (Share of Employees in Last Month Normalized by Office Size) Each panel displays monthly averages for the full sample of congressional offices (left), Democratic offices (top right), and Republican offices (bottom right). The yellow dashed vertical line marks the onset of COVID-19 restrictions (March 2020), and the red dashed line marks January 6, 2021. Blue dashed lines show the counterfactual forecast based on pre-COVID trends. Shaded regions indicate the COVID lockdown period. Horizontal lines denote mean separation rates before COVID (black) and after January 6 (red). Overlaid by-party averages are shown in Section C.2.

are more likely to have a higher share of workers with advanced degrees — but it also shows that Democratic offices experienced relatively higher turnover, as well as declines in experience and degree holding after January 6.

Empirical Strategy

To build the intuition for our empirical strategy and estimation, we focus on our primary outcome, the proportion of staffers in an office each month who leave their jobs. We employ a difference-in-differences design comparing changes in office-level turnover rates in the months before and after January 6 across Democratic and Republican offices. In the parlance of difference-in-differences designs, Democratic offices in our case represent the "treated" group, in that these offices and their staffers would be more adversely effected by the events of January 6, compared to Republican offices and their staffers. To mitigate any bias stemming from the entry and exit of officeholders, our main results focus on a balanced panel of member-offices in the House and Senate from January 2017 (the start of the 115th Congress) to December 2022.

With our panel data on congressional offices by month-year, we estimate a two-way fixed effect model accounting for time-invariant office-level characteristics and common time trends to estimate the difference-in-differences quantity of interest. To formalize this strategy, let y_{it} be the proportion of staffers in office i and month-year t who are leaving their jobs. Let α_i be the office fixed effect accounting for all characteristics of offices that do not vary across time, and γ_t be the month-year fixed effect accounting for characteristics of any time t that are commonly experienced across offices. As such, our estimation makes within-office comparisons across time, comparing turnover rates before and after January 6 within the same office. We then interact an indicator variable for whether a given time t is after January 6, 2021 with an indicator variable for whether office i is Democratic in time t. Finally, let $\mathbf{X}_i$ be a vector of time-varying office-level controls including whether the MC is a committee chair, the number of years they have been in Congress, whether the MC is the majority or minority leader, and which chamber of congress – Senate or House – they are in. Standard errors are clustered at the office-level, as that is the level at which "treatment" (party) is assigned (Abadie et al., 2023), to account for correlated errors within office across time.

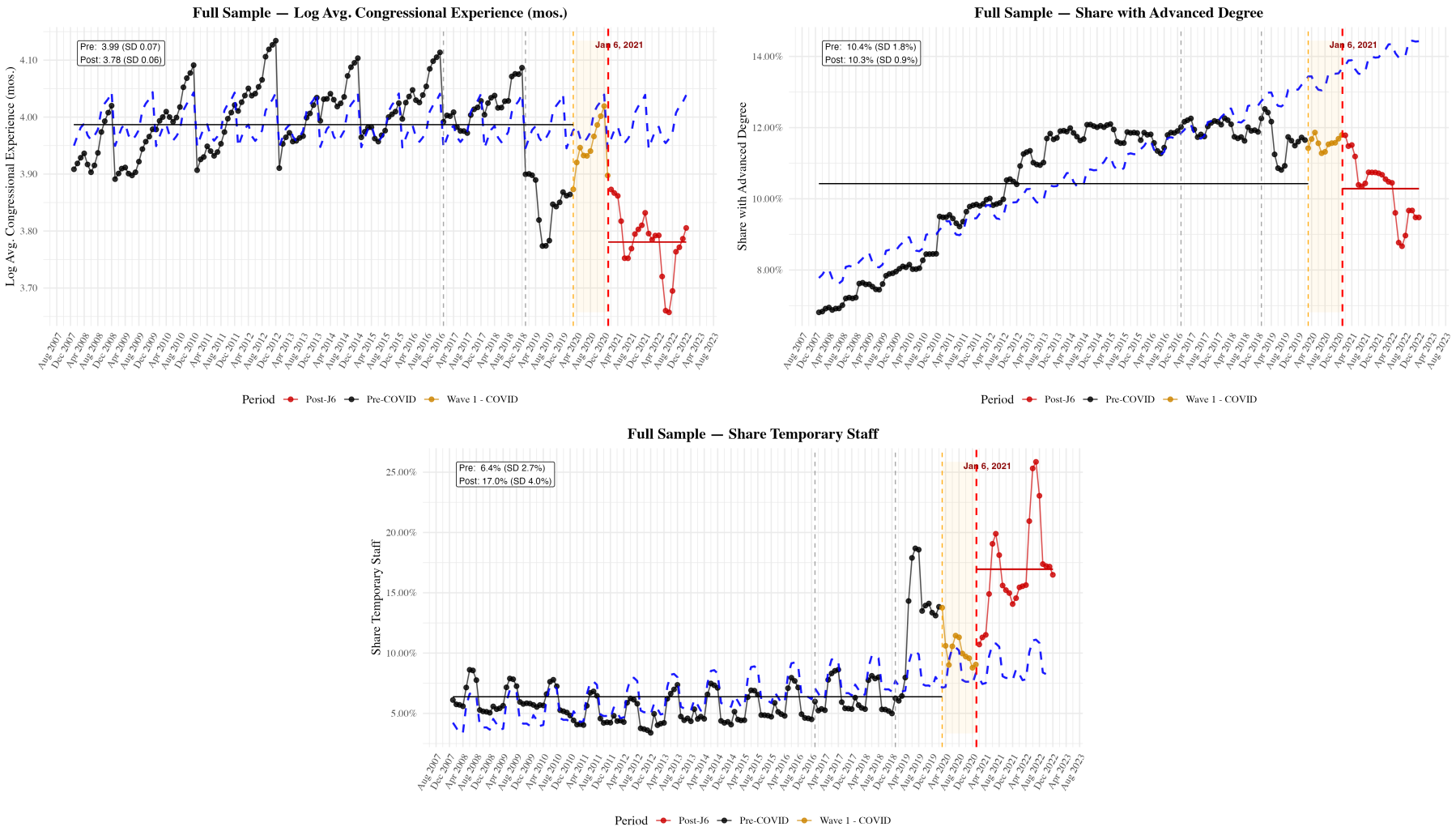

Figure 3: Average Office-level Human Capital Each panel shows monthly averages for the full sample of congressional offices. The yellow dashed vertical line marks the onset of COVID-19 restrictions (March 2020), and the red dashed line marks January 6, 2021. Blue dashed lines show the counterfactual forecast based on pre-COVID trends. Shaded regions indicate the COVID lockdown period. Horizontal lines denote mean separation rates before COVID (black) and after January 6 (red). Overlaid by-party averages are shown in Section C.2.

We estimate the following linear model:

$$y_{it} = \alpha_i + \gamma_t + \theta \text{Post Jan. 6, 2021} \times \text{Democrat}_{it} + \beta \mathbf{X}_i \epsilon_i \quad (1)$$

Our quantity of interest is θ, which represents the difference-in-differences estimate of the causal effect of January 6 on Democratic (compared to Republican) office turnover.

This cross-party comparison holds constant temporal trends in office turnover that are similarly experienced for both Democratic and Republican offices. Interpretation of the difference-in-differences estimate as the causal effect of January 6 is thus reliant on the parallel trends assumption — that, absent the events of January 6, the difference between Democratic and Republican office-level turnover rates would remain constant over time. We take several steps in our estimation strategy to strengthen and test the robustness of this assumption, detailed below. In doing so we grapple with inferential difficulties specific to our data and case.

Office employment data are highly seasonal, and while Democratic and Republican offices experience many common trends in the hiring cycle, they also may experience seasonality that is specific to a given party. For example, changes in chamber control, presidential transitions, and party-specific strategic priorities (e.g., shifts between governing, oversight, or electoral messaging) can induce asymmetric waves of separations and hiring. Failure to account for such differential seasonality could violate the parallel trends assumption and bias results. To this point, initial evaluation of by-party trends in turnover from 2017-2020 (the years leading up to Jan. 6th) in Figure 2 shows evidence of differential trends by party, signaling potential violations of the parallel trends assumption that straightforward difference-in-differences technique cannot account for. In response to this evidence, we estimate two alternative versions of the two-way fixed effect estimator that re-balance the treated and control groups so as to better make counterfactual comparisons. In the first, we implement a matching design (Ho et al., 2007) where we match units based on pre-trend average levels of the outcome, pre-trend slopes, average pre-trend outcome levels in months capturing different points in the seasonal cycle, and pre-trend levels of office size. We generate nearest neighbor Mahalanobis distance weights from these variables (using the `Matchit` software in R). We then reduce the data to matched pairs and re-estimate Equation 1. This matching process

reconstructs the treated and control groups to only include units that are more similar on pre-trend characteristics, thus better accounting for potential pre-trend violations.

Taking this logic a step further, we also use an alternative estimation using entropy balancing (Hainmueller, 2012). With this approach, we reweight the control group (Republican offices) to look more like the treated group (Democratic offices) by calculating the minimum entropy distance subject to balance constraints using the same variables used in our matching process (implemented using the `weightit` package in R). We then estimate our two-way fixed effects specification from Equation 1 with a weighted linear estimator. This approach reduces pre-trend differences by construction along the dimensions included in the weights estimation. Both the matching and entropy-balancing strategies strengthen our causal claim by constructing pre-treatment comparisons in which Democratic and Republican offices are better balanced on pre-treatment outcome levels, overall trends, and seasonal patterns.

To make further progress on these causal inference challenges, we also estimate synthetic difference-in-differences (Arkhangelsky et al., 2021). This approach extends the logic of synthetic control methods (Abadie, Diamond and Hainmueller, 2010; Xu, 2017), and is increasingly used in difference-in-differences style designs with challenging parallel trends assumptions (e.g., Lal and Thompson (2024)). To implement synthetic difference-in-differences in the context of our study, we simultaneously calculate office-level weights such that the pre-treatment turnover trends in Republican offices are as equal as possible to trends in Democratic offices and calculate month-year (across offices) weights that emphasize pre-treatment periods where Republican office turnover trends best predict the treated group's pre-treatment trends. The synthetic difference-in-differences estimator (implemented using `synthdid` in R) then estimates a reweighted regression using both of these weights. We compute bootstrapped standard errors resampled at the office-level for the synthetic estimates, which is analogous to office-clustered standard errors as computed in the two-way fixed effect models (Arkhangelsky et al., 2021).

The advantage of this synthetic difference-in-differences approach is that by reconstructing the counterfactual comparison to better make the Republican office time series look like the Democratic office time series, interpretation of ensuing estimates as the causal effect of January 6, 2021 on Democratic office turnover (relative to Republican offices) requires a more manageable identification

assumption better supported by the data. Rather than require parallel trends between Democratic and Republican offices before and after January 6, 2021, the synthetic difference-in-differences estimate can be interpreted as the causal effect so long as the potential outcomes for Democratic offices can be approximated by a weighted average of Republican office trends and the weighted average of pre-period time shocks. In our setting, where raw pre-trends differ but can be more closely matched after reweighting, this approximation requirement is far more plausible than strict parallel trends.[15]

Synthetic difference-in-differences goes further than our matching and entropy balancing strategies in estimating the effect of January 6, 2021. Those strategies substantially reduce pre-treatment differences by constructing more balanced pre-trend groupings of treated and control units but they differ from synthetic difference-in-differences in that they only balance on units. Synthetic difference-in-differences uses both unit and time weights to offer a more flexible projection of the counterfactual trends in turnover for Democratic offices after January 6, 2021 (Arkhangelsky et al., 2021). Therefore, in our presentation of the results we refer to the results from the synthetic difference-in-differences estimation as our preferred estimates for summarizing the effects of January 6, 2021. However, we present these preferred estimates alongside estimates from our two-way fixed effect model in Equation 1 and the versions of that model with matching and entropy balancing so as to demonstrate the consistency of the estimates across estimation strategies.

Personnel Responses to January 6

The Effect of January 6 on Office Turnover

To address Hypothesis 2, we formally measure the effect of January 6 on Democratic versus Republican turnover by estimating the panel designs described in the Empirical Strategy section. Table 3 reports the results from Equation 1 and from the versions of Equation 1 with matching and entropy balancing. The table also reports the results from our synthetic difference-in-differences estimation as well as a version of the synthetic difference-in-differences estimation run on the matched sample.

[15]Visualizations of the synthetic pre-trends are shown in SI Figures D1-D4.

Table 3: Effect of January 6th on Democrat Offices' Staff Turnover

	(1)	(2)	(3)	(4)	(5)
Office-Level Separation Rate					
Democrat x After Jan. 6	0.0146***	0.0124***	0.0159***	0.0145***	0.0151***
	(0.0025)	(0.0026)	(0.0026)	(0.0026)	(0.0029)
Obs.	23,760	23,760	21,516	23,760	21,516
Dep. Var. Mean	0.0552	0.0552	0.0547	0.0552	0.0547
Office-Level Exit Congress Rate					
Democrat x After Jan. 6	0.0133***	0.0117***	0.0147***	0.0138***	0.0139***
	(0.0021)	(0.0022)	(0.0022)	(0.0026)	(0.0025)
Obs.	23,760	23,760	21,516	23,760	21,516
Dep. Var. Mean	0.0412	0.0412	0.0407	0.0412	0.0407
TWFE	✓	✓	✓		
Synth.				✓	✓
Entropy balancing		✓			
Matching			✓		✓

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

We first present results for two distinct turnover outcomes: the proportion of staffers in a congressional office each month where that month is their final month working in that congressional office (top panel), and the proportion of workers each month who stop working in the office *and* leave working in Congress all together (bottom panel).[16] Focusing on our preferred synthetic difference-in-differences estimates in column 4, we find that Democratic monthly turnover post-January 6 increased 1.45 percentage points relative to Republican monthly turnover. This point estimate is very similar to the point estimates from our two-way fixed-effects models with and without matching or entropy balancing, and similar to the estimate from synthetic difference-in-differences with matching. We interpret this consistency as evidence that any differential party seasonality or other trends in the pre-treatment period are largely orthogonal to our treatment contrasts, thus supporting our identifying assumption and strengthening the interpretation of the party gap in turnover to differential responses to the events of January 6.

We further find, in the bottom panel, that January 6 produced substantial increases in the rate of staffers not only leaving their jobs but also leaving Congress – meaning they do not transition

[16]In SI Section D.12, we also provide results for employee counts and alternative turnover metrics.

to work at a different congressional office but rather go work outside of Congress. The effects on this outcome are consistent across estimation strategies (1.17 percentage points to 1.47 percentage points). Further, we can infer from the effect sizes (1.38 percentage point increase in exit Congress rate in our preferred estimation) that almost all of the increase in turnover reflected in the upper panel are staffers leaving Congress all together.

The magnitude of the effects of January 6 on turnover can be compared to the average rates of monthly turnover. Each month, 5.52% of staffers leave their job. Thus, the 1.45 percentage point coefficient constitutes an approximate 26% increase in staff turnover. For the exit congress outcome, 4.12% of staffers leave their job and exit Congress completely each month, so the 1.38 percentage point effect on this outcome represents about a 33% increase. January 6 produced large effects on Democratic office turnover relative to Republican offices.

In SI Section D.1, we present additional analyses to test the robustness of our synthetic difference-in-differences estimates. We present diagnostics of the synthetic difference-in-differences identifying assumption, including overlayed plots for treated and synthetic paths (SI Figures D1-D5), synthetic donor and pre-period weight diagnostics (SI FiguresD6-D10), hold-out pre fit tests (Newey and West, 1987) (SI Table D1), comparisons of pre and post period root mean squared predicted error (SI Table D2), and leave-out donor analysis to test sensitivity to top-weighted control units (SI Figures D11-D12). These diagnostics strongly support the synthetic difference-in-differences identification assumption. We also present alternative synthetic difference-in-differences estimation strategies, reported in SI Table D3. These include re-estimating the synthetic difference-in-differences estimation after residualizing the outcome data based on pre-period characteristics (Doudchenko and Imbens, 2016; Ferman and Pinto, 2021), implementing generalized synthetic control (Xu, 2017), and estimating augmented synthetic control (Ben-Michael, Feller and Rothstein, 2021). The results are generally consistent across these estimation strategies, although we note that the augmented synthetic control estimates are less precise in part due to the approach taken by this methodology in quantifying uncertainty.

We also demonstrate further robustness of our two-way fixed effect estimation by estimating multiple alternative specifications that in different ways account for unmodeled trends in our main specifications. We estimate specifications interacting the month-year fixed effects with office-level

controls (whether the MC is a committee chair, the number of years they have been in Congress, whether they are the majority or minority leader, and which chamber of congress – Senate or House – they are in) so as to account for trends that are common within these variables across time. We also estimate specifications that include indicator variables for the interaction of which year in a presidential term a given month-year is and party, to account for differential seasonality by party that is tied to the presidential term cycle. Lastly, we estimate specifications with sine and cosine party-specific Fourier terms, as an alternative strategy to account for party specific seasonality. The estimated effect on turnover is consistent across estimation strategies, and the results are shown in SI Tables D4 and D5.

Additional Findings & Robustness Checks

Placebo Check: Republican presidential transition 2016-2017

One threat to identification in our design is that the events of January 6, 2021 coincided with other shifts in the labor market conditions of congressional staffers that could spuriously generate an increase in Democratic office turnover relative to Republican offices. In particular, when one political party assumes the presidency from another, the parties may adopt different patterns of staff turnover as firms, lobbying organizations, and public agencies reorient toward the incoming administration, anticipating greater opportunities for access and influence (Furnas, Heaney and LaPira, 2019).

Hypothesis 1 implies that any such transition-related dynamics should be insufficient to explain the magnitude or breadth of turnover we observe. As an additional test of this possibility, we conduct a placebo test in which we re-estimate our empirical strategy, but assign the "treatment" date to January 6, 2017. This period corresponds to a presidential transition in which Republicans assumed control of the executive branch from a Democratic administration. If the patterns we document are driven primarily by party-specific labor market opportunities associated with entering presidential power, we would expect to observe elevated turnover in Republican offices relative to Democratic offices during this earlier period. Comparing the magnitude of these placebo estimates to our main results therefore provides a direct test of whether the post–January 6, 2021 turnover reflects routine

transition dynamics or a distinct, generalized disruption consistent with our hypothesis.

To estimate this placebo test, we take data on congressional offices from February 2013 through December 2018. This time period matches the February 2017 to December 2022 period in our main analysis. We again drop January observations, limit to Democratic and Republican offices, and balance the panel to only keep offices observed in all month-years. We then set February 2017 as the first post-period month-year, and re-estimate our empirical strategies, except in this case we designated the "treated" group as Republican offices, since this is the party assuming the presidency in this case.

Figure 4 reports the results from this placebo estimation and compares them to the effects of January 6, 2021.[17] The placebo estimates for turnover fall well below the main estimates and are generally close to zero. For office-level separation rate, both the two-way fixed effects and synthetic difference-in-differences estimation strategies return positive and significant estimates for the placebo effect on turnover, but these estimates are much smaller (0.36 percentage points in our preferred synthetic difference-in-differences estimation) than the effects for Jan 6th, 2021 reported previously (1.45 percentage points). For the office-level exit Congress rate, the two-way fixed effect estimation is significant but similarly small and close to zero, while the the estimates from synthetic difference-in-differences are not statistically distinguishable from zero. Overall, in addition to being consistent with H1, these placebo results support our interpretation of of the main estimates as the effect of January 6, 2021, rather than being a result of differential party responses to presidential transition periods.

Alternative Specification: 2020 Election Certification

In Appendix Section D.4, we re-specify treatment using members' roll-call position on certifying the 2020 Electoral College results. Legislators who voted to certify the election constitute the *control* group and those who voted against the certification serve as the *treated* group. Using this alternative specification, we find that offices with members who opposed certification exhibit lower post-January 6 turnover than those members who voted to certify. However, when we restrict the sample to Republican offices, the estimates are small and statistically indistinguishable from zero,

[17] Regression results for all main outcomes can be found in Appendix Section D.3.

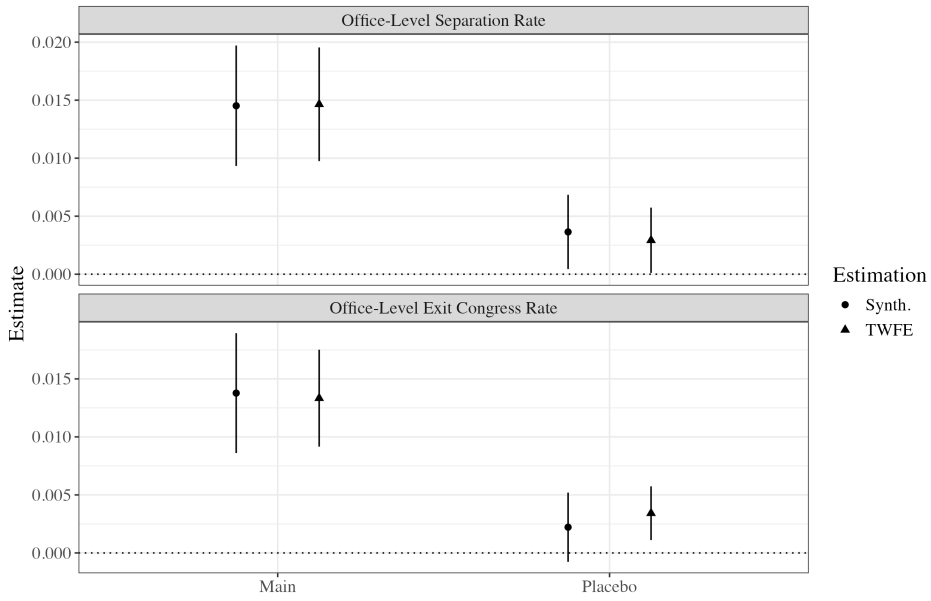

Figure 4: Placebo versus main estimates. Main estimates represent coefficients from our two-way fixed effect (triangular points) and synthetic difference-in-differences (circular points) estimation of the effect of January 6, 2021 on Democratic compared to Republican office turnover. The placebo estimates represent the same estimation but on the effect of January 6, 2017 on Republican compared to Democratic turnover. These treated groups are constructed such that in each case the treated group is the party assuming the presidency. Results are presented for office-level separation rate and office-level exit Congress rate. Error bars represent 95% confidence intervals with standard errors clustered at the office-level.

suggesting that the full-sample results reflect between-party rather than within-party differences.

Alternative Explanation: The Effect of COVID-19

The events of January 6 unfolded against the backdrop of the ongoing COVID-19 pandemic. Given previous work documenting partisan differences in pandemic-related behaviors — and specifically evidence that Democrats behaved more cautiously during the pandemic (e.g., Allcott et al., 2020; Baxter-King et al., 2022) — one potential concern is that the post–January 6 increase in Democratic staff departures reflects deferred turnover: Democratic staffers disproportionately postponed exits earlier in the pandemic and then resigned after January 6. In this scenario, although January 6 could serve as a push factor, the observed increase in Democratic turnover would not represent a behavioral response to the attack itself.

To assess this possibility, in SI Section E we examine the effects of the pandemic's onset on congressional staff outcomes. We estimate an alternative difference-in-differences specification comparing changes in Democratic and Republican staff behavior in the months before and after March

2020, when the first major U.S. outbreaks began. Following the logic above, if Democrats disproportionately delayed turnover during the pandemic, we would expect Democratic offices to exhibit lower turnover relative to Republican offices in the months after March 2020. To analyze the effects of the first COVID-19 outbreak, we limit the data month-years before January 2021 and define the pre and post-periods for analysis with respect to March 2020.

SI Table E1 reports the results from the COVID-19 estimation, presenting results from our two-way fixed effect models and our synthetic difference-in-differences estimation. Our preferred synthetic difference-in-differences estimation indicates that Democratic offices experienced no change in turnover relative to Republican offices across the COVID-19 outbreak, which is inconsistent with the deferred-exit hypothesis. While we do observe a positive turnover effect for Democratic offices from our two-way fixed effects estimation, these effects are substantively much smaller compared to our analysis of effects of January 6, 2021. Taken together, these results provide no evidence that Democrats accumulated delayed exits during the pandemic, and therefore make it less likely that the post–January 6 increase in Democratic turnover reflects stored-up turnover from an earlier period.

Alternative Explanation: Changing Partisan Labor Markets

We also consider the possibility that the post–January 6 labor market for staffers with Democratic-office experience changed as a result of pandemic-related shifts in managers' labor demands that correlate with partisanship or the transition from President Trump to President Biden driving interest from the lobbying and government relations sectors. Together, shocks like these could have increased demand for staffers with certain partisan backgrounds or office expertise, affecting their outside options rather than their underlying willingness to remain in Congress. To address this, we analyze monthly job-listing data for Washington, D.C., Maryland, and Virginia from WageScape, a vendor that compiles postings from publicly available aggregated job boards covering the period from May 2016 (the earliest data available) to the present.[18] To attribute specific organizations to Democrat or Republican offices, we use our personnel data to identify which organizations have

[18]Our geographic focus is consistent with previous findings that few people, including those from white collar positions, move from their original employer's commuter zones (e.g., Chinoy and Koenen, 2024).

hired a majority of their former-staffer employees from either party between January 2008 and December 2016.[19] We discuss our full merging process in SI Section B.

In SI Tables D4 and D5, we include estimates from a TWFE specification where we control for lagged office- and party-specific job listings. For the office-specific control we include the rolling three-month sum of job postings from organizations that have ever hired a former employee of that office, using employment data from January 2008 through December 2016. For the party-specific control, we include the rolling three-month sum of job postings from organizations whose former congressional employees are majority Democratic or majority Republican. The estimates from this specification are extremely similar to those from our main TWFE specifications, and to our preferred synthetic difference-in-differences estimation. As such, we conclude that our turnover results are not driven by job market fluctuations in the availability of exit options for congressional staffers.

Heterogeneity: Members

In the Supporting Information, we assess heterogeneity by ideological extremity, previous session legislative effectiveness, chamber, and seniority (SI Sections D.6–D.9). Effects do not vary systematically by ideology, productivity, or seniority, and are largely concentrated among House members.

Heterogeneity: Staffers

Are particular types of staffers more likely to be impacted by January 6? In SI Sections D.10-D.11, we estimate monthly separation effects disaggregated by seniority and job title. For seniority, we consider junior staffers to be those in Congress for at most a year, and senior staffers to be those with at least a standard deviation more congressional experience than the average staffer in a given year. For job titles, we classify positions as administrative, communications, or legislative.[20] Across specifications, turnover does not vary systematically across the office hierarchy.[21]

[19] For example, if a firm employed ten former congressional staffers during this period — seven from Democratic offices and three from Republican offices — we classify it as a "Democratic" firm. Similarly, if a majority of its associated staffers previously worked in Republican offices, we classify it as a "Republican" firm. Firms employing an equal mix of Democratic and Republican staffers are coded as "Mixed."

[20] A detailed description of how we classify job titles can be found in Appendix A.2.

[21] In SI Section D.13, we also provide TWFE estimates for these variables and other outcomes.

The Consequences of January 6 for Congressional Labor

Turnover alone does not reveal how Congressional offices reorganized after January 6 or whether the characteristics of workers entering Congress changed in systematic ways. In the months that followed, offices could have responded by deliberately altering their staffing strategies or by adjusting to constraints in the Congressional labor market. Hypothesis 3 posits that post-January 6 staffing changes reflected selection effects, such that offices were less able to make first-best hires along key dimensions of human capital. Because we do not observe applicant pools, we evaluate this hypothesis by examining a set of observable implications of a constrained labor market. Specifically, we analyze changes in staff human capital, employment composition, office demographics, and assortative replacement patterns. We close the section with a discussion of our other findings on office productivity.

Office Human Capital

If congressional offices face difficulty recruiting or retaining high-human-capital workers after January 6, average staff experience and educational attainment should decline and remain persistently suppressed. Table 4 evaluates this prediction by estimating the effect of January 6 on human capital in Democratic relative to Republican offices.[22] In the top panel, we report results on the logged average tenure in months across all staff, as a measure of the depth of institutional knowledge within the office. Focusing on the synthetic difference-in-differences estimate in column 4, relative to Republican offices, Democratic offices' average intra-Congressional experience fell by about 0.125 logged average months of experience per month after January 6. Importantly, these patterns are not mechanically lower in Democratic than in Republican offices merely because turnover was higher. The inflow includes both lateral hires from other offices and re-entrants with prior Hill experience, so average experience among new hires can vary independently of contemporaneous separations.

We report the synthetic difference-in-differences results for the share of office employees with advanced degrees in the bottom panel of Table 4. The synthetic difference-in-differences estimates provide evidence of a 1.51 percentage point decline in the monthly share of staffers with advanced

[22] We present synthetic difference-in-differences diagnostics for human capital outcomes in SI Section D.1.

Table 4: Effect of January 6th on Democrat Offices' Human Capital

	(1)	(2)
Log Avg. Congressional Experience (mos.)		
Democrat x After Jan. 6	-0.1257***	-0.1238***
	(0.0216)	(0.0247)
Obs.	23,760	21,516
Dep. Var. Mean	3.9611	3.9597
Share with Advanced Degree		
Democrat x After Jan. 6	-0.0151**	-0.0185**
	(0.0054)	(0.0058)
Obs.	23,760	21,516
Dep. Var. Mean	0.1174	0.1154
Share Temporary Staff		
Democrat x After Jan. 6	0.0259**	0.0253**
	(0.0079)	(0.0085)
Obs.	23,760	21,516
Dep. Var. Mean	0.1170	0.1166
Synth.	✓	✓
Matching		✓

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

degrees in Democratic offices compared to Republican offices after January 6, 2021. Compared to the 11.54% monthly share of staffers with advanced degrees, this is a 16% decrease.

If offices faced difficulty filling or retaining durable full-time positions after January 6, they should rely more heavily on temporary staffing as a stopgap. The bottom panel of Table 4 reports the effect on the share of temporary staff in Democratic compared to Republican offices. We find that Democratic offices increased the share of their staffers who were temporary by 2.59 percentage points compared to Republican offices following January 6, 2021. Notably, this shift occurs alongside stable overall office headcount (S.I. Table D19), indicating substitution toward more flexible employment arrangements rather than a general contraction in staffing.

Taken together, these results show that post-January 6 turnover was followed by reductions in the amount of legislative experience among the average staffer in Democratic offices, diminished average educational attainment, and increased hiring of temporary workers.

Office Demographics

If January 6 altered the congressional labor market through selection effects, demographic change should arise not only because particular groups are more likely to exit, but also because offices are less able to replace them at historical rates.

Staffer accounts suggest that minority staffers were among the most negatively affected by the event (e.g., Korten, 2022; Tully-McManus, 2022).[23] Following our theoretical framework, the racist rhetoric and Confederate imagery associated with January 6 may have constituted an additional identity threat for staffers of color (Morrison, 2021). Similarly, female staffers may have responded to rioters' targeting of prominent women such as Speaker Pelosi, and public opinion research suggests that women experienced more negative emotional reactions than men (Anderson and Coduto, 2024), potentially contributing to exits in Republican offices where gendered differences were especially salient. These mechanisms suggest that exits may have been especially pronounced for Black and female staff.

Figure 5 plots Black and female staffing over time. Following January 6, offices experienced increases in turnover among Black and female staffers, resulting in sustained reductions in both groups' office shares. Consistent with Hypothesis 1, turnover exceeded levels observed during routine political transitions and earlier identity-threatening episodes, including the 2010 Congressional Black Caucus scandal and the 2018 #MeToo movement, which appear in the shaded regions and are not qualitatively distinct from surrounding months. Notably, the reduction in representation lasts well beyond the immediate post-event period, indicating that elevated exits were not offset by subsequent hiring.

While these patterns document substantial demographic change in their own right, they do not distinguish between two explanations. Persistent declines in representation could reflect a deliberate reorientation of offices' hiring priorities, or they could arise if offices were unable to replace existing staff along dimensions where preferences remained stable. We turn next to this distinction.

[23] Also see Bolton, Hassell and McCrain 2025 for previous work on racio-ethnic minority staffers' turnover rates.

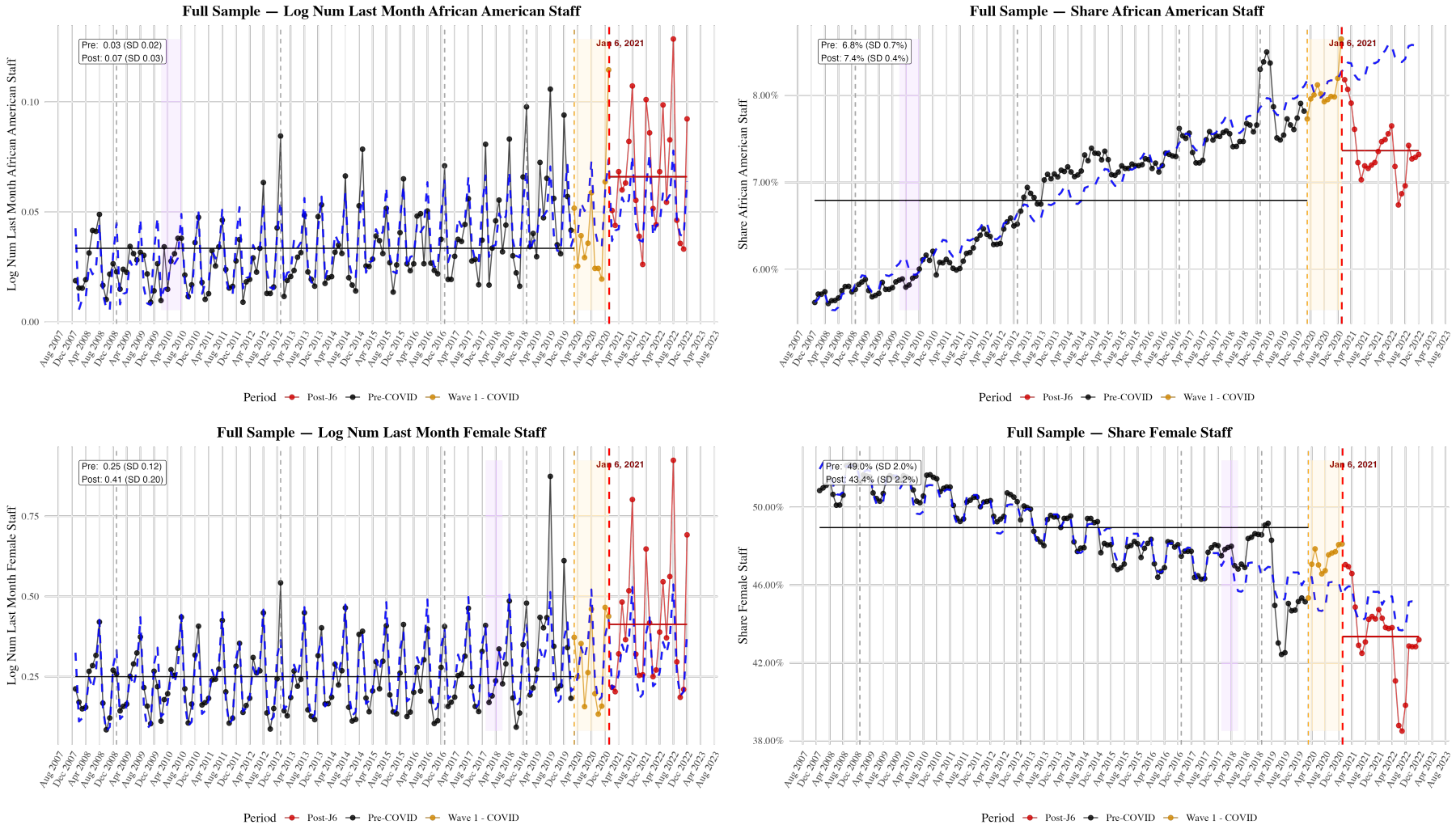

Figure 5: Average Full-Time Staff Demographics Each panel displays monthly averages for the full sample of congressional offices. The left column shows the logged number of non-temporary workers in their last month for Black (the top row) and female staffers (the bottom row). The right column shows the group's share in the office. Before and after January 6 means are shown in black and red, respectively, and reported on the figure alongside sample standard deviations.) Purple shading denotes the 6 months after the revelation of the Congressional Black Caucus scandal for Black staffers, and the start of the #MeToo movement for female staffers.

Self-Selection or Strategy? Examining Assortative Hiring

Hypothesis 3 implies that post-January 6 staffing changes primarily reflect self-selection by departing staff rather than offices' shifting hiring preferences. If so, offices should struggle to replace workers along dimensions where their hiring preferences have historically been stable. Congressional offices hire in an assortative fashion: for example, Black members tend to employ a higher share of Black staff than non-Black members, and female members employ a higher share of female staff than male members (for further examples, see S.I. Table F1). These patterns enable the following test: If preferences for assortative hiring remain intact after January 6 but the relevant labor pool contracts, offices should continue attempting to hire preferred types of workers yet fail to fully replenish them following elevated exits.

To benchmark our analysis, we use prior presidential transitions: the six months following January 6, the first six months of the Trump administration in 2017, and the first six months of the Obama administration in 2009. As our previous results have shown, these are periods of heightened turnover and reorganization within offices, but these transitions did not involve events that challenged staffers' professional identities. Hence, presidential transitions provide a reference point for how effectively offices typically replace staff under conditions of high turnover but relatively unconstrained labor supply. We compare post–January 6 patterns to the first six months of the Trump administration in 2017 and the first six months of the Obama administration in 2009 to assess whether replacement failures observed after January 6 reflect an unusually constrained hiring environment rather than normal transition dynamics.

Figure 6 examines exits, hires, and replacement rates for Black and female staff across the three transition periods. The top panel focuses on Black staff in Black and non-Black Democratic members' offices, while the bottom panel presents analogous patterns for female staff in female and male Republican members' offices. We measure the cumulative replacement rate as the number of hires in month t and the subsequent two months, divided by the number of Black staff who exited in month $t-1$, capturing the extent to which attrition is replenished within a three-month window. Offices without strong assortative hiring preferences for these groups — non-Black Democratic members and male Republican members — provide a comparison group. These offices face the

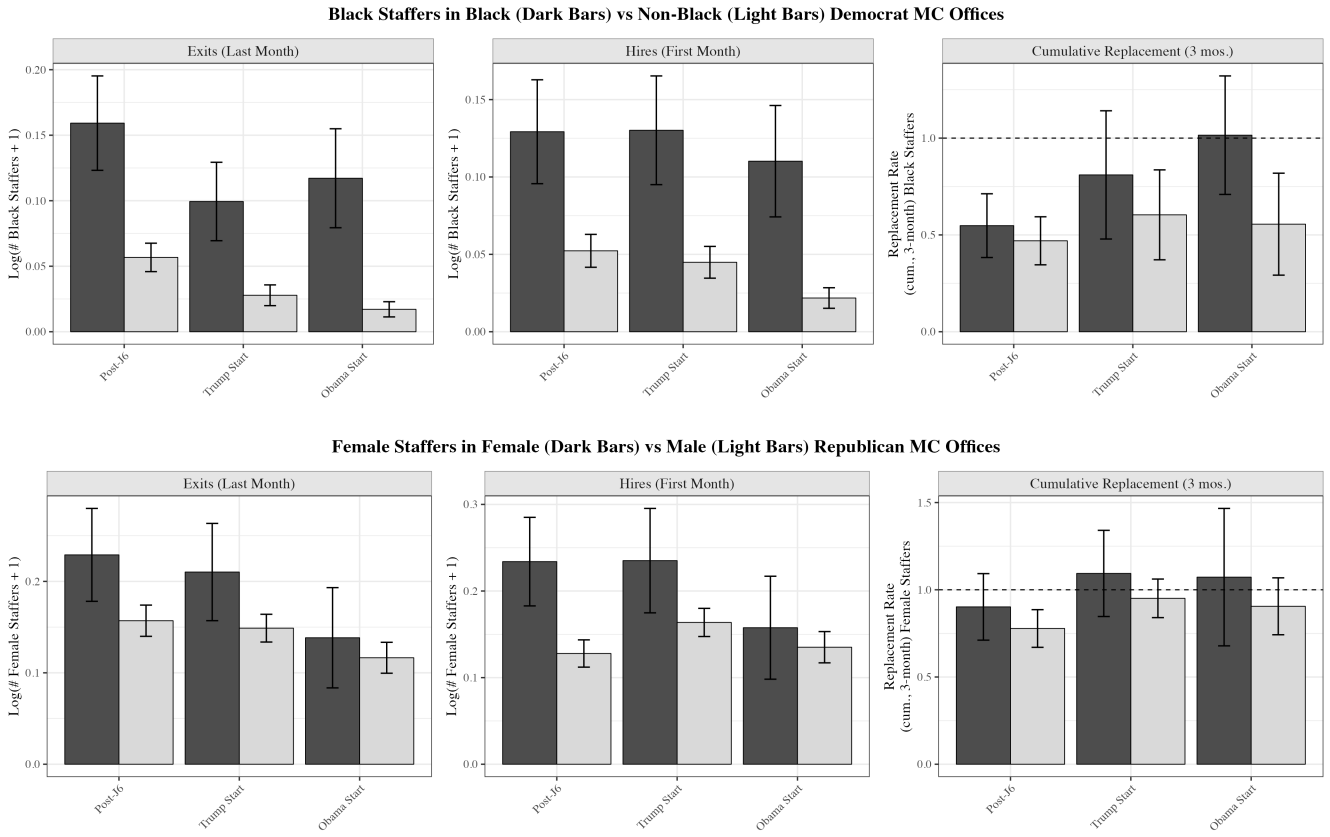

Figure 6: Turnover, Hiring, and Replacement in Congressional Offices The top panel shows descriptive patterns for Black staffers in Black and non-Black Democratic members' offices, comparing the logged number of Black staffers in their last month (separations and exits), the logged number in their first month (hires), and an office-level cumulative replacement rate (horizontal dotted line denotes the full replacement rate). Estimates are shown for three six-month periods: the six months following January 6, 2021; the first six months of the Trump administration in 2017; and the six months following the start of the Obama administration. The bottom panel presents the same descriptives for female staffers in female and male Republican members' offices. Points represent means across member-months within each period. Error bars indicate 95 percent confidence intervals. Office shares of each group are shown in S.I. Figure F1.

same general post-January 6 hiring environment but do not preferentially recruit from the specific, smaller labor pools (Black staff, female staff) that contracted most sharply after the attack.

Following January 6, Black staff exits are higher in Black Democratic members' offices relative to prior transitions. Black members continue to hire Black staff at levels comparable to earlier transition periods, indicating that demand for Black staff does not decline. Nevertheless, elevated exits are not offset by subsequent hiring. Even allowing a three-month window for recruitment, Black members replace 55 percent of exited Black staff within 3 months, on average, after January 6, compared to higher replacement or near-complete replacement rates during the Trump and Obama transitions, respectively. As a result, the share of Black staff in Black members' offices declines

and fails to recover (also presented as a continuous time series in S.I. Figure F2). By contrast, Black staffers' turnover, hiring, and relative replacement in non-Black Democratic members' offices are roughly similar across transitions. Taken together, these patterns indicate that post–January 6 replacement shortfalls emerge precisely where preferences for hiring Black staff are strongest, consistent with a contraction in the available labor pool rather than a shift in offices' hiring priorities. The bottom panel reveals similar dynamics for female staff in female Republican offices: exits increase, hiring attempts persist, but replacement remains suppressed relative to earlier transitions.[24]

Additional Findings

Our results indicate that staffing changes after January 6 reflect constraints in the congressional labor market. Democratic offices experienced persistent declines in staff experience and educational attainment, increased reliance on temporary workers, and sustained losses of Black and female staff. These demographic declines coincided with a weakening of assortative replacement, even as offices continued to hire preferred types. Together, these patterns are consistent with the selection effects posited in Hypothesis 3, in which offices face enduring constraints in recruiting and retaining their preferred staff following January 6.

A natural question is whether the above-outlined turnover and human capital results influenced staff performance, and in turn, member productivity. Directly measuring staff performance is difficult, however, because most outputs are joint products of members and their teams, complicating attribution. Ideally, we would analyze casework — tracking request volumes and response times across offices — since this is a staff-dominant activity. But such data are rarely accessible, and the few studies that have secured access (e.g., Quinn et al., 2025) necessarily draw on selective samples, which constrains inference beyond the covered offices and periods.

In SI Section D.5, we present results for two sets of outcomes at the office-month level. We study newsletters sent to constituents collected from *DCInbox*. These newsletters frequently include

[24]These patterns extend beyond race and gender in expected ways. Estimates for Ivy League–educated staffers are less precisely estimated due to smaller underlying counts and greater month-to-month volatility, but S.I. Section F.3 presents analogous descriptives for alumni Democratic and Republican members' offices. Point estimates are directionally consistent across all groups examined. While changes in the representation of Black or female staff might plausibly reflect strategic considerations tied to specific legislative or constituent-service demands, such arguments are less compelling for staff educational pedigree.

information about members' activities on the Hill, and are part of an office's general communications strategy. We might expect that changes in office composition affect both the volume and content of constituent newsletters. Empirically, we find no partisan differences in the number of emails sent after January 6. By contrast, Democratic offices reduced the share of political language in their emails relative to Republican offices, indicating a post-January 6 shift toward less politicized communication. While this pattern is consistent with our theoretical argument, we view these results as suggestive rather than definitive. Changes in the content of office communications may reflect a variety of strategic considerations unrelated to staff composition, and these outcomes should not be interpreted as providing direct evidence on ideological positioning or staff preferences. Second, we examine bill introductions by identifying the sponsors and co-sponsors of legislation at the office-month level. We model the number of bills sponsored, the number of bills co-sponsored at introduction, and the number of new co-sponsorships. We observe limited but nontrivial differences. In particular, we find post-January 6 decline in Democratic offices' bill initiation when sponsorship is measured as primary sponsors plus original co-sponsors who are understood to be part of the bill's development (Curry and Roberts, 2023). Because bill initiation is jointly produced with committees and external stakeholders, we treat these outcomes as indirect indicators of office capacity rather than staff-dominant outputs.

Accordingly, these patterns should be read as suggestive of post-January adjustments, not as comprehensive evidence on productivity. The forms of capacity most affected by persistent turnover and compositional change, such as institutional knowledge, are poorly captured by standard observable outputs. Offices may therefore sustain baseline levels of observable activity even as underlying organizational capacity erodes, consistent with our broader argument that January 6 imposed enduring constraints on congressional labor markets.

Discussion

This paper develops and tests a theory of exit among public sector workers following identity-threatening institutional shocks. Before concluding, we situate our findings within prior work, discussing limitations, and outlining directions for future research.

A natural concern is whether our results are driven by violence *as such*. A growing literature argues that the perpetration of and tolerance for political violence deserves more central attention as a driver and moderator of political outcomes (e.g., in the U.S. context see Uscinski et al., 2021; Kalmoe and Mason, 2022; King, 2025). We share that concern, but in our setting this interpretation is too narrow. The violence of January 6 matters in our framework not primarily through physical danger, but as a symbolic act directed at an institution and, for staff unaligned with the rioters, an extreme continuation of contentious politics (McAdam, Tarrow and Tilly, 2001; Tilly, 2014) that can be psychologically and professionally injurious to workers who tie their self-concept to institutional stability. Moreover, the attack was followed by operational breakdowns — evacuations and lock-downs on January 6, followed by militarized security and persistent disruption — that constrained staffers' ability to perform and reaffirm their professional roles. This is consistent with existing views of political violence as a "*repertoire* of actions oriented at inflicting physical, psychological, and symbolic damage" (Bosi and Malthaner, 2015, pp. 439, emphasis added).

Importantly, neither our argument nor our results hinge on direct exposure to physical harm. Many staffers worked remotely due to the pandemic, and we observe comparable turnover responses across staff strata, including among junior and administrative roles as well as more senior staff (SI Table D17). Here, the consequences of political violence depend on how the event is framed and interpreted within the organizational setting (Sewell Jr, 1996), processes shaped by partisanship and professional norms. At the same time, the broader rise in threats and harassment targeting public officials in the United States (Simi and Hughes, 2025) plausibly increases the expected costs of remaining in and entering public service through the channels we outline here. Future work should more directly isolate the role of political violence from other forms of dysfunction.

Seen this way, the relevance of January 6 lies less in its exceptional violence than in the configuration of conditions it created. Comparable dynamics appear in other professional settings following crises that combine professional identity threat with workplace dysfunction. For example, as Vaughan (1982) argues, the *Challenger* space shuttle disaster posed professional identity threats for engineers at NASA and its contractor Morton Thiokol, as ignored safety warnings and a catastrophic failure they believed preventable were followed by public investigations that placed engineers more centrally at the locus of blame, diminishing their professional standing. These identity

threats were compounded by workplace dysfunction, as non-scientist manager's efforts to reassert control constrained employee' ability to return to meaningful work — leading some to exit (Boisjoly, Curtis and Mellican, 1989; Mahler, 2009; McDonald, 2012). More recently, attorneys working in the Department of Justice at the start of President Trump's second term have described tensions between professional identity and increased caseloads, causing exit (Lowell, 2025; The Daily, 2025). In both of these cases, exit patterns cannot be explained solely by workload, compensation, or risk. Instead, they reflect whether workers can continue to see their roles as meaningful expressions of professional values within functioning organizations. Our framework helps unify these cases by identifying when crises are most likely to produce durable human capital consequences.

Because of our focus on congressional staffers, our findings speak most directly to political appointees rather than civil servants. Importantly, congressional staffers' lack of employment protections that characterize much of the civil service can make instances of identity threat and workplace dysfunction all the more costly and cognitively taxing to bear (Sverke, Hellgren and Näswall, 2002). Civil service laws such as tenure protections and professional insulation may therefore buffer career bureaucrats from the identity-dysfunction dynamic we describe, even when they face comparable organizational stress (e.g., Bhatti, Gørtz and Pedersen, 2015; Howell-Moroney and Ertas, 2025).[25] This contrast between staffers and career bureaucrats suggests a potentially important scope condition of our argument. Future research could leverage institutional variation in bureaucratic design to test how employment protections and professionalization moderate the relationship between crisis, turnover, and recruitment.

January 6 did more than trigger a temporary spike in resignations. It altered the stability, experience, and composition of the congressional workforce in ways that persisted well beyond the immediate aftermath. These changes matter because staffers are central to legislative capacity (Fox and Hammond, 1977) and institutional memory (Mills and Selin, 2017; Ommundsen, 2023). More broadly, our theoretical contribution is to claim that certain kinds of crises — those that simultaneously threaten professional identity and disable the workplace — can generate enduring human capital consequences. Understanding these dynamics is essential for explaining how governmental

[25]This buffering may be incomplete: prior work shows that politically misaligned bureaucrats do exit in response to changes in administration (e.g., Doherty, Lewis and Limbocker, 2019; Bolton, De Figueiredo and Lewis, 2021).

institutions respond to stress, and why some workplace crises are more injurious than others.

Conclusion

In this paper, we study how public sector employees respond to workplace crises. We leverage the events of January 6, 2021 as a salient institutional shock that coupled acute workplace dysfunction with a direct threat to congressional staffers' professional identities. To assess personnel responses, we assemble career histories for congressional staff from 2008 to 2022. We find that exits increased in the months following January 6, with the effects concentrated in Democratic offices. These separations were followed by measurable shifts in workforce composition: Democratic offices experienced declines in average legislative experience and educational attainment, alongside greater reliance on temporary staffers. Finally, we document post-January 6 declines in racial and gender diversity of congressional staff.

Beyond contextualizing recent research on January 6, we offer a framework for when and how shocks in public workplaces drive exit. Shocks that undermine staffers' professional identities and simultaneously impede the routines that sustain those identities have the potential to be particularly injurious. The post-event increase in separations, especially in Democratic offices, suggests that polarized conflict can impose asymmetric human-capital costs on the side most directly targeted. In this sense, polarization is not merely a feature of preferences or roll-call behavior; it can operate through a capacity channel, hollowing out the expertise and continuity required for effective lawmaking. Additionally, when crisis trigger departures of experienced staff and shift hiring toward temporary workers, offices lose institutional memory, procedural fluency, and policy specialization. These inputs structure what members can credibly attempt and how quickly they can respond to fast-moving legislative agendas.

More broadly, the stability and quality of public institutions hinge on the people who staff them (Fox and Hammond, 1977), and those people respond not only to wages and promotion ladders but also to whether their work remains identity-congruent and doable during moments of crisis. Institutional crises can therefore degrade governance indirectly by reshaping who stays and who joins.

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How Crisis Reshapes Government Talent

Online Appendix

Table of Contents

A Data Descriptives

A.1 Descriptives: Staffer Exit Locations

Our analysis tracks Congressional staff transitions using employment records from LegiStorm, which comprehensively covers Congressional positions and the many private-sector organizations that interact with MCs (lobbying firms, think tanks, government affairs roles). However, as expected, a substantial portion of staff transitions lead to positions outside our data coverage. We present descriptive statistics on individuals with and without post-Congressional employment records in Table A1. Panel A in the Table shows descriptives for individuals exiting during our 2017 to 2024 analysis period in the main text, and Panel B shows descriptives for the exits in the period spanning 2008 to 2016 (which we use in the construction of our labor market controls, discussed in Section B).

Table A1: Comparison of Staff with and without Observed Next Occupation

Panel A: 2017–2024 Exits
Table A2:

	Has next occupation (N=5839)		Missing next occupation (N=10704)			
	Mean	Std. Dev.	Mean	Std. Dev.	Diff. in Means	Std. Error
Exit year	2019.514	1.763	2019.614	1.729	0.099	0.028
Tenure (months)	55.986	48.698	44.927	56.831	−11.059	0.841
Has advanced degree	0.174	0.379	0.104	0.305	−0.070	0.006
Has LinkedIn	0.930	0.256	0.653	0.476	−0.277	0.006
Junior staffer	0.112	0.315	0.257	0.437	0.146	0.006
Senior staffer	0.172	0.377	0.036	0.186	−0.136	0.005
Communications role	0.182	0.386	0.084	0.277	−0.098	0.006
Legislative role	0.380	0.486	0.323	0.468	−0.057	0.008
Administrative role	0.158	0.365	0.222	0.415	0.063	0.006
Democratic office	0.514	0.500	0.552	0.497	0.037	0.008

Panel B: 2008–2016 Exits
Table A3:

	Has next occupation (N=7686)		Missing next occupation (N=14769)			
	Mean	Std. Dev.	Mean	Std. Dev.	Diff. in Means	Std. Error
Exit year	2012.617	2.358	2011.810	2.560	−0.808	0.034
Tenure (months)	50.775	44.728	41.864	49.220	−8.911	0.651
Has advanced degree	0.177	0.382	0.053	0.223	−0.124	0.005
Has LinkedIn	0.861	0.346	0.300	0.458	−0.561	0.005
Junior staffer	0.129	0.336	0.265	0.441	0.135	0.005
Senior staffer	0.139	0.346	0.032	0.176	−0.107	0.004
Communications role	0.170	0.376	0.069	0.254	−0.101	0.005
Legislative role	0.390	0.488	0.288	0.453	−0.102	0.007
Administrative role	0.174	0.379	0.294	0.456	0.120	0.006
Democratic office	0.539	0.498	0.577	0.494	0.038	0.007

Because LegiStorm's data collection process relies in part on LinkedIn records, individuals for whom we observe subsequent occupation data almost all have, or at some point had, LinkedIn accounts. This is especially the case in the pre-2017 period. Moreover, although we only present descriptive results for non-temporary staff (i.e., excluding interns, temps, and individuals on fellowships), staffers with subsequent occupation data also tend to stay longer than one year in Congress.

Table A1 and the upper panel of Figure A1 show that non-junior staff are, on average, 10 percentage points more likely to have subsequent employment records. The bottom panel of the Figure show that within non-juniors, the individuals with subsequent employment data are more likely to be in non-administrative roles or roles we did not classify for the purposes of our analysis (e.g., caseworkers or district workers).

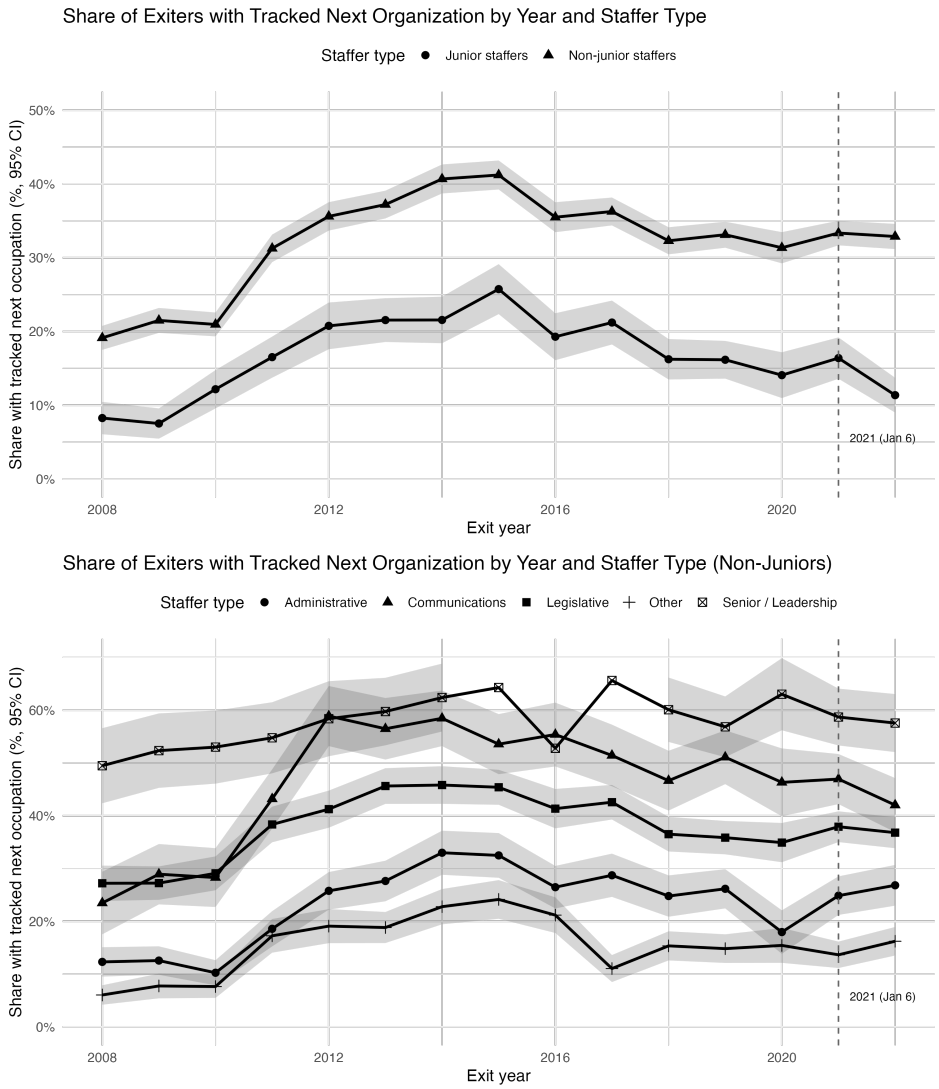


Figure A1: Share of Full-time Staff Exiters with Subsequent Career Data Data plots the share of exiting, full-time Congressional staffers with some subsequent career data by tenure (in the top panel) and job type (in the bottom panel).

While the 31% coverage of post-Congressional jobs could be cause for concern, we emphasize two things. First, LegiStorm's employment records within Congress come directly from official *Statement of Disbursements* filings, ensuring that start and end dates reflect actual payroll activity rather than retrospective self-reports. This is why we consider both "separations" (Congressional-to-Congressional moves) and "exits" (Congressional-to-outside moves) in the main text. To the extent we do rely on post-Congressional career outcomes, we either use them in auxiliary analyses as control variables or focus our analysis on individuals for whom coverage is most complete: senior staff, legislative staff, and those with advanced degrees.

Second, as we discuss above and show, coverage of post-Congress employment is not random: individuals in the very types of positions most visible to external employers are disproportionately represented among those with identified next occupations. This bias reflects the nature of LegiStorm's data collection, and, as a result, our observed exits probably over-represent staff with more private-sector oriented or career-mobile profiles. This selection works against our main theoretical expectation that public-service-motivated staff, who are less private-sector oriented, would be more likely to leave following January 6. In that sense, any bias introduced by data availability in our revolving door analyses would downward bias our estimates.

A.2 Job Titles

We code staff positions mainly following the procedures outlined by (Montgomery and Nyhan, 2017; McCrain, 2018). We hand code our "Administrative staff" job titles based off of job titles not already categorized by the aforementioned sources with at least 50 employees over the course of our analysis period. Scheduling, HR, administrative, and "assistants" were coded following "Position Descriptions" featured on the U.S. Senate Employment Office website.[26]

Table A4: Job Titles

Chief of Staff
chief of staff‡; legislator director
Legislative Staff Positions
legislative correspondent; legislative assistant*; legislative aide*; legislative coordinator; legislative adviser; policy analyst; legislative fellow; policy adviser*; senior adviser*; policy aide; policy director; director of policy; policy coordinator; counsel; policy specialist; research assistant; policy analyst; fellow*; law clerk; research director; legislative research assistant; legislative clerk; legislative analyst; u.s. senate aide; national security adviser; special adviser; appropriations associate; legislative associate; senior legislative associate; legal fellow; transition aide; appropriations director; adviser; legislative liaison
Administrative / Operations / Support¶
Scheduling, HR, Admin: staff assistant; scheduler; scheduling assistant; scheduling director; scheduling coordinator; scheduling correspondent; receptionist; front office coordinator; front office manager; front office supervisor; office manager; office administrator; office assistant; office coordinator; office director; administrative assistant; administrative aide; administrative specialist; administrative manager; administrative director; operations director; operations manager; operations coordinator; operations assistant; chief of operations; executive assistant; special assistant; personal assistant; executive secretary; secretary; clerk; records manager; archivist; archives specialist; front desk; appointment desk; telephone operator; driver; messenger driver; page; page coordinator; intern coordinator; grants coordinator; travel coordinator; flag coordinator; mail manager; mailroom manager; mailroom coordinator; mailroom assistant; mailroom director; mailroom staff assistant; correspondence director; correspondence manager; correspondence specialist; correspondence coordinator; correspondence aide; correspondence clerk; correspondence operator; financial administrator; financial manager; financial director; financial coordinator; accountant; accounts manager; accounts representative; accounting manager; accounting representative; accounts payable specialist; accounts administrator; accounts assistant; accounts clerk; accounts director; payroll and benefits generalist; payroll and benefits specialist; chief administrative officer; house operations director; human resources [HR] director; human resources manager; human resources specialist; HR coordinator; HR assistant; *Assistants:* assistant to the chief of staff; assistant to chief of staff; assistant to the staff director; assistant to staff director; assistant to the chair; assistant to the speaker; assistant to the leader; assistant to the vice president; assistant to the senator; assistant to the member; assistant to the representative; assistant to rep; personal secretary; executive clerk; chief receptionist; *IT & Systems:* systems administrator; systems engineer; systems manager; systems analyst; systems technician; systems integrator; systems support; network operations specialist; network infrastructure specialist; network support specialist; network engineering specialist; network operations manager; network infrastructure manager; network support manager; network engineering manager; network implementation specialist; information technology director; information technology manager; information technology specialist; information technology analyst; information technology architect; information technology officer; information systems director; information systems manager; information systems specialist; information systems analyst; information systems architect; information systems officer; information security director; information security manager; information security specialist; information security analyst; information security officer; computer operator; computer specialist; computer operations supervisor; technical support engineer; technical support specialist; technical support representative; technical support director; technical solutions engineer; technical assistance specialist; technical operations engineer; technical operations specialist; technical operations representative; software engineer; software specialist; cybersecurity specialist; chief information security officer

‡ anything containing "Chief of Staff" and not "assistant to." ∗ anything containing `legislative` or `policy` that is not captured as Chief of Staff.

[26] https://employment.senate.gov/position-descriptions/

B Labor Demand for Congressional Staff

We consider the possibility that the labor market for staffers with Democratic-office experience changed for reasons unrelated to January 6, confounding our turnover effect. For example, first, the COVID-19 pandemic reshaped hiring priorities and work practices across many sectors, potentially in ways correlated with political orientation — for instance, through changes in hiring priorities in partisan-tied industries (e.g., tech and electronics for Democrats or oil and gas for Republicans) or differing expectations about remote work. Second, the transition from President Trump to President Biden may have generated heightened interest in Democratic staffers from lobbying and government-relations firms anticipating renewed access to policymaking networks. Taken together, these two shocks could have increased demand for individuals with certain partisan backgrounds or specific office experience, thereby influencing their external labor-market opportunities rather than their intrinsic willingness to remain in Congress.

To assess this, we analyze monthly job-listing data for Washington, D.C., Maryland, and Virginia (the "DMV") from WageScape, a data vendor that compiles postings from publicly available, aggregated job boards covering the period from May 2016 (the earliest available date) through the present. In this section, we describe how we operationalize these data, present descriptive results on the DMV-area labor market for Democrat and Republican staffers. We present our results accounting for office- and partisan-specific labor market demand in Columns 10 and 11 of SI Table D4 and D5 and find few differences with our main specification findings.

B.1 Incorporating the WageScape Data

WageScape is a data vendor that primarily contracts with human resources departments and recruiting firms to provide insights into changes in labor market dynamics. Access to these data was provided to the authors by DeweyData. For our purposes, we rely on job listings from the DMV area for the full period WageScape provides — May 2016 through October 2025 — which captures both pre- and post-pandemic labor-market conditions as well as the 2020 presidential transition.[27] The raw data includes information about a listing's original date of posting, its location (city and state), the position's title, and the name of the firm posting the advertisement.

To better understand partisan dynamics in post-Congressional employment, we draw on our personnel records to identify organizations that predominantly hired former staffers from either Democratic or Republican member offices spanning the period January 2008 to December 2016. Across these years, our data include 8,907 staff departures from members' offices to 4,353 distinct non-Congressional employers identified by LegiStorm. As we note in the main text and in Section A.1, this is likely not an exhaustive accounting of post-Congressional employment. These tracked departures make up only 32% of the 27,491 full-time staff exits in this period.

Using these 4,353 employers for the tracked employees, we can categorize employer-organizations as favoring Democrat or Republican staffers based on the share of hires in the 2008 - 2016 period with experience in different members' offices.

[27] Our focus on the DMV area likely *under-counts* the number of jobs that Congressional staffers are likely to be attracted to, though previous work in labor economics shows that individuals often do not leave their original employer's commuter zone when they transition to a new employer (Chinoy and Koenen, 2024).

B.2 The DMV Labor Market for Democrat and Republican Staffers

Table B1 presents differences between Democratic and Republican offices from February 2017 to December 2022. On average, Democratic offices are linked to a larger set of alumni organizations, with former staffers moving to approximately 13 distinct employers, compared to about 10 for Republican offices. Although offices in both parties are equally likely to be connected to at least one alumni organization that posts a job opening during the analysis period, Democratic staffers are associated with a greater volume of such postings overall.

Table B1: Balance on Labor Market Controls by Party

Panel A: Member-month controls

Table B2:

	Democratic offices (N=19223)		Republican offices (N=19318)			
	Mean	Std. Dev.	Mean	Std. Dev.	Diff. in Means	Std. Error
Alumni organizations (count)	13.244	13.234	10.490	9.653	−2.755	0.118
Any alumni org posting (past 3m)	0.369	0.483	0.354	0.478	−0.015	0.005
Alumni orgs with postings (count, past 3m)	0.636	1.099	0.576	0.996	−0.060	0.011
Any alumni org posting, policy-job (past 3m)	0.046	0.210	0.045	0.207	−0.002	0.002
Alumni orgs with postings, policy-job (count, past 3m)	0.049	0.230	0.049	0.236	−0.001	0.002

Panel B: Party-month controls

Table B3:

	Democratic offices (N=71)		Republican offices (N=71)			
	Mean	Std. Dev.	Mean	Std. Dev.	Diff. in Means	Std. Error
Any party org posting (past 3m)	1.000	0.000	1.000	0.000	0.000	0.000
Party orgs with postings (count, past 3m)	47.451	9.076	25.704	4.416	−21.746	1.198
Any party org posting, policy-job (past 3m)	0.775	0.421	0.648	0.481	−0.127	0.076
Party orgs with postings, policy-job (count, past 3m)	1.986	1.599	1.042	1.152	−0.944	0.234

C Timeseries

C.1 Forecasting Exercise

For our preliminary analysis, we conduct a descriptive time-series analysis of congressional staff outcomes to establish baseline trends and provide context for our causal estimates. The exercise serves two related purposes. First, COVID-19 introduced a large exogenous disruption to staffing patterns beginning in March 2020. In the absence of an adjustment, simple mean reversion after the shock could mechanically generate the appearance of an "effect" in the post-COVID period. Second, because all congressional offices were exposed to the January 6, 2021 attack, it is not possible to define untreated control units. This descriptive analysis is therefore valuable in its own right, as it allows us to evaluate whether staffing trajectories exhibit a clear intercept shift at the time of January 6 relative to forecasted counter-factuals.

To generate counterfactual staffing trajectories absent COVID-19 and January 6, we estimate simple forecasting models using pre-COVID data (up to February 2020). For each outcome, we aggregate observations into a balanced monthly panel. The model specification is:

$$Y_t = \alpha + \beta_t + \sum_{m=1}^{12} \gamma_m D_{mt} + \varepsilon_t,$$

where Y_t is the mean outcome in month t, t is a linear time index, and D_{mt} are month-of-year indicators capturing seasonal patterns. The linear-seasonal model is fit on pre-COVID data, and predictions are generated for the entire sample period through December 2022.

We then compare the observed post–COVID and post–January 6 outcomes to the forecasted counterfactual series. This design allows us to visually inspect whether deviations in staffing patterns reflect simple mean reversion to pre–COVID levels or instead a discontinuous break coinciding with January 6. Because all offices were treated, we cannot separately identify a control group; the forecast exercise instead serves as a synthetic baseline, providing evidence on the plausibility of January 6 as a discontinuous shock.

C.2 Additional Timeseries: Party Overlay

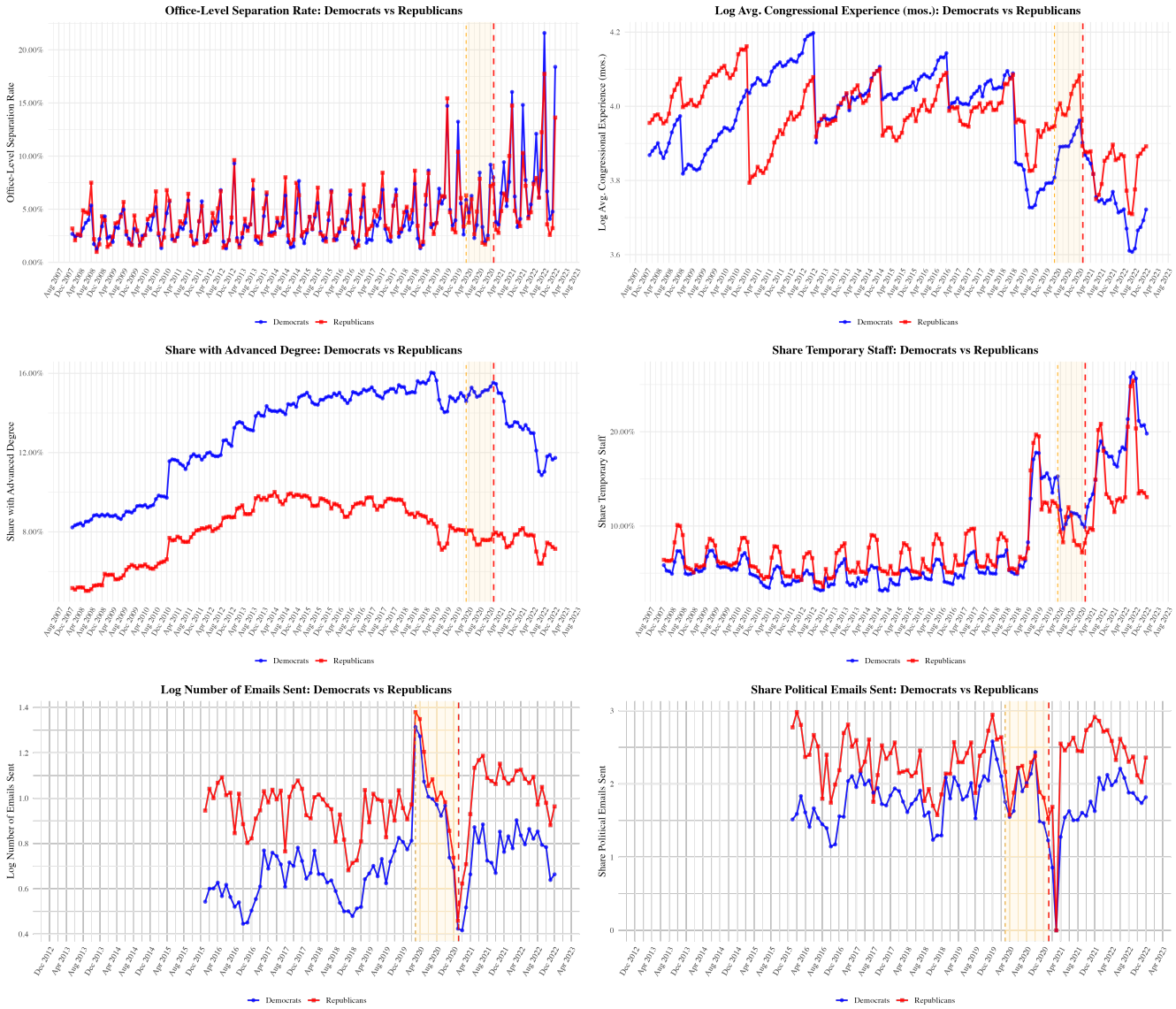


Figure C1: By-Party Trends in Main Outcomes Each panel displays monthly averages for Democrat (blue circles) and Republican (red squares) congressional member offices. The vertical red dotted line marks January 6, 2021. The vertical yellow dotted line denotes the start of the COVID-19 pandemic in March 2020.

D Additional January 6 Analyses

D.1 Synthetic difference-in-differences diagnostics

To test the validity of our synthetic difference-in-differences design, we conduct several diagnostics. First, we visualize the overlaid path of the treatment group (Democratic offices) and the reweighted path of the synthetic control group in the months leading up to and after January 2021. We do so for all outcomes in the manuscript. By construction, synthetic difference-in-differences reconstructs a synthetic control group aimed at better matching the treated groups path in terms of absolute levels and month-to-month changes. These visualizations, to the extent that synthetic paths follow treatment paths in the pre-period, offer illustrative evidence of the identification assumptions for synthetic difference-in-differences. For example, in Figure D1, we see very similar paths for Democratic offices and the synthetic control group, supporting the inferences made for this outcome. We see similar coherence in the paths for Office-Level Exit Congress rate in Figure D2. The paths for the human capital in Figures D3-D5 differ more than the turnover paths, but still overall demonstrate the close tracking of paths after synthetic reweightings.

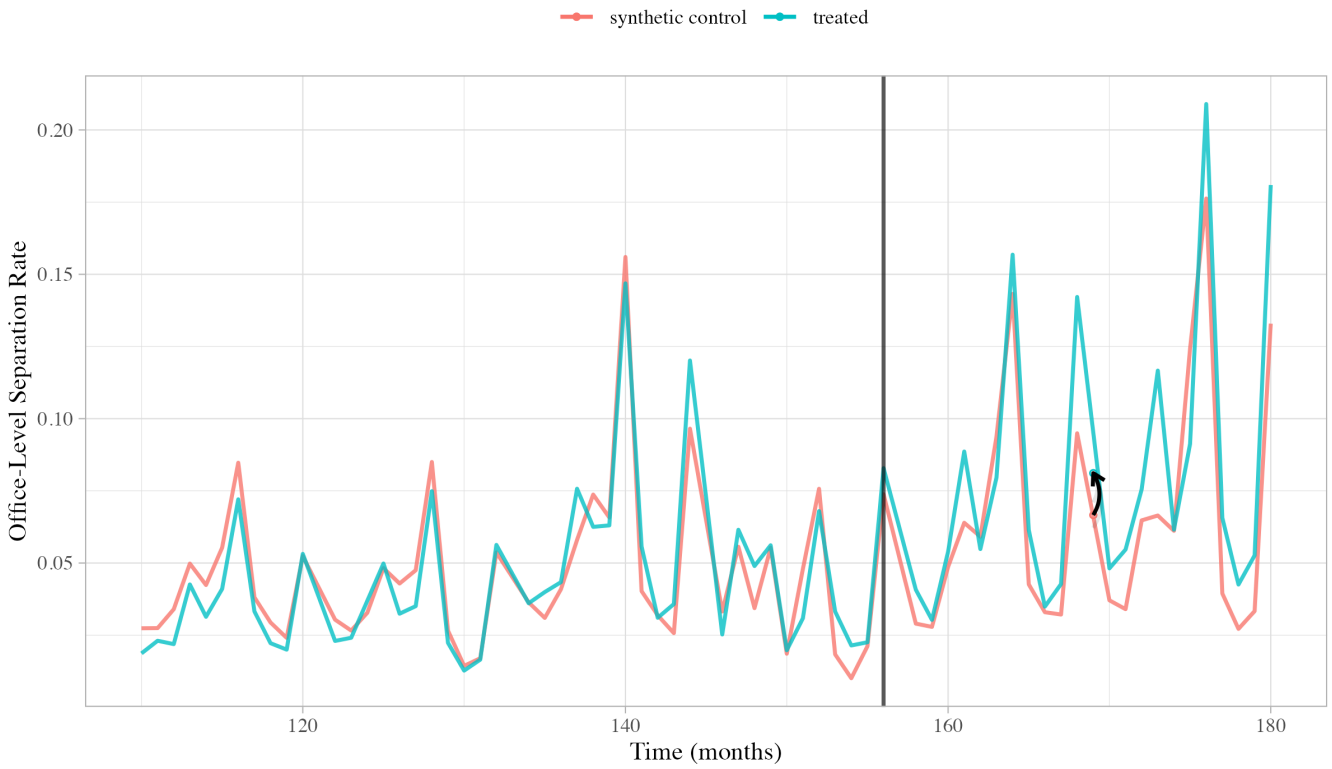


Figure D1: Office-level separation rate treated versus synthetic paths. Lines plot treated (blue) and synthetic control (red) paths before and after January 6, 2021 for office-level separation rate. Black arrow displays treatment effect, the average difference in the change in turnover for Democratic offices versus Republican offices after January 6, 2021. Horizontal black line signifies beginning of post-treatment period.

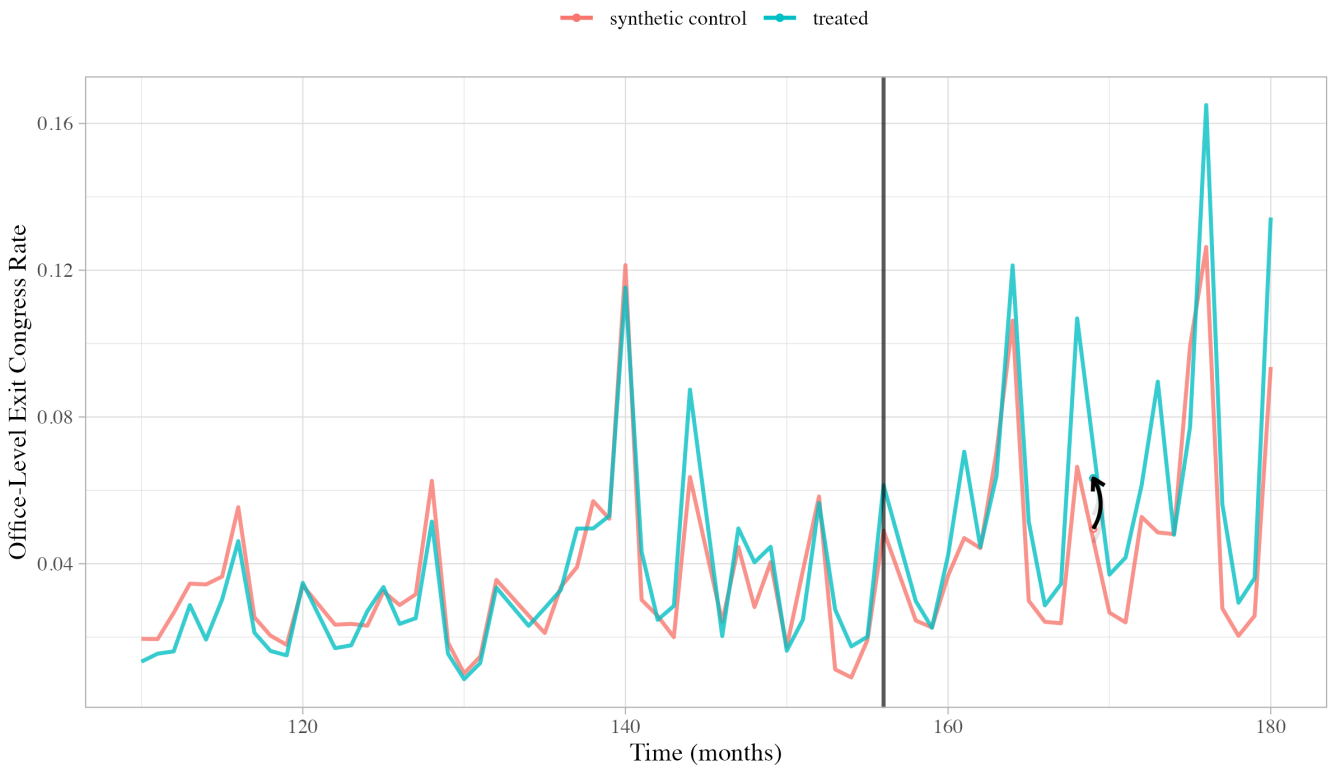


Figure D2: Office-level exit Congress rate treated versus synthetic paths. Lines plot treated (blue) and synthetic control (red) paths before and after January 6, 2021 for office-level exit Congress rate. Black arrow displays treatment effect, the average difference in the change in turnover for Democratic offices versus Republican offices after January 6, 2021. Horizontal black line signifies beginning of post-treatment period.

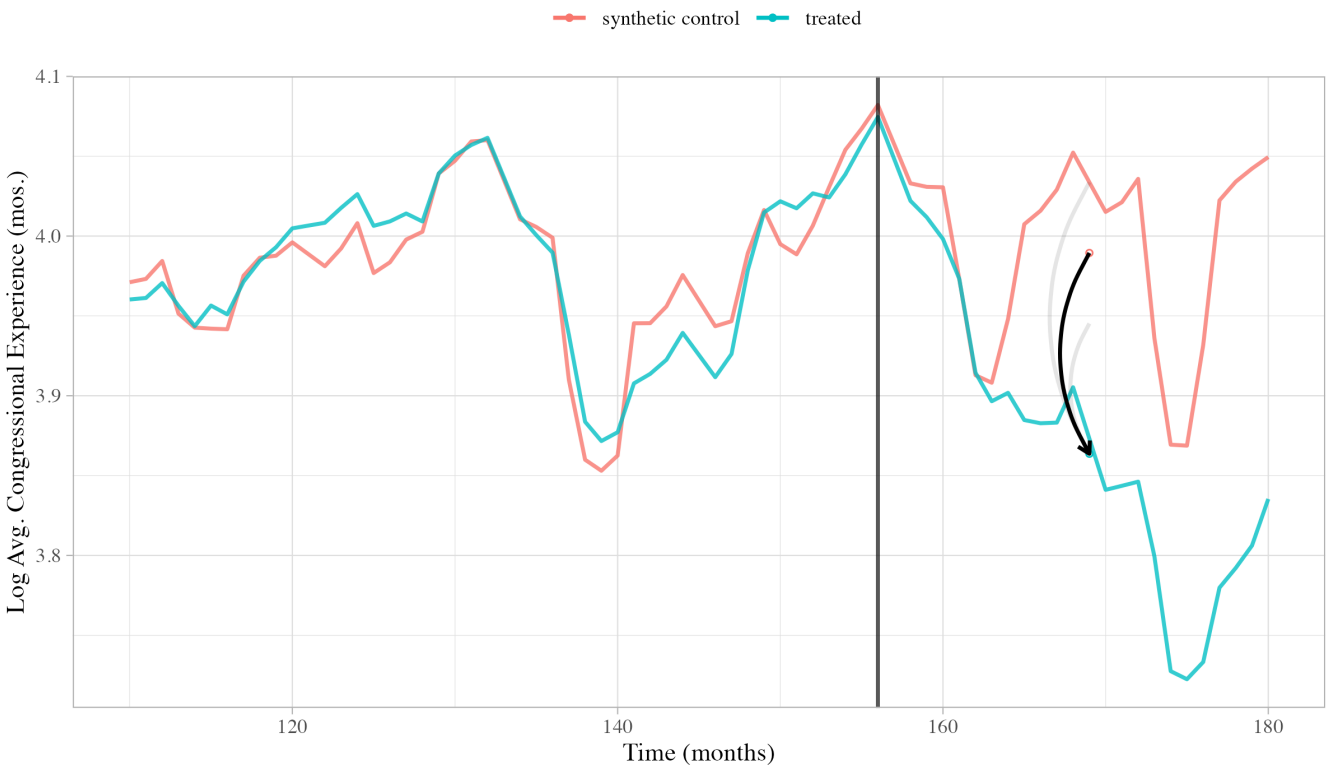


Figure D3: Logged average Congressional experience treated versus synthetic paths. Lines plot treated (blue) and synthetic control (red) paths before and after January 6, 2021 for logged average Congressional experience. Black arrow displays treatment effect, the average difference in the change in Congressional experience for Democratic offices versus Republican offices after January 6, 2021. Horizontal black line signifies beginning of post-treatment period.

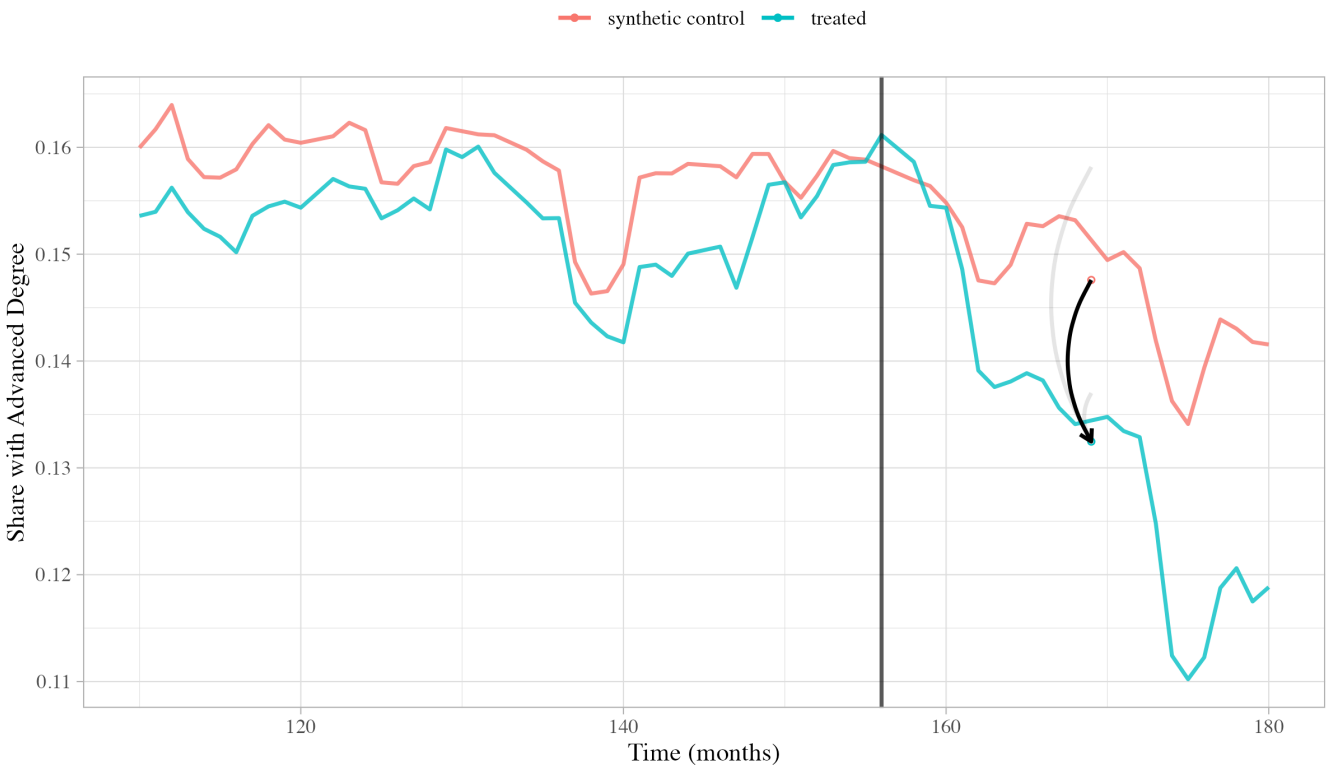


Figure D4: Share advanced degree treated versus synthetic paths. Lines plot treated (blue) and synthetic control (red) paths before and after January 6, 2021 for share with an advanced degree. Black arrow displays treatment effect, the average difference in the change in share with an advanced degree for Democratic offices versus Republican offices after January 6, 2021. Horizontal black line signifies beginning of post-treatment period.

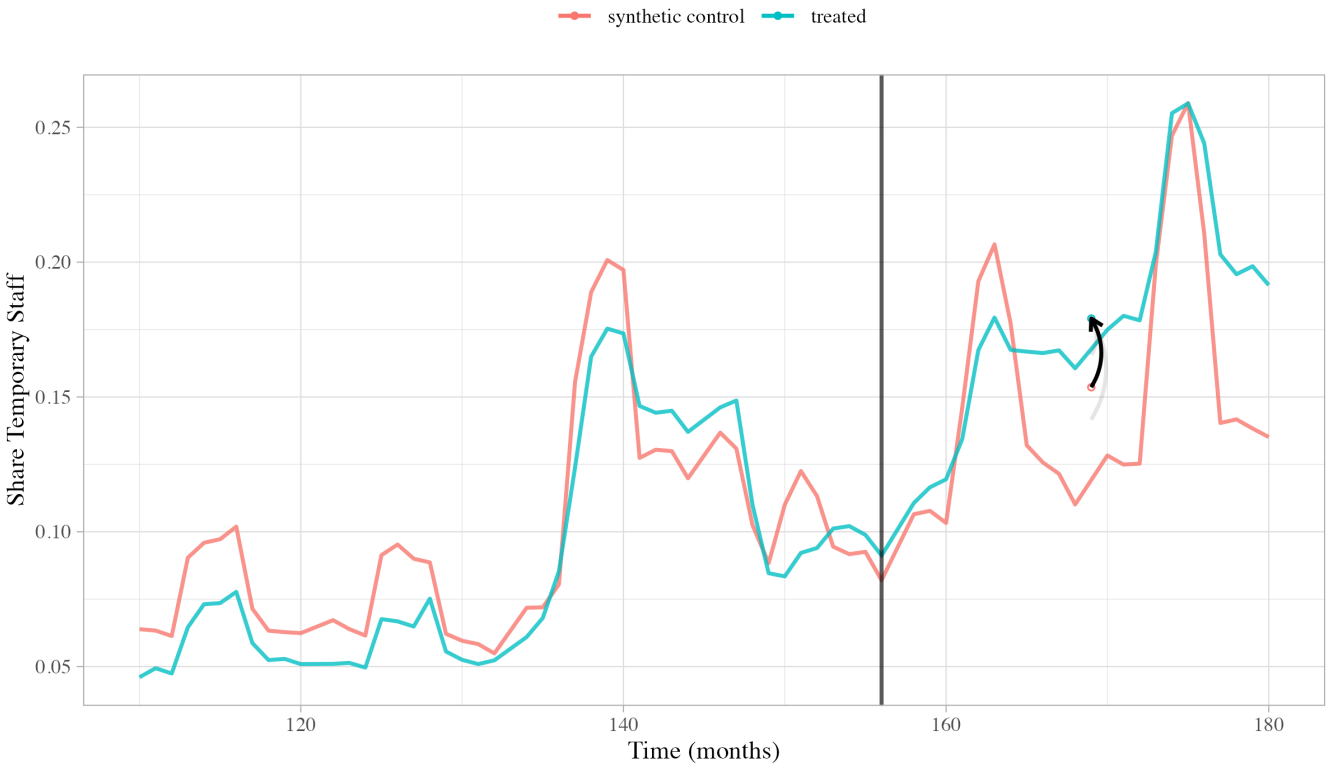
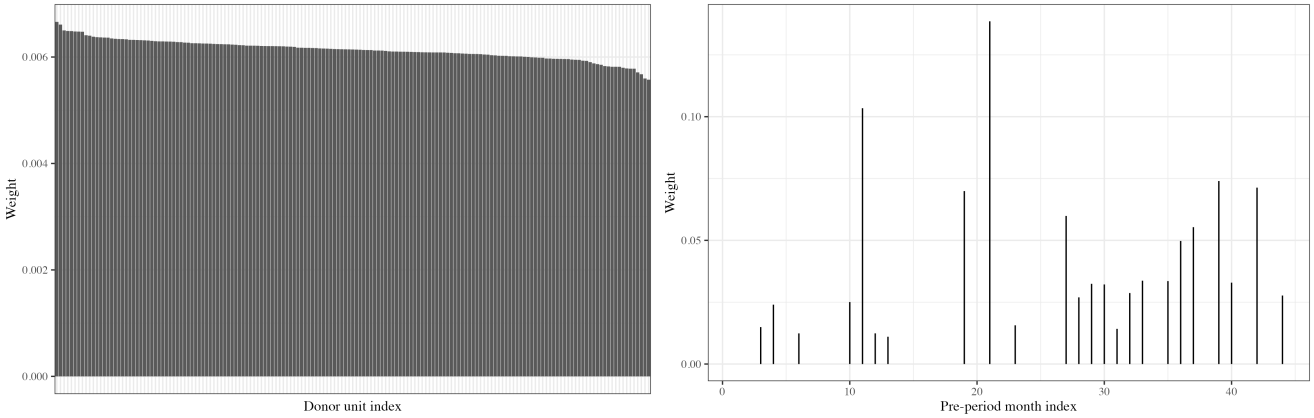


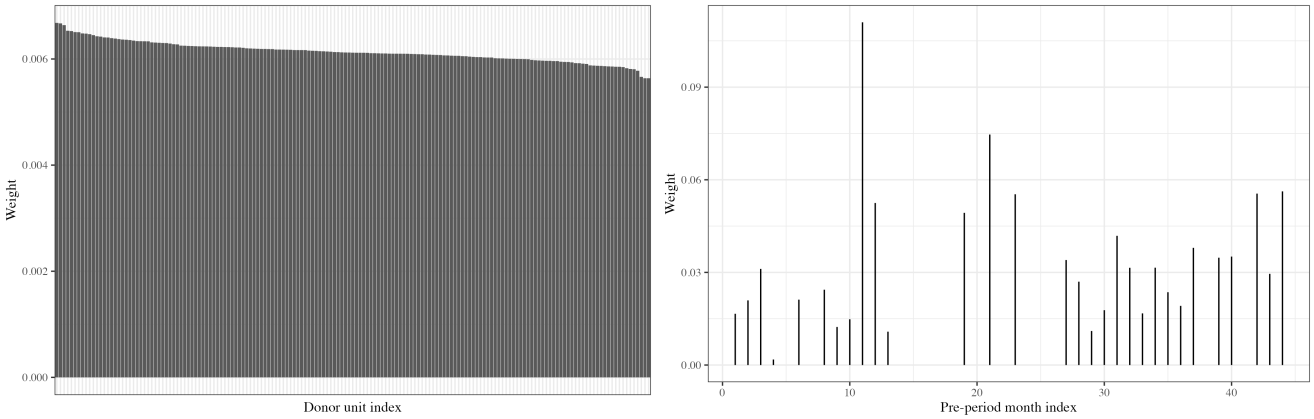
Figure D5: Share temporary staff treated versus synthetic paths. Lines plot treated (blue) and synthetic control (red) paths before and after January 6, 2021 for share of temporary staff. Black arrow displays treatment effect, the average difference in the change in share of staff who are temporary workers for Democratic offices versus Republican offices after January 6, 2021. Horizontal black line signifies beginning of post-treatment period.

Next, we present summary statistics on the distribution of unit and time weights in the synthetic difference-in-differences analysis for each of our manuscript outcomes. In Figures D6-D10, we present side-by-side figures, with the left figure plotting the weight values across all synthetic control units, ordered from largest to smallest weight, and the right figure plotting time weights across pre-period month-years ordered by earliest to latest in time. Both plots give a sense of how weights are distributed across units and inform later analyses of sensitivity to leaving out units with high weights or removing time periods from analysis.



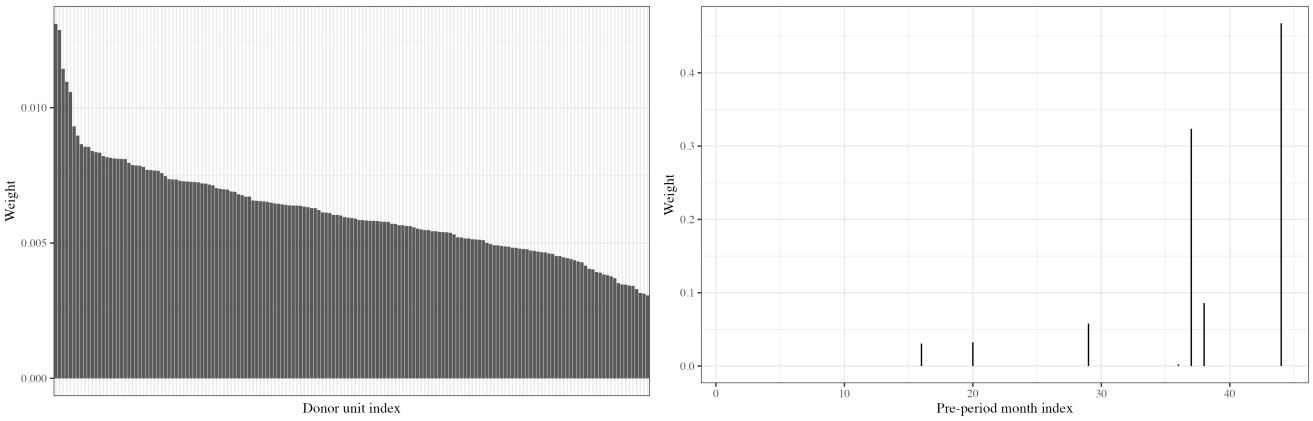
(a) Unit weights **(b)** Time weights

Figure D6: Synthetic unit and time weights – Office-level separation rate. Left panel plots the synthetic difference-in-differences weight for each donor (control) unit, ordered from largest to smallest weight. Right panel plots the time weights for each pre-treatment month-year, ordered from earliest to latest month-year.



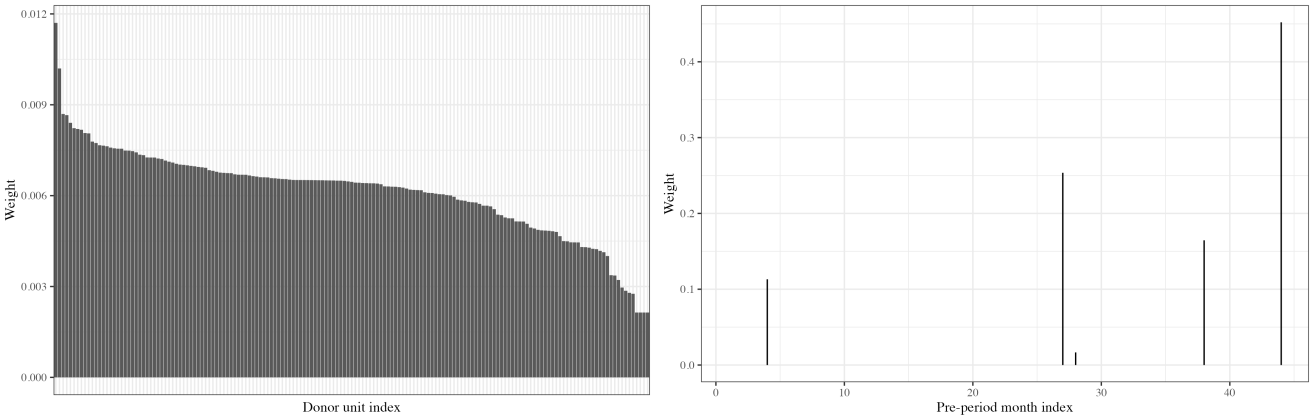
(a) Unit weights **(b)** Time weights

Figure D7: Synthetic unit and time weights – Office-level exit Congress rate. Left panel plots the synthetic difference-in-differences weight for each donor (control) unit, ordered from largest to smallest weight. Right panel plots the time weights for each pre-treatment month-year, ordered from earliest to latest month-year.



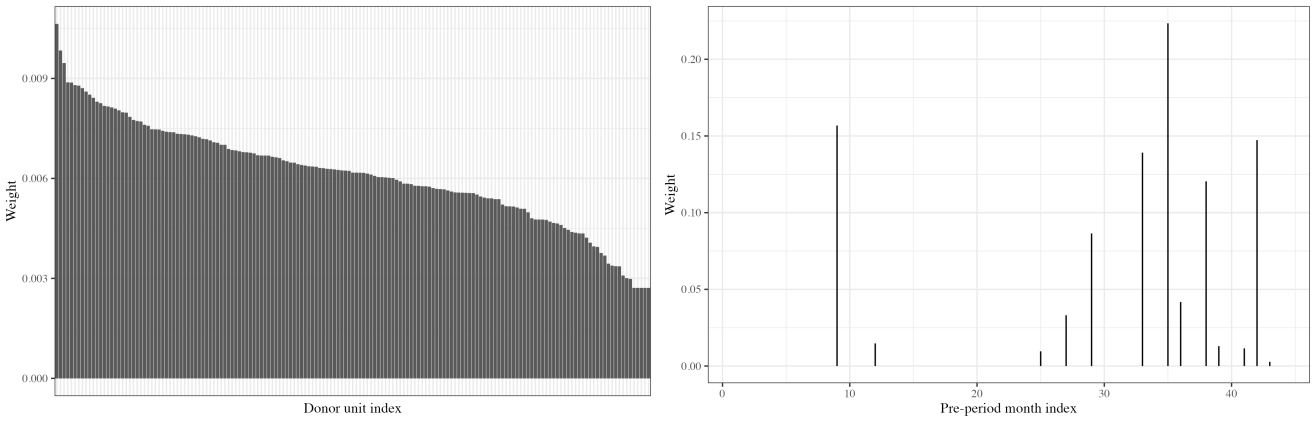
(a) Unit weights **(b)** Time weights

Figure D8: Synthetic unit and time weights – Logged average congressional experience. Left panel plots the synthetic difference-in-differences weight for each donor (control) unit, ordered from largest to smallest weight. Right panel plots the time weights for each pre-treatment month-year, ordered from earliest to latest month-year.



(a) Unit weights **(b)** Time weights

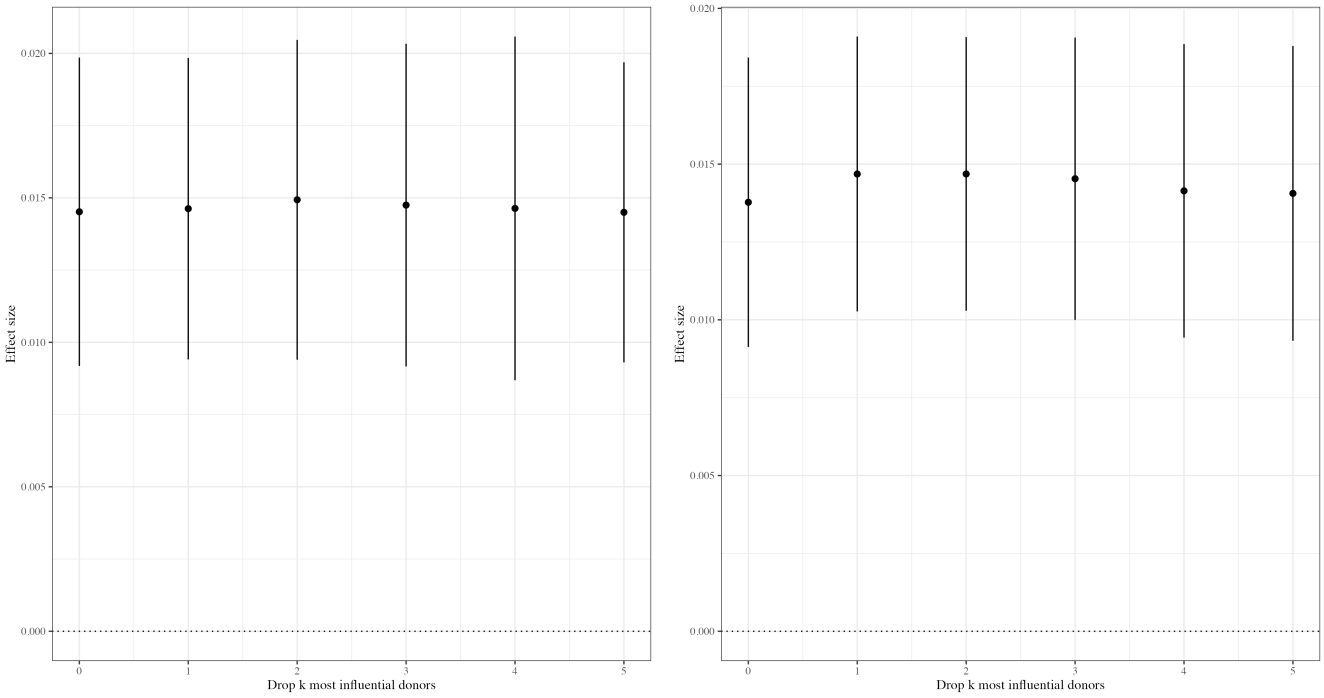
Figure D9: Synthetic unit and time weights – Share advanced degree experience. Left panel plots the synthetic difference-in-differences weight for each donor (control) unit, ordered from largest to smallest weight. Right panel plots the time weights for each pre-treatment month-year, ordered from earliest to latest month-year.



(a) Unit weights **(b)** Time weights

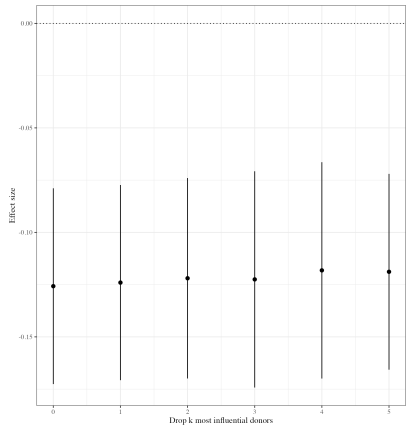
Figure D10: Synthetic unit and time weights – Share temporary staff. Left panel plots the synthetic difference-in-differences weight for each donor (control) unit, ordered from largest to smallest weight. Right panel plots the time weights for each pre-treatment month-year, ordered from earliest to latest month-year.

In Figures D11-D12, we plot estimates from leave-out analyses where we leave out up to 5 of the highest weighted donor units in the synthetic control group. Across all outcomes and across all leave-out analyses, we observe very stable point estimates, indicating that our results are not sensitive to a small number of donor units.

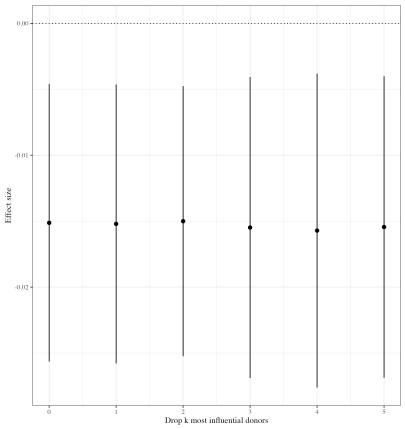


(a) Office-level separation rate **(b)** Office-level exit Congress rate

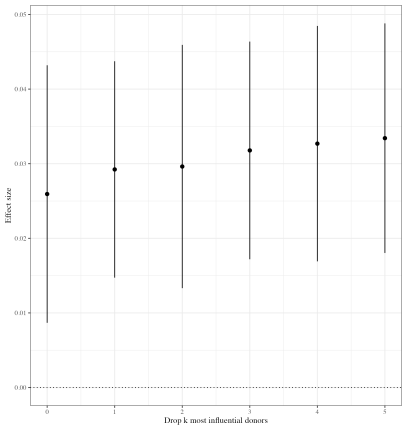
Figure D11: Leave-out robustness of synthetic estimates – Turnover outcomes. Points represent synthetic difference-in-differences estimates across datasets where the k donor control units with the highest weights are dropped, with k ranging from 0 to 5. Horizontal dotted line represents the 0 effect size line. Error bars report 95% confidence intervals from bootstrapped standard errors resampled at the office level.



(a) Logged average congressional experience



(b) Share advanced degree



(c) Share temporary staff

Figure D12: Leave-out robustness of synthetic estimates – Human capital outcomes. Points represent synthetic difference-in-differences estimates across datasets where the k donor control units with the highest weights are dropped, with k ranging from 0 to 5. Horizontal dotted line represents the 0 effect size line. Error bars report 95% confidence intervals from bootstrapped standard errors resampled at the office level.

In Table D1, we report results from a hold-out pre-fit test where train the synthetic estimator on the earliest two-thirds of the pre-period month-years and use that data to predict the paths for held out pre-period. From this analysis we report the mean gap between treated and synthetic paths in the test data along with standard errors and p-values for the significance test as to whether this gap is statistically distinguishable from zero. A low mean gap and a lack of statistical significance in this test is evidence in support of the synthetic difference-in-differences identification assumption, as it demonstrates that synthetic weights more likely generalize beyond the period used to construct the control analysis in the synthetic estimation.

As Table D1 shows, the mean gap for our turnover outcome is quite small. The p-value for Office-Level Separation Rate is 0.113, meaning we cannot reject the null hypothesis that there is no mean gap in the test data. The p-value for the other outcomes (except share temporary staff) do fall below conventional statistical significance threshhold ($\alpha < 0.05$) but we note that at least for the Office-Level Exit Congress Rate and Logged Average Congressional Experience outcomes the means gaps are still quite small.

Next, we conduct pre- and post-treatment calculations of root mean squared prediction error (RMSPE) for each outcome in the pre- and post-periods. This test is used to test for a sharp divergence in pre versus post RMSPE, which would be consistent with synthetic and treated paths that align in the pre-period then differ in the post-period (due to a the effect of January 6). Table D2 reports the pre and post RMSPE statistica and their ratio. For both turnover outcomes, Office-Level Separation Rate and Office-Level Exit Congress Rate, the synthetic difference-in-differences estimation returns extremely close pre-treatment fit, as evidenced by very small pre RMSPE. The post RMPSE for each outcome is more than twice as large as the pre RMSPE, demonstrating substantial divergence after January 6. The pre-post ratio for Logged Average Congressional Experience is even larger, with a ratio of over 7, meaning that for this outcome we see a very large increase after January 6, and a small RMSPE in the pre-period (indicative of good pre-period fit). The RMSPE ratio for Share Advanced Degree is not strong, at less than 1, indicating little if any divergence. Share Temporary Staff show strong pre versus post fit, similar to the turnover outcomes. Overall, RMSPE diagnostics strongly support the credibility of synthetic difference-in-differences estimates for turnover and congressional experience outcomes, but suggest caution for the educational attainment outcome.

Table D1: Hold-out test of synthetic pre-fit

Outcome	Mean gap (hold-out)	SE	t-stat	p-value	T_0 (train)	T_0 (test)
Office-Level Separation Rate	0.004	0.002	1.693	0.113	29	15
Office-Level Exit Congress Rate	0.005	0.002	3.466	0.004	29	15
Log Avg. Congressional Experience (mos.)	−0.023	0.010	−2.361	0.033	29	15
Share with Advanced Degree	0.070	0.003	23.205	0.000	29	15
Share Temporary Staff	0.013	0.007	1.938	0.073	29	15

Table D2: Pre- and post-treatment root mean squared prediction error for synthetic difference-in-differences

Outcome	Pre RMSPE	Post RMSPE	Post/Pre Ratio
Office-Level Separation Rate	0.010	0.023	2.423
Office-Level Exit Congress Rate	0.008	0.021	2.585
Log Avg. Congressional Experience (mos.)	0.020	0.144	7.028
Share with Advanced Degree	0.068	0.058	0.854
Share Temporary Staff	0.016	0.041	2.650

D.2 Additional Specifications

D.2.1 Synthetic difference-in-differences alternate estimation strategies

In Table D3, we present synthetic difference-in-differences estimates from alternative samples and synthetic esitmators. These include:

1. Synthetic difference-in-differences on the 2019-2022 sample (rather than 2017-2022 in the manuscript).

2. Synthetic difference-in-differences on the 2020-2022 sample.

3. Synthetic difference-in-differences using residualized outcomes wherein we predict outcomes for treated and control units using pre-period data. We include in the model dummy variables for month and year, control for which chamber of congress an office is in, whether the MC is the majority leader, interaction of party and month dummies and interaction of party and the chamber control variable, and logged average congressional experience, logged number of employees, and share with an advanced degree. When running this analysis on any outcomes included in the above list, we omit that control variable. We then create residualized outcomes that are the difference between observed outcomes and the predicted values from that model. After this residualizing process we run synthetic difference-in-differences on these new outcomes. This method is designed to test robustness to possible violations of the synthetic difference-in-differences assumptions by removing them (to the extent they can be picked up in the above model) from the outcome prior running the analysis (Doudchenko and Imbens, 2016; Ferman and Pinto, 2021). While we cannot compare the magnitude of the residualized outcomes to our main estimates, we look for consistency in sign and significance as evidence in support of the identifying assumption in our main synthetic difference-in-differences analysis.

4. Gsynth alternative synthetic control estimator from Xu (2017), with two-way (unit and time) forcing method.

5. Gsynth alternative synthetic control estimator from Xu (2017), with unit forcing method.

6. Augsynth alternative synthetic control estimator from (Ben-Michael, Feller, and Rothstein, 2021). We note that this method does not return standard errors by design and that the p-value for this estimate is in reference to the placebo distribution. As a result, this method is known to be much more conservative in terms of significance testing than methods relying on more common asymptotic sampling uncertainty estimates.

Table D3: Effect of January 6th on Democrat Offices - Alternative Synthetic Estimation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Office-Level Separation Rate							
Democrat x After Jan. 6	0.0145***	0.0109***	0.0096***	0.0055*	0.0156***	0.0162***	0.0107.
	(0.0026)	(0.0028)	(0.0029)	(0.0022)	(0.0024)	(0.0027)	-
Obs.	23,760	15,840	11,880	23,760	23,760	23,760	23,760
Office-Level Exit Congress Rate							
Democrat x After Jan. 6	0.0138***	0.0099***	0.0080**	0.0055**	0.0144***	0.0155***	0.0119
	(0.0025)	(0.0026)	(0.0026)	(0.0019)	(0.0020)	(0.0025)	-
Obs.	23,760	15,840	11,880	23,760	23,760	23,760	23,760
Log Avg. Congressional Experience (mos.)							
Democrat x After Jan. 6	-0.1257***	-0.1228***	-0.1182***	-0.1141***	-0.0832**	-0.1281***	-0.1392
	(0.0216)	(0.0249)	(0.0232)	(0.0246)	(0.0304)	(0.0198)	-
Obs.	23,760	15,840	11,880	23,760	23,760	23,760	23,760
Share with Advanced Degree							
Democrat x After Jan. 6	-0.0151**	-0.0161**	-0.0174**	-0.0127*	-0.0151*	-0.0197***	0.0019
	(0.0054)	(0.0060)	(0.0055)	(0.0059)	(0.0070)	(0.0056)	-
Obs.	23,760	15,840	11,880	23,760	23,760	23,760	23,760
Share Temporary Staff							
Democrat x After Jan. 6	0.0259**	0.0247**	0.0186*	0.0084	0.0181.	0.0311***	0.0224
	(0.0079)	(0.0078)	(0.0076)	(0.0061)	(0.0101)	(0.0083)	-
Obs.	23,760	15,840	11,880	23,760	23,760	23,760	23,760
Synth. (main)	✓						
Synth. (2019+)		✓					
Synth. (2020+)			✓				
Synth. (residualized)				✓			
Gsynth (two-way)					✓		
Gsynth (unit)						✓	
Augsynth							✓

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: Augsynth does not report standard errors by construction. P-values are from placebo distribution.

D.2.2 TWFE difference-in-differences alternate estimation strategies

Table D4: Effect of January 6th on Democrat Offices Office-Level Separation Rate – TWFE Specifications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Democrat x After Jan. 6	0.0147***	0.0146***	0.0156***	0.0109***	0.0104***	0.0164***	0.0197***	0.0146***	0.0146***	0.0144***	0.0146***
	(0.0025)	(0.0025)	(0.0024)	(0.0026)	(0.0027)	(0.0035)	(0.0030)	(0.0025)	(0.0025)	(0.0025)	(0.0025)
Senate	−0.0014	−0.0040		−0.0076.	−0.0031	−0.0291	−0.0064	−0.0040	−0.0040	−0.0046	−0.0045
	(0.0021)	(0.0089)		(0.0045)	(0.0084)	(300.5310)	(0.0102)	(0.0089)	(0.0089)	(0.0092)	(0.0087)
Seniority	−0.0004.	0.0022		0.0060**	0.0020	−1.7597	0.0015	0.0022	0.0022	0.0020	0.0021
	(0.0002)	(0.0024)		(0.0019)	(0.0022)	(1111.5314)	(0.0026)	(0.0024)	(0.0024)	(0.0024)	(0.0024)
Majority Leader	0.0119**	0.0023		−0.0001	0.0026		0.0016	0.0023	0.0023	0.0022	0.0022
	(0.0042)	(0.0048)		(0.0066)	(0.0044)		(0.0048)	(0.0048)	(0.0048)	(0.0048)	(0.0048)
Minority Leader	0.0025	−0.0077		−0.0027	−0.0072	−0.0639	−0.0069	−0.0077	−0.0077	−0.0075	−0.0079
	(0.0038)	(0.0053)		(0.0058)	(0.0061)	(2148.7199)	(0.0052)	(0.0053)	(0.0053)	(0.0052)	(0.0053)
Committee Chair	−0.0021	−0.0012		−0.0009	−0.0011	−0.0179	−0.0021	−0.0012	−0.0012	−0.0014	−0.0014
	(0.0029)	(0.0030)		(0.0034)	(0.0031)	(220.0300)	(0.0029)	(0.0030)	(0.0030)	(0.0030)	(0.0030)
Democrat x Fourier sine (harmonic 1)								0.0021*	0.0016		
								(0.0011)	(0.0010)		
Democrat x Fourier cosine (harmonic 1)								0.0074***	0.0066***		
								(0.0014)	(0.0014)		
Democrat x Fourier sine (harmonic 2)									−0.0054***		
									(0.0012)		
Democrat x Fourier cosine (harmonic 2)									0.0002		
									(0.0014)		
Lagged job postings (same-party firms)										0.0002*	
										(0.0001)	
Lagged job postings (prev. hired from office)											−0.0022***
											(0.0005)
Num.Obs.	23 760	23 760	23 760	15 840	23 760	23 760	23 760	23 760	23 760	23 760	23 760
Office FE		✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Month-Year FE	✓	✓	✓	✓	✓		✓	✓	✓	✓	✓
Month-Year x Controls FE						✓					
Party FE	✓										
Pres. Term Year x Party FE							✓				
Weights					✓						
Trimmed sample				✓							

. p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table D5: Effect of January 6th on Democrat Offices Office-Level Exit Congress Rate – TWFE Specifications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Democrat x After Jan. 6	0.0135***	0.0133***	0.0144***	0.0100***	0.0101***	0.0143***	0.0180***	0.0133***	0.0133***	0.0131***	0.0133***
	(0.0021)	(0.0021)	(0.0020)	(0.0023)	(0.0023)	(0.0031)	(0.0025)	(0.0021)	(0.0021)	(0.0021)	(0.0021)
Senate	0.0021	0.0051		0.0075*	0.0057	−0.1851	0.0028	0.0051	0.0051	0.0044	0.0045
	(0.0018)	(0.0061)		(0.0037)	(0.0061)	(212.1370)	(0.0071)	(0.0061)	(0.0061)	(0.0064)	(0.0059)
Seniority	−0.0003.	0.0027.		0.0067***	0.0026	−0.7211	0.0021	0.0027.	0.0027.	0.0025	0.0027
	(0.0002)	(0.0016)		(0.0015)	(0.0016)	(781.9563)	(0.0018)	(0.0016)	(0.0016)	(0.0017)	(0.0016)
Majority Leader	0.0072*	0.0013		−0.0025	0.0007		0.0007	0.0013	0.0013	0.0012	0.0012
	(0.0031)	(0.0034)		(0.0052)	(0.0034)		(0.0033)	(0.0034)	(0.0034)	(0.0034)	(0.0034)
Minority Leader	−0.0026	−0.0100**		−0.0052	−0.0103*	−1.2552	−0.0093*	−0.0100**	−0.0100**	−0.0098**	−0.0102**
	(0.0023)	(0.0037)		(0.0044)	(0.0047)	(1515.8946)	(0.0037)	(0.0037)	(0.0037)	(0.0037)	(0.0038)
Committee Chair	−0.0020	−0.0015		−0.0008	−0.0015	−0.0860	−0.0024	−0.0015	−0.0015	−0.0018	−0.0018
	(0.0025)	(0.0026)		(0.0028)	(0.0026)	(128.7814)	(0.0025)	(0.0026)	(0.0026)	(0.0026)	(0.0026)
Democrat x Fourier sine (harmonic 1)								0.0002	−0.0001		
								(0.0009)	(0.0009)		
Democrat x Fourier cosine (harmonic 1)								0.0052***	0.0046***		
								(0.0012)	(0.0012)		
Democrat x Fourier sine (harmonic 2)									−0.0033**		
									(0.0011)		
Democrat x Fourier cosine (harmonic 2)									0.0002		
									(0.0012)		
Lagged job postings (same-party firms)										0.0002*	
										(0.0001)	
Lagged job postings (prev. hired from office)											−0.0022***
											(0.0005)
Num.Obs.	23 760	23 760	23 760	15 840	23 760	23 760	23 760	23 760	23 760	23 760	23 760
Office FE		✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Month-Year FE	✓	✓	✓	✓	✓		✓	✓	✓	✓	✓
Month-Year x Controls FE						✓					
Party FE	✓										
Pres. Term Year x Party FE							✓				
Controls	✓	✓		✓	✓	✓	✓	✓	✓	✓	✓
Weights					✓						
Trimmed sample				✓							

. p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

D.3 2016-2017 Presidential Transition Placebo

Table D6: Effect of 2016-2017 Presidential Transition on Democrat Offices

	(1)	(2)
Office-Level Separation Rate		
Republican x After Jan. 2017	0.0029*	0.0036*
	(0.0014)	(0.0016)
Obs.	25,476	25,476
Dep. Var. Mean	0.0339	0.0339
Office-Level Exit Congress Rate		
Republican x After Jan. 2017	0.0034**	0.0022
	(0.0012)	(0.0015)
Obs.	25,476	25,476
Dep. Var. Mean	0.0240	0.0240
Log Avg. Congressional Experience (mos.)		
Republican x After Jan. 2017	-0.0249	-0.0273.
	(0.0194)	(0.0142)
Obs.	25,476	25,476
Dep. Var. Mean	4.0881	4.0881
Share with Advanced Degree		
Republican x After Jan. 2017	-0.0058	-0.0019
	(0.0061)	(0.0043)
Obs.	25,476	25,476
Dep. Var. Mean	0.1210	0.1210
Share Temporary Staff		
Republican x After Jan. 2017	-0.0010	0.0002
	(0.0035)	(0.0035)
Obs.	25,476	25,476
Dep. Var. Mean	0.0592	0.0592
TWFE	✓	
Synth.		✓

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table D7: Effect of Jan 2017 on Republican Offices – TWFE Specifications

	Office-Level Separation Rate	Office-Level Exit Congress Rate	Log Avg. Congressional Experience (mos.)	Share with Advanced Degree	Share Temporary Staff
Democrat x After Jan. 6	0.0029*	0.0034**	−0.0249	−0.0058	−0.0010
	(0.0014)	(0.0012)	(0.0194)	(0.0061)	(0.0035)
Senate	−0.0055	−0.0035	−0.1206	−0.0711*	0.0329
	(0.0153)	(0.0113)	(0.1071)	(0.0328)	(0.0347)
Seniority	−0.0014	−0.0026	0.0176	−0.0138**	0.0057
	(0.0051)	(0.0041)	(0.0234)	(0.0047)	(0.0092)
Majority Leader	−0.0004	0.0008	0.0233	0.0024	−0.0031
	(0.0023)	(0.0017)	(0.0300)	(0.0116)	(0.0057)
Minority Leader	0.0011	0.0021	−0.0100	−0.0054	0.0074
	(0.0023)	(0.0020)	(0.0239)	(0.0078)	(0.0059)
Committee Chair	0.0021	0.0022.	−0.0322	−0.0066	0.0026
	(0.0019)	(0.0013)	(0.0272)	(0.0069)	(0.0052)
Num.Obs.	25 476	25 476	25 476	25 476	25 476
RMSE	0.04	0.04	0.15	0.05	0.05
Office FE	✓	✓	✓	✓	✓
Month-Year FE$_{c}ode$	✓	✓	✓	✓	✓

. p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

D.4 Election Deniers

In this section, we consider the position that legislators took in certifying the 2020 presidential election. Compared to our main analysis that looks at the partisan differences in response to January 6, here we only consider those that voted against the certification of the election as the *treated* group, and all remaining legislators — those who voted to certify — constitute the *control* group. In this case, the *control* group includes some Republicans.

Members in the treated group tended, on average, to have more ideologically extreme positions and publicly contested the 2020 result. As a result, their offices likely attracted and retained personnel whose preferences were aligned with those positions. By contrast, offices of members who voted to certify were more likely to face post-January 6 pressures that could amplify stress or perceived mismatch between role demands and personal safety or values. Accordingly, we anticipate and below document that staff in members' offices that voted against certification were less likely to separate in the aftermath than staff in objection members' offices.

Table D8: Effect of January 6th on Election-Denying Offices' Turnover

	(1)	(2)	(3)	(4)
Office-Level Separation Rate				
Election-Denier x After Jan. 6, 2021	-0.0102***	-0.0096**	-0.0018	0.0002
	(0.0027)	(0.0030)	(0.0033)	(0.0039)
Obs.	23,760	23,760	10,758	10,758
Dep. Var. Mean	0.0552	0.0552	0.0535	0.0535
Office-Level Exit Congress Rate				
Election-Denier x After Jan. 6, 2021	-0.0103***	-0.0102***	-0.0030	-0.0025
	(0.0024)	(0.0024)	(0.0027)	(0.0030)
Obs.	23,760	23,760	10,758	10,758
Dep. Var. Mean	0.0412	0.0412	0.0391	0.0391
Sample	Full	Full	Republicans	Republicans
TWFE	✓		✓	
Synth.		✓		✓

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table D9: Effect of Jan 6, 2021 on Election-Denying Offices – TWFE Specifications

	Full sample		Republican sample	
	Office-Level Separation Rate	Office-Level Exit Congress Rate	Office-Level Separation Rate	Office-Level Exit Congress Rate
Election Denier x After Jan. 6	−0.0102***	−0.0103***	−0.0018	−0.0030
	(0.0027)	(0.0024)	(0.0033)	(0.0027)
Senate	−0.0023	0.0068	0.0039	0.0106
	(0.0072)	(0.0048)	(0.0171)	(0.0092)
Seniority	0.0025	0.0031**	0.0008	0.0019
	(0.0018)	(0.0011)	(0.0022)	(0.0012)
Majority Leader	0.0053	0.0039	0.0063	0.0039
	(0.0049)	(0.0034)	(0.0078)	(0.0051)
Minority Leader	−0.0082	−0.0104**	0.0006	−0.0034
	(0.0053)	(0.0037)	(0.0042)	(0.0024)
Committee Chair	0.0032	0.0024	0.0055	0.0035
	(0.0029)	(0.0025)	(0.0038)	(0.0032)
Num.Obs.	23 760	23 760	10 758	10 758
Office FE	✓	✓	✓	✓
Month-Year FE	✓	✓	✓	✓

. p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

D.5 Productivity

D.5.1 Data

Constituency Communications Given that turnover erodes organizational memory and capacity, we expect offices with higher exit rates to exhibit weaker performance. In particular turnover likely weakens on-boarding, socialization, and the transmission of office norms to new hires (Krause, 1999; DiBenigno, 2022; Gailmard and Gailmard, 2025). However, evaluating staff performance poses substantial challenges, as most observable outcomes in congressional offices reflect joint production by members and their teams rather than staff-specific contributions. We therefore focus on a staff-dominant activity — constituency outreach — operationalized using the content of mass emails from *DCInbox*. These communications are typically drafted by staff with limited direct involvement from members, providing a cleaner window into staff performance. Examining such constituent outreach is useful for understanding impacts on productivity, because any effects should be most visible in domains with limited MC oversight.

As a measure of constituency outreach, we use data from *DCInbox*. The data compiles email newsletters sent by members of Congress from January 2010 to December 2022. Full-text data for March 2021 are missing due to a system error that failed to collect email content. We measure an office's constituency contact using the total number of emails sent by an office in a month. We further measure the extent to which such constituent outreach contains political language. We use the licensed off-the-self LIWC dictionary (Boyd et al., 2022), which contains a comprehensive list of political words.[28]

These two measure of constituency communication capture two important theoretical concepts. First, the total number of emails sent informs as to whether staffers spend more or less time on constituent outreach (as opposed to legislative activities), in response to the disruption to their workplace following January 6, 2021. The extent to which this outreach contains political language, furthermore, measures whether constituent outreach becomes more overtly political following this highly-politicized example of workplace political violence.

Bills We use bill-level data from the *ProPublica* Congress API to construct monthly office-level measures of legislative activity. For each bill, we recover the sponsor, the set of original co-sponsors listed at introduction, and the date of subsequent co-sponsorships. Because original co-sponsorship is understood to reflect participation in bill's development (Curry and Roberts, 2023, 2025), we report three outcomes: (1) number of sponsors, (2) the number of sponsors and cosponsors at introduction, and (3) the number of subsequent co-sponsorships.

D.5.2 Results

Table D10 reports the synthetic difference-in-difference estimates for these outcomes. We find no effect of January 6 on the logged number of emails sent each month by Democratic offices compared to Republican offices, with estimates not statistically distinguishable from zero at conventional thresholds. We do find that Democratic offices decreased the share of political words in these emails relative to Republican offices. These effect sizes are substantively large: on a monthly basis, Democratic offices decreased the share of political words in their constituent outreach emails by 32.25 percentage points compared to Republican offices. This decreases signals a shift toward less politicized communication post-January 6. For bills, we see mixed results when only looking at

[28]Because LIWC dictionaries are proprietary, we do not reproduce the full politic word list. For illustration, the category includes terms such as "legislative," "impeach," "immigrant," "under-represented," and "sanctions."

primary sponsorship, consistent post–January 6 decline for the combined count of primary sponsorships plus original co-sponsorships, and no systematic change in overall co-sponsorships after introduction.

Table D10: Effect of January 6th on Democrat Offices' Productivity

	(1)	(2)
Log Number of Emails Sent		
Democrat x After Jan. 6	-0.0507	-0.0730
	(0.0507)	(0.0505)
Obs.	23,760	23,760
Dep. Var. Mean	0.8569	0.8569
Share Political Words in Emails		
Democrat x After Jan. 6	-0.3420**	-0.3225**
	(0.1032)	(0.1162)
Obs.	23,760	23,760
Dep. Var. Mean	1.9817	1.9817
Log Number of Bills Introduced (Primary)		
Democrat x After Jan. 6	-0.0237	-0.0699**
	(0.0192)	(0.0269)
Obs.	23,760	23,760
Dep. Var. Mean	0.5927	0.5927
Log Number of Bills Introduced (Primary and Intro. Cospon.)		
Democrat x After Jan. 6	-0.4117***	-0.3998***
	(0.0270)	(0.0537)
Obs.	23,760	23,760
Dep. Var. Mean	1.8730	1.8730
Log Number of Bills Cosponsored		
Democrat x After Jan. 6	-0.0194	-0.0464
	(0.0276)	(0.0336)
Obs.	23,760	23,760
Dep. Var. Mean	1.9347	1.9347
TWFE	✓	
Synth.		✓

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

D.6 Heterogeneity: Ideology

A natural source of heterogeneity we can explore is member ideology. In Online Appendix D.6, we examine heterogeneity across the ideological spectrum using both DW-NOMINATE and CFscores from Bonica (2023). We find no differential effects.

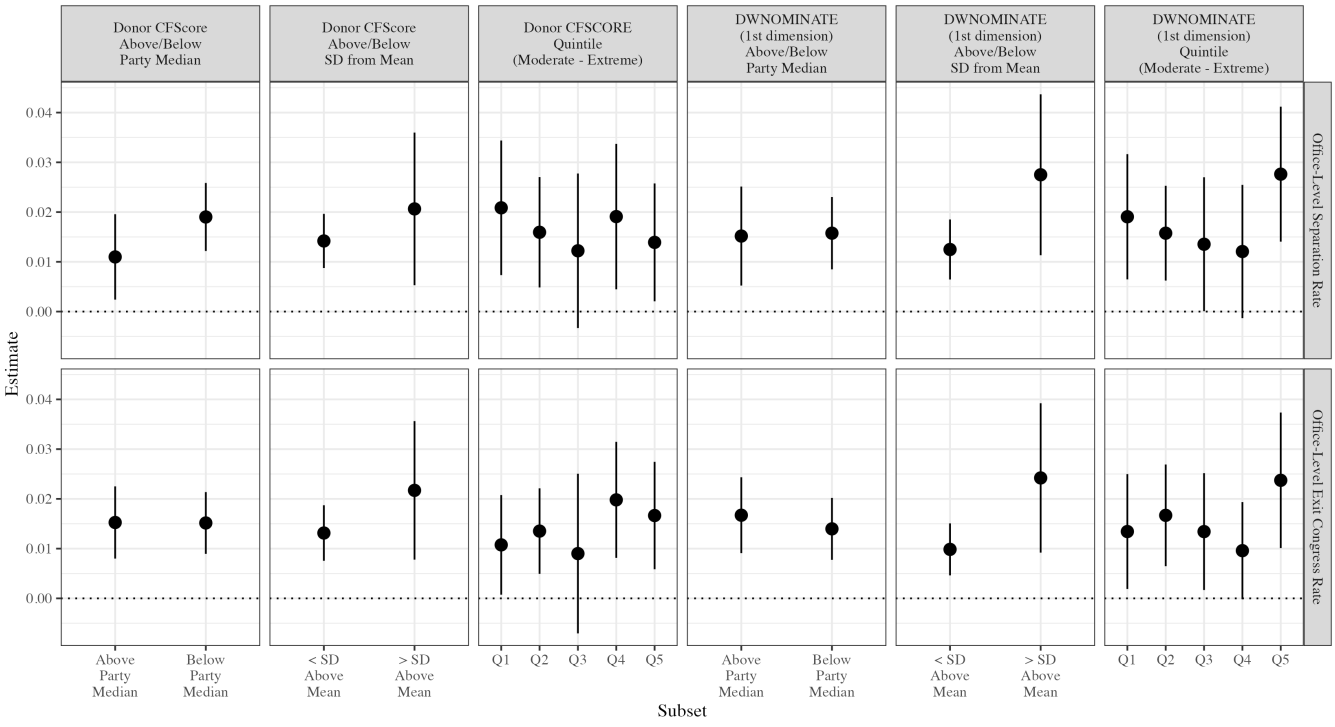


Figure D13: Effect of Jan. 6 on Turnover Results by Ideology. Points report estimates from synthetic difference-in-differences estimation within different ideological subsets. Error bars report 95% confidence intervals from bootstrapped standard errors computed by resampling at the office-level.

Table D11: Effect of January 6th on Democratic Offices' Turnover – Heterogeneity by Ideology (CFScore)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Office-Level Separation Rate									
Democrat x After Jan. 6, 2021	0.0190***	0.0110*	0.0142***	0.0206**	0.0209**	0.0159**	0.0122	0.0191*	0.0139*
	(0.0035)	(0.0044)	(0.0028)	(0.0078)	(0.0069)	(0.0057)	(0.0079)	(0.0075)	(0.0060)
Obs.	12,210	11,550	20,592	3,036	4,356	5,148	4,488	4,884	4,092
Dep. Var. Mean	0.0535	0.0569	0.0549	0.0574	0.0520	0.0539	0.0556	0.0581	0.0565
Office-Level Exit Congress Rate									
Democrat x After Jan. 6, 2021	0.0152***	0.0153***	0.0131***	0.0217**	0.0108*	0.0135**	0.0090	0.0198***	0.0166**
	(0.0032)	(0.0037)	(0.0028)	(0.0071)	(0.0051)	(0.0044)	(0.0082)	(0.0059)	(0.0055)
Obs.	12,210	11,550	20,592	3,036	4,356	5,148	4,488	4,884	4,092
Dep. Var. Mean	0.0403	0.0421	0.0409	0.0433	0.0392	0.0404	0.0417	0.0426	0.0422
hline Sample	Below Party Median	Above Party Median	¡ SD Above Mean	¿ SD Above Mean	Q1	Q2	Q3	Q4	Q5
Synth.	✓	✓	✓	✓	✓	✓	✓	✓	✓

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table D12: Effect of January 6th on Democratic Offices' Turnover – Heterogeneity by Ideology (DWNOMINATE 1st Dimension)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Office-Level Separation Rate									
Democrat x After Jan. 6, 2021	0.0158***	0.0152**	0.0125***	0.0275***	0.0191**	0.0158**	0.0135*	0.0121.	0.0276***
	(0.0037)	(0.0051)	(0.0031)	(0.0083)	(0.0064)	(0.0049)	(0.0069)	(0.0068)	(0.0069)
Obs.	11,880	11,814	20,328	3,432	4,026	4,554	4,686	4,488	4,818
Dep. Var. Mean	0.0525	0.0579	0.0536	0.0642	0.0529	0.0527	0.0542	0.0554	0.0609
Office-Level Exit Congress Rate									
Democrat x After Jan. 6, 2021	0.0140***	0.0167***	0.0098***	0.0242**	0.0134*	0.0167**	0.0134*	0.0096.	0.0237***
	(0.0032)	(0.0039)	(0.0027)	(0.0077)	(0.0059)	(0.0052)	(0.0060)	(0.0050)	(0.0069)
Obs.	11,880	11,814	20,328	3,432	4,026	4,554	4,686	4,488	4,818
Dep. Var. Mean	0.0394	0.0430	0.0401	0.0474	0.0402	0.0392	0.0403	0.0408	0.0453
hline Sample	Below Party Median	Above Party Median	¡ SD Above Mean	¿ SD Above Mean	Q1	Q2	Q3	Q4	Q5
Synth.	✓	✓	✓	✓	✓	✓	✓	✓	✓

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

D.7 Heterogeneity: Effectiveness

We also examine heterogeneity by members' office effectiveness. If low effectiveness reflects organizational dysfunction and latent dissatisfaction, staff in these offices may be especially responsive to events like January 6. Consistent with this expectation, Online Appendix D.7 shows that low-performing offices experienced higher post-January 6 turnover than their more effective counterparts.

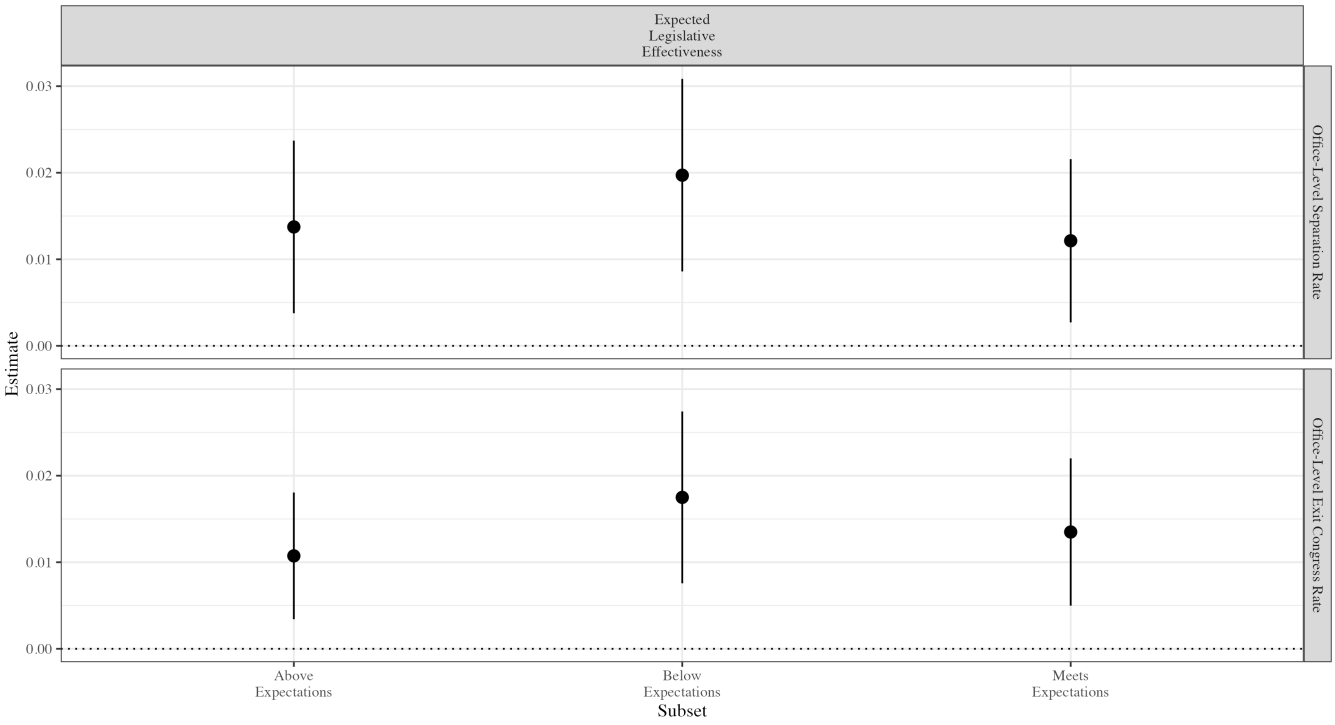


Figure D14: Effect of Jan. 6 on Turnover Results by Legislative Effectiveness. Points report estimates from synthetic difference-in-differences estimation within subsets defined office legislative effectiveness. Error bars report 95% confidence intervals from bootstrapped standard errors computed by resampling at the office-level.

Table D13: Effect of January 6th on Democratic Offices' Turnover – Heterogeneity by Legislative Effectiveness

	(1)	(2)	(3)
Office-Level Separation Rate			
Democrat x After Jan. 6, 2021	0.0197***	0.0121*	0.0137**
	(0.0057)	(0.0048)	(0.0051)
Obs.	6,930	11,418	5,016
Dep. Var. Mean	0.0576	0.0562	0.0509
Office-Level Exit Congress Rate			
Democrat x After Jan. 6, 2021	0.0175***	0.0135**	0.0107**
	(0.0051)	(0.0043)	(0.0037)
Obs.	6,930	11,418	5,016
Dep. Var. Mean	0.0431	0.0422	0.0373
Sample	Below Expectations	Meets Expectations	Above Expectations
Synth.	✓	✓	✓

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

D.8 Heterogeneity: Chamber

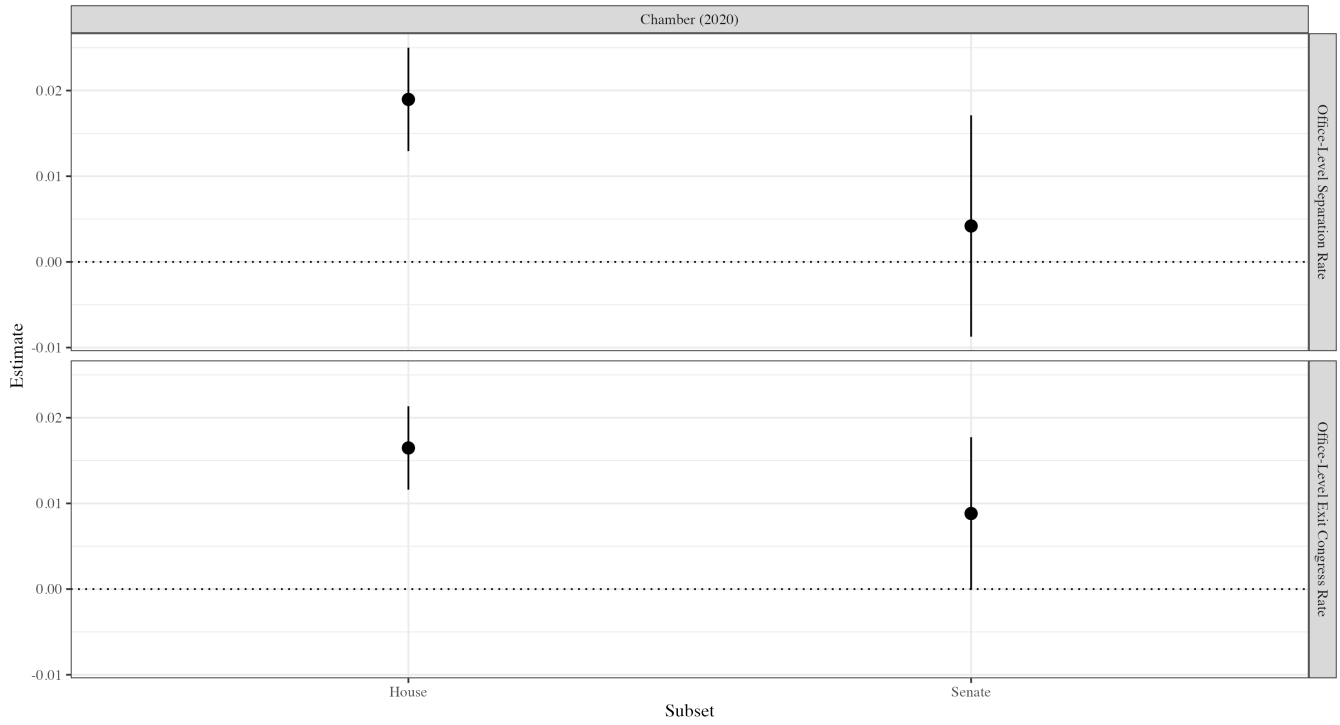


Figure D15: Effect of Jan. 6 on Turnover Results by Congressional Chamber. Points report estimates from synthetic difference-in-differences estimation within subsets defined by Congressional chamber. Error bars report 95% confidence intervals from bootstrapped standard errors computed by resampling at the office-level.

Table D14: Effect of January 6th on Democratic Offices' Turnover – Heterogeneity by Congressional Chamber

	(1)	(2)
Office-Level Separation Rate		
Democrat x After Jan. 6, 2021	0.0190***	0.0042
	(0.0031)	(0.0066)
Obs.	18,348	5,412
Dep. Var. Mean	0.0554	0.0544
Office-Level Exit Congress Rate		
Democrat x After Jan. 6, 2021	0.0165***	0.0088.
	(0.0025)	(0.0045)
Obs.	18,348	5,412
Dep. Var. Mean	0.0408	0.0426
Sample	House	Senate
Synth.	✓	✓

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

D.9 Heterogeneity: Seniority

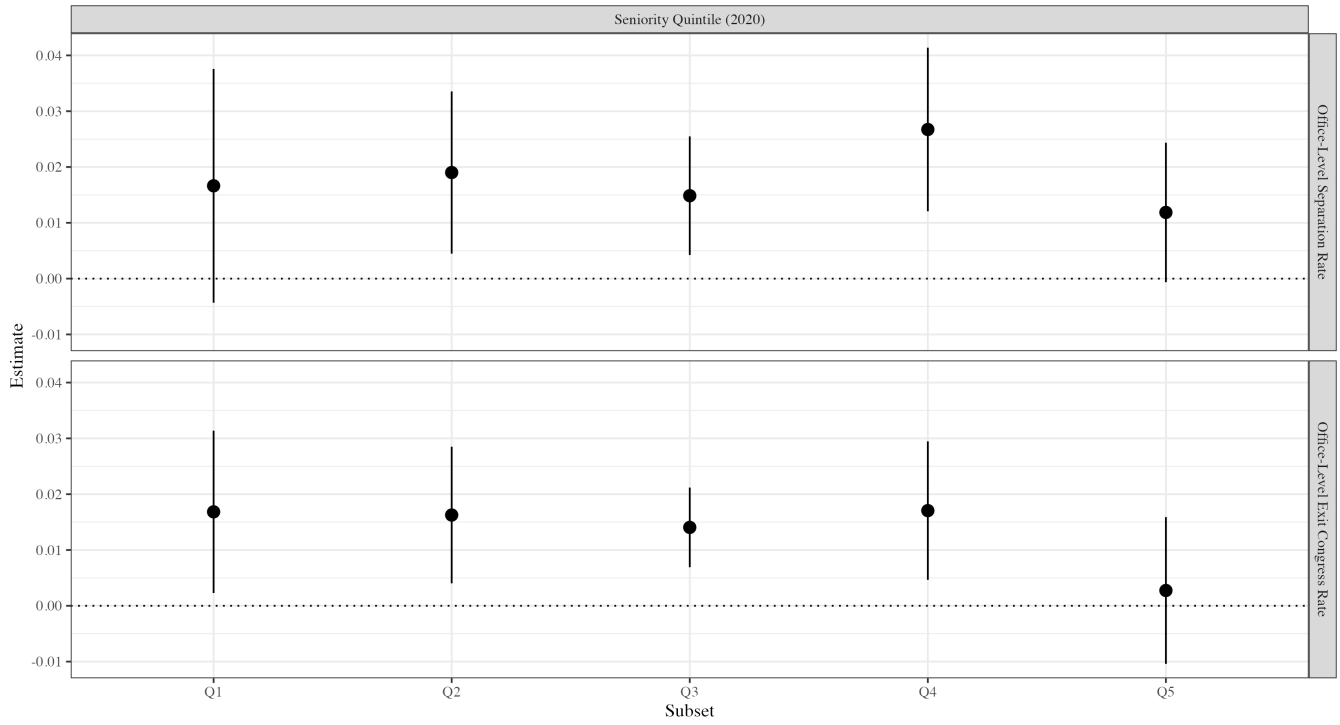


Figure D16: Effect of Jan. 6 on Turnover Results by Seniority. Points report estimates from synthetic difference-in-differences estimation within subsets defined by quintiles of how long the MOS for a given office has been in Congress (as of 2020). Error bars report 95% confidence intervals from bootstrapped standard errors computed by resampling at the office-level.

Table D15: Effect of January 6th on Democratic Offices' Turnover – Heterogeneity by Seniority

	(1)	(2)	(3)	(4)	(5)
Office-Level Separation Rate					
Democrat x After Jan. 6, 2021	0.0166	0.0190*	0.0149**	0.0267***	0.0119.
	(0.0107)	(0.0074)	(0.0054)	(0.0075)	(0.0064)
Obs.	3,564	3,234	6,864	4,422	5,676
Dep. Var. Mean	0.0559	0.0544	0.0571	0.0550	0.0529
Office-Level Exit Congress Rate					
Democrat x After Jan. 6, 2021	0.0168*	0.0163**	0.0140***	0.0171**	0.0027
	(0.0074)	(0.0062)	(0.0036)	(0.0063)	(0.0067)
Obs.	3,564	3,234	6,864	4,422	5,676
Dep. Var. Mean	0.0411	0.0404	0.0429	0.0412	0.0396
Sample	Q1	Q2	Q3	Q4	Q5
Synth.	✓	✓	✓	✓	✓

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

D.10 Seniority

Table D16: Effect of January 6th on Democrat Offices' Staff Turnover by Seniority or Job Title

	(1)	(2)
Log Number Junior Leavers		
Democrat x After Jan. 6	0.0071	0.0064
	(0.0062)	(0.0078)
Obs.	23,760	23,760
Dep. Var. Mean	0.0630	0.0630
Log Number Junior Leavers (Grad Degree)		
Democrat x After Jan. 6	0.0005	-0.0047.
	(0.0021)	(0.0026)
Obs.	23,760	23,760
Dep. Var. Mean	0.0088	0.0088
Log Number Senior Leavers (Experience-Basis)		
Democrat x After Jan. 6	0.0046	0.0047
	(0.0042)	(0.0050)
Obs.	23,760	23,760
Dep. Var. Mean	0.0252	0.0252
Log Number Senior Leavers (Titles-Basis)		
Democrat x After Jan. 6	0.0202***	-0.0017
	(0.0045)	(0.0068)
Obs.	23,760	23,760
Dep. Var. Mean	0.0266	0.0266
TWFE	✓	
Synth.		✓

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

D.11 Occupation type

Table D17: Effect of January 6th on Democrat Offices' Turnover by Occupation Type

	(1)	(2)
Log Number Admin. Staff Leavers		
Democrat x After Jan. 6	0.0069	0.0107.
	(0.0061)	(0.0061)
Obs.	23,760	23,760
Dep. Var. Mean	0.0719	0.0719
Log Number Comms. Staff Leavers		
Democrat x After Jan. 6	0.0070	0.0077
	(0.0053)	(0.0069)
Obs.	23,760	23,760
Dep. Var. Mean	0.0434	0.0434
Log Number Legislative Staff Leavers		
Democrat x After Jan. 6	0.0234**	0.0108
	(0.0075)	(0.0086)
Obs.	23,760	23,760
Dep. Var. Mean	0.1166	0.1166
TWFE	✓	
Synth.		✓

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table D18: Effect of January 6th on Democrat Offices' Share of Staffers

	(1)	(2)
Share Administrative Staff		
Democrat x After Jan. 6	-0.0013	0.0015
	(0.0056)	(0.0054)
Obs.	23,760	23,760
Dep. Var. Mean	0.1683	0.1683
Share Interns		
Democrat x After Jan. 6	0.0384***	0.0265**
	(0.0075)	(0.0083)
Obs.	23,760	23,760
Dep. Var. Mean	0.1180	0.1180
Share Legislative Staff		
Democrat x After Jan. 6	-0.0144*	-0.0175**
	(0.0066)	(0.0063)
Obs.	23,760	23,760
Dep. Var. Mean	0.2538	0.2538
TWFE	✓	
Synth.		✓

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

D.12 Employee counts and alternative turnover metrics

Table D19: Effect of January 6th on Democrat Offices' Number of Employees

	(1)	(2)
Log Office Size		
Democrat x After Jan. 6	0.0380*	-0.0088
	(0.0162)	(0.0166)
Obs.	23,760	23,760
Dep. Var. Mean	2.9943	2.9943
Log Office Size (No Temps)		
Democrat x After Jan. 6	-0.0095	-0.0411**
	(0.0174)	(0.0152)
Obs.	23,760	23,760
Dep. Var. Mean	2.8694	2.8694
Log Number of Leavers		
Democrat x After Jan. 6	0.1042***	0.0854***
	(0.0181)	(0.0201)
Obs.	23,760	23,760
Dep. Var. Mean	0.5745	0.5745
Office-Level Separation Rate (No Temps)		
Democrat x After Jan. 6	0.0086***	0.0062***
	(0.0017)	(0.0019)
Obs.	23,755	23,628
Dep. Var. Mean	0.0279	0.0279
TWFE	✓	
Synth.		✓

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

D.13 Full TWFE regression tables – other outcomes

Table D20: Effect of January 6th 2021 on Democratic Offices – Other Outcomes – TWFE Specifications

	Office-Level Separation Rate (No Temps)	Log Number of Leavers	Log Number Senior Leavers (Experience-Basis)	Log Number Senior Leavers (Titles-Basis)	Log Number Junior Leavers	Log Number Junior Leavers (Grad Degree)	Log Number Legislative Staff Leavers	Log Number Admin. Staff Leavers	Log Number Comms. Staff Leavers
Democrat x After Jan. 6	0.0086***	0.1042***	0.0046	0.0202***	0.0071	0.0005	0.0234**	0.0069	0.0070
	(0.0017)	(0.0181)	(0.0042)	(0.0045)	(0.0062)	(0.0021)	(0.0075)	(0.0061)	(0.0053)
Senate	−0.0028	0.3375**	0.0168	−0.0103	0.0436	0.0225	0.0756*	0.0367	0.0067
	(0.0046)	(0.1142)	(0.0203)	(0.0116)	(0.0386)	(0.0148)	(0.0335)	(0.0392)	(0.0324)
Seniority	0.0001	0.0444	0.0039	0.0024	−0.0001	0.0017	−0.0058	−0.0025	0.0055
	(0.0007)	(0.0425)	(0.0038)	(0.0026)	(0.0050)	(0.0024)	(0.0053)	(0.0058)	(0.0048)
Majority Leader	−0.0020	0.0405	0.0032	−0.0008	0.0270.	0.0057	0.0056	−0.0003	−0.0026
	(0.0028)	(0.0357)	(0.0061)	(0.0090)	(0.0144)	(0.0047)	(0.0175)	(0.0095)	(0.0077)
Minority Leader	−0.0032	−0.0494	−0.0081	−0.0016	0.0020	0.0007	−0.0052	−0.0111	0.0044
	(0.0024)	(0.0413)	(0.0085)	(0.0086)	(0.0138)	(0.0042)	(0.0140)	(0.0114)	(0.0094)
Committee Chair	−0.0034*	−0.0033	−0.0026	−0.0031	−0.0047	−0.0004	−0.0314**	−0.0013	0.0049
	(0.0015)	(0.0273)	(0.0063)	(0.0049)	(0.0085)	(0.0028)	(0.0105)	(0.0090)	(0.0073)
Num.Obs.	23 755	23 760	23 760	23 760	23 760	23 760	23 760	23 760	23 760
Office FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Month-Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓

. p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table D21: Effect of January 6th 2021 on Democratic Offices – Other Outcomes – TWFE Specifications

	Log Office Size	Log Number of New Hires	Log Office Size (No Temps)	Log Number of New Hires (No Temps)	Log Number of New Temp Hires	Log New Hires with Advanced Degree	Share Interns	Share Legislative Staff
Democrat x After Jan. 6	0.0380*	0.0695***	−0.0095	0.0252*	0.0499**	0.0007	0.0384***	−0.0144*
	(0.0162)	(0.0166)	(0.0174)	(0.0100)	(0.0164)	(0.0048)	(0.0075)	(0.0066)
Senate	0.6999**	0.2732	0.7552**	0.1612*	0.1584	0.0958.	−0.0517	0.0737
	(0.2451)	(0.1670)	(0.2336)	(0.0769)	(0.1275)	(0.0499)	(0.0361)	(0.0656)
Seniority	0.0445	0.0108	0.0422	−0.0072	0.0223	0.0103.	0.0019	−0.0127
	(0.0475)	(0.0376)	(0.0399)	(0.0109)	(0.0379)	(0.0061)	(0.0101)	(0.0183)
Majority Leader	0.0577.	0.0531	0.0558.	0.0159	0.0441	0.0024	0.0036	−0.0085
	(0.0307)	(0.0360)	(0.0320)	(0.0229)	(0.0393)	(0.0090)	(0.0167)	(0.0123)
Minority Leader	0.0377	−0.0434	0.0438	−0.0095	−0.0420	0.0092	−0.0032	−0.0108
	(0.0325)	(0.0420)	(0.0347)	(0.0247)	(0.0461)	(0.0100)	(0.0178)	(0.0119)
Committee Chair	−0.0154	0.0246	−0.0210	0.0055	0.0233	0.0062	0.0043	−0.0247***
	(0.0210)	(0.0234)	(0.0193)	(0.0142)	(0.0229)	(0.0068)	(0.0091)	(0.0074)
Num.Obs.	23 760	23 760	23 760	23 760	23 760	23 760	23 760	23 760
Office FE	✓	✓	✓	✓	✓	✓	✓	✓
Month-Year FE	✓	✓	✓	✓	✓	✓	✓	✓

. p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table D22: Effect of January 6th 2021 on Democratic Offices – Other Outcomes – TWFE Specifications

	Share Administrative Staff	Number of Emails Sent	Log Number of Emails Sent	Share Political Words in Emails	Log Number of Bills Introduced (Primary)	Log Number of Bills Introduced (Primary and Intro. Cospon.)	Log Number of Bills Cosponsored
Democrat x After Jan. 6	−0.0013	−0.2313	−0.0507	−0.3420**	−0.0237	−0.4117***	−0.0194
	(0.0056)	(0.2534)	(0.0507)	(0.1032)	(0.0192)	(0.0270)	(0.0276)
Senate	−0.0332	−1.1619	−0.2510	1.6212.	0.4914*	0.5280***	−0.5374***
	(0.0330)	(1.1557)	(0.4203)	(0.9566)	(0.2090)	(0.1442)	(0.0399)
Seniority	−0.0082*	−0.1816	−0.0575	0.1042	0.0142	−0.0393	−0.0617***
	(0.0037)	(0.1377)	(0.0483)	(0.1055)	(0.0317)	(0.0369)	(0.0141)
Majority Leader	−0.0055	0.4355	0.2704*	0.7725**	0.0370	−0.0616	−0.0634
	(0.0110)	(0.3919)	(0.1061)	(0.2463)	(0.0386)	(0.0624)	(0.0601)
Minority Leader	−0.0168.	0.2437	0.1150	0.3926	−0.0118	0.0256	0.0276
	(0.0090)	(0.3129)	(0.0878)	(0.2418)	(0.0459)	(0.0599)	(0.0603)
Committee Chair	−0.0019	−0.0211	0.0128	0.1903	0.0548*	−0.0355	−0.0362
	(0.0067)	(0.2048)	(0.0613)	(0.1386)	(0.0223)	(0.0310)	(0.0282)
Num.Obs.	23 760	23 760	23 760	23 760	23 760	23 760	23 760
Office FE	✓	✓	✓	✓	✓	✓	✓
Month-Year FE	✓	✓	✓	✓	✓	✓	✓

. p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

E COVID-19 Analysis

Table E1: Effect of March 1, 2020 COVID-19 Outbreak on Democrat Offices

	(1)	(2)
Office-Level Separation Rate		
Democrat x After Jan. 6	0.0054*	-0.0001
	(0.0023)	(0.0026)
Obs.	15,480	15,480
Dep. Var. Mean	0.0552	0.0449
Office-Level Exit Congress Rate		
Democrat x After Jan. 6	0.0059**	0.0038
	(0.0021)	(0.0027)
Obs.	15,480	15,480
Dep. Var. Mean	0.0412	0.0328
Log Avg. Congressional Experience (mos.)		
Democrat x After Jan. 6	-0.0257	0.0330*
	(0.0194)	(0.0154)
Obs.	15,480	15,480
Dep. Var. Mean	3.9611	3.9796
Share with Advanced Degree		
Democrat x After Jan. 6	0.0126*	0.0105**
	(0.0064)	(0.0037)
Obs.	15,480	15,480
Dep. Var. Mean	0.1174	0.1237
Share Temporary Staff		
Democrat x After Jan. 6	0.0129.	-0.0072
	(0.0078)	(0.0095)
Obs.	15,480	15,480
Dep. Var. Mean	0.1170	0.0913
TWFE	✓	
Synth.		✓

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table E2: Effect of March COVID-19 Outbreak on Democratic Offices – TWFE Specifications

	Office-Level Separation Rate	Office-Level Exit Congress Rate	Log Avg. Congressional Experience (mos.)	Share with Advanced Degree	Share Temporary Staff
Democrat x After Jan. 6	0.0054*	0.0126*	−0.0257	0.0129.	0.0059**
	(0.0023)	(0.0064)	(0.0194)	(0.0078)	(0.0021)
Senate	0.0044	0.0281**	0.2146*	−0.0739**	0.0086
	(0.0086)	(0.0090)	(0.1087)	(0.0253)	(0.0078)
Seniority	0.0019	−0.0004	0.1192***	−0.0058**	0.0022.
	(0.0013)	(0.0012)	(0.0153)	(0.0022)	(0.0013)
Majority Leader	−0.0022	0.0137	−0.0234	−0.0080	−0.0009
	(0.0044)	(0.0132)	(0.0613)	(0.0233)	(0.0039)
Minority Leader	−0.0129*	−0.0032	0.1658.	−0.0108	−0.0137**
	(0.0055)	(0.0184)	(0.0861)	(0.0209)	(0.0043)
Committee Chair	−0.0023	0.0032	−0.0600.	0.0032	−0.0021
	(0.0036)	(0.0156)	(0.0308)	(0.0091)	(0.0033)
Num.Obs.	15 480	15 480	15 480	15 480	15 480
Office FE	✓	✓	✓	✓	✓
Month-Year FE	✓	✓	✓	✓	✓

. $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

F Assortative Hiring

F.1 Data Descriptive

In Table F1, we present sample descriptives on assortative hiring. Members of different racio-ethnic groups, genders, and social backgrounds (Ivy league alumni status) tend to employ more individuals of their group. Assortative hiring appears more pronounced across groups for Democrat offices, whereas it is concentrated among MCs of color (Black and Latino) for Republicans. Our data vendor, LegiStorm, manually classifies gender and race based on their professional and social media profiles. For unclassified staffers, we impute these attributes from their names using the R packages `rethnicity` and `gender` (Mullen, Blevins, and Schmidt, 2015; Xie, 2022). For these staff we rely on codings with posterior probabilities of at least 80 percent.

Table F1: Office-month staff composition by member characteristics

	Group mean (SD)	Non-Group mean (SD)	Diff (SE)
Democrat offices			
Member race (Black)			
Share Black staff	0.455 (0.183)	0.081 (0.084)	0.375 (0.026)***
Share Latino staff	0.074 (0.101)	0.121 (0.144)	-0.047 (0.018)**
Share White staff	0.299 (0.156)	0.620 (0.171)	-0.322 (0.026)***
Member race (Latino)			
Share Black staff	0.033 (0.052)	0.168 (0.189)	-0.134 (0.016)***
Share Latino staff	0.378 (0.141)	0.078 (0.093)	0.300 (0.026)***
Share White staff	0.405 (0.150)	0.578 (0.209)	-0.173 (0.031)***
Member gender (Female)			
Share Female staff	0.567 (0.127)	0.540 (0.125)	0.027 (0.015)+
Member education (Ivy)			
Share Ivy staff	0.047 (0.056)	0.029 (0.045)	0.018 (0.008)*
Republican offices			
Member race (Black)			
Share Black staff	0.238 (0.044)	0.025 (0.044)	0.214 (0.003)***
Share Latino staff	0.041 (0.030)	0.024 (0.062)	0.017 (0.004)***
Share White staff	0.555 (0.058)	0.822 (0.116)	-0.267 (0.008)***
Member race (Latino)			
Share Black staff	0.019 (0.035)	0.026 (0.047)	-0.007 (0.011)
Share Latino staff	0.264 (0.232)	0.020 (0.042)	0.244 (0.126)+
Share White staff	0.550 (0.186)	0.825 (0.109)	-0.276 (0.101)**
Member gender (Female)			
Share Female staff	0.519 (0.129)	0.483 (0.127)	0.036 (0.028)
Member education (Ivy)			
Share Ivy staff	0.012 (0.024)	0.010 (0.025)	0.003 (0.005)

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

To accompany main text Figure 6, Figure F1 presents the share of full time Black and female staff for same-group MCs.

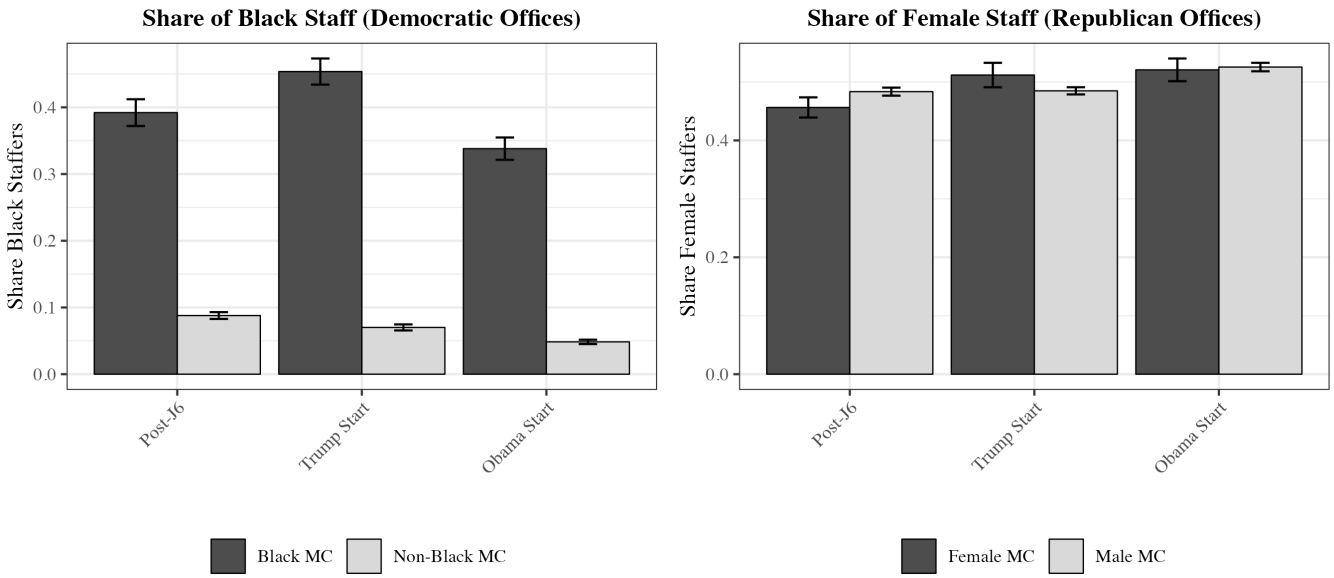


Figure F1: Black and Female Employment in Congressional Offices Figure compares average share of Black and female staffers in Congressional offices during Presidential transition periods.

F.2 Assortative Employment Over Time

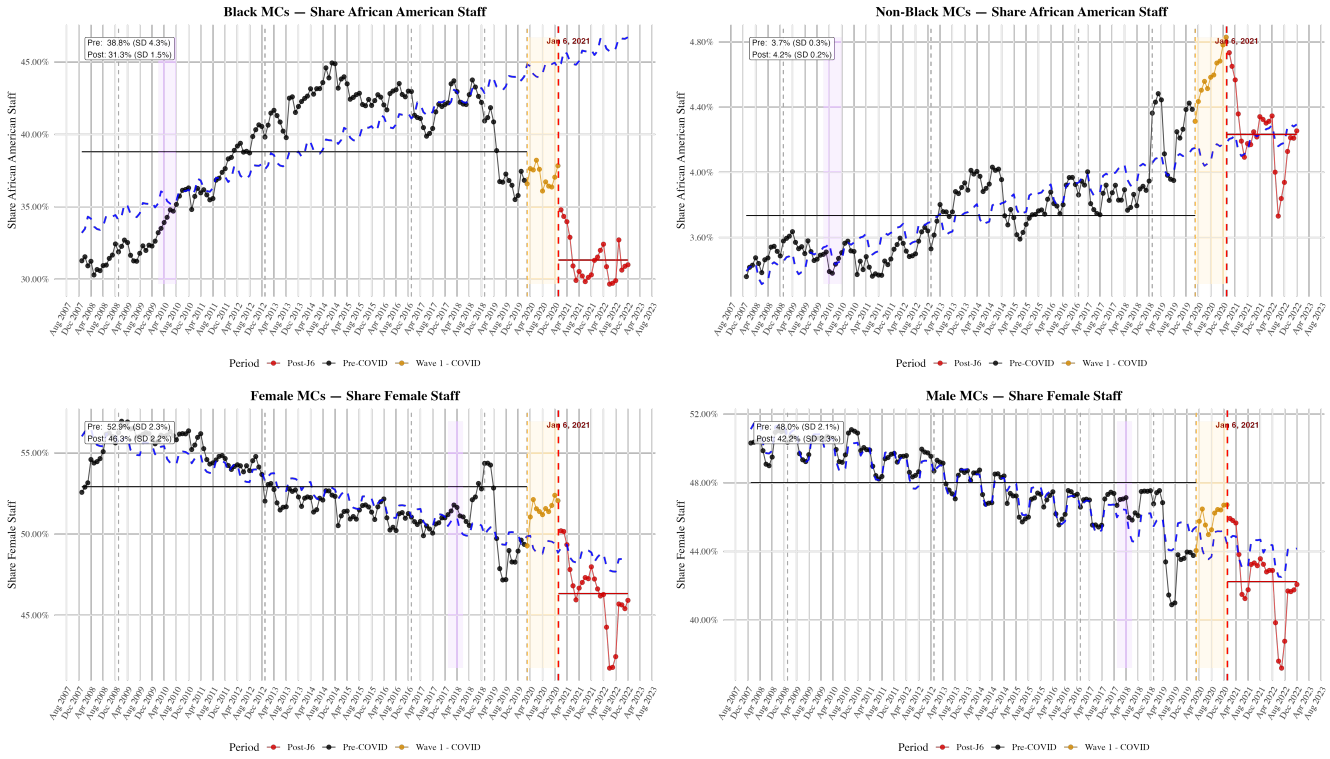


Figure F2: Black and Female Employment in Congressional Offices Over Time Figure plots average share of Black and female staff over time in MCs' offices.

F.3 Ivy Members and Staff

Analagous to main text Figure 6, Figures F3 and F4 plot turnover, hiring, office shares, and average cumulative replacement in the 6 months afters January 6, the start of President Trump's first term in January 2017, and the start of President Obama's first term in January 2009 for staff with Ivy League degrees.

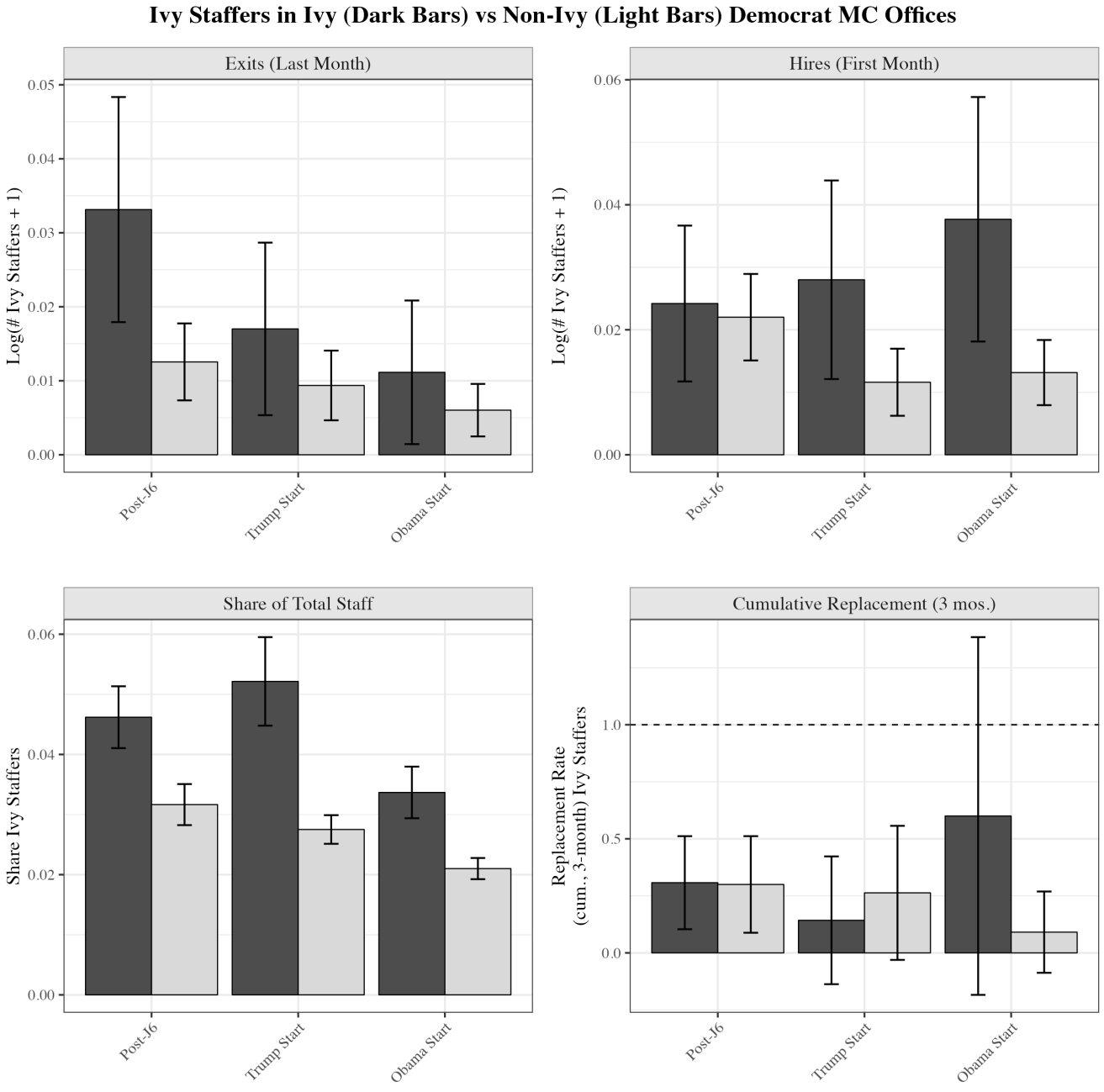


Figure F3: Turnover, Hiring, Office Shares, and Replacement in Ivy Alum Democrat Offices

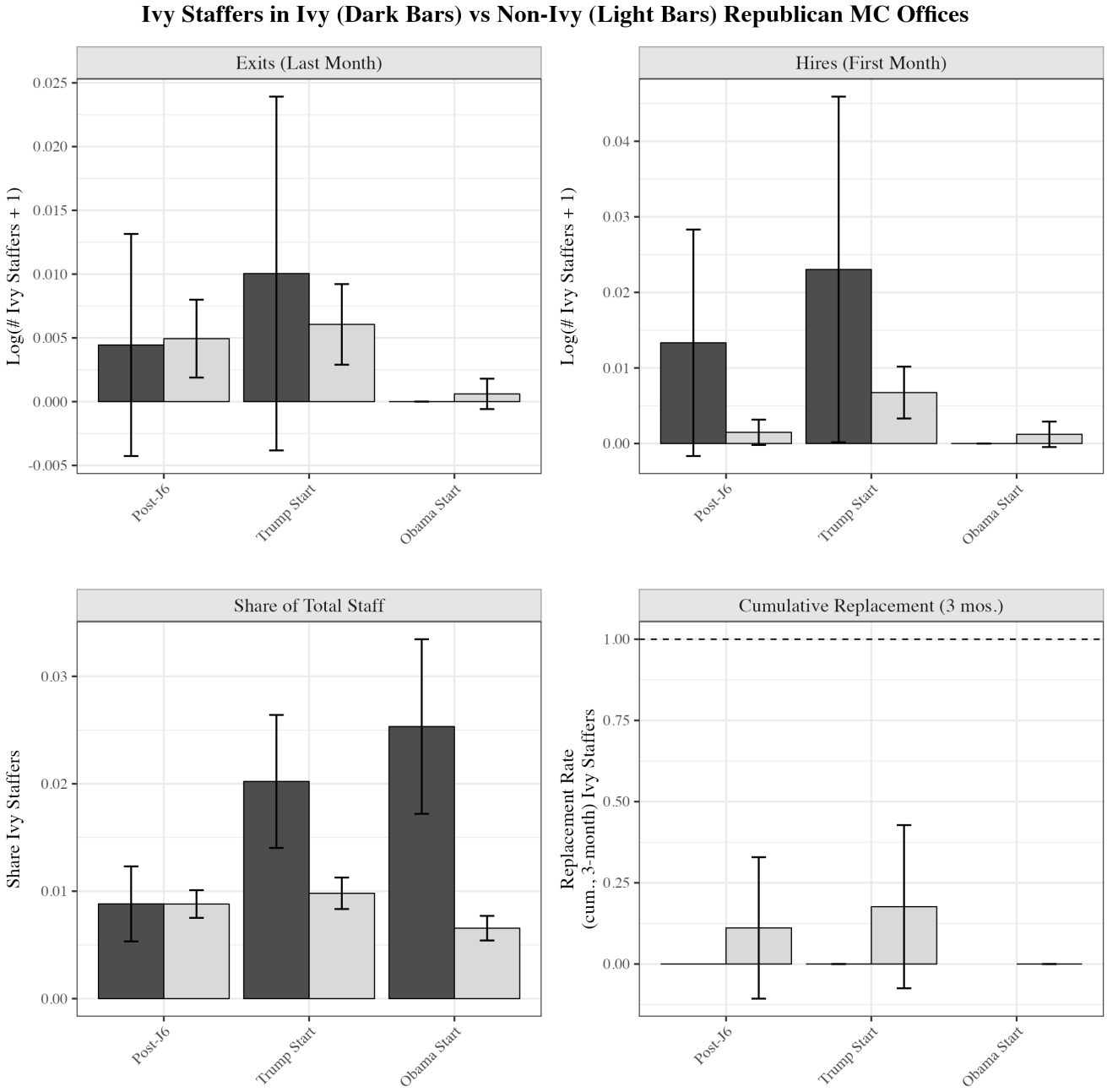


Figure F4: Turnover, Hiring, Office Shares, and Replacement in Ivy Alum Republican Offices

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