Sorting for K Street:

Post-Employment Regulations and Strategic Wage Setting in Congress

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Abstract

While post-employment regulations are a common tool to slow the revolving door in government, little is known about their effectiveness and consequences. Using the 2007 Honest Leadership and Open Government Act (HLOGA), I argue that policymakers strategically adjust their behaviors to maintain lucrative career options in the lobbying industry. HLOGA prohibited staffers-turned-lobbyists who earn at least 75% of a Congress member's salary from contacting their ex-employers in Congress for one year. Using data on the complete set of congressional staff (2001-2016), I show that staffers sort below the salary threshold post-HLOGA. Employing various panel data analyses, I also find that selecting out of the regulation increases a staffer's probability to become a lobbyist and ensures a substantial premium in revenues at the beginning of their lobbying career. These results explain why reforms of the revolving door fail and provide insights on institutional determinants of career incentives for non-elected public officials.

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1 Introduction

As the lobbying industry and its influence have expanded over the years (Baumgartner et al., 2009), lobbying regulations and ethics laws have become ubiquitous. Intentions to regulate lobbyists and their clients range from restrictions – such as rules on post-government employment and limits on gifts for public office-holders – to registration and reporting requirements – including regular disclosure of lobbying and fundraising activities. These regulations help to ensure transparency of interest group influence and can prevent potential conflicts of interest for public officials. Thus, the purpose is to strengthen the accountability of decision-makers and to level the playing field of democratic representation.

At the same time, many lobbying regulations involve opaque exceptions and discretionary cutoffs. For instance, mandatory cooling-off periods for public officials are a common tool to reign in the revolving door between government service and the lobbying industry. While 43 US states prohibit former legislators or staff from working as lobbyists for up to two years, many of these ethics laws provide for unique exceptions or nuances. Some states exempt uncompensated lobbying with expenses reimbursed, or set thresholds for procurement contracts that determine officials' ability to accept employment with government contractors.[1] Similarly, although federal efforts to restrict the revolving door have been extensive – recently culminating in executive orders from the Obama, Trump, and Biden administrations that impose limits on lobbying for former executive branch employees – these restrictions only apply to senior personnel and have repeatedly been subject to waivers (Drutman, 2013). Similar omissions and manipulable thresholds are present in related regulations of money in politics, such as bans on gifts, loans, and other benefits from lobbyists (Kerns and Wood, 2015) or limits to the discretion of procuring entities in allocating government contracts (Palguta and Pertold, 2017; Bosio et al., 2022). These exceptions to lobbying regulations are often seen as a way to balance ethics standards with the government's objective of attracting

[1] See a survey of US revolving door regulations at https://www.ncsl.org/research/ethics/50-state-table-revolving-door-prohibitions.aspx, accessed on 09/20/2022.

experienced and qualified individuals to public service and leveraging valuable information for policy-making (Law and Long, 2011; Arnsdorf, 2016). Yet, they may also open room for unchecked influence peddling, thus jeopardizing the intentions of accountability reforms and regulations.

In this article, I shed light on how such discretion impacts the effectiveness of lobbing rules by answering two questions: How do affected public officials strategically react to self-imposed restrictions? And how does this shape the strategy of lobbying firms and their clients? I use the 2007 Honest Leadership and Open Government Act (HLOGA) to study how congressional staffers adjust their behavior to circumvent cooling-off periods and if these strategic adjustments are rewarded with lucrative future employment in the lobbying industry. To minimize possible conflicts of interest, HLOGA prohibited staffers-turned-lobbyists to contact their ex-employers and colleagues in Congress for one year. Yet, it only affected staffers who earn at least 75% of what a member of Congress (MC) is paid annually. I argue that this incentivized staffers to self-select out of HLOGA's coverage to maintain lucrative outside options in the lobbying industry. By keeping their salaries below the cutoff – hence avoiding HLOGA's waiting period – these staffers maintained their attractiveness to the lobbying industry. Therefore, I also expect that "switching" out of the regulation increases the probability that a staffer becomes a lobbyist and increases their revenues as lobbyists during the cooling-off period.

Using data on the entirety of full-time staffers employed in Congress between 2001 and 2016, I present four sets of findings. First, I show evidence that staffers sort below the 75% salary threshold after HLOGA took effect. Second, this bunching is stronger for staffers with connections of especially high value for the lobbying market, namely committee staff and Senate staff. Third, I use a within-staffer panel design using two-way fixed effects to study the relationship between moving below the salary threshold and lobbying employment. The results show that by switching out of HLOGA coverage, staffers can increase their chance to be hired as a lobbyist immediately after leaving Congress by about two percentage points,

thereby more than doubling the 1.5% average annual turnover to lobbying among staffers. Last, to study how this behavior influences strategies of lobbying in Congress, I further examine the revenues generated by staffers-turned-lobbyists and find that those who switch out of coverage attract higher revenues than HLOGA-covered ex-staffers in their first year as lobbyists. This indicates that switchers are compensated in the lobbying industry for their willingness to reduce their congressional salary and their ability to fully exploit their personal connections to lawmakers directly following their exit from Congress.

This study adds to our understanding of regulatory policies, both with respect to government accountability more broadly and revolving-door regulations specifically. As regards the former, I highlight a particular dilemma of institutional reforms that affect lawmakers and their staff. Because policymakers have little motivation to thoroughly regulate their own behavior and career options, respective regulatory efforts often include substantial loopholes, thus precluding far-reaching institutional change. Importantly, I show that reforms aimed at strengthening government accountability are particularly vulnerable if they are defined over variables that regulated groups can control (e.g., salary levels). This finding contributes to literature on the impact of political institutions on policymakers' behavior in two main ways. First, a large body of work stresses the importance of institutional rules for democratic accountability, such as government audits (Ferraz and Finan, 2011; Wood and Grose, 2022), term limits (Alt et al., 2011), or court rulings on campaign spending limits (Abdul-Razzak et al., 2020). Yet, this research has predominantly considered policies where the relevant variables are *outside* of officials' choice sets, thus neglecting the vulnerability of accountability reforms to regulatory evasion. Second, several studies show that politicians respond to monetary incentives and regulations of campaign financing when deciding whether to run for office (Groseclose and Krehbiel, 1994; Barber, 2016; Avis et al., 2017; Weschle, 2021). This article, in turn, establishes that institutional features and regulatory efforts can affect career choices of unelected policymakers – beyond their widely observed impacts on elected officials.

Additionally, this article informs the policy debate on revolving-door restrictions. Imposing ever stricter rules and longer cooling-off periods for public personnel – as subsequent regulations of the Obama, Trump, and Biden administrations mandated – will likely miss the objective. The evidence suggests that ill-designed lobbying reforms may not only distort the incentives for affected officials, but also influence contracting and remuneration in the lobbying market resulting in clients paying significantly more for "fully connected" lobbyists. Hence, reform efforts like HLOGA may merely shift and obscure the channels through which special interests influence Congress, instead of reducing their pressure. Thus, this research contributes to the growing number of studies looking at the effects of revolving-door regulations (Law and Long, 2011; Cain and Drutman, 2014; LaPira, 2016; Palmer and Schneer, 2019; Weschle, 2021). The study most closely related to my research is Cain and Drutman's (2014) evaluation of HLOGA. Using a difference-in-differences (DiD) design, the authors assess the effectiveness of HLOGA's revolving-door provisions for different types of congressional staffers. The authors find that after HLOGA, "covered" staff (those earning at least 75% of a member's salary) were less likely to become lobbyists within one year than "high-level" staff making between 60% and 75% of a member's pay. Yet, as the authors note themselves, these findings rest on the important assumption that staffers do not manipulate their treatment status by moving between salary bands. My study addresses this issue explicitly, indicating that such regulatory evasion indeed weakens revolving-door rules.[2]

2 The 2007 Honest Leadership and Open Government Act

Shaken by the scandals around Jack Abramoff, a lobbyist sentenced for fraud and federal conspiracy in relation to his lobbying activities, Congress was determined to regulate the revolving door after the 2006 midterm elections. The resulting HLOGA strengthened lobbying and donation disclosure requirements, strictly limited travel and gifts sponsored by lobbyists, and increased penalties for non-compliance with ethics regulations. Most importantly

[2]See Appendix C for a more detailed comparison with Cain and Drutman (2014).

for this research, HLOGA intended to decrease the flow of personnel between Congress and the lobbying industry by strengthening existing cooling-off periods. Covered officials who go through the revolving door to lobbying are now prohibited from contacting former colleagues in Congress for up to two years, depending on their previous government position. In addition to senators and representatives, the regulation affects only those staff members earning at least 75% of a MC's annual pay rate for at least 60 days in the year before they leave Congress.[3] As the annual pay rate for MCs has been $174,000 since 2009 (Congressional Research Service, 2021), the threshold for staffers leaving Congress after 2009 is $130,500 aggregated annual pay. However, the post-employment restrictions apply in slightly different ways. Former House staffers who meet the threshold are prohibited for one year from communicating with members, committees, and offices for whom they worked in Congress. Following HLOGA, covered Senate staffers, in contrast, cannot contact *any* Senate offices for one year if they become lobbyists (Cain and Drutman, 2014). Importantly, however, for both Senate and House staffers the cooling-off period limited contacts of all covered employees regardless of whether they become, or are hired by, *registered* lobbyists.

It is important to note that federal legislative branch officials were subject to some post-employment restrictions prior to HLOGA, with similar distinctions for employees of different ranks. In particular, the one-year lobbying ban for congressional staffers, including the 75% salary threshold, has been part of the federal criminal code (18 US Code §207) since the Ethics Reform Act of 1989 (Committee on Ways and Means, 1995). Yet, while there were few mechanisms in place to ensure the necessary enforcement of existing restrictions before HLOGA, the 2007 reform significantly strengthened the enforcement of cooling-off periods.[4] HLOGA not only requires that MCs and covered staff inform Congress about ongoing employment negotiations with the private sector within three business days, but

[3]For employees of legislative offices other than personal staff, committee staff, and leadership staff the threshold is the basic rate of pay for level IV of the Executive Schedule (between $149,000 and $160,300 since 2009).

[4]See Appendix A for more details on the specific regulatory changes and how staffers learn about the relevant restrictions.

also requires the Clerk of the House and the Secretary of the Senate to notify legislative officials of their post-employment coverage restrictions upon leaving Congress and to post the details of such notifications publicly.[5] I, therefore, focus on staffers' strategic wage setting and use of the revolving door after HLOGA.

The law was celebrated as a historic achievement across the aisle. Introduced by Senate majority leader Harry Reid and co-sponsored by minority leader Mitch McConnell, the lobbying reform pushed both parties to cooperate on a Senate's important first bill for the first time in 32 years (Arnsdorf, 2016). After the law was enacted on September 14, 2007, then-senator Barack Obama praised the law as the "most sweeping ethics reform since Watergate" (Hiltzik, 2015) and Harry Reid promised that "this legislation will slow the revolving door that shuffles lawmakers and top staff between federal jobs and the private sector" (Arnsdorf, 2016). Indeed, some commentators and scholars claim that HLOGA significantly changed lobbying practices and curbed the revolving door in Washington (Rehr, 2012; Cain and Drutman, 2014).

Other research, however, argues that HLOGA is merely a paper tiger because it incentivized affected public officials to simply circumvent the new rules after leaving Congress by avoiding to register as shadow lobbyists (LaPira, 2016; LaPira and Thomas, 2017; Ban et al., 2019) or choosing a different unregulated revolving door (Palmer and Schneer, 2019). In this study I argue that congressional staffers use another loophole of HLOGA to avoid post-employment restrictions: reducing their salary while in Congress to remain below the coverage threshold. Given the limited effectiveness of HLOGA, the various other ways to avoid cooling-off periods, and the law's narrow influence on only a subset of high-earning staffers, the odds might seem stacked against finding that HLOGA pushed staffers to lower their congressional salaries to cash in on their political connections later. Hence, the evidence presented here crucially highlights the influence of the lobbying market on the career choices

[5] https://disclosures-clerk.house.gov/PublicDisclosure/PostEmploymentNotification, accessed on 04/29/2022; https://www.senate.gov/legislative/termination_disclosure/report2018.htm, accessed on 04/29/2022

of public officials in Washington and the importance of political connections for lobbyists on K-Street.

It might seem limiting to focus on the strategic behavior of Congress staffers and thus zoom in on only one particular population affected by HLOGA. However, it is important to note that the grand majority of revolving-door lobbyists have worked in Congress. Among 4,733 revolving-door lobbyists who submitted lobbying reports from 1998 to 2014, 78.2% were congressional staffers, whereas 7.5% were members of Congress and 14.3% were lobbyists from the executive branch (Shepherd and You, 2020). This implies that examining congressional staffers can inform us about the strategies of an important subpopulation of lobbyists and how incentives to move into the lobbying industry influence their ex ante behavior in government.

3 Career Incentives of Staff and Wage Setting in Congress

My argument rests on three main assumptions. First, the cooling-off period stipulated in HLOGA only diminishes the attractiveness of ex-staffers for the lobbying market if revolving-door lobbyists are hired primarily for their political contacts as opposed to their policy expertise or knowledge of congressional procedures. Several studies on the revolving door in Congress show a strong relationship between lobbyists' connectedness and their revenues, and suggest that lobbying clients value connections more than policy expertise (Blanes i Vidal et al., 2012; Bertrand et al., 2014; LaPira and Thomas, 2014; McCrain, 2018; Ban et al., 2019).

Second, my argument requires that career incentives in general – and the revolving door to lobbying in particular – influence staffers' behavior while they still serve in Congress. This assumption also is borne out by a substantial body of literature showing that the challenging working conditions together with the high premium for political connections in the lobbying industry incentivize staffers to seek more lucrative future careers outside Congress (Salisbury and Shepsle, 1981; Cain and Drutman, 2014; Shepherd and You, 2020).

Third, and most importantly, the argument assumes that senior staffers have sufficient influence on their salaries in Congress. Hiring and wage setting in Congress is very flexible and decentralized, which leaves room for MCs and their staff to adjust staffers' compensation. Both House and Senate members receive annual allowances to pay for official expenses, including personal staff, mail, travel, and office equipment. While some components vary by MC based on the characteristics of their congressional district or state – such as the distance to Washington, DC – both House Members' Representational Allowance (MRA) and Senators' Official Personnel and Office Expense Account (SOPOEA) assign an initially identical amount of resources for use on personnel (Brudnick, 2018). Nevertheless, MCs can substitute between types of expenditures as they see fit.

While there are some regulations governing staffing decisions in Congress – House members cannot employ more than 18 full-time staffers and there are certain caps on maximum pay for both House and Senate staffers[6] – the process gives members and offices of Congress substantial discretion in how they allocate their available resources across staffers and positions. This opens ways for staffers to influence the wage-setting process. As one former staff member put it: "Everything is very flexible in Congress around wages. There's a lot of isomorphism, but there's no formal reason for this to be the case. Staff can negotiate wages, benefits, etc. There's nothing that dictates pay."[7] What is more, there is anecdotal evidence that staffers are able to achieve a certain salary figure. As a committee professional staffer indicated, some mid-level staff avoid moving beyond the "senior staff" salary threshold[8] in order to circumvent the necessity to file quarterly financial disclosure forms, adding that "[staffers] have more discretion to turn down a raise than trying to advocate for a raise."[9] Another senior staffer recounted the strategy of a former colleague who preferred to move

[6]These are $168,411 ($172,500) and $169,459 ($171,315) for personal staff (committee staff) in the House and Senate, respectively. These caps, as well as the annual salary for MCs of $174,000, have remained unchanged since 2009 (Brudnick, 2018).

[7]Email correspondence with former staffer, July 6, 2020.

[8]The triggering salary is at least 120% of the federal GS-15 base level salary, which ranged from $114,468 in 2008 to $135,468 in 2022. Importantly, the threshold does not coincide with the HLOGA threshold in my sample period.

[9]Interview 1, October 28, 2022.

into lobbying rather than follow their MC to another committee. During wage negotiations, this staffer told their employer: "Don't give me this particular pay increase, because I don't want to get this extra lobbying ban."[10]

Since HLOGA's restriction affects senior staffers (i.e., chiefs of staff and legislative directors who are often responsible for wage setting themselves), the mechanism of strategic wage manipulation does not necessarily require MCs and congressional offices to be complicit – a fact corroborated by a committee professional staff member.[11] Additionally, while staffers are well aware of post-employment regulations from annual ethics training sessions and the staff handbook, most MCs and offices are not familiar with the specificities of these rules.[12] Yet, even in cases where congressional members and offices have direct influence on wage negotiations and knowledge about the post-employment regulations, they likely have an interest in supporting staffers in their strategic wage adjustments. Reducing the annual salaries of individual staffers could free up resources for other types of expenditures, such as travel to and from the district. Additionally, recent research shows that the revolving door to lobbying can incentivize congressional staff to showcase their skills, leading to higher legislative productivity for their MCs (Shepherd and You, 2020). Hence, while staffers' sorting behavior reveals their willingness to leave Congress and may signal low commitment to their congressional employers, congressional offices may play along to reap possible gains in staffers' productivity. More importantly, as former leadership office and committee staffers indicated,[13] lobbyists' political connections are not only an asset for lobbying clients, but also for lawmakers themselves. For MCs, lobbyists often function as "service bureaus" or "adjuncts to staff" who subsidize congressional work with their policy-specific information (Hall and Deardorff, 2006, p.76). MCs, therefore, have an interest in helping staffers land prestigious lobbying jobs.

These theoretical and empirical regularities imply several predictions about how HLOGA

[10]Interview 3, November 12, 2022.
[11]Interview 1, October 28, 2022.
[12]Interview 1, October 28, 2022.
[13]Interview 2, October 28, 2022; Interview 3, November 12, 2022.

affected the underlying incentives of the revolving door. It made covered staffers less attractive to the lobbying industry because of the restrictions on whom they can lobby (Cain and Drutman, 2014). Staffers, in turn, now have an incentive to avoid coverage to maintain valuable outside options in lobbying and reap gains from their personal connections to Congress. Hence, congressional staffers in higher pay grades are willing to accept pay cuts or forgo salary increases while in Congress in exchange for a lucrative future career in lobbying.

> **Hypothesis 1**: Post-HLOGA, congressional aides set their salary to just below the coverage threshold to avoid revolving-door regulations.

Yet, we should see considerable heterogeneity across types of staffers in their sorting behavior. First, the regulation should have more influence on strategic salary manipulation for committee staffers than for personal staff. Previous research not only shows that lobbyists specifically target committees (Bertrand et al., 2014; Hojnacki and Kimball, 1998), lobbying revenues also increase considerably with connections of ex-staffers to congressional committees (Blanes i Vidal et al., 2012; McCrain, 2018). Hence, connections to legislators and staffers in committees are especially valuable for revolving-door lobbyists, and committee staff have higher incentives to be strategic in avoiding these constraints. Additionally, HLOGA is more relevant to committee staff on the margins because their salaries are higher and more committee staff are therefore covered by HLOGA (see Figure 1). Overall, HLOGA's restrictions are more salient for committee staff than personal staff.

Second, I expect stronger sorting in the Senate than in the House. As indicated above, HLOGA was more restrictive for Senate staff than House staff. Additionally, procedural rights in the Senate concentrate power in the hands of individual senators, granting them more ability to influence legislation (Krehbiel, 1998). Hence, former Senate staffers are likely preferred by lobbying firms because they can offer more power to affect legislation than former House staffers (Lazarus et al., 2016).

> **Hypothesis 2**: Post-HLOGA, sorting around the threshold of 75% of a member's

annual salary is stronger for committee staff and Senate staff than for personal staff and House staff.

If staffers intentionally sort out of coverage by revolving-door restrictions, this raises an immediate question: To what extent do staffers benefit from their willingness to restrict their congressional salaries? Avoiding coverage makes staffers more attractive to the lobbying industry because communication with their personal network in Congress is unrestricted. Moreover, since reducing their salary is a costly signal to pave their way into lobbying, staffers may not want to keep their salaries subdued for long periods. Therefore, one may expect that "switching" out of the revolving-door restriction increases a staffer's likelihood to leave Congress for the lobbying sector. Additionally, given the substantial premiums associated with ties to legislators and colleagues in Congress (Blanes i Vidal et al., 2012; Bertrand et al., 2014; McCrain, 2018), the lobbying industry should place a higher price tag on lobbyists who can fully exploit their connections to Congress. Ex-staffers who successfully switched out of the lobbying restrictions before moving into the lobbying industry likely advertise their coverage status to lobbying firms and clients. Also, firms are aware of who is affected by lobbying restrictions through the public disclosure of post-employment notifications after HLOGA. Hence, staffers who switched out of coverage in their last year in Congress should attract more revenue and larger contracts in their first year in lobbying than HLOGA-covered ex-staffers. This premium should attenuate in later years when the cooling-off period has passed and all staffers-turned-lobbyists can equally leverage their connections to lawmakers in Congress.

Hypothesis 3: Congressional staffers are more likely to join the lobbying sector in years when they move from being covered to being uncovered by HLOGA's restrictions.

Hypothesis 4: Staffers-turned-lobbyists who switched from being covered to being uncovered by HLOGA's restrictions in their last year in Congress attract more revenue than covered lobbyists during the cooling-off period.

4 Data

To test these hypotheses, I compile data on the full universe of staffers recorded on the congressional payroll system between 2001 and 2016. The information comes from LegiStorm, a for-profit organization that assembles staffer salary data from official statements of disbursement provided by the Clerk of the House and the Secretary of the Senate. In addition to salary information for each staffer and employment period, these records include staff names, positions, the employing office or member, and the length of employment. I remove part-time employees, interns, drivers, and staffers who worked in Congress for less than six months. The remaining sample includes 59,471 full-time staffers (39,697 after HLOGA).

To calculate staffers' annual salaries and compare them to a member's annual pay, I aggregate salaries for an entire year. To account for the fact that staffers may not work the full year, I follow Cain and Drutman (2014) and calculate daily pay rates based on the number of days worked per calendar year.[14] This results in a staffer-year data set. In each year, a staffer is classified as covered by the revolving-door restriction if her daily pay rate amounts to at least 75% of a member's daily pay rate (see Cain and Drutman (2014) for a similar approach).[15] For administrative staff, who account for about 14% of my sample, a staffer is covered if her daily pay rate is at least the annual pay for level IV of the Executive Schedule.

Turning to the data on staffers' revolving doors to lobbying, I use information from

[14]Staff disbursements happen quarterly. For each staffer and year, I obtain their total salary across all disbursements as well as the number of days employed. I then divide the total annual salary by the total number of days worked. Some staffers simultaneously receive salary from several offices. In calculating the number of days worked, I counted each day only once.

[15]Note that the coverage rule in HLOGA further specifies a time period (60 days) in which a staffer must earn more than the coverage threshold. To build a consistent staffer-calendar year panel, I follow Cain and Drutman (2014) and do not include the 60-day refinement in the coding of coverage because doing so may cause staffers to artificially drop out of coverage in their last year in office if they leave before 60 working days in that year. This coding, therefore, provides a conservative measure for my main variable of interest (switching out of coverage). Yet, to evaluate the robustness of this coding decision, I also provide evidence using two alternative treatment codings that account for the days of coverage in Table D10, D11 and Figure E8. The main analysis is largely robust to these alternative operationalizations, albeit some of the estimates are less precisely estimated.

disclosure reports that lobbyists file with the Senate Office of Public Records. These reports have been publicly available since 1998 and are compiled and released quarterly by the Center for Responsive Politics.[16] For each lobbying report and lobbyist, these data include the revenue that clients pay lobbying firms for their activities and a description of their previous experience in government if a lobbyist worked for a legislative or executive agency. To identify staffers-turned-lobbyists, I build on Shepherd and You (2020) and merge lobbyists who registered between 1998 and 2017 to the payroll data using lobbyists' names and the details of their past positions in Congress.[17] There are 5,040 unique lobbyists who also appeared in the staff data between 2001 and 2016. My final data set of full-time staffers includes 4,527 unique lobbyists, of whom 2,736 worked in Congress after 2007.

For the analysis of lobbyist revenues, I focus on staffers-turned-lobbyists who become lobbyists within one year after leaving Congress. Additionally, for this section of the analysis I follow related work (Blanes i Vidal et al., 2012; McCrain, 2018) and restrict my sample to revolving-door lobbyists who work for lobbying firms. I exclude ex-staffers who exclusively work as in-house lobbyists because lobbying disclosure reports do not list the revenue for these lobbyists explicitly. The sample includes 2,027 individual lobbyists, of whom 1,110 joined the lobbying industry after HLOGA took effect. Note that I only use the restricted sample for the analysis of Hypothesis 4. All other analyses use the full set of staffers-turned-lobbyists, including in-house lobbyists. To estimate lobbyists' revenues by year, I use the weighted measure of aggregate revenues described in Blanes i Vidal et al. (2012).[18] I then build a lobbyist-year panel for the years 2001-2018 by aggregating each lobbyist's weighted inflation-adjusted revenues across all reports per year.

While these data offer very detailed information on staffers' careers in Congress as well as in the lobbying market, some limitations merit additional discussion. Although staffers-

[16] https://www.opensecrets.org/bulk-data, accessed on 04/29/2022.

[17] See Appendix B for a further description of this matching procedure.

[18] For each lobbying report I attribute a share of the total revenue to each lobbyist depending on the number of lobbyists listed on the report. For example, if seven lobbyists are named on a report with a value of $70,000, each lobbyist is allocated $10,000 in revenue for this report. In Figure E3, I show estimation results with the unweighted measure of lobbying revenues. The results remain essentially unchanged.

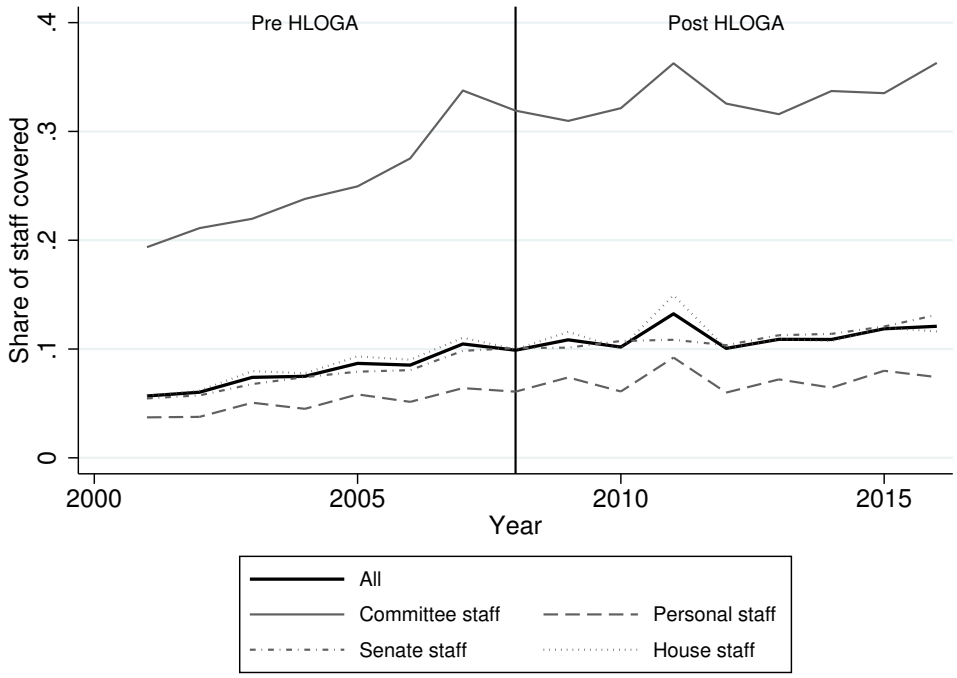
turned-lobbyists are required to register and disclose their previous government positions, I acknowledge that some ex-staffers may fail to register or may deliberately withhold information about their previous government service (Cain and Drutman, 2014; LaPira and Thomas, 2017). While the extensive manual checks of names and positions I conducted should alleviate these concerns, the analysis presented below necessarily relies on self-reporting by lobbyists and can only offer inferences on the effects of strategic wage setting on the careers of *registered* lobbyists. Additionally, lobbying revenues are certainly an imperfect measure for lobbyists' value and disregarding in-house lobbyists considerably restricts my sample for the analysis of Hypothesis 4.[19] Yet, I believe that this measure offers a reasonable proxy for lobbyists' value. The amount clients spend on each lobbying contract depends on the performance of lobbying firms and their lobbyists (McCrain, 2018). Firms, in turn, assign their most effective lobbyists to their largest lobbying contracts (Blanes i Vidal et al., 2012; McCrain, 2018). Lobbying revenues should, therefore, be closely related to the value of lobbyists servicing the specific contracts.

Table D1 presents summary statistics on staffers' salaries and the revolving door to lobbying. With respect to staffers' annual pay, the figures bolster the account of a flexible labor market and wage setting in Congress. On average, a staffer is paid $63,576 per year. Yet, there is considerable variation in annual pay rates for individual staffers. About 0.5% of staffers move from being covered by HLOGA to being uncovered per year. The turnover rate indicates that more than 15% of full-time staffers leave Congress every year, with higher rates in the final years of congressional terms. About one percent of staffers leave for the lobbying market every year.

To illustrate who and how many staffers are affected by HLOGA's coverage rule across time, Figure 1 shows the share of covered staff by office type, chamber, and year. Overall, between 10% and 13% of all staffers were covered post-HLOGA, thus restricting the lobbying ability of about 2,000 staffers each year. Importantly, between 2008 and 2016, a total of

[19]There are 1,058 ex-staffers who directly moved from Congress to lobbying and exclusively worked as in-house lobbyists after HLOGA.

Figure 1: Number of covered staff over time



Note: Depicted is the share of staffers (by chamber, office type, and overall) who receive annual salaries above the 75% threshold and are thus covered by HLOGA.

5,772 staffers (15%) were affected by HLOGA. While the coverage rate is lower (6%-9%) for personal staff – which is likely due to hard budget constraints that MCs face for their MRAs – this figure is substantially higher for committee staff, ranging between 32% and 36% after HLOGA. The differences in coverage rates for House and Senate staff, in contrast, are not as stark.

5 Testing for Strategic Sorting

I first present descriptive evidence for Hypotheses 1 and 2. In particular, I use McCrary density estimates (McCrary, 2008) to illustrate sorting by staffers around the 75% salary threshold before and after the introduction of HLOGA and for different types of staffers. This test estimates the density of the running variable separately on both sides of the cutoff and checks whether the density drops substantially at the threshold. The test first estimates a fine-gridded histogram and then uses local linear regressions on both sides of the threshold

to smooth the histogram.

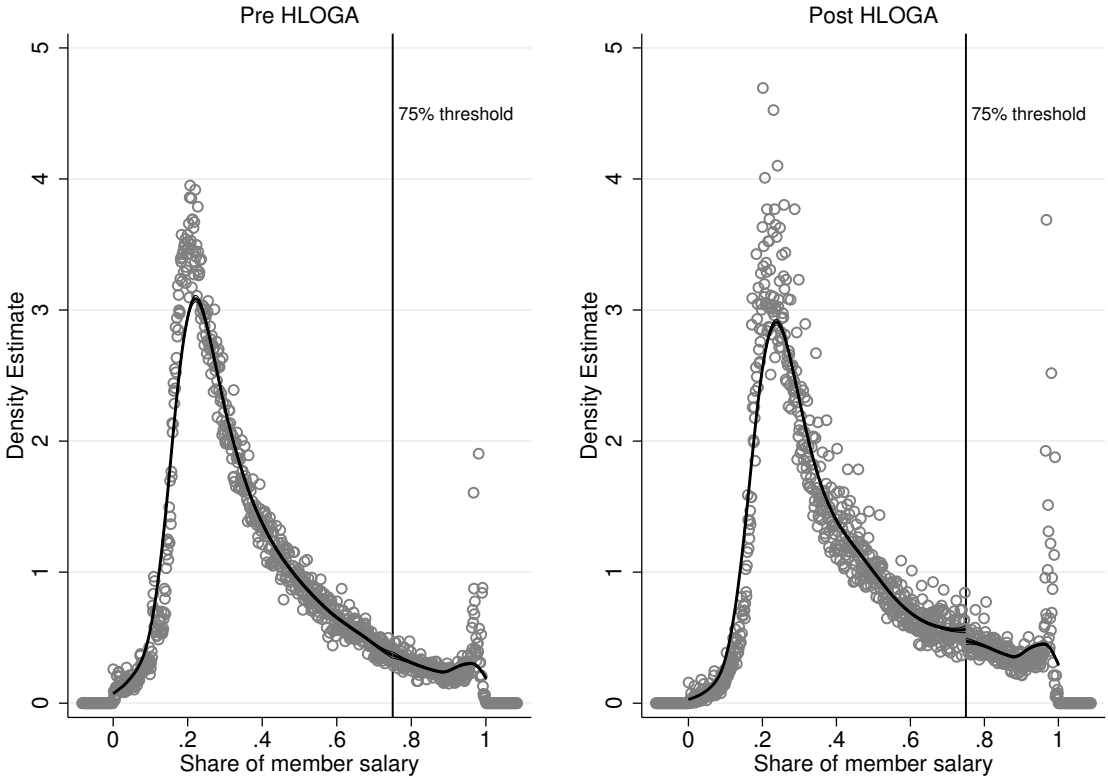
Figure 2 depicts the McCrary (2008) density estimates based on staffers' annual salaries for the years before (2001-2007) and after (2008-2016) HLOGA. Staffer salaries are shown as shares of a member's annual salary.[20] The distribution of salaries is highly right-skewed, and the general shape differs little between the two time periods. Yet, staffers close to the 75% threshold seem to have behaved differently across the periods: Post-HLOGA, there is a negative and statistically significant discontinuity in the density of staffer salaries at the threshold. The McCrary estimate is -0.18 ($se = 0.04$), indicating that there are about 18% more observations barely below the cutoff than immediately above it. Substantively, while 8,294 staffer-year observations are clustered within the McCrary (2008) optimal bandwidth below the cutoff (i.e., 66-75% of a member's salary) in the post-HLOGA period (2008-2016), only 6,515 staffer-year observations are located within the same bandwidth above the threshold (i.e., 75-84%).[21] Before HLOGA, however, the log densities seem smooth. The McCrary estimate is very small (-0.002) and far from statistically significant ($se = 0.05$).

According to my manipulation hypothesis, the sorting behavior arises from staffers' strategic salary adjustments during their time in Congress. However, one may also think of selection effects that can explain the bunching patterns. For instance, MCs may set the annual salary of *new* hires below the 75% threshold instead of adjusting the pay of existing staff. Similarly, if senior staffers self-select out of Congress because of the reduced attractiveness of these positions after HLOGA, we might observe a similar jump at the cutoff. To further substantiate that the sorting behavior is due to salary adjustments, Figure 3 shows the number of staffers who move across the 75% threshold as a result of salary increases or decreases. Consequently, these figures capture salary adjustments by *existing* staffers as opposed to changes in the income distribution due to entry and exit of staffers. For each salary

[20] Note that I drop 1% of observations that turned out to be strong outliers. In particular, I remove negative salaries and staffer-year observations that are higher than 100% of a member's salary. As the LegiStorm codebook notes, these observations are likely accounting adjustments (e.g., in cases of repayments or large extra benefits).

[21] Note that these absolute estimates do not incorporate the triangular kernel weighting included in the McCrary (2008) density estimates.

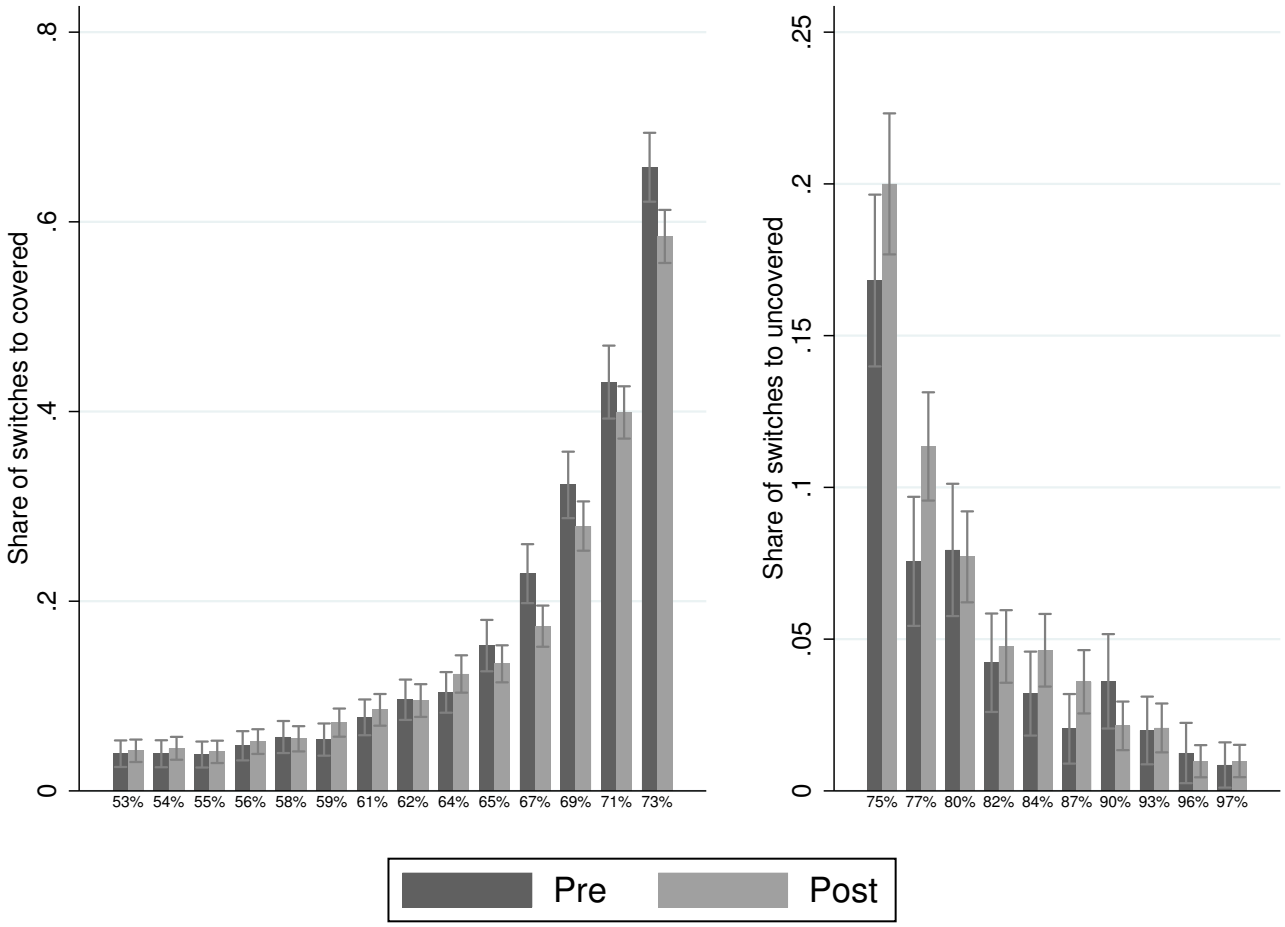
Figure 2: McCrary density estimates, before and after HLOGA



Note: Depicted are estimated densities and local linear regression results of the McCrary analysis before HLOGA (2001-2007) and after HLOGA (2008-2016). Observations on the staffer-year level, $N = 301,485$. 3081 observations (1%) with salaries below zero or above 100% removed.

quantile of staffers, the bars indicate the share of staffer-year observations that constitute a switch to being covered (movement from below the cutoff via salary increases) or a switch to being uncovered (movement from above the cutoff via salary reductions). Staffers become more likely to switch into coverage as they approach the 75% cutoff both before and after HLOGA. Yet, for staffers earning between 65% and 75% of a member's salary the number of switches to covered status is consistently and significantly lower after HLOGA than before HLOGA. Hence, staffers close to the 75% cutoff seem to minimize salary increases to remain below the threshold after HLOGA. Additionally, there is some evidence that staffers above the threshold were more likely to reduce their salary to sort below the cutoff post-HLOGA. Except for the quantile earning between 80% and 82% of a member's pay, the share of switches to uncovered is consistently higher for staffers making between 75% and 90% of a

Figure 3: Number of switches across the threshold by salary quantiles, before and after HLOGA



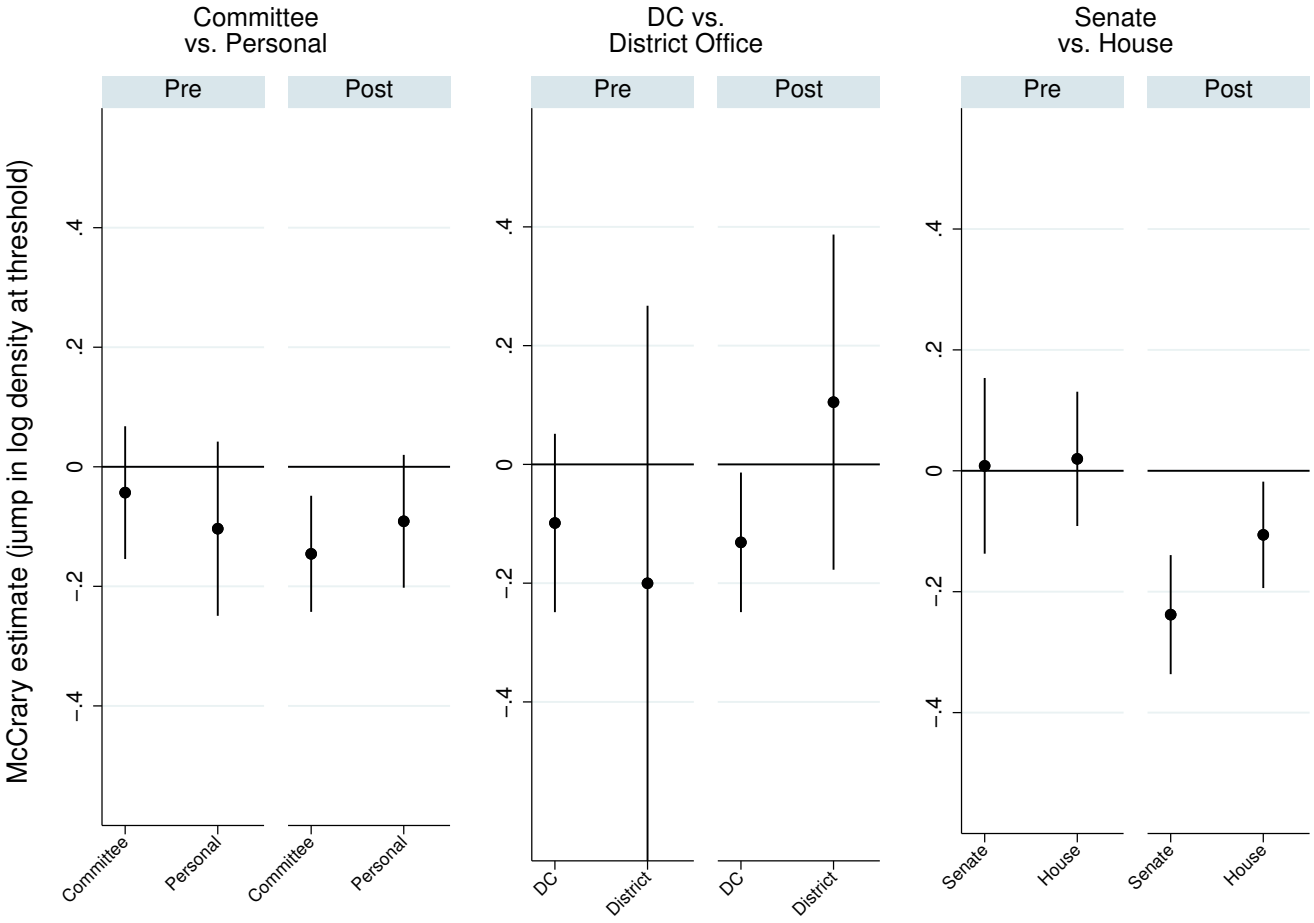
Note: Depicted is the share of staffer-year observations that constitute a salary increase/drop across the 75% threshold by quantiles of staffers pre-HLOGA (2001-2007) and post-HLOGA (2008-2016). Each quantile includes about 1,800 staffer-year observations. Labels on the x-axis indicate the lower bound of the quantiles. Administrative staff excluded.

member's salary after the reform than before. Yet, these differences do not reach statistical significance at conventional levels. These descriptive patterns lend some support to the idea that staffers close to the cutoff amount forgo salary increases or accept reductions in their salary after HLOGA to sort below the 75% threshold.[22]

To test whether the sorting behavior differs by type of staffers, I run the McCrary analysis

[22]Table D2 shows corresponding regression estimates, suggesting that staffers right below the cutoff (65-75% of member pay) were 3 percentage points less likely to switch above the threshold after HLOGA and staffers right above the cutoff were 1.5 percentage points more likely to switch to below the threshold after HLOGA, on average.

Figure 4: McCrary density estimates by type of staffer, before and after HLOGA



Note: Depicted are McCrary estimates together with 95% confidence intervals for different staffer types, before HLOGA (2001-2007) and after HLOGA (2008-2016). Staffers are classified based on whether they ever worked as this type in a given year.

for subsamples of congressional aides. The results in Figure 4 provide descriptive evidence for the claim that sorting is stronger among specific types of staffers.[23] First, the overall discontinuity in staffer salaries around the threshold post-HLOGA seems to be mostly driven by committee staffers as opposed to personal staff. While the estimated discontinuity at the threshold is -0.15 for committee staffers, the McCrary estimate is only -0.09 for personal staff and does not reach statistical significance post-HLOGA. However, when distinguishing personal staff by their location (DC staff vs. district/state office staff), I find that personal

[23]Figure E1 shows density estimates by additional staffer covariates. There is some indication that male staffers, better educated staffers, and staffers with more years of experience in Congress sort more than others. Yet, these differences do not reach statistical significance.

staffers working in Washington show a similar discontinuity of −0.13. As expected, the effect seems to be somewhat stronger for Senate staff than for House staff. The McCrary estimate is −0.24 for the Senate, while staffers in the House only show a discontinuity of −0.11. This difference is marginally significant at the 5% level.[24] For all categories of staffers, the McCrary estimates are small and insignificant for the years before HLOGA took effect.

6 Estimating the Returns to Switching out of Coverage

6.1 The Effect of Switching on the Revolving Door

The evidence on staffers' sorting behaviors is consistent with the argument that HLOGA has changed the strategies of staffers when setting their congressional salaries, especially for staffer types whose connections are valuable to the lobbying sector. Yet, whether staffers can in fact land prestigious lobbying jobs by avoiding coverage remains an open question. To analyze whether salary manipulation by staffers is related to their propensity to become lobbyists, I use the following two-way fixed effects design:

$$\text{Lobbying}_{it} = \alpha_{\mathbf{i}} + \beta \text{ Switch to Uncovered}_{it} + \mathbf{X_{it}}'\gamma + \delta_{\mathbf{t}} + \epsilon_{it} \tag{1}$$

Lobbying_{it} is a dummy equal to one if staffer i leaves Congress in year t to become a lobbyist in that same or the next calendar year. $\text{Switch to Uncovered}_{it}$ is a dummy that indicates whether the staffer crossed the coverage threshold from above in year t. According to Hypothesis 3, I expect $\beta > 0$. The staffer fixed effects $\alpha_{\mathbf{i}}$ in the model account for staffer-specific time-invariant characteristics, such as their unobserved career ambitions or skills. The time fixed effects $\delta_{\mathbf{t}}$, in turn, absorb common changes to the political system that influence both salary manipulations and employment in the lobbying industry, such as

[24] Since the McCrary estimate is asymptotically distributed normal, we can use a simple t-test for independent samples to compare the density estimates: $\frac{\beta_1 - \beta_2}{\sqrt{se_1^2 + se_2^2}}$. The test statistic for Senate vs. House staffers is $\frac{-0.238 + 0.106}{\sqrt{0.050^2 + 0.045^2}} = -1.959$.

changes in the allowances for members or turnover of congressional terms. I use a linear probability model to estimate the regression equation, with standard errors clustered at the staffer level. The sample for the main analysis spans the years after HLOGA was introduced, whereas earlier years serve as a placebo period in Section 7.[25]

I further account for time-varying characteristics of staffers that may confound the relationship between switching and staffers' future lobbying prospects, such as the density of their networks in Congress. Therefore, I include a vector of time-varying staffer-specific controls, $\mathbf{X_{it}}$, to condition on the number of days worked in a year and a staffer's full years in office when leaving her position in Congress.[26] Additionally, I control for whether a staffer ever worked for a specific office in a year as committee staff, personal staff, Senate staff, leadership office staff, district staff, or minority party staff.

A few clarifying comments are important. I have argued that HLOGA incentivizes staffers to *sort* below the cutoff to avoid coverage by lobbying restrictions, either by forgoing salary increases or by reducing their annual salary to below the cutoff.[27] Yet, in the panel models I

[25]In Table D12 I estimate a DiD model where I include both pre- and post-HLOGA periods and interact Switch to Uncovered$_{it}$ with a post-HLOGA dummy. Reassuringly, the results suggest that the effect of switching on lobbying employment is stronger post-HLOGA than pre-HLOGA. However, the estimates are more imprecisely estimated and sensitive to more demanding specifications. I do not opt for a DiD model in my main specification for several reasons. First, my primary interest lies in the *marginal* effect of strategic behavior (switching out of coverage) on the probability of moving into lobbying post-HLOGA rather than the *difference* in the effect of switching across periods. Since the cooling-off period technically existed before HLOGA, staffers' potential strategic wage setting before HLOGA – albeit to a lesser degree – could weaken estimated differences in effect sizes and thus distract from the significant relationship of switching and lobbying success after HLOGA. Second, the validity of a DiD design heavily relies on the parallel trends assumption. However, unlike the usual DiD setting with fixed group assignments, my treatment is time-variant, and since my outcome is exiting Congress for lobbying, staffers who switch in year t necessarily have values of zero in the outcome variable for years $t-1, t-2, ...$ to be observed in my sample. Pre-trends in the probability of exiting between staffers switching in year t and those not switching in year t are thus mechanically parallel and equal to zero. Finally, given the limited number of sorting and switching staff together with the high turnover rate of staffers, a DiD setting poses several challenges for inference. A *within-staffer* DiD setup estimates the difference in the effect of switching on staffers' propensity to exit for lobbying across periods *for a given staffer*, i.e. only staffers with variation in the DiD terms contribute to the DiD estimate. Hence, staffer fixed effects reduce the effective sample size to only a few staffers who switched out of coverage *both* before and after HLOGA. Similarly, the limited number of switchers before HLOGA (430 compared to 796 after HLOGA) cause substantial power issues in a DiD setting.

[26]For years in office, I only have reliable information for staffers joining Congress after 2001, which is the beginning of my panel. For staffers joining before 2001, I impute the years of experience with LegiStorm's manually entered records of congressional offices for which the staffers worked.

[27]After HLOGA, 796 staffers switched from covered to uncovered, and 3,674 staffers earned between 70-75% of a member's salary.

concentrate on the effect of *switching* out of coverage for two main reasons. First, if staffers actively reduce their salaries, this likely sends a particularly strong and credible signal to the lobbying market. Staffers who simply keep their salaries below the cutoff, in contrast, share the asset of unrestricted connections to Congress but they compete against switchers in the lobbying market who previously worked in more superior positions and earned higher salaries. The overall effect of salary manipulations on staffers' success in lobbying is, therefore, likely driven by switchers. The second reason is more practical. While I can illustrate staffers' sorting behaviors using McCrary (2008) density estimates, the counterfactual salaries of sorters are unobserved and I cannot cleanly identify individual staffers who forgo salary to remain below the cutoff.[28] Therefore, estimating the effect of sorting in a panel setting is difficult.

Similarly, I use an indicator for switching out of coverage in a year (Switch to Uncovered$_{it}$) instead of a dummy that simply indicates whether a staffer earns a salary above or below the cutoff. This is because the switching indicator is most commensurable with the immediate signaling effect of salary manipulations presented in Section 3. Using a coverage indicator as the explanatory variable, in contrast, would capture the average difference in staffers' propensity to become lobbyists in all years when they are covered compared to when they are uncovered by HLOGA.[29]

Table 1 reports the results for the within-staffer design.[30] Models 1-3 show estimates using the sample between 2008 and 2016, while models 4-6 extend the sample back to 2007. This accounts for possible anticipation effects, as several staffers intentionally left Congress in 2007 when HLOGA was already being debated on the floor (Shepherd and You, 2020). The results indicate that switching out of coverage significantly increases a staffer's chance to become a lobbyist within a year. The estimated effect is highly consistent across models

[28] One could possibly infer unobservable salary trajectories from previous developments. However, this would likely introduce considerable measurement error when distinguishing staffers who strategically subdue their pay from those reaching a natural ceiling in their salaries.

[29] This is more closely related to the setup in Cain and Drutman (2014) who estimate the direct effect of HLOGA on staffers' propensity to lobby rather than the effect of behavioral responses to the regulation.

[30] Figure E2 shows further descriptive evidence on lobbying rates for different groups of staffers.

and predicts that congressional staffers are, on average, between 2-3 percentage points more likely to leave Congress for the lobbying sector when they switch from above to below the threshold. With an annual average lobbying rate of only 1.5% in the sample, these effect sizes suggest that staffers use the revolving door 2-3 times as often as the average staffer after switching to below the cutoff.

Table 1: Regression Models for Becoming a Lobbyist

	2008-2016			2007-2016		
	(1)	(2)	(3)	(4)	(5)	(6)
Switch to Uncovered	0.030***	0.019*	0.022*	0.032***	0.021**	0.022*
	(0.009)	(0.008)	(0.009)	(0.008)	(0.008)	(0.009)
Staffer FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Position controls		✓	✓		✓	✓
Experience control			✓			✓
Mean of DV	0.014	0.014	0.015	0.015	0.015	0.015
Observations	143,745	143,745	128,067	159,890	159,890	140,194
Number of staffers	37,744	37,744	34,438	41,264	41,264	36,921
R^2	0.014	0.056	0.059	0.012	0.058	0.061

Note: Linear probability OLS regressions with staffer and year fixed effects (not reported); all models include a constant. Position controls include days worked per calendar year and indicators for committee staff, personal staff, majority party staff, minority party staff, DC office staff and leadership office staff. Experience controls include tenure in years as staffer and its squared term. Full models shown in Table D3. Dependent variable: $RD_{i,t}$. Standard errors clustered by staffer in parentheses. *** p<0.001, ** p<0.01, * p<0.05

What is the mechanism of these switches out of coverage? Are staffers who aim for lobbying jobs in the future negotiating lower pay for similar work, or are they willing to accept demotions in titles to increase their lobbying prospects? To address these questions, I use information on the rank of staffer titles based on the hierarchy and chain of command within congressional offices, position qualifications, and position descriptions from Ritchie and You (2021). Using this data, I am able to identify the ranking of 85% (260,449) of staffer-year observations in my panel.[31] I then regress an indicator of whether a staffer experienced

[31]If staffers have more than one title and rank in a given year, I assign the maximum rank or title with the maximum pay for that year.

a demotion in rank on whether they switched out of coverage together with staffer and year fixed effects. The results in Table D4 suggest that switching below the cutoff increases the probability of demotion by six percentage points – a meaningful effect relative to an average demotion rate of only 2%. Yet, these estimates also suggest that a substantial amount of switches below the coverage (i.e., $> 90\%$) are not accompanied by a demotion in ranks, but are due to the significant variability of wages *within* titles and ranks. Table D5 sheds more light on this mechanism. For each rank the table depicts intraclass correlation coefficients (ICC) for staffer random intercepts. If all variation in wages for a given rank is explained by differences *between* staffers rather than within staffers, these estimates are expected to be close to one. However, especially for senior ranks, including (deputy) chief of staff and state/district directors, the ICC are below 0.5, thus indicating that the correlation of wages for a given staffer and rank is low. Taken together, both demotions and changes in salary for similar work drive the mechanism of switching out of coverage.

6.2 The Effect of Switching on Lobbying Revenues

This evidence supports the claim that staffers' willingness to reduce their congressional pay is rewarded with career opportunities in the lobbying market. But are staffers switching below the cutoff also monetarily compensated once they become lobbyists? To estimate the effect of switching out of coverage on staffers' future revenues as lobbyists, I use the lobbyist-year panel described above and estimate the following OLS model:

$$\begin{aligned} \log \text{Revenue}_{jt} = &\alpha + \beta \text{ Switcher}_j + \eta \text{ Years Since Exit}_{jt} + \\ &\theta \left(\text{Switcher}_j \times \text{Years Since Exit}_{jt}\right) + \mathbf{X_j}'\gamma + \delta_{\mathbf{t}} + \epsilon_{jt} \end{aligned} \tag{2}$$

Revenue_{jt} is the revenue attributed to staffer-turned-lobbyist j in year t. As the distribution of this variable is highly skewed, I log this variable to account for skewed residuals. The main independent variable of interest is now the interaction between Switcher_j, an indicator

of whether a staffer switched out of coverage in her last year in Congress or remained above the threshold, and Years Since Exit$_{jt}$, a vector of dummy variables indicating the number of years since a staffer left Congress to become a lobbyist. If Hypothesis 4 holds, I expect β to be positive, whereas the interaction terms should yield negative coefficients θ.[32] Since ex-staffers' characteristic of being a switcher is constant across years as lobbyists, I cannot include lobbyist fixed effects. This raises concerns that unobserved staffer-level characteristics could bias the estimates. However, the fact that switchers and covered staffers are similar regarding their salaries and traits correlated with salaries may attenuate these concerns. Additionally, I still add all control covariates included in Equation (1) as well as year fixed effects. The covariates now correspond to staffers' characteristics in their last year in Congress. In addition, I account for staffers' log maximum pay throughout their Congressional careers.[33] I cluster standard errors on the lobbyist level and restrict the main sample to staffers leaving Congress and joining the lobbying sector after HLOGA took effect in September 2007. Staffers departing Congress in earlier years again serve as a placebo sample in Section 7.

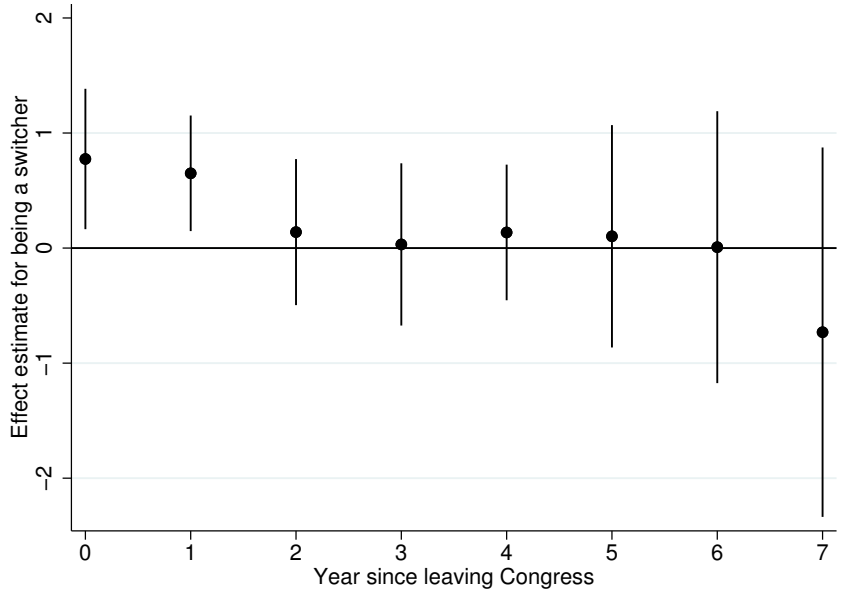
Figure 5 shows the marginal effect of being a switcher as compared to being covered by HLOGA by staffers' years after leaving Congress, holding all covariates in Equation (2) at their means.[34] Staffers who are willing to reduce their congressional salaries to below the cutoff in their last year in Congress attract significantly higher lobbying revenues than covered staffers-turned-lobbyists during the one-year cooling-off period. The estimated parameters of Equation (2) suggest that covered ex-staffers earn a revenue of \$98,481 from lobbying contracts ($\widehat{\log \text{Revenue}}_{jt} = 11.5$) immediately after exiting Congress (Years since Exit$_{jt} = 0$). Staffers who successfully switched below the threshold, in contrast, are estimated to earn \$215,038 in lobbying revenue ($\widehat{\log \text{Revenue}}_{jt} = 12.28$) in that year. Hence, staffers who avoid

[32] Since the cooling-off period is 12 months from the time staffers leave Congress and, therefore, covers part of the first year after exiting Congress, we should observe a significant drop in lobbyist revenue only for Years Since Exit$_{jt} \geq 2$.

[33] The results are robust to alternatively controlling for log average pay.

[34] Table D6 reports the full estimation results.

Figure 5: Average marginal effects of being a switcher on log lobbying revenue, by year since leaving Congress



Note: Depicted are average marginal effects of switching below the cutoff in the last year in Congress on annual lobbying revenues with 95% confidence intervals, based on model (3) in Table D6. The baseline is covered staffers earning salaries above the threshold in their last year in Congress. The sample includes congressional staffers joining the lobbying industry after HLOGA (excluding in-house lobbyists). Observations on the lobbyist-year level, $N = 1,950$.

coverage by HLOGA – and, therefore, may contact any former employers and colleagues in Congress – secure a premium in annual lobbying revenues of about $116,557. These figures are largely in line with previous estimates of the value of political connections in Washington's lobbying industry (Blanes i Vidal et al., 2012; Bertrand et al., 2014). Interestingly, while switchers receive a similar benefit in the year after leaving Congress, the premium drops considerably after the cooling-off period has expired in the second year after staffers leave Congress and it remains close to zero in later years. While this drop only reaches significance for some of the interactions (see Table D6), the clear and sudden reduction in the effect size provides some evidence for Hypothesis 4.[35] As soon as both covered and uncovered staffers-turned-lobbyists may leverage their contacts to Congress, lobbying clients

[35]Note that the insignificance mainly results from a power issue of the analysis. When expanding the sample to all staffers leaving in 2007 – which adds an additional 186 staffers-turned-lobbyists – the estimates are essentially identical, but the interactions reach significance at conventional levels for almost all interaction terms in the model.

are no longer willing to dig deep into their pockets for staffers who circumvented HLOGA.

Simple back-of-the-envelope calculations can help to further put these estimates into perspective. Staffers who switch out of coverage in their last year in Congress and are hired as lobbyists in the same year incur an average reduction of their inflation-adjusted annual salary of about $111,900[36] – an amount that is essentially identical to the premium in annual lobbying revenues they can secure through this strategic behavior. Hence, the market for connections in Washington seems to converge to an equilibrium in which lobbying firms almost perfectly match staffers' cost of avoiding post-employment restrictions.

To further reveal the mechanisms behind this revenue premium, Figure E4 shows the effect of being a switcher on lobbyists' total number of reports serviced per year and the average size of each report according to its revenue. Although the estimates are noisier for the number of lobbying reports, the results indicate that, in comparison to covered ex-staffers, switchers not only work on more lobbying contracts in their first years but also secure higher revenues for each lobbying activity, especially right after they leave Congress. This implies both that switchers lobby more and are compensated more for their work at the beginning of their lobbying careers.

7 Alternative Explanations and Robustness

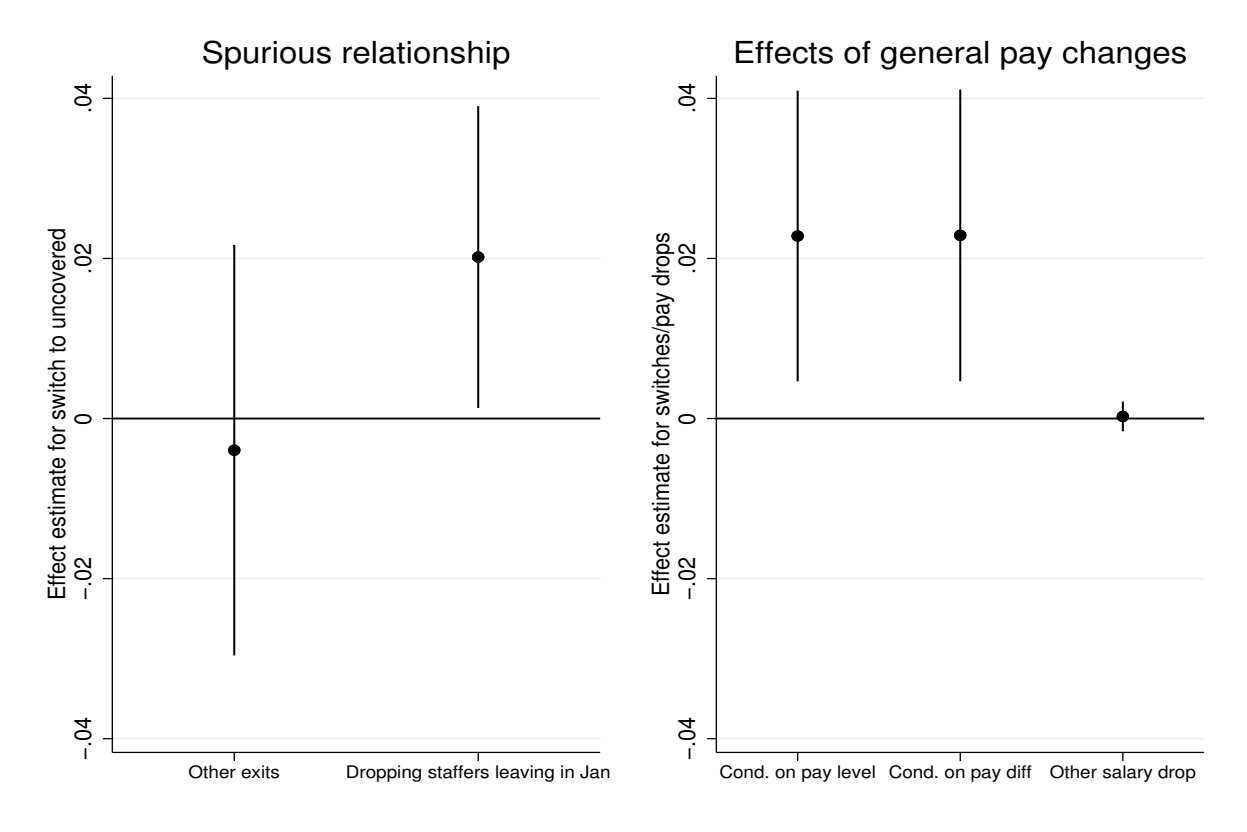
Since I analyze the effect of strategic behaviors of staffers, the switches out of coverage are by definition non-exogenous. I am, therefore, limited in my ability to identify the effect of strategic salary reductions on careers in the lobbying sector. However, I provide several checks that test my proposed mechanism and help to rule out alternative explanations.

With respect to the effect of switching below the cutoff on the propensity to become a lobbyist, one may be concerned that the estimated results are due to a spurious correlation between movements across the threshold and lobbying. There may be other reasons that

[36]The average salary drop for *all* switchers out of coverage post-HLOGA is $42,000. Figure E5 shows the entire distribution of salaries that switchers move to and from for non-administrative staffers.

make switching more likely toward the end of staffers' careers in Congress, (e.g., because their salaries become more variable in their final years). If the effects are in fact due to such mechanical relationships, we would expect that switching below the cutoff also predicts leaving Congress for non-lobbying jobs. Another concern is the turnover of congressional terms. Each Congress normally ends on January 2 and annual budgets for the new Congress are authorized on January 3 (Brudnick, 2018). The main analysis included staffers who left Congress due to congressional turnover in early January. These individuals might possibly drive the results if their annual salary drops in the last few days on Capitol Hill because their compensation is still covered under the budget of the previous Congress. The first plot in Figure 6 demonstrates the robustness of the main results against these alternative explanations. Switching below the salary cutoff does not predict staffers' exits for other reasons. Additionally, dropping staffers who leave Congress in early January does not affect the estimate of switching to uncovered on staffers' use of the revolving door.

Figure 6: Robustness of the effect of switching on becoming a lobbyist



A second alternative explanation for the effect of switching on becoming a lobbyist could be that pay cuts predict career decisions generally. Staffers who experience a drop in their salary may find outside career options, including lobbying, more attractive. To account for such alternative explanations, I first control for staffers' pay levels and year-to-year differences in pay. Additionally, I estimate the effect of general pay cuts that do not cross the threshold. If the mechanism of strategic salary manipulation holds, we should not find substantial effects of such general movements on staffers' compensation. The second plot in Figure 6 again shows that accounting for these concerns does not change the main results. Controlling for pay levels or pay differences leaves the effect estimate of switching essentially unchanged. Drops in staffers' salaries that do not cut the threshold, in contrast, are estimated to have a negligible and insignificant effect on staffers' probability to become lobbyists.

Further, I present additional results using alternative model specifications and estimation samples. To substantiate that the effects of switching on lobbying success are not driven by dynamics of non-switchers in the control group, Table D7 shows results for the sample of staffers who switch to below the cutoff at some point during their tenure in Congress. The point estimates are similar to the main results, although less precisely estimated in this substantially smaller sample of staffers. Next, I augment my main specification slightly to account for possible temporal dependence in the outcome variable of my binary time-series cross-sectional model (Beck et al., 1998). Instead of controlling for staffers' experience in Congress through a continuous variable, I add years worked in Congress as a series of dummy variables to the linear probability model in Equation (1). Reassuringly, the results are virtually unchanged (Table D8).

Finally, I run several placebo analyses. To bolster my claim about staffers' bunching behavior at the 75% salary threshold, Figure E6 shows McCrary density estimates for hypothetical discrete salary thresholds between 20% and 80% of an MC's annual salary. While we find significant jumps in the density of staffers at a few hypothetical cutoffs, it is also evident that the discontinuity is by far the largest at the true cutoff of 75%. I further es-

timate the panel models for years before HLOGA was discussed on the congressional floor and implemented in 2007 (Table D9). While the effect of switching out of coverage is still positive, it is smaller in size and insignificant across the specifications. Similarly, Figure E7 shows the effect of being a switcher on lobbying revenues for staffers leaving Congress before September 2007. The estimates are all statistically insignificant and mostly negative.

8 Conclusion

HLOGA has been one of the major attempts to slow the revolving door in Washington to date. In this study, I have argued that this lobbying reform has important unintended consequences: it incentivizes some congressional staffers to set their salaries just below the threshold to avert coverage by HLOGA and thus maintain attractive outside options in the lobbying industry. The evidence largely supports this view. After HLOGA was introduced, staffers sort below the 75% cutoff. However, this bunching is strongest for staffers with better outside options (i.e., committee staff and Senate staff). I further find that switching out of HLOGA coverage significantly increases staffers' chances to successfully walk through the revolving door to lobbying and clients seem to reward these staffers-turned-lobbyists with larger contracts and higher revenues in their first year as lobbyists.

These findings have at least three implications for public policy and our understanding of the political economy of special interests. First, the implications go beyond the narrow scope of the particular revolving-door regulations of HLOGA. Various revolving-door laws that US federal lawmakers have enacted since the 1970s have similar exemptions and allow discretion in how they are applied to different officials (Maskell, 2014). For executive branch officials, for example, the federal cooling-off periods only apply to certain senior personnel. Additionally, specific agencies, such as the SEC, managed to receive exemptions from post-employment restrictions for their senior officials and both the Obama and Trump administrations were heavily criticized when granting waivers of their own executive regulations to specific officials (Drutman, 2013). Similarly, while many other OECD countries, including the UK and Japan,

as well as several EU institutions, have introduced revolving-door regulations, their cooling-off periods often contain similar loopholes. For instance, the new Ethics Framework at the European Central Bank (ECB) restricts the revolving door for ECB staff, yet the regulation's intensity differs by salary band and task.[37] By highlighting the effects of such inconsistencies in the application of regulatory efforts, this study provides insights into the effectiveness and optimal design of revolving-door regulations.

Second, this study helps to better understand the labor market of public officials and the incentives to which they react. In particular, the findings highlight the robustness of career incentives provided by the lobbying market and speak to existing evidence that connectedness is a highly valuable asset on K Street (Blanes i Vidal et al., 2012; Bertrand et al., 2014; McCrain, 2018). If a strong imbalance of salaries on and off the Hill persists, staffers will likely continue to avoid revolving-door regulations to capitalize on their connections in the lobbying market.

Last, this study highlights a particular dilemma of institutional reforms that directly affects lawmakers and their staff. While the public outcry about the Abramoff scandal pushed revolving-door regulations onto the national agenda in 2007, MCs remained reluctant to close loopholes in the post-employment restrictions of HLOGA's final bill. To be clear, the evidence indicates that only a relatively small fraction of staffers actively avoid HLOGA's restrictions (796 staffers switched from covered to uncovered, and 3,674 staffers earned between 70-75% of a member's salary), and the reform may have somewhat slowed the revolving door overall (Cain and Drutman, 2014). Yet, the results also imply that the problem of self-regulation inherent in such reforms undermines far-reaching institutional change. While one can only speculate about the exact intentions of lawmakers in crafting revolving-door regulations, the attractiveness of shifting part of legislators' expenses to lobbying firms may have made staffers' sorting behavior an intended effect of the law rather than an accidental outcome. Hence, although the resulting incentive distortions for staffers

[37] https://eur-lex.europa.eu/legal-content/EN/TXT/PDF/?uri=CELEX:52015XB0620(01)&from=SK, accessed on 04/29/2022

could be avoided by removing arbitrary exceptions from revolving-door regulations, lawmakers may not have sufficient interest in doing so, thus pointing toward broader issues with such regulatory approaches.

Nevertheless, a few caveats about the analysis deserve attention. Due to limitations in cleanly identifying "sorters" (i.e., staffers who strategically avoid salary increases to remain below the threshold), this study could only examine the effect of switching out of coverage on staffers' lobbying careers as opposed to the effect of strategic salary manipulations more broadly. This complicates the analysis of lobbying revenues by reducing statistical power. Additionally, it may lead to an underestimation of the effect of salary manipulation on staffers' revolving doors because sorting staffers are included in the baseline for this analysis. While switching likely carries a stronger signaling effect than sorting, the results of this study should be interpreted with these caveats in mind and future research might aim at further teasing out the effect of different types of salary manipulations.

Additionally, several possible implications remain unexplored. For instance, the willingness of staffers to forgo salary increases may free up considerable amounts of resources. How do MCs and congressional offices take advantage of this bump in their budgets? Are these additional resources spent on other personnel or redistributed to other types of expenditures? Additionally, the incentive distortions of HLOGA may have further repercussions for the selection and performance of staffers in Congress. On the one hand, by providing incentives for career-oriented staffers above the threshold to self-select out of their positions, HLOGA may have decreased the talent pool among senior staff. On the other hand, by imposing restrictions on staffer salaries, HLOGA may have made congressional positions relatively more attractive for publicly minded personnel with higher efficacy and motivation for congressional work. Hence, prima facie it remains unclear whether and how HLOGA affected the selection of congressional staff and the legislative productivity of offices employing them. I leave these additional questions for future research.

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Appendix: Supporting Information for *Sorting for K Street*

A Further Background on HLOGA

The Ethics Reform Act of 1989 enacted, for the first time, statutory post-employment regulations for certain legislative branch officials. These limitations were codified at section 207 of the Federal Criminal Code (Title 18) and went into effect with the swearing of the 102nd Congress in January 1991. The cooling-off period specified that for one year after leaving office, a covered former legislative employee may not "knowingly [make], with the intent to influence, any communication to or appearance before [the specific Member/committee/office for whom that former staffer was an employee] on behalf of any other person (except the United States) in connection with any matter" on which the former officeholder seeks official action (Select Committee on Ethics, 2003). Even before HLOGA, the law applied only to Members of Congress, officers, and employees who earn at a rate of pay at least 75% of a Member's annual salary.

Yet, HLOGA strengthened these rules and their enforcement in multiple ways. First, §101 of HLOGA amended §207 of US Code 18 such that all elected officers and staff of the Senate are now prohibited from contacting *any* Senator or *any* officer or employee of the Senate with the intent to influence and seek action by the Senate. Second, §103 of HLOGA prescribes that the Clerk of the House, after consultation with the Committee on Standards of Official Conduct, or the Secretary of the Senate, need to notify covered officials of the beginning and ending date of the prohibitions that apply under §207(e) of US Code 18. As a result, the Clerk of the House and the Secretary of the Senate now disclose all covered officials together with the exact dates of coverage on their public website. Third, §301 and §532 of HLOGA amends the Rules of the House and the Standing Rules of the Senate, respectively, and determines that covered employees of either chamber need to notify the relevant Ethics Committee that they are negotiating or have an agreement of future employment of compensation. By strengthening the disclosure requirements for covered officials as well as their awareness of the regulations and penalties, these additional regulations significantly improved the enforceability of post-employment restrictions in Congress. It is important to note that covered employees are limited in their contacts regardless of whether they become, or are employed by, *registered* lobbyists.

Following HLOGA, the statute stipulates that a covered former employee may not, for a period of one year after leaving Congress:

- Knowingly communicate with or appear before the employee's former employing office or committee – or for Senate employees any Senator or any officer or employee of the Senate – with the intent to influence, on behalf of any other person, the official actions or decisions by a Member, officer, or employee in such office or committee (§207(e)(3)-(7)).

- Knowingly represent a foreign government or foreign political party before any federal official (including any Member of Congress) with the intent to influence a decision of such official in official duties (§§207(f)(1)(A) and (i)(1)(B)).

- Knowingly aid or advise a foreign government or foreign political party with the intent to influence a decision of any federal official (including Members of Congress) in carrying out their official duties (§207(f)(1)(B)).

Besides the official notification of coverage at the time of leaving Congress, staffers receive information about the relevant restrictions and penalties through various means. First, all new Members, officers and employees of both House and Senate need to receive a compulsory ethics training within 60 days of their start date in Congress.[1] Additionally, following HLOGA both the Senate Select Committee on Ethics and House Committee on Ethics published multiple memoranda that detailed the exact regulations together with the coverage rules and exceptions.[2]

B Data Description

This paper uses two primary sources of data: 1) LegiStorm's congressional staff salaries data and 2) OpenSecret's lobbying disclosure data.

The original congressional salary records are detailed in the quarterly House and biannual Senate disbursement books and represent salary payments made during the particular time period in which a staffer was listed on the congressional payroll. These payments can include bonuses in addition to base salary payments; reimbursed expenses and benefits are excluded. LegiStorm –a non-partisan, for-profit organization that researches and publishes information about Members of Congress and congressional staff (http://www.legistorm.com) – has collected and cleaned the compensation records of all congressional staffers since 1998. In order to resolve the numerous discrepancies and inconsistencies in the raw data, LegiStorm individually verifies all congressional staffers' names (including the various variants used across records), checks the employment information on staffers' offices and harmonizes title information. Due to the extensive manual checking done by LegiStorm, this data source is the most accurate, timely and comprehensive database for contact and biographical data on congressional staff used in the literature to date (Bertrand et al., 2014; Cain and Drutman, 2014; McCrain, 2018; Shepherd and You, 2020; Ritchie and You, 2021). As illustrated in the main text, the LegiStorm data includes salary disbursement data, information on staffers' names, their titles, and the offices for which they worked, as well as some background information, such as education and awards won. From the overall 1,282,854 records of staff employees across the US House and Senate in the full data, I retain 59,471 full-time staff who worked in Congress between 2001 and 2016.

[1] https://www.ethics.senate.gov/public/index.cfm/ethics-faqs;
https://ethics.house.gov/legislation/schedule/faqs-about-training

[2] e.g. see
https://www.ethics.senate.gov/public/_cache/files/bf9ea0f9-2593-4f49-83b3-f581f86b9098/guidance-on-the-post-employment-contact-ban.pdf;
https://ethics.house.gov/sites/ethics.house.gov/files/Members%20and%20Officers.pdf

To identify staffer-turned-lobbyists, I merge this set of congressional staffers to the list of registered lobbyists included in quarterly lobbying disclosure reports since 1998 (obtained from OpenSecrets). I first build on (Shepherd and You, 2020) who identified 4,696 individual lobbyists with prior work experience in Congress. Since these authors only focus on personal staffers and consider the revolving door only up until 2016, I expand their list of staffers-turned-lobbyists in the following way. First, I identify lobbyists whose records include keywords related to congressional positions when disclosing their previous employment. In particular, I search for

> *SENAT*, *SEN.*, *SEN *, *REP *, *REP.*, *TRADE REP.*, *REPRESENTATIVE*, *TRADE REPRESENTATIVE*, *REPRESENTATIVE OF*, *LEGISLATIVE REPRESENTATIVE*, *AA*, *AIDE*, *CHAIR*, *LEG FELLOW OFFICE*, *STAFF*, *LEGISLATIVE DIRECTOR*, *LEG ASST*, *LEGISLATIVE ASST*, *COMMITTEE*, *COMM.*, *CMTE*, *HASC*, *HOUSE*, *WHITE HOUSE*, *CONGRESS *, *CONGRESSMAN*, *CONGRESSWOMAN*, *MEMBER OF CONGRESS*, *MBR OF CONG*, *WHIP*, *DEMOCRATIC*, *REPUBLICAN*, *SPEAKER*

Next, I merge this set of lobbyists to the full list of congressional staffers using a fuzzy name merge (Stata's `matchit` command). For each lobbyist, I retain the record in LegiStorm that returns the maximum similarity scores between names in OpenSecrets and LegiStorm (based on the bigram method in `matchit`). I then manually check each of these merges and verify that the match is correct based on the exact past employment information about lobbyists and their positions in Congress included in the LegiStorm data. I only retain matches where the office or MC listed in the lobbyist's past employment information is also included in the congressional records and the names overlap substantially (apart from some differences in spelling). For female lobbyists with past congressional employment information who could not be matched in this way (allegedly due to name changes through marriage), I further check staffers with identical first names, search for individuals online, and verify whether their employment records overlap with these staffers with the same first name but different last name. This procedure yields a total of 5,040 unique lobbyists who also appeared in the congressional records between 2001 and 2016.

To determine a staffer's HLOGA coverage status, I largely follow (Cain and Drutman, 2014) and classify a staffer as covered by the revolving-door regulation if her daily pay rate is at least 75% of a member's daily pay rate throughout a calendar year. While this disregards HLOGA's condition that a staffer needs to earn above this salary threshold for at least 60 days in a year, it provides a conservative measure of a staffer's decision to switch out of coverage and allows me to build a consistent staffer-year panel. To evaluate the robustness of this coding decision, I also provide evidence in Table D10, Table D11 and Figure E8 using two alternative treatment codings that account for the days of coverage: 1) A staffer is covered if she earned at least 75% of a member's daily salary during any employment period for at least 60 days in a calendar year and 2) a staffer is covered if she earned at least 75% of a member's annual salary or if she was covered in December of the previous calendar year. The later accounts for the fact that staffers may be moved above the threshold by end-of-year bonus payments. Additionally, I verified my coding using the names of covered

staff disclosed by the US House Office of the Clerk and the Secretary of the US Senate.[3] I used a similar iterative fuzzy name matching procedure to match the congressional records to this list of staffer names. For the years 2008-2016, this yields 2,767 unique staffers that appear in both the LegiStorm records and the congressional post-employment notifications data. Reassuringly, in 79% of cases my coding of the coverage in a staffer's final year in Congress coincides with the list of covered staff disclosed by Congress.

C Comparison to Cain and Drutman (2014)

Cain and Drutman (2014) examine the effectiveness of the post-employment rules of HLOGA in slowing the revolving door between Congress and the lobbying industry. Similar to this article, the authors exploit the one-year ban on contacts of ex-staffers with their former colleagues in Congress to estimate the effect of HLOGA on lobbying registration rates of Congressional staffers. Cain and Drutman (2014) use a difference-in-differences (DiD) design and LegiStorm data between 2001 and 2011 to compare lobbying registration among "covered" staff earning 75% or more of a member's salary (the treatment group) to registration among "high-level" staff making between 60% and 75% of a member's annual pay (the control group). The authors find that the tendency to register as a lobbyist within a year of leaving Congress declines more for covered staff than uncovered staff relative to the pre-HLOGA period. This decline is strongest for committee staff, Senate staff and majority party staff. Additionally, the authors show some substitution effects in the lobbying market, i.e. post HLOGA demand for high-level uncovered Senate committee staffers increased.

Cain and Drutman (2014) make important contributions to the literature on the revolving door and the effectiveness of respective regulations. In particular, the authors shed light on whether one of the most ambitious revolving-door regulations to date achieves its intended effects of slowing the flow of public officials into the lobbying industry. Additionally, Cain and Drutman (2014) add to existing evidence that connections and personal contacts tend to be of higher value in the lobbying market than policy expertise.

The DiD design and results in Cain and Drutman (2014) rest on two important assumptions. First, as the authors acknowledge, they treat the treatment and control groups as exogenous. If HLOGA creates perverse incentives for staffers to manipulate their salary to move from covered to uncovered status, the implied effectiveness of HLOGA in reducing lobbying registration among covered staff may be an artifact of movements between treatment and control groups rather than an actual reduction in the revolving door. Second, the DiD design treats the pre-HLOGA years as control period. However, as noted in Appendix A of this article, the revolving door restriction was already on the books since the Ethics Reform Act in 1991. Thus, the DiD analysis provides evidence on the effect of *strenghtening* revolving-door regulations rather than imposing new rules.

This article builds on and moves beyond Cain and Drutman (2014) in various ways. First, my research differs in its contribution and approach. While Cain and Drutman (2014) evaluate the overall effectiveness of HLOGA, I uncover strategic reactions in the behavior of the regulated group. That is, instead of using the coverage of HLOGA at face value, I focus

[3] https://disclosures-clerk.house.gov/PublicDisclosure/PostEmploymentNotification; https://www.senate.gov/legislative/termination_disclosure/report2018.htm

on *changes* in coverage by HLOGA, measured by staffers' sorting around and movement below the salary threshold. My aim is to capture the value of staffers' signal to the lobbying market (i.e. avoiding coverage) beyond the value of their political connections.

Second, I employ different methodology. While I rely on pre-HLOGA years for placebo analyses, I do not incorporate them in a DiD design for several reasons. First, my primary interest lies in the *marginal* effect of strategic behavior (switching out of coverage) on the probability of moving into lobbying post-HLOGA rather than the *difference* in the effect of switching across periods. Since the cooling-off period technically existed before HLOGA, staffers' potential strategic wage setting before HLOGA – albeit to a lesser degree – could weaken estimated differences in effect sizes and thus distract from the significant relationship of switching and lobbying success after HLOGA. Second, the validity of a DiD design heavily relies on the parallel trends assumption. However, unlike the usual DiD setting with fixed group assignments, my treatment is time-variant, and since my outcome is exiting Congress for lobbying, staffers who switch in year t necessarily have values of zero in the outcome variable for years $t-1, t-2, ...$ to be observed in my sample. Pre-trends in the probability of exiting between staffers switching in year t and those not switching in year t are thus mechanically parallel and equal to zero. Finally, given the limited number of sorting and switching staff together with the high turnover rate of staffers, a DiD setting poses several challenges for inference. A *within-staffer* DiD setup estimates the difference in the effect of switching on staffers' propensity to exit for lobbying across periods *for a given staffer*, i.e. only staffers with variation in the DiD terms contribute to the DiD estimate. Hence, staffer fixed effects reduce the effective sample size to only a few staffers who switched out of coverage *both* before and after HLOGA. Similarly, the limited number of switchers before HLOGA (430 compared to 796 after HLOGA) cause substantial power issues in a DiD setting. Hence, instead of DiD analyses, I rely on McCrary (2008) density estimates, fixed effects models and a within-staffer design, which allows me to account for any staffer-specific determinants of their revolving door.

Third, the implications we draw from this article are substantially different. Cain and Drutman (2014) provide key insights in the overall effectiveness of revolving-door regulations, such as HLOGA. This article, in contrast, uncovers an important dilemma of self-regulation in government by highlighting how accountability reforms like HLOGA can elicit regulatory evasion of regulated officials. Importantly, as discretionary cutoffs and loopholes are present in many regulations of money in politics, this finding may have broader implications for the success of ethics rules and reforms.

D Tables

Table D1: Summary Statistics by Year

Year	No. Staff	Av. Annual Compensation ($)	Mean Change in Annual Pay Rate (%)	Switches to uncovered status (%)	Turnover (%)	Lobbying (%)
2001	18,291	63,220	-	-	9.0	0.3
2002	19,547	63,219	15.7	0.4	17.5	0.6
2003	20,117	60,805	25.8	0.4	21.4	1.0
2004	18,506	65,149	12.0	0.3	14.1	1.0
2005	19,981	61,455	20.3	0.4	18.4	1.2
2006	19,049	64,754	12.7	0.6	15.2	1.2
2007	20,398	61,139	32.3	0.7	20.4	1.9
2008	18,833	66,765	11.8	0.7	13.3	1.2
2009	20,237	65,218	29.0	0.6	16.2	1.1
2010	19,274	70,012	10.2	0.4	13.6	1.0
2011	20,642	60,754	61.2	0.8	20.9	1.5
2012	18,297	66,290	9.9	0.5	13.7	0.9
2013	19,021	60,227	18.1	0.8	20.3	1.4
2014	17,477	64,494	13.3	0.3	13.9	1.1
2015	18,538	61,610	24.8	0.5	17.6	1.1
2016	16,358	62,111	10.7	0.4	-	2.0

Note: The table shows statistics for all full-time staff on the congressional payroll for 2001-2016. Observations on the staffer-year level. Average Annual Compensation is mean annual salary in 2015-dollar terms. Mean Change in the Annual Pay Rate is the average of the absolute year-to-year percentage change in inflation-adjusted annual pay by staffer. Switches to uncovered status is the share of staffers who moved from being covered in the previous year to being uncovered by HLOGA in a given year. Turnover is the rate of staffers in each year who do not appear on the payroll in the next year. Lobbying gives the share of departing staff who became registered lobbyists within one year.

Table D2: Regression Models for Switching Across Threshold

	Switch to covered from 65-75% pay		Switch to uncovered from 75-90% pay	
	(1)	(2)	(3)	(4)
After HLOGA	-0.032***	-0.028**	0.014**	0.015**
	(0.010)	(0.010)	(0.005)	(0.005)
Controls		✓		✓
Mean of DV	0.320	0.320	0.081	0.081
Observations	9,751	9,751	10,846	10,846
R^2	0.001	0.008	0.001	0.027

Note: Linear probability OLS regressions of switching. Controls include days worked per calendar year and indicators for committee staff, personal staff, DC office staff and leadership office staff. Dependent variable: Dummy for switch across the threshold. Robust standard errors in parentheses. *** p<0.001, ** p<0.01, * p<0.05

Table D3: Regression Models for Becoming Lobbyist - Main Specification

	2008-2016			2007-2016		
	(1)	(2)	(3)	(4)	(5)	(6)
Switch to Uncovered	0.030***	0.019*	0.022*	0.032***	0.021**	0.022*
	(0.009)	(0.008)	(0.009)	(0.008)	(0.008)	(0.009)
Days Worked		-0.000***	-0.000***		-0.000***	-0.000***
		(0.000)	(0.000)		(0.000)	(0.000)
Committee Staff		-0.005	-0.005		-0.008**	-0.007**
		(0.003)	(0.003)		(0.003)	(0.003)
Personal Staff		-0.011*	-0.014**		-0.013***	-0.016***
		(0.004)	(0.004)		(0.004)	(0.004)
Senate Staff		-0.000	-0.001		0.000	-0.001
		(0.003)	(0.003)		(0.003)	(0.003)
Majority Party Staff		-0.003	-0.002		-0.003	-0.001
		(0.003)	(0.003)		(0.003)	(0.003)
Minority Party Staff		-0.011***	-0.009**		-0.011***	-0.008**
		(0.003)	(0.003)		(0.003)	(0.003)
DC Office Staff		0.007***	0.008***		0.007***	0.008***
		(0.001)	(0.002)		(0.001)	(0.002)
Leadership Office Staff		0.000	0.000		-0.002	-0.003
		(0.005)	(0.005)		(0.004)	(0.004)
Hill Experience			0.003***			0.003***
			(0.000)			(0.000)
Hill Experience sq.			0.000			-0.000
			(0.000)			(0.000)
Staffer FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Mean of DV	0.014	0.014	0.015	0.015	0.015	0.015
Observations	143,745	143,745	128,067	159,890	159,890	140,194
Number of staffers	37,744	37,744	34,438	41,264	41,264	36,921
R^2	0.014	0.056	0.059	0.012	0.058	0.061

Note: Linear probability OLS regressions with staffer and year fixed effects (not reported); all models include a constant. Dependent variable: Lobbying$_{it}$. Standard errors clustered by staffer in parentheses. *** p<0.001, ** p<0.01, * p<0.05

Table D4: Regression Models: Switching Below Threshold and Demotions in Rank

	(1)	(2)
Switch to Uncovered	0.064***	0.062***
	(0.011)	(0.011)
Staffer FE	✓	✓
Year FE		✓
Mean of DV	0.022	0.022
Observations	84,303	84,303
Number of staffers	27,162	27,162
R^2	0.002	0.006

Note: Linear probability OLS regressions of demotions in title ranks for staffers switching to below the cutoff post-HLOGA. Rank information obtained from Ritchie and You (2021). Dependent variable: Dummy for demotion in rank. Standard errors clustered by staffer in parentheses. *** p<0.001, ** p<0.01, * p<0.05

Table D5: Intraclass Correlation Coefficients for Staffer Random Intercepts on Annual Salary

Rank	Title Group	ICC
Rank 1	Chief of Staff	0.224*** (0.081)
Rank 2	Deputy Chief of Staff	0.382*** (0.024)
Rank 3	State/District Director	0.485*** (0.014)
Rank 4	Legislative Director	0.509*** (0.017)
Rank 5	Communications Director	0.460*** (0.012)
Rank 6	Legislative Assistant	0.605*** (0.006)
Rank 7	Legislative Correspondent/ Deputy Press Secretary/ Specials Director/ Deputy State/District Director	0.530*** (0.008)
Rank 8	Executive Assistant/ Office Manager/ Caseworker/ Staff Assistant	0.703*** (0.003)

Note: Intraclass correlation coefficients (with standard errors in parentheses) for staffer random intercepts, obtained from multi-level mixed effects models by staffer ranks. Rank information obtained from Ritchie and You (2021). Dependent variable: Inflation adjusted annual pay. *** p<0.001, ** p<0.01, * p<0.05

Table D6: Regression Models for Lobbying Revenue

	(1)	(2)	(3)
Switcher	0.936**	0.922**	0.774*
	(0.312)	(0.306)	(0.310)
Year Since Leaving Congress = 1	0.634***	0.672***	0.765***
	(0.135)	(0.144)	(0.139)
Year Since Leaving Congress = 2	1.066***	1.090***	1.133***
	(0.160)	(0.195)	(0.197)
Year Since Leaving Congress = 3	1.220***	1.247***	1.311***
	(0.161)	(0.195)	(0.195)
Year Since Leaving Congress = 4	1.300***	1.312***	1.400***
	(0.177)	(0.213)	(0.204)
Year Since Leaving Congress = 5	1.219***	1.201***	1.299***
	(0.199)	(0.249)	(0.242)
Year Since Leaving Congress = 6	1.139***	1.126***	1.269***
	(0.238)	(0.305)	(0.299)
Year Since Leaving Congress = 7	1.309***	1.241***	1.308***
	(0.204)	(0.302)	(0.296)
Year Since Leaving Congress = 8	1.226***	1.146**	1.143**
	(0.257)	(0.356)	(0.395)
Year Since Leaving Congress = 9	0.917*	0.866	0.683
	(0.448)	(0.544)	(0.645)
Year Since Leaving Congress = 10	0.825	0.752	0.635
	(0.562)	(0.692)	(0.800)
Year Since Leaving Congress = 11	1.939***	1.778***	1.697***
	(0.253)	(0.338)	(0.365)
Switcher x Year Since Leaving Congress = 1	-0.251	-0.233	-0.124
	(0.316)	(0.321)	(0.309)
Switcher x Year Since Leaving Congress = 2	-0.614	-0.626	-0.635
	(0.413)	(0.411)	(0.452)
Switcher x Year Since Leaving Congress = 3	-0.757*	-0.740*	-0.743*
	(0.325)	(0.332)	(0.377)
Switcher x Year Since Leaving Congress = 4	-0.640	-0.618	-0.639
	(0.358)	(0.359)	(0.417)
Switcher x Year Since Leaving Congress = 5	-0.706	-0.657	-0.672
	(0.454)	(0.455)	(0.581)
Switcher x Year Since Leaving Congress = 6	-0.516	-0.505	-0.767
	(0.513)	(0.514)	(0.658)
Switcher x Year Since Leaving Congress = 7	-0.870	-0.921	-1.506
	(0.588)	(0.601)	(0.847)
Switcher x Year Since Leaving Congress = 8	-0.330	-0.200	-0.813
	(0.537)	(0.537)	(0.623)
Switcher x Year Since Leaving Congress = 9	-0.206	-0.244	-1.044
	(0.708)	(0.728)	(0.732)
Switcher x Year Since Leaving Congress = 10	0.778	0.703	
	(0.625)	(0.632)	
Year FE		✓	✓
Mean of DV	13.570	13.570	13.610
Observations	2200	2200	1950
R^2	0.042	0.047	0.106

Note: OLS regressions of log annual lobbying revenue on staffer characteristics and year fixed effects; Model (3) includes covariates described in Equation (2); all models include a constant. Dependent variable: $\log R_{jt}$. Standard errors clustered by lobbyist in parentheses. *** p<0.001, ** p<0.01, * p<0.05

Table D7: Regression Models for Becoming Lobbyist - Switching staffers only

	2008-2016			2007-2016		
	(1)	(2)	(3)	(4)	(5)	(6)
Switch to Uncovered	0.029***	0.012	0.016	0.032***	0.014	0.016
	(0.009)	(0.008)	(0.009)	(0.008)	(0.008)	(0.008)
Days Worked		-0.000***	-0.000***		-0.000***	-0.000***
		(0.000)	(0.000)		(0.000)	(0.000)
Committee Staff		-0.006	-0.005		-0.007	-0.008
		(0.009)	(0.010)		(0.009)	(0.010)
Personal Staff		0.016	0.004		0.021	0.007
		(0.020)	(0.020)		(0.018)	(0.019)
Senate Staff		-0.013	-0.017		-0.004	-0.003
		(0.013)	(0.014)		(0.013)	(0.013)
Majority Party Staff		-0.013	-0.011		-0.010	-0.009
		(0.015)	(0.016)		(0.014)	(0.015)
Minority Party Staff		-0.011	-0.009		-0.012	-0.012
		(0.016)	(0.016)		(0.015)	(0.015)
DC Office Staff		-0.009	-0.003		-0.008	0.000
		(0.010)	(0.010)		(0.009)	(0.009)
Leadership Office Staff		-0.010	-0.001		-0.010	-0.005
		(0.015)	(0.014)		(0.013)	(0.013)
Hill Experience			0.007**			0.007**
			(0.003)			(0.002)
Hill Experience sq.			-0.000			-0.000
			(0.000)			(0.000)
Staffer FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Mean of DV	0.022	0.022	0.023	0.023	0.023	0.022
Observations	5,086	5,086	4,440	,5815	5,815	5,014
Number of staffers	928	928	790	1,011	1,011	835
R^2	0.026	0.089	0.095	0.024	0.088	0.092

Note: Linear probability OLS regressions with staffer and year fixed effects (not reported); all models include a constant. Dependent variable: Lobbying$_{it}$. Standard errors clustered by staffer in parentheses. *** p<0.001, ** p<0.01, * p<0.05

Table D8: Regression Models for Becoming Lobbyist - Event History Analysis

	2008-2016	2007-2016
	(1)	(2)
Switch to Uncovered	0.022*	0.021*
	(0.009)	(0.009)
Days Worked	-0.000***	-0.000***
	(0.000)	(0.000)
Committee Staff	-0.005	-0.007**
	(0.003)	(0.003)
Personal Staff	-0.013**	-0.016***
	(0.004)	(0.004)
Senate Staff	-0.002	-0.001
	(0.003)	(0.003)
Majority Party Staff	-0.002	-0.001
	(0.003)	(0.003)
Minority Party Staff	-0.008**	-0.008**
	(0.003)	(0.003)
DC Office Staff	0.008***	0.008***
	(0.002)	(0.002)
Leadership Office Staff	0.000	-0.003
	(0.005)	(0.004)
Staffer FE	✓	✓
Year FE	✓	✓
Duration FE	✓	✓
Mean of DV	0.015	0.015
Observations	128067	140194
Number of staffers	34438	36921
R^2	0.060	0.063

Note: Linear probability OLS regressions with staffer and year fixed effects (not reported); all models include a constant. Dependent variable: Lobbying$_{it}$. Standard errors clustered by staffer in parentheses. *** p<0.001, ** p<0.01, * p<0.05

Table D9: Placebo Regressions for Becoming Lobbyist

	2001-2006		
	(1)	(2)	(3)
Switch to Uncovered	0.015	0.010	0.039
	(0.014)	(0.013)	(0.021)
Days Worked		-0.000***	-0.000***
		(0.000)	(0.000)
Committee Staff		-0.004	-0.000
		(0.004)	(0.005)
Personal Staff		-0.014*	-0.005
		(0.006)	(0.008)
Senate Staff		0.001	0.003
		(0.005)	(0.006)
Majority Party Staff		0.003	0.001
		(0.005)	(0.007)
Minority Party Staff		0.003	0.002
		(0.005)	(0.007)
DC Office Staff		-0.007***	-0.007***
		(0.001)	(0.002)
Leadership Office Staff		-0.005	-0.010
		(0.006)	(0.008)
Hill Experience			0.005***
			(0.001)
Hill Experience sq.			0.000***
			(0.000)
Staffer FE	✓	✓	✓
Year FE	✓	✓	✓
Mean of DV	0.012	0.012	0.014
Observations	80,797	80,797	41,776
Number of staffers	29,056	29,056	16,278
R^2	0.015	0.067	0.111

Note: Linear probability OLS regressions with staffer and year fixed effects (not reported); all models include a constant. Dependent variable: Lobbying$_{it}$. Standard errors clustered by staffer in parentheses. *** p<0.001, ** p<0.01, * p<0.05

Table D10: Regression Models for Becoming Lobbyist - Alternative Treatment Coding: Incorporate Days Covered

	2008-2016			2007-2016		
	(1)	(2)	(3)	(4)	(5)	(6)
Switch to Uncovered	0.064***	0.036***	0.040***	0.066***	0.037***	0.039***
(inc. days)	(0.006)	(0.006)	(0.007)	(0.006)	(0.006)	(0.006)
Days Worked		-0.000***	-0.000***		-0.000***	-0.000***
		(0.000)	(0.000)		(0.000)	(0.000)
Committee Staff		-0.006*	-0.006*		-0.008**	-0.008**
		(0.003)	(0.003)		(0.003)	(0.003)
Personal Staff		-0.011*	-0.014**		-0.013***	-0.016***
		(0.004)	(0.004)		(0.004)	(0.004)
Senate Staff		-0.000	-0.001		0.001	-0.000
		(0.003)	(0.003)		(0.003)	(0.003)
Majority Party Staff		-0.003	-0.002		-0.003	-0.001
		(0.003)	(0.003)		(0.003)	(0.003)
Minority Party Staff		-0.011***	-0.009**		-0.011***	-0.008**
		(0.003)	(0.003)		(0.003)	(0.003)
DC Office Staff		0.007***	0.008***		0.007***	0.008***
		(0.001)	(0.002)		(0.001)	(0.002)
Leadership Office Staff		-0.000	-0.001		-0.002	-0.003
		(0.005)	(0.005)		(0.004)	(0.004)
Hill Experience			0.003***			0.003***
			(0.000)			(0.000)
Hill Experience sq.			0.000			-0.000
			(0.000)			(0.000)
Staffer FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Mean of DV	0.014	0.014	0.015	0.015	0.015	0.015
Observations	143,745	143,745	128,067	159,890	159,890	140,194
Number of staffers	37,744	37,744	34,438	41,264	41,264	36,921
R^2	0.018	0.057	0.060	0.017	0.059	0.063

Note: Linear probability OLS regressions with staffer and year fixed effects (not reported); all models include a constant. Dependent variable: Lobbying$_{it}$. Standard errors clustered by staffer in parentheses. *** p<0.001, ** p<0.01, * p<0.05

Table D11: Regression Models for Becoming Lobbyist - Alternative Treatment Coding: Incorporate December Previous Year

	2008-2016			2007-2016		
	(1)	(2)	(3)	(4)	(5)	(6)
Switch to Uncovered (adj)	0.012*	0.008	0.012	0.011*	0.007	0.009
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Days Worked		-0.000***	-0.000***		-0.000***	-0.000***
		(0.000)	(0.000)		(0.000)	(0.000)
Committee Staff		-0.005	-0.005		-0.008**	-0.007**
		(0.003)	(0.003)		(0.003)	(0.003)
Personal Staff		-0.011*	-0.014**		-0.013***	-0.016***
		(0.004)	(0.004)		(0.004)	(0.004)
Senate Staff		-0.000	-0.001		0.000	-0.001
		(0.003)	(0.003)		(0.003)	(0.003)
Majority Party Staff		-0.003	-0.002		-0.003	-0.001
		(0.003)	(0.003)		(0.003)	(0.003)
Minority Party Staff		-0.011***	-0.008**		-0.011***	-0.008**
		(0.003)	(0.003)		(0.003)	(0.003)
DC Office Staff		0.007***	0.008***		0.007***	0.008***
		(0.001)	(0.002)		(0.001)	(0.002)
Leadership Office Staff		0.000	0.000		-0.002	-0.003
		(0.005)	(0.005)		(0.004)	(0.004)
Hill Experience			0.003***			0.003***
			(0.000)			(0.000)
Hill Experience sq.			0.000			-0.000
			(0.000)			(0.000)
Staffer FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Mean of DV	0.014	0.014	0.015	0.015	0.015	0.015
Observations	143,745	143,745	128,067	159,890	159,890	140,194
Number of staffers	37,744	37,744	34,438	41,264	41,264	36,921
R^2	0.013	0.055	0.059	0.012	0.058	0.061

Note: Linear probability OLS regressions with staffer and year fixed effects (not reported); all models include a constant. Dependent variable: Lobbying$_{it}$. Standard errors clustered by staffer in parentheses. *** p<0.001, ** p<0.01, * p<0.05

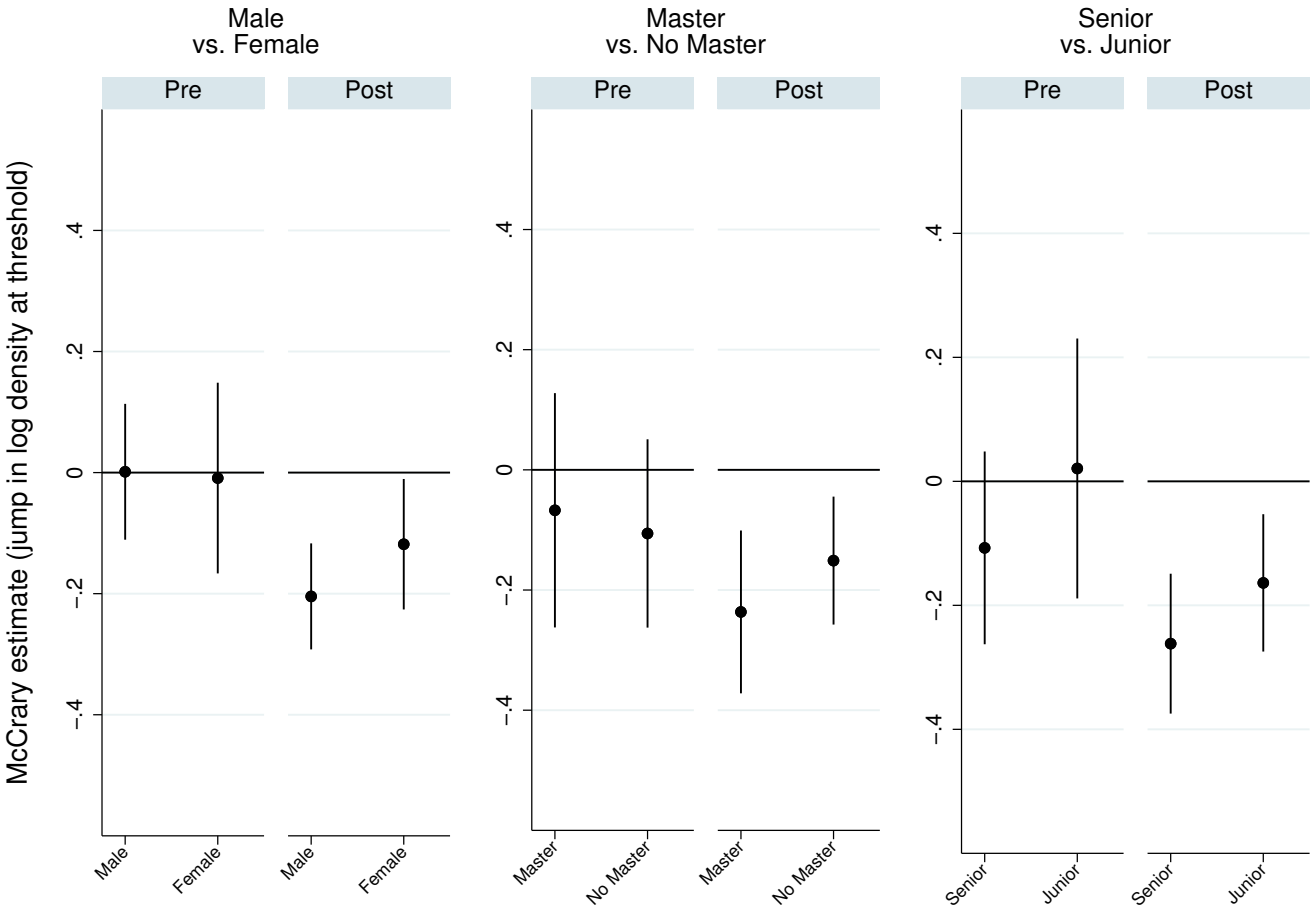
Table D12: Regression Models for Becoming Lobbyist - DiD Specification

	(1)	(2)	(3)	(4)	(5)
Switch to Uncovered × Post-HLOGA	0.011	0.028†	0.028†	0.017	-0.014
	(0.015)	(0.015)	(0.015)	(0.014)	(0.019)
Switch to Uncovered	0.038**	0.012	0.013	0.009	0.038*
	(0.012)	(0.012)	(0.012)	(0.012)	(0.017)
Post-HLOGA	0.003***	0.066***	0.065***	0.025***	-0.018
	(0.000)	(0.002)	(0.002)	(0.002)	(0.022)
Committee Staff			-0.012***	-0.004†	-0.003
			(0.002)	(0.002)	(0.002)
Personal Staff			-0.021***	-0.016***	-0.020***
			(0.003)	(0.003)	(0.003)
Senate Staff			0.000	0.001	-0.000
			(0.002)	(0.002)	(0.002)
Majority Party Staff			-0.007**	-0.001	0.001
			(0.002)	(0.002)	(0.002)
Minority Party Staff			-0.009***	-0.008***	-0.006*
			(0.002)	(0.002)	(0.003)
DC Office Staff			0.005***	0.006***	0.009***
			(0.001)	(0.001)	(0.001)
Leadership Office Staff			-0.005	0.001	-0.000
			(0.003)	(0.003)	(0.004)
Days Worked				-0.000***	-0.000***
				(0.000)	(0.000)
Hill Experience					0.006***
					(0.002)
Hill Experience sq.					-0.000***
					(0.000)
Year FE		✓	✓	✓	✓
Staffer FE		✓	✓	✓	✓
Mean of DV	0.014	0.014	0.014	0.014	0.015
Observations	240,687	240,687	240,687	240,687	181,970
Number of staffers		55,604	55,604	55,604	42,707
R^2	0.001	0.013	0.014	0.064	0.074

Note: Linear probability OLS regressions with staffer and year fixed effects (not reported); all models include a constant. Dependent variable: Lobbying$_{it}$. Standard errors clustered by staffer in parentheses. *** p<0.001, ** p<0.01, * p<0.05, † p<0.01

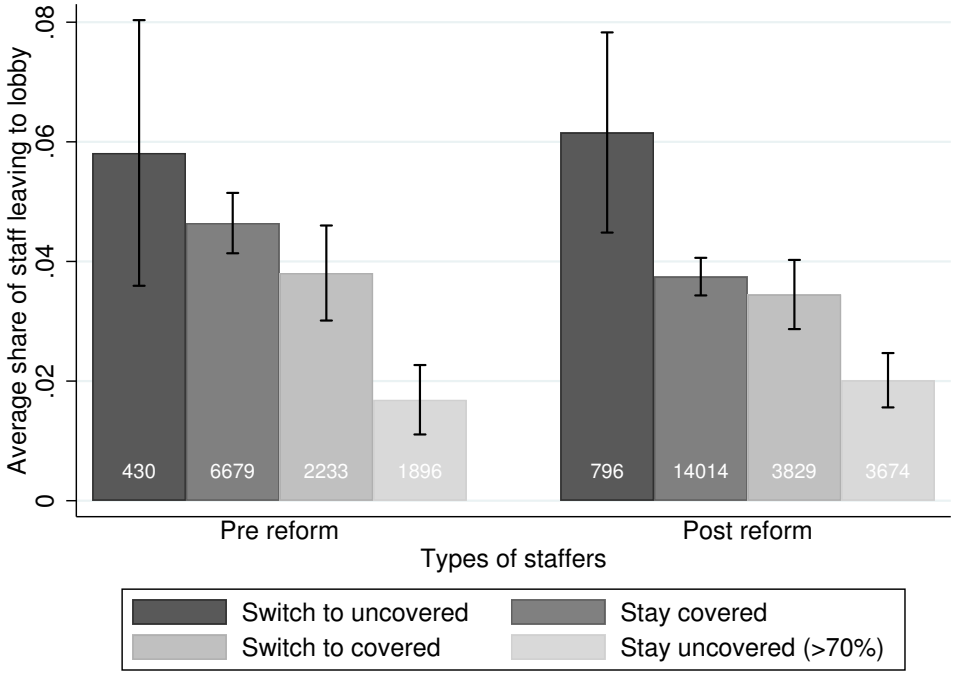
E Figures

Figure E1: McCrary Density Estimates by Staffer Covariates, Before and After HLOGA



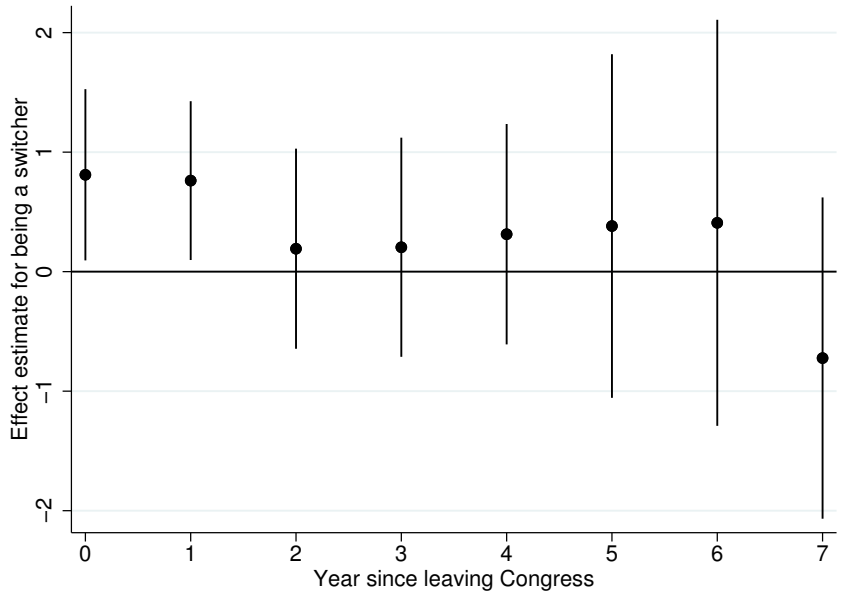
Note: Depicted are McCrary estimates together with 95% confidence intervals for different staffer types, before HLOGA (2001-2007) and after HLOGA (2008-2016). "Master" indicates whether a staffer has received a master's degree or higher (information missing for 59% of the sample). "Senior" indicates whether a staffer is above the 75th percentile of years of experience (8 years after HLOGA, 4 years prior to HLOGA; information missing for 24% of the sample).

Figure E2: Share of Staffers Leaving to Become Lobbyists, By Coverage Type



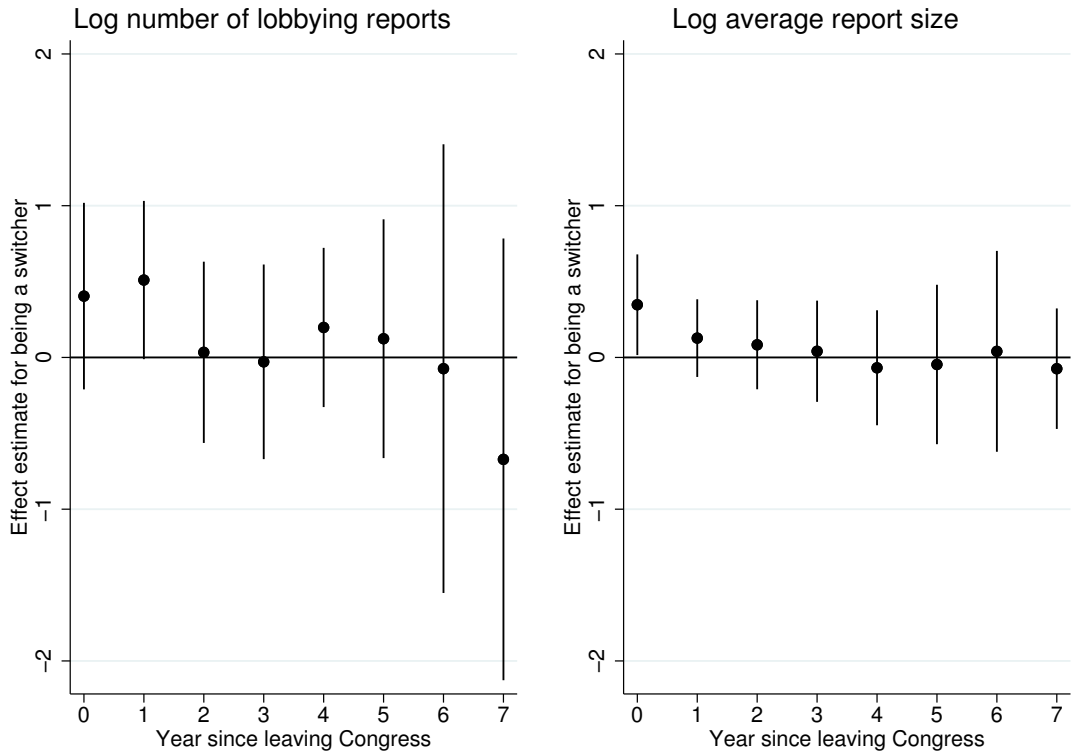
Note: Depicted are average shares of staff leaving to become lobbyists in a year by coverage status pre-reform (2001-2007) and post-reform (2008-2016), together with 95% confidence intervals. White figures indicate the number of staffers in each group.

Figure E3: Average Marginal Effects of Being a Switcher on Unweigthed Lobbying Revenue, By Year After Leaving Congress



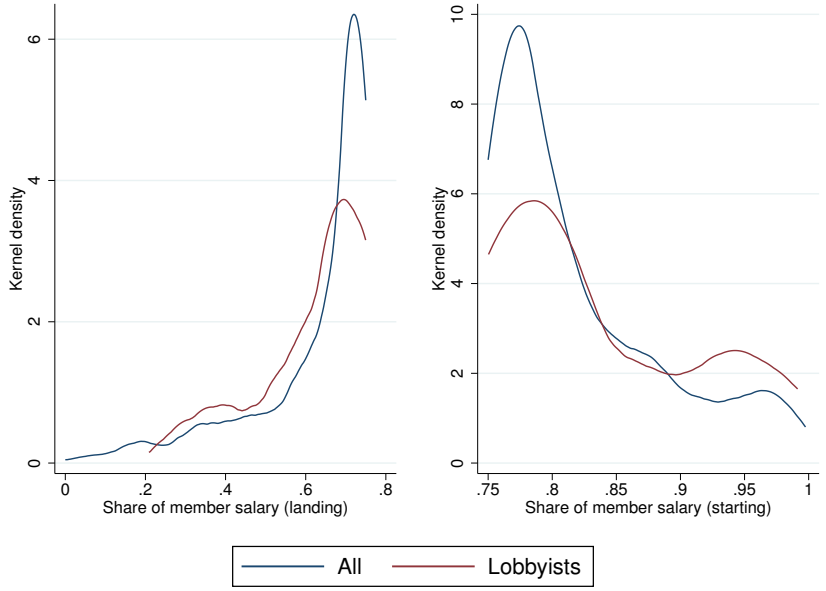
Note: Depicted are average marginal effects of switching below the 75% cutoff in the last year in Congress before becoming a registered lobbyist. The baseline is covered staffers earning salaries above the 75% threshold in their last year in Congress. The estimation results are based on a version of Equation (2) with unweighted lobbying revenue as dependent variable. The sample includes Congressional staffers joining the lobbying industry after HLOGA. The level of observation is on the lobbyist-year, $N = 1,950$.

Figure E4: Average Marginal Effects of Being a Switcher on Number and Size of Lobbying Reports, By Year Since Leaving Congress



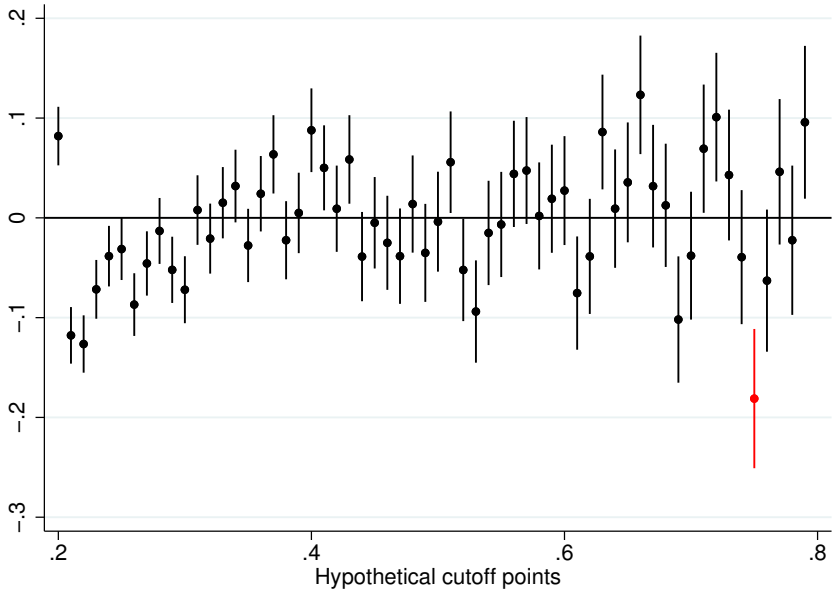
Note: Depicted are average marginal effects of switching below the 75% cutoff in the last year in Congress before becoming a registered lobbyist. The baseline is covered staffers earning salaries above the 75% threshold in their last year in Congress. The underlying models are identical to Equation (2), except for the dependent variables. The level of observation is on the lobbyist-year, $N = 1,950$.

Figure E5: Share of Member Salary That Switchers Move To



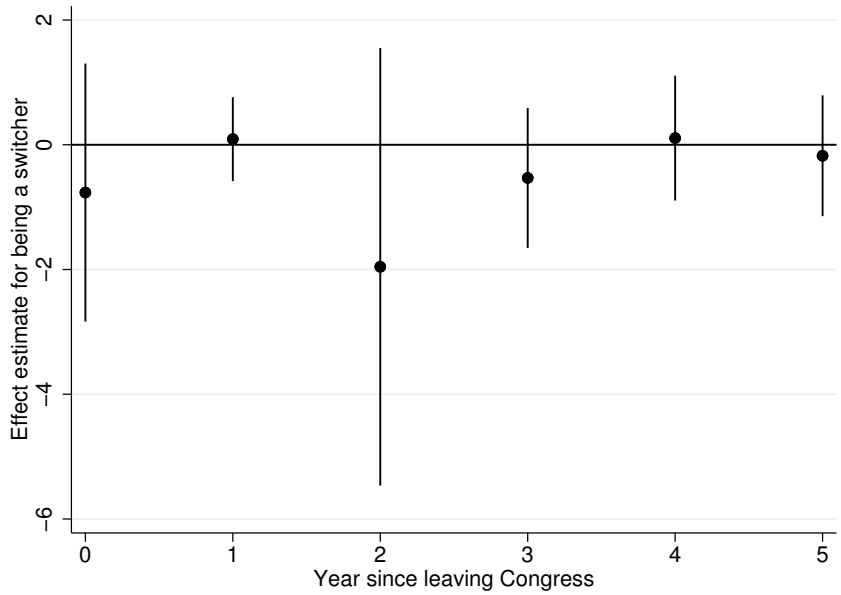
Note: Depicted are kernel density estimates for salary distributions (as a share of a member's salary) for switching staff after HLOGA took effect (2008-2016). The left panel shows salary distributions in the year after switching, the right panel shows salary distributions in the year of switching (5% of observations with shares above 1 not shown). Administrative staff excluded.

Figure E6: McCrary Density Estimates at Hypothetical Salary Cutoffs, After HLOGA



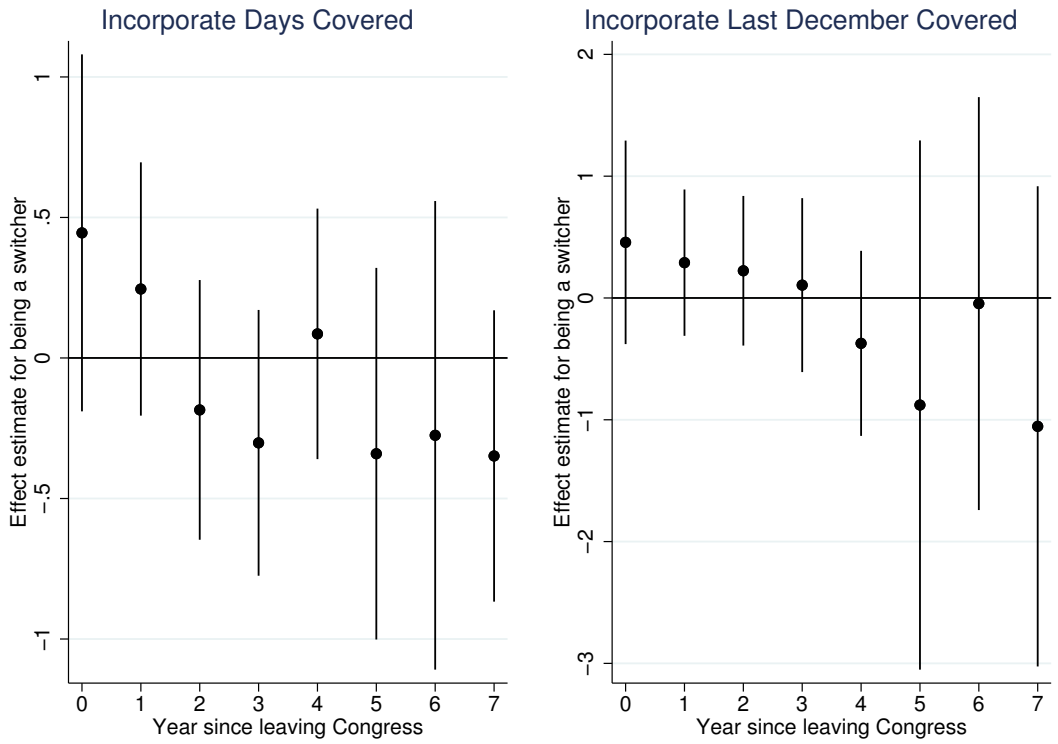
Note: Depicted are McCrary density estimates at hypothetical salary thresholds between 20% and 80% for years after HLOGA, together with 95% confidence intervals. The effect at the actual threshold of 75% is shown in red.

Figure E7: Placebo Analysis for the Effect of Switching on Lobbying Revenue



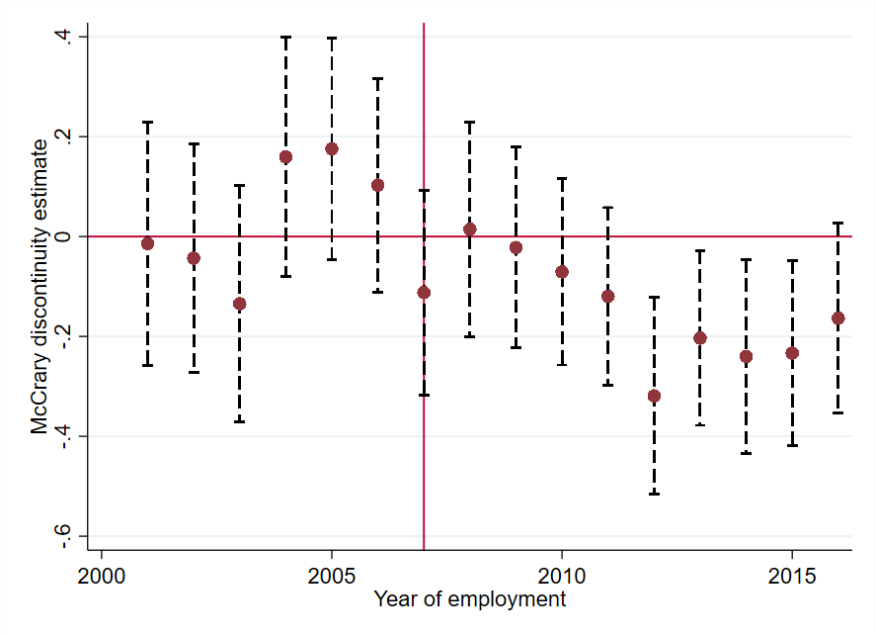
Note: Depicted are average marginal effects of switching below the 75% cutoff in the last year in Congress before becoming a registered lobbyist for staffers leaving Congress before HLOGA. The baseline is covered staffers earning salaries above the 75% threshold in their last year in Congress. The models include year fixed effects and staffer-level controls (see Equation (2)). Observations are on the lobbyist-year level, $N = 558$.

Figure E8: Average Marginal Effect of Being a Switcher on Lobbying Revenue: Alternative Treatment Coding



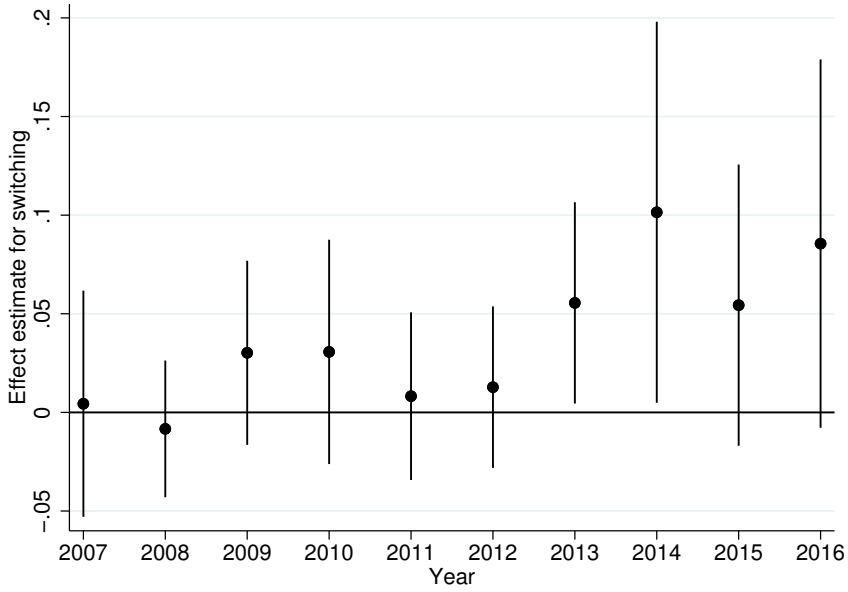
Note: Depicted are average marginal effects of switching below the 75% cutoff in the last year in Congress before becoming a registered lobbyist for staffers leaving Congress before HLOGA. The baseline is covered staffers earning salaries above the 75% threshold in their last year in Congress. The models include year fixed effects and staffer-level controls (see Equation (2)). Observations are on the lobbyist-year level.

Figure E9: Year-specific McCrary Density Estimates



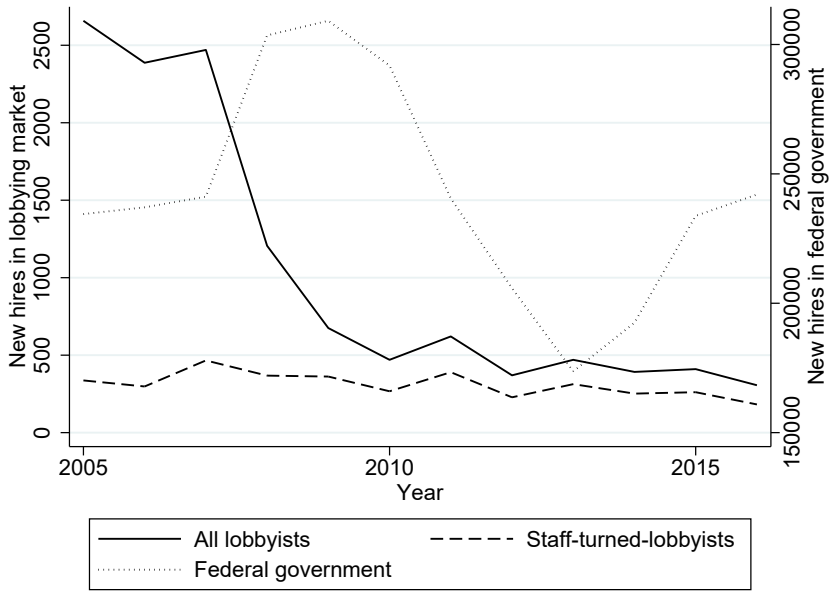
Note: Depicted are year-specific McCrary estimates together with 95% confidence intervals.

Figure E10: Year-specific Estimates of Switching out of Coverage



Note: Depicted are year-specific estimates of the effect of switching out of coverage on lobbying employment, together with 95% confidence intervals.

Figure E11: Hiring in Industries Relevant for Post-Congress Employment



Note: Depicted are accession numbers in the lobbying industry (from lobbying registration records) and in the federal government (from OPM FedScope records).

Figure E12: Histogram of Staffer Salaries by Year