

Revolving Door Benefits?

The Consequences of the Revolving Door for Political Access

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Abstract

Do policymakers grant greater access to organized interests employing their former colleagues? While a growing literature examines the “revolving door” between business and politics, we know little about whether organized interests can extract political favors by hiring revolvers. Combining novel data on the career trajectories of European Union officials and politicians with access data to the European Commission, we use a difference-in-differences strategy to estimate how hiring revolvers shapes access to meetings with policymakers. Surprisingly, we find limited evidence that employing politically connected staff shapes access overall. However, we uncover substantial heterogeneities in the effects, suggesting that revolvers do increase access to meetings about the broader legislative agenda when institutional friction is low. Moreover, contract lobbying firms, which arguably possess the weakest level of legitimacy, experience increased access. Our article sheds new light on the circumstances under which revolvers with political connections help private interests and have important implications for understanding the political effects of the revolving door.

Keywords: Lobbying the EU Commission; the revolving door; lobbying and agenda-setting; contract lobbying firms.

The flow of politicians and public officials into the private sector has become a stable feature of modern politics. As an example, Alter-EU reports that six out of thirteen departing European Commissioners in 2009-10 went into corporate or lobbying jobs and noted that similar patterns existed for high-level officials from the European Union (EU) institutions in general.¹ Both academics and commentators speculate that the significant financial transactions associated with the 'revolving door' imply that special interests can secure political favors by hiring former political insiders. A smaller number of studies have for example examined the consequences of the revolving door for individual lobbyists. This research suggests that connections through the revolving door pay off for lobbyists, as the employers of revolvers obtain an economic premium (e.g. Blanes i Vidal, Draca, and Fons-Rosen 2012; McCrain 2018) and see improved results in the stock market (Luechinger and Moser 2020). Not surprisingly, the labor market prospects for people with a background in politics are therefore quite good (McCrain 2018; Strickland 2020; Egerod and Tran 2023; Palmer and Schneer 2016, 2019). However, there remains a gap in our understanding of the situations in which individuals with political ties utilize their connections to advance private interests following their employment transitions from the public sector.

In this article, we aim to investigate whether political access plays a role in facilitating the ability of these individuals and their employers to solicit political favors and gain advantages through the revolving door. To achieve this, we systematically evaluate whether organized interests² that employ former civil servants and politicians are subsequently granted greater access to policymakers.

We find limited evidence that all organized interests are able to use revolvers to obtain access to the political system as a whole. However, we do find that revolvers can facilitate access to subsets

¹ <https://www.alter-eu.org/the-revolving-door-in-detail>

² Defined as non-public actors with organizational structure involved in lobbying, e.g. business associations, NGOs and firms (see Baroni et al. 2014).

of meetings pertaining to the setting of the legislative agenda, although only for a short period of time. Importantly, prior research (e.g. Jones, Larsen-Price, and Wilkerson 2009) has found that this is when institutional friction is still relatively low. Furthermore, we demonstrate that the positive access gained from hiring revolvers is concentrated within contract lobbying firms.

Our analysis uses a difference-in-differences (DiD) strategy to estimate the impact of the revolving door on access to policymakers with a new unique dataset from the EU. We start from a list of bureaucrats and politicians who retire from the EU Commission (Commission), then cross-reference the names of their new employers with official records documenting the Commission's interactions with external interests. Additionally, we leverage a distinctive aspect of the EU system, wherein the Commission publishes concise descriptions of meeting topics. We explore this unique textual monthly data on Commission meetings to measure what organized interests discuss, and with which policymakers they discuss it. The design allows us to use highly granular behavioral data to examine how the propensity for an organized interest to gain meetings with the Commission changes *after* the revolver joins the organization.

The article's contribution is both methodological and empirical. It represents the first study to evaluate the impact of the revolving door in EU institutions using a difference-in-differences identification strategy to estimate how career transitions to the private sector affect access to meetings with policymakers. This helps shed light into consequences of the revolving door rarely studied, i.e. whether hiring revolvers is ultimately a benefit for obtaining access to policymakers and whether this effect differs for different types of organized interests and meetings. In addition, it potentially helps understand the related finding in the literature that contract lobbying firms that hire revolvers on average secure economic gains (e.g. Blanes i Vidal, Draca, and Fons-Rosen 2012; McCrain 2018; Luechinger and Moser 2020). Hence, it suggests that part of the mechanism rendering revolvers valuable sources of income for these types of organized interests could be their enhanced capacity to

access the political system regarding certain issues, enabling them to directly communicate their concerns to policymakers. While there are many types of “favors” organized interests can extract from hiring revolvers, access is one of the most important ones, since it is an important step towards influencing the political agenda (e.g. Bouwen 2004; Miller 2021).

Our findings offer grounds for both optimism and pessimism. On a positive note, they imply that the influence of revolvers, while significant, may be more restricted in its reach than feared by critics. We demonstrate that employing revolvers does not automatically ensure access to meetings across the entire policymaking process. By analyzing meeting agendas, we reveal a concentration of advantages in meetings concerning broader political agenda topics, rather than specific legislative matters. Nevertheless, the presence of varied effects among different actor types, where contract lobbying, that possess less democratic legitimacy than other groups, can obtain political favors such as high-level meetings by utilizing revolving doors, raises significant normative questions about the broader ramifications of this practice.

Theoretical framework: Lobbying, Political Connections and Information

A large literature investigates the role of political connections and expertise in lobbying (e.g. Blanes i Vidal, Draca, and Fons-Rosen 2012; Bertrand, Bombardini, and Trebbi 2014; Bouwen 2004; LaPira and Thomas 2017; McCrain 2018; Shepherd and You 2020). We build on it to argue *that organized interests that hire former civil servants and politicians should gain by subsequently being granted greater access to policymakers (H1)*. While access does not necessarily guarantee a group's ability to influence policy, it is often considered a crucial element in the process. This is the case, because – for access to happen – policymakers must spend their scarce time on meetings with organized interests, and they would not do so unless they expected to get something in return (Binderkrantz, Christiansen, and Pedersen 2015; Binderkrantz, Pedersen, and Beyers 2017).

Why employers should gain access from hiring revolvers

The reason revolvers should be able to help their private sector employers gain access to policymakers is that their careers in public service can be expected to have endowed them with a particular set of skills. We can broadly distinguish between the importance of expertise and connections, with the former being further sub-divided into procedural and substantive expertise (LaPira and Thomas 2017). First, in relation to expertise, Bouwen (2004) argues that policymakers interact with organized interests, because they are in an information deficit. In particular, when seeking to construct business regulation, policymakers have imperfect information about the cost structure facing the individual firm, and how a change in regulation would impact it (Grossman and Helpman 2001, chapter 3). Therefore, in order to construct a successful lobbying campaign that allows the organized interest to gain access to policymakers, they need to construct high-quality policy input (Bouwen 2004), delivered in a way that can easily be implemented in policy (Drutman 2015). In other words, information alone is typically insufficient; the suggestions need to be actionable policy advice (Drutman 2015). Revolvers can be helpful for organized interests that seek to obtain these assets. While many policymakers – particularly in top positions – are legislative generalists with little issue-specific expertise (Hall and Deardorff 2006), revolving door bureaucrats or legislative staffers have considerable expertise in specific policy-making areas (LaPira and Thomas 2017). Moreover, while many organized interests seeking to influence policy may already possess the requisite knowledge about their operations and how a regulatory change might impact them (Bouwen 2004), translating such knowledge into actionable policy is challenging (Drutman 2015). Revolvers, however, possess the expertise necessary to accomplish this task convincingly to policymakers (Strickland 2020). Second, connections facilitate access to policymakers. During their public service, revolvers will have established personal relationships with their colleagues, which will assist them in their work for private sector employers once they have transitioned into their new roles as lobbyists (Strickland 2020). A substantial and expanding body of literature suggests that lobbyists can utilize their social

connections to policymakers as assets to secure access on behalf of their clients (Bertrand, Bombardini, and Trebbi 2014; Blanes i Vidal, Draca, and Fons-Rosen 2012; Hirsch et al. 2019; McCrain 2018). However, connections and information are likely to complement each other. In particular, when choosing whom to meet with, policymakers face the problem that there are many groups that seek access, and the substantive expertise that they offer might be biased (Hall and Deardorff 2006). This places the policymaker in a conundrum: They need information to construct policy, but it is often too costly to validate the information offered by the organized interests. Repeated personal connections between lobbyists and policymakers are important to solve this problem – lobbyists must deliver honest information if they want to benefit from future access. By awarding some lobbyists a privileged insider status in return for information, policymakers can avoid problems with cheap talk (Groll and Ellis 2017). Revolvers play an important role because they have pre-existing social links to the current policymakers, making them more likely to be trusted both in terms of their honesty and the quality of their information. Hence, they are more likely to be granted the privileged insider status all lobbyists seek (Hirsch et al. 2019). This implies that policymakers do not have to validate the quality of information themselves – they can rely on the revolver for this (McCrain 2018). A similar way connections can play a role is that the policymaker is likely to know the biases of the lobbyists with whom they have had a professional relationship, and they can use this to “debias” the information they receive (Grossman and Helpman 2001, chapter 3). Lobbyists who lack personal relationships with policymakers cannot guarantee that they will only introduce them to organized interests with the most valuable information. The profit motive incentivizes them to represent as many clients as possible. However, due to their personal connections with policymakers, revolvers can more credibly commit to only introducing organized interests with high-quality information (Hirsch et al. 2019). In this way, political connections facilitate the transmission of substantive expertise between organized interests and policymakers.

While it has been theorized that revolvers might help their future employers access the political system (McCrain 2018), empirical research on the potential gains of hiring employers has focused on broader economic effects rather than political access (e.g. Blanes i Vidal, Draca, and Fons-Rosen 2012; Luechinger and Moser 2020). Shepherd and You (2020) is an exception. However, they look at whether members of congress employing more soon-to-be revolvers grant more access to prospective employers *before* staff moves into the private sector rather than whether organized interests gain access *after* hiring revolvers, which is the focus of this study. Furthermore, akin to the majority of literature on the revolving door, their examination is centered on the United States.

The relevance of hiring revolvers in the European Union context

Despite the US centered focus in existing research, we can expect the discussed assets of revolvers to be equally pertinent for benefiting future employers within other political contexts, such as the EU, which is the focus of our analysis. While not a traditional state, the EU bears resemblance to a political system in several crucial respects, particularly because its member states have delegated significant law-making competences to it, rendering it the primary source of regulation in numerous policy domains (Hix and Hoyland 2011). To examine whether hiring revolvers leads to access to policymakers, we specifically analyze meetings with one of the key EU institutions, the EU Commission. The Commission has the right of initiative, meaning that it is responsible for planning, preparing and proposing new EU legislation, which then proceeds to the European Parliament and the Council of Ministers. This privileged position in agenda-setting renders access to the Commission particularly significant. Additionally, because Commission officials do not run for election, it offers an interesting opportunity to investigate the revolving door phenomenon in the absence of electoral pressure. The Commission is structured into distinct policy departments known as directorates-general (DGs), each overseen by a commissioner. Due to the vast scope of producing European legislation, the Commission faces resource constraints and must engage with external stakeholders to

fulfill its legislative mandate (Bouwen, 2004). This characteristic renders the EU context, especially the European Commission, particularly suitable for examining the theory. Existing work by Coen and Vannoni (2016) of EU Business-Government relations – including the revolving door – demonstrates that, while career moves between the political and private sector are not as widespread in the EU as in the US, they do occur. In support of this, recent survey evidence of hiring practices of organized interests lobbying the EU by Belli and Bursens (2023) shows that a little over a third of groups report having staff with a public sector background.

Recent literature has also started examining the consequences of such hires in an EU context. Crucially, Luechinger and Moser (2020) show that investors expect political connections to matter as they do in the US: Firms that hire former EU Commissioners see improved stock market returns. Additionally, Belli and Beyers (2023) have recently published a study looking at whether those organizations that report having (any amount of) staff from the EU institutions in a survey experience a different likelihood of having meetings with the European Commission than those which do not. Their analysis provides important, suggestive evidence that the revolving door might also have political consequences for some types of organized interests in an EU context even if their data does not demonstrate longitudinal within-case variation that allows them to make causal claims.

As we will outline below, we complement existing work by linking granular behavioral data on movement of EU officials and politicians into the private sector with data of the meetings between the EU Commission and outside interests. This enables us to use a difference-in difference design to look at whether different types of interest organizations experience gains in access *following* their hires of EU revolvers (see below for a detailed discussion of the model). Additionally, we examine variation in the potential benefits of the revolving door among both the types of actors utilizing revolving door practices and the nature of meetings with the Commission. Consequently, our database of meeting records enables us to classify meeting content, thereby facilitating an investigation into

differences in the impact of the revolving door on accessing different types of meetings, likely associated with distinct phases of the policymaking process.

Why gains from hiring revolvers might may vary across *meeting content* and *actor types*

We argue that access gains for organizations hiring revolvers may not be constant across meetings of different levels of “policy specificity”. We draw a theoretical distinction between meeting topics related to a) broad policy discussions, b) medium-broad discussions on specific economic sectors, and c) specific policies under negotiation or adopted. These categories highlight the spectrum from agenda-setting to specific policy outcomes with broad discussions often aimed at introducing new topics to the agenda, while specific discussions usually occur in later policy stages, focusing on existing proposals or laws. We expect *organized interests to capitalize more on hiring lobbyists for obtaining access to meetings involving broader discussions, likely to happen early on in policy processes, than to meetings involving more narrow discussions of policy outputs later in the policy cycle (H2)*.

First, during the early stages of the policy process, policymakers may demonstrate an increased demand for the assets typically associated with 'revolvers' discussed in the previous sections. During this phase, the documented information deficit of policy-makers (Bouwen 2004) and the demand for technical policy input are likely to be more pronounced, underscoring the significance of the expertise offered by revolvers in specific policy-making domains (LaPira and Thomas 2017). Using similar argumentation, the interest group literature has also convincingly argued and shown that exerting policy influence should generally be easier earlier than later in the policy process (e.g. Baumgartner, Berry, et al. 2009; Olzak and Soule 2009; Soule and Olzak 2004). Second, apart from need for information, the literature on agenda setting also convincingly argues that different stages of the policy process exhibit varying levels of "institutional friction," which refers to formal structures that increase decision and transaction costs (Baumgartner, Breunig, et al. 2009; Bevan and Jennings

2014; Jones and Baumgartner 2005; Jones, Larsen-Price, and Wilkerson 2009). Early stages typically exhibit lower institutional friction compared to later stages, allowing decision-makers more flexibility to be responsive to public priorities, and potentially also give them greater latitude in addressing the concerns of organized interests. Lower friction may afford organized interests greater opportunities to benefit from having hired revolvers, as it may become easier to accommodate meeting requests compared to later stages of the policy process. In these later stages, policies may be more entrenched and locked in, and decision-makers may be compelled to prioritize listening to all relevant stakeholders involved in a given policy decision, regardless of whether the organizations in question offer specific connections and expertise through hired revolvers.

Finally, we contend that the importance of hiring revolvers should not be uniform across all types of organized interests in securing access. This is because, beyond seeking expertise and connections, policymakers also interact with organized interests for their potential to provide legitimacy and contribute to political representation. The latter can be understood as a process where representatives claim to represent particular societal interests (Saward 2006). Organized interest face the challenge that they are not directly elected but self-authorized representatives. However, provided they are *authorized by* and *accountable to* the affected constituency they claim to represent they may be seen as contributing to democratic representation (Montanaro 2012). When policymakers are aware of the segments of society represented by a group, they also gain insight into the biases it may harbor. This insight is valuable for policymakers when assessing the quality of information (Grossman and Helpman 2001). Yet, the “representative potential” of different organized interests varies, meaning that they may not be equally dependent on hiring revolvers for getting access.

For certain organized interests, claims to represent either broader segments of interests or a large share of the potential constituency of a given type of interest are more likely to be sufficient for

obtaining access. Interacting with broadly representative organized interests not only helps the policymakers assess the information offered by them – their representativeness carries legitimacy that will increase the value of meeting with them (Junk 2019). NGOs are a good example: Many of them represent broader societal interests and are widely perceived as having a broader potential to representative society (e.g. Flöthe and Rasmussen 2019). Policymakers need to engage in close dialogue with such organized interests to boost the legitimacy of their organization and policies, irrespective how many former ex-officials and politicians with technical expertise and political connections these organized interests have on their payroll. These NGOs still employ revolvers but may experience fewer direct benefits from doing so, as they might have a high likelihood of securing a seat at the table regardless.

Instead, the hiring of revolvers might have a more significant impact for organized interests with weaker "representative" and "legitimizing" potential. A prime example would be contract lobbyists—professional lobbyists employed by specific clients—many of whom lack a mission tied to broader societal concerns. Rather than representing constituencies associated with broader societal issues, they cater to specific interests. When policymakers consider granting access to contract lobbyists, concerns about enhancing legitimacy may carry less weight, while other factors, such as their level of professionalism and expertise, may exert a stronger influence. For contract lobbyists, employing former staffers and politicians from the EU apparatus may thus have a more significant impact on the political benefits they receive. Additionally, business associations and companies are likely to occupy a middle ground between NGOs and contract lobbyists. While they may not inherently claim to represent society broadly, they are expected to carry a degree of legitimacy, dependent on the proportion of any given sector they represent. In summary, we anticipate that *the potential access effect of hiring revolvers should be more pronounced among contract lobbying firms*

than other types of organized interests, given their comparatively lower representative and legitimizing potential (H3).

Research Design

Our analysis relies on a difference-in-differences design and longitudinal data from the EU that allow us trace how the likelihood of an organized interest of obtaining access to EU policymakers changes after they hire revolvers, who have previously been employed in the EU institutions. As we detail below, we extend our analysis beyond overall access by engaging in content coding of the topics of the meetings held between organized interests and policymakers.

Data

We rely on several data sources to measure our variables of interest. First, to estimate the effect of gaining a political connection by hiring a former Commission official or politician on access to meetings, we start by constructing a binary indicator of the year during which the organized interests hire revolvers. For this, we rely on work by Corporate Europe Observatory (CEO). CEO used a combination of desk research and available online documents provided by the Commission to construct a list of Commission officials and politicians that transferred to positions in the private sector. This list contains information on which organizations the 41 top EU officials and politicians in the dataset moved to. We collected data on the timing of the change in employment for the revolvers, and – in doing so – we also validated each case in the dataset. As we do not know when the revolver leaves the group again, we consider treatments during the first year only. That is, since we know which month the revolver arrived, we count a group as treated from the month of arrival and for twelve months into the future. If we were to extend the period beyond one year, and certain revolvers leave before others, our estimates would be biased. It is important to note that few

Commission officials spin through the revolving door, and our results rely on relatively few revolvers. Because our data is very granular data, we still obtain a quite large sample size overall, allowing us to detect some effects. However, future research should probe the results further in larger samples with more treatment events.

To measure the access that an organization gains to the Commission, we rely on Transparency International's data gathering effort. The organization collected publicly available information on meetings from the websites of Commission DGs and combined it into a single file, including details about the name of the organization that is meeting with the Commission and the subject they discuss. This information provides a potent method for assessing access, aligning closely with the theoretical definition of access (Binderkrantz, Pedersen, and Beyers 2017), and drawing on higher-quality data than typically accessible to researchers (Miller 2021). We rely on two measures of access in our main specification. First, to construct a measure of access at the intensive margin, we use the *number of monthly* meetings that an organization has with top-level staff in the Commission within a four-year period (between late 2015 and 2018). In our analyses, we log transform this measure.³ This captures the frequency with which the group meets with policymakers. Second, to construct a measure of access at the extensive margin, we use a binary measure of whether an organization has a meeting with the Commission in each month in our data. This captures the likelihood of gaining any form of access.

Figure 1 presents an overview of the types of revolvers that previously worked in the Commission, with which the Commission held meetings as well as a distribution of the total number of meetings and the top 16 actors that obtained access. According to panel C, Companies and lobbying firms account by far for the largest number of meetings between organizations and the Commission,

³ We have also estimated fixed effects poisson regressions where the outcome variable is the count of meetings. This yields statistically significant results when there are no group-year interactive fixed effects included.

while e.g. NGOs and Think tanks account for a considerably lower share. The same applies to trade unions and business associations, who are included in the “other category” along with for example law firms and public entities.

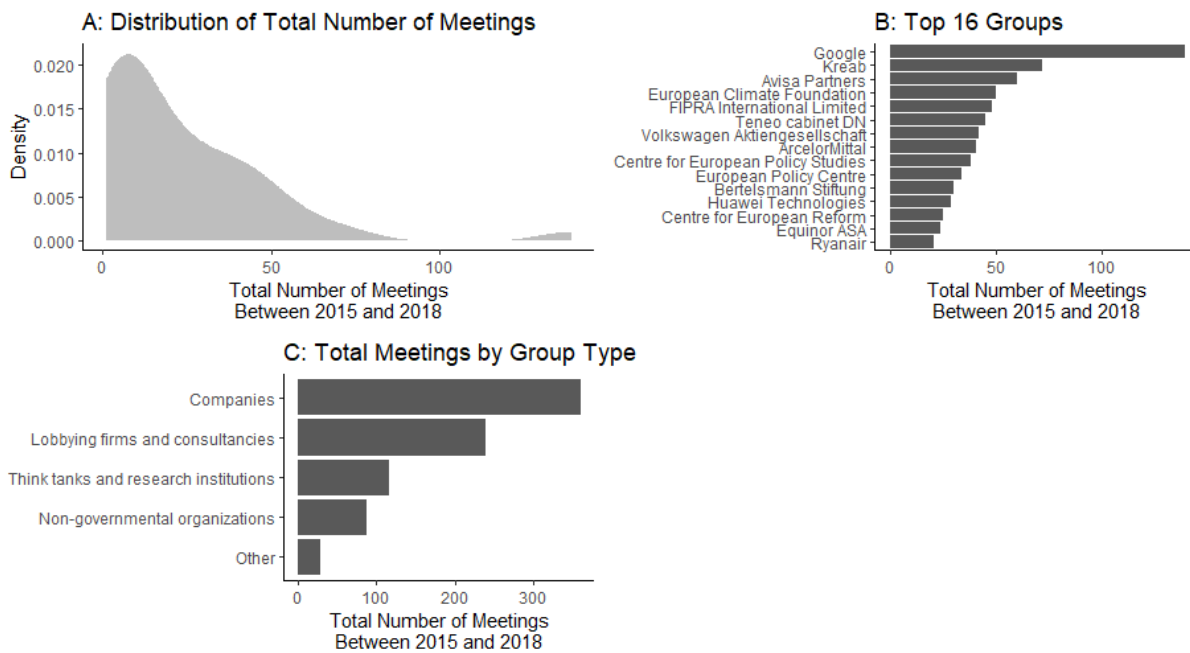


Figure 1. Meetings and (types of) organized interests that hired Commission Revolvers

Figure 2 instead shows the types of actors employing revolvers in our dataset irrespective the number of meetings held. Importantly, individual firms and commercial lobbying companies employ far more revolvers than other types of actors. However, trade associations – traditionally, the main vehicle for business power – do not employ more revolvers than for example think tanks and NGOs. This is interesting, as it contrasts the findings from the US federal level in LaPira and Thomas (2017, p 116-117) that, both companies and trade associations hire revolvers more than most other types of actors. It also contrasts Strickland’s work (2020) on lobbying in the US states, which finds that while

companies, other types of private interests and government reform groups hire the former legislators⁴, public interests in general hire quite a large number as well.

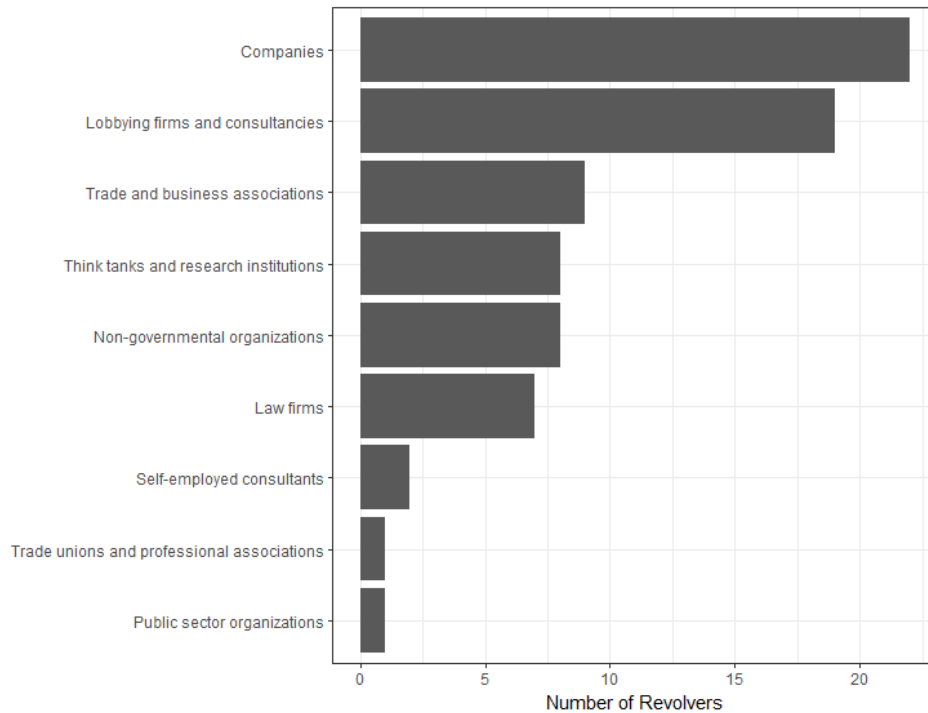


Figure 2. Number of Revolvers Hired by Actor Type.

While this gives us a sense about broad patterns of access and hires, it leaves an obvious remaining question: What do the organized interests use this access for? Is the goal to influence specific policy outcomes or to put new issues on the EU agenda? Crucially, given that the Commission registers the topics of the meetings, we can construct a robust proxy measure for our

⁴ Part of the reason for the differences in results relative to Strickland (2020) and LaPira and Thomas (2017) might be that we examine different types of revolvers. Our data on Commission revolvers includes only policymakers in the EU executive branch. We include both commissioners (i.e. the highest policymakers in a DG) and other top-level policymakers, while Strickland (2020) examines former legislators. LaPira and Thomas (2017) includes both revolvers from the legislative and executive branches, but their results on which groups hire revolvers do not differentiate between revolvers in these branches.

variable of interest: whether the organized interest's objective in the meeting is to influence a specific policy outcome or to raise awareness of an issue within the EU agenda.

We categorize the subjects of meetings in a simple scheme capturing whether the meeting is related to a) very broad policy discussions that are not currently covered by a policy proposal or active legislation, b) discussions of moderate to broad scope regarding a particular sector of the economy, or c) specific policies and issues either in preparation or already are in force. These categories of meetings match the expectations outlined previously.

Importantly, this measure of “policy specificity” captures agenda-setting and specific policy outcomes and issues as two extreme cases. First, how closely a meeting’s subject relates to an actual policy proposal will naturally be related to the stage of the policy process. Very abstract meetings about subjects where the EU has not formulated a policy yet will typically have the goal of putting a new topic on the EU’s agenda, or deciding what should be discussed before an actual proposal is developed. In contrast, meetings of higher specificity, which concern specific proposals or active legislation, are more likely to concern later stages of the policy processes. A more detailed codebook can be found in Appendix A.

Table 1: Examples of Different Meeting Types

Meeting Classification:	Example Subject Description:	Participant:
Very Broad Discussions/ Agenda-Setting Meetings	“Economic developments and rule of law in Serbia”	APCO Worldwide
Medium-Broad Discussions/ Economic Sector Meetings	“Current issues in the banking sector”	BNP Paribas
Highly Specific Discussions/ Policy Specific Meetings	“Anti-dumping case on steel”	ArcelorMittal

Our final dataset is based on 58 organized interests hiring 32 revolvers during our time frame. The final dataset includes 849 organized interest-months, where 831 meetings were held. We emphasize that this is a limited dataset, and while the results we present are informative, future research should probe them further by collecting larger datasets. Table 2 presents descriptive statistics split according to whether the revolver had a past in the Commission or another EU institution. According to Panel A of Table 2, the number of meetings for organizations hiring Commission revolvers averages to approximately 0.7 each month, while the probability of having a meeting in a given month is about one-in-three. Approximately one-tenth of all organized interest-months are treated with a connection as a benchmark. To provide a benchmark, panel B shows similar descriptives on revolvers from other EU institutions. Interestingly, revolvers from other institutions have fewer Commission meetings – the probability of gaining a meeting in any month is approximately 20 percent. Yet, they hire the same proportion of revolvers – in approximately one in ten months, the organized interests have a revolver appointed.

Table 2: Descriptive Statistics

Statistic	N	Mean	St. Dev.	Min	25 th Pctl	75 th Pctl	Max
<i>Panel A: Commission Revolvers</i>							
Meeting?	1,190	0.303	0.460	0	0	1	1
# Meetings	1,190	0.697	1.442	0	0	1	12
Months with Revolver Employed	849	0.098	0.297	0.000	0.000	0.000	1.000
<i>Panel B: Revolvers from Other EU Institutions</i>							
Meeting?	680	0.215	0.411	0	0	0	1
# Meetings	680	0.531	1.480	0	0	0	12
Months with Revolver Employed	467	0.096	0.295	0.000	0.000	0.000	1.000

Identification

To identify the effect of gaining connections on access to Commission policymakers, we rely on a DiD design. In the model, the "difference" refers to the change in outcomes before and after the intervention, and the "differences" refer to the contrasts in these changes between the treatment and control groups. By comparing these differences, the DiD model attempts to isolate the causal effect of the treatment from other factors that may influence the outcome. In this way, the design is well-suited to estimate the effect of hiring revolvers on access. One main reason is that it allows us to control for all time-constant variables that could threaten identification. The DiD relies on the assumption that trends would have evolved in parallel if the newly connected organized interest had not hired a revolver. To increase the plausibility of this assumption, we add two features to the typical design, which make our particular DiD design even more preferable to alternative design choices. First, we only compare organized interests that are treated now to organized interests that will be treated in the near future. That is, we exclude all organized interests that never hire revolvers. This is a powerful design choice, since it is not random which organized interests choose to become connected. Excluding never-connected organized interests eliminates bias stemming from specific group types opting to employ revolvers.⁵ As a second way of dealing with unobserved shocks, we make use of the high granularity of our data. Because we track meetings and hirings within years, we can control away all confounders that change from year to year. While real-world factors change between months, most of the confounders we could hope to collect data on would be on the yearly level. One important example includes organizational strategies which will tend to change relatively slowly. This strategy purges our estimates of all such unobservables.

⁵ Note that since our data on revolvers starts before the Commission started collecting and publicizing data on meetings, all organized interests in our dataset are not treated within the time period we study. This decreases treatment events, but minimizes problems related to Goodman-Bacon bias.

We estimate the DiD through OLS⁶ regression models:

$$Access_{gmy} = \beta connect_{gmy} + \gamma_g + \delta_m + I_y * \gamma_g + \epsilon_{gmy}$$

Access denotes one of our dependent variables capturing meetings and different types of meetings with the Commission, and *g* represents the organized interest, *m* represents the month and *y* is the year. *connect* is our indicator of whether the organized interest hires a Commission revolver in a given year-month period, which makes β our parameter of interest – the effect of hiring a revolver on the organized interest’s access. γ represents an organized interest fixed effect, while δ is set of year-month (calendar-time) fixed effects. The organized interest fixed effect removes all time-invariant factors (e.g. group type), while the time fixed effect deals with homogeneous monthly shocks to the system. The inclusion of these fixed effects make this a difference-in-differences model (Goodman-Bacon 2021). Finally, the interaction between dummies for the year and the organized interest (*I* and γ) allows for unobserved shocks to each organized interest every year. This is quite powerful as it holds constant any confounder that changes on an annual basis.^{7,8} Fixed effects can be problematic estimator of DiD (Goodman-Bacon 2021). In Appendix D, we show that our results are not biased by issues arising from staggered treatment and heterogeneous effects over time.

⁶ Using linear regression is appropriate, since a logistic regression would be biased by the incidental parameters problem that arises when too many parameters are included. As a general rule, logistic regression is biased by the inclusion of unit fixed effects.

⁷ Note that this is different from the approach that adds a linear time-trend for each group. Rather, we interact the group fixed effects with dummies for year, which allows us to hold constant everything that changes at the yearly level without making the parametric assumption that confounders change linearly as an interaction between group and month would impose.

⁸ This specification is quite restrictive. However, estimating the model only with group and time fixed effects yields similar substantive results.

Results

Table 2 presents our main results for both the probability of obtaining meetings (columns 1 and 2) and the number of meetings (columns 3 and 4) for revolvers from the EU Commission. Surprisingly, we do not uncover strong evidence that hiring these revolvers increases access in general. This holds for both the extensive and intensive margins. In column 1, we find that the probability of obtaining a meeting increases by 5 percentage points after the revolver arrives. However, the standard error is of the approximately the same size as the coefficient, and the estimate is not statistically significant. In column 2, to guard against the potential that groups that lobby more are more likely to both gain meetings and hire revolvers, we include the log of groups' spending on Brussels lobbying as a control. Since most groups do not update their entries in the Transparency Register, and spending remains more or less constant over time, we include it in an interaction with time fixed effects. The results remain statistically insignificant. In columns three and four, we examine the logged number of meetings using the same specifications. The specification in column four, including the interactive control for logged spending, is significant at the ten per cent level. This could happen by chance and should not be considered robust evidence that hiring revolvers increases meetings with the Commission in general.

In sum, while the coefficients are all large, they only meet common standards for statistical significance in one case. The lack of significance in support for hypothesis 1 could be driven by low power, since we have a relatively small number of organizations. However, we have estimated less restrictive models without interactions between group and year fixed effects, which yield the same results. Additionally, in Appendix K, we include groups that never hire a revolver. This increases our sample size noticeably and helps with potential issues of low power. However, the results remain statistically insignificant. In Appendix I, we also run a specification with linear time trends rather than an interaction between group and year fixed effects. This does not change the results. Finally,

since the monthly data is likely to be quite noisy, we also aggregate it to a quarterly panel in Appendix J and estimate our main set of regressions. This does not change the results either. Overall, we thus find no compelling evidence for an average effect on all types of meetings – neither concerning the probability of obtaining a meeting (the extensive margin) nor the frequency of meetings (the intensive margin).

Table 3: Hiring Former Commission Employees and Commission Meetings

	<i>Dependent variable:</i>			
	Meeting?		ln (# Meetings + .5)	
	(1)	(2)	(3)	(4)
Hire EU Official	0.054 (0.053)	0.078 (0.061)	0.077 (0.046)	0.103* (0.058)
Organized Interest FE?	Yes	Yes	Yes	Yes
Year-Month FE?	Yes	Yes	Yes	Yes
Organized Interest X Year FE?	Yes	Yes	Yes	Yes
Spending X Year-Month FE?	No	Yes	No	Yes
Observations	849	847	849	847

*Note: Robust standard errors with organized interest level clustering in parentheses. *, ** and *** indicates statistical significance at the 10, 5 and 1 per cent levels. Dependent variables: in columns 1 and 3: a binary indicator for monthly meeting and in columns 2 and 4: logged monthly number of meetings.*

Next, to delve further into this puzzling null result, we investigate the type of meetings organized interests do gain access to. According to Table 3, we find support for our expectation in hypothesis 2 that organized interests are more likely capitalize on hiring lobbyists for obtaining access to meetings involving broader discussions, which are likely to occur early on in policy processes, than for meetings involving narrower discussions of policy outputs later in the cycle. Column one of Table 3 shows that hiring a lobbyist is associated with an increase in the probability of getting at least one meeting for agenda setting purposes by approximately 15 percentage points. This estimate is statistically significant – the estimate is more than three times the size of its standard error, and we

can confidently reject the null of no effect. Importantly, we estimate a noisy drop in the number of meetings held about specific policies (column three), and no change in meetings of a medium level specificity related to a certain economic sector (column two). Both these estimates are statistically insignificant at conventional levels, but they are still highly indicative of an important pattern. They show why we see no average effect: The effect is heterogeneous and concentrated on agenda-setting meetings.

These are important findings suggesting that hiring lobbyists primarily leads to increased access to meetings with broader discussions. That is, they gain access to meetings early in the policy cycle but are less utilized to gain access to the more detailed stages of the policy-making process. This reinforces the notion that the role of former politicians and top bureaucrats is not primarily to influence the outcomes of very specific policy processes (e.g., the decision of who gets a specific procurement contract). Rather, they leverage their connections with the goal of steering the direction of the broader policy agenda. An alternative explanation for these findings is that organized interests that hire revolvers might want to hide the content of their meetings with the Commission. If this were driving our results, we would expect the description of the meeting content to be shorter and less linguistically diverse after a revolver is hired. In Appendix C, we show that this is not the case.

Table 4: Hiring Former EU Official or Politician and Meeting Content

	<i>Dependent variable:</i>		
	Agenda-Setting? (1)	Sector-Related? (2)	Specific Policy? (3)
Hire EU Official	0.148*** (0.050)	0.023 (0.035)	-0.079 (0.047)
Organized Interest FE?	Yes	Yes	Yes
Month FE?	Yes	Yes	Yes
Organized Interest X Year FE?	Yes	Yes	Yes
Observations	849	849	849
Residual Std. Error	0.314	0.340	0.333

*Note: Robust standard errors with organized interest-level clustering in parentheses. *, ** and *** indicates statistical significance at the 10, 5 and 1 per cent levels.*

Finally, we examine whether the positive effect of the revolving door varies between organized interests representing different types of constituents. To do so, we allow for different effects depending on the type of organized interest and use a binary indicator of any meeting with the Commission as our dependent variable. We distinguish between lobbying firms, companies, and NGOs, which are the actor types for which we have a sufficient number of meetings to run comparisons. Because we encounter issues of perfect collinearity when including interactions with each of the group type indicators, we estimate separate models. Each of these models include an interaction between one of the group types and the revolving door treatment. Figure 3 shows that in line with our expectations in hypothesis 3, the potential access-effect of hiring revolvers is more pronounced among contract lobbying firms, which enjoy lower representative and legitimizing potential than other types of organized interests. They see large gains in meetings when they hire revolvers, with an increase in meetings of approximately 18 percentage points. In contrast, the increases observed in the two other types of organized interests examined are smaller and not statistically significant.

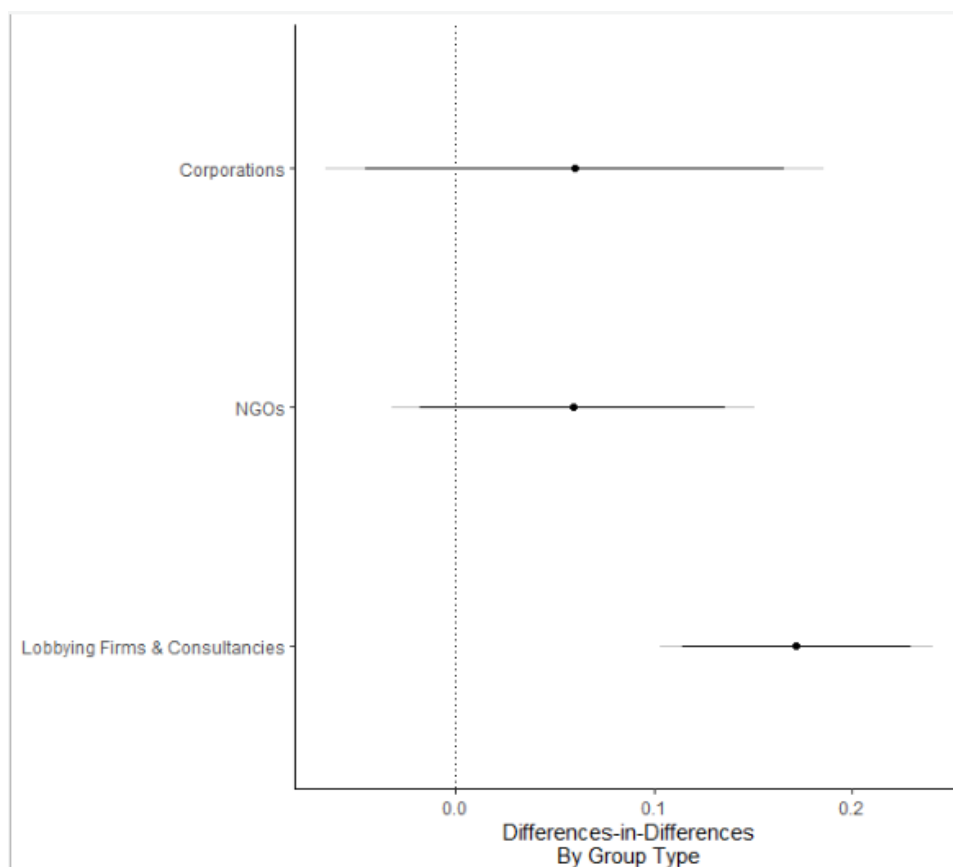


Figure 3: Effects are Driven by Corporations and Lobby Firms

Note: Results show the marginal effect of hiring a Commission revolver. The dependent variable is a binary indicator of at least one meeting for each type of organized interest. Estimates are from three separate models, each with one interaction by type of organized interest. Models include fixed effects for organized interest, month and an interaction between organized interest and year. Confidence intervals are cluster-robust 90 per cent (black) and 95 per cent (grey). Think tanks are left out, since coefficients could not be estimated for them and lobbying firms simultaneously.

Additional Analyses and Robustness Checks

We conduct a wide range of robustness tests. First, we test for the presence of differential pre-trends, finding no evidence of this (Appendix E), and show that the effect on agenda-setting meetings is very short-lived (Appendix F). To guard our estimates against the possibility that organized interests pursue other influence-seeking strategies simultaneously with hiring revolvers, we show that organized interests do not increase lobbying activity when they hire revolvers (Appendix G). We also

investigate effect heterogeneity by showing that hiring revolvers from the EU's other institutions (the Parliament and the Permanent Representation) is only noisily associated with access to the Commission (Appendix H).

Conclusion

A large literature investigates the revolving door between business and politics and has documented that hiring revolvers might be associated with economic gains (e.g. Blanes i Vidal, Draca, and Fons-Rosen 2012; McCrain 2018). However, we know much less about whether organized interests can gain political favors by hiring revolvers in the form of obtaining access to policymakers. We have gathered a unique dataset on the revolving door within the EU and employ a difference-in-differences design to examine whether various types of organized interests benefit from hiring revolvers and the subsequent access they gain to different types of high-level meetings with the European Commission.

Overall, our findings indicate that political access could be a component of the mechanism enabling revolvers and their employers to attain certain economic benefits associated with the revolving door in prior research. However, our results also suggest that the impact of revolvers on access to the EU Commission is transient, smaller than could be expected from existing work, and highly contingent. Most importantly, the benefits they derive from hiring revolvers do not extend to gaining access to all types of meetings, but rather to those focused on agenda-setting, which involve discussions on broad policy topics not currently addressed by a policy proposal or active legislation. This aligns with our expectation that the expertise provided by hiring revolvers should be more readily utilized in the early stages of the policy process when institutional friction is low.

Additionally, it is worth discussing how these results should be interpreted in a causal sense. Our design ensures that most confounders are either differenced out by including group and calendar-

time fixed effects or held constant by interacting year and group dummies. However, one factor we cannot exclude is whether the group has meetings with the EU Commission as its strategy. It is possible that when groups perceive these meetings as a strategic priority, they opt to enlist the help of revolvers to advance their objectives. This implies two possible interpretations of our results – either hiring revolvers leads to access (for some group types and for agenda-setting meetings), or the revolvers are an integral part of successful access-seeking strategies (for some group types and for agenda-setting meetings). The latter interpretation suggests that at the very least, from a causal perspective, revolvers are utilized by groups to secure access under specific circumstances, even though these groups might have gained access without the involvement of a revolver.

There are three potential theoretical explanations for these findings. First, it has previously been argued that information is particularly important for lobbying the EU Commission (Bouwen 2004). Although some revolvers have important substantive expertise, non-revolver lobbyists are able to build similar information capacities by working on certain issues and building expertise (Miller et al. 2024). Thus, one potential explanation for the overall null finding is simply that connections matter less in the context studied here, and that – as argued in previous research – information is much more important when lobbying the EU Commission. Second, however, we also show that the arrival of a revolver does increase access among commercial lobbying firms. This speaks to the Ellis and Groll’s (2018) model, which shows that organized interests that engage in repeated interactions with policymakers will build the required level of trust needed to obtain insider status and preferential access to policymakers. According to this theory, lobbyists’ political connections matter, because policymakers come to trust the lobbyists they know. Importantly, the business model of commercial lobbying firms is quite often to rely on personal connections to obtain access on behalf of their clients. Therefore, we would conjecture that revolvers should be more important for commercial lobbying firms, because it allows them to “buy” the trust of policymakers by hiring someone they already have

a relation to. Conversely, organized interests – that represent a well-defined constituency and have repeated interactions with policymakers – would build trust-based relations with policymakers at the organizational level and would not have to rely on individual lobbyists. While this is somewhat speculative, it is also consistent with Belli and Beyer’s (2023) finding that access is greater for what they refer to as ‘professionalized organizations’, i.e. organizations where staff and organizational leadership dominate advocacy and policy work. This finding also aligns with prior research on revolvers as contract lobbyists (McCrain 2018). Overall, this supports our expectation that lobby firms – lacking broad representativeness, and a relation to policymakers at an organizational level – must rely on hiring revolvers to obtain meetings with policymakers, whereas other types of groups are able to amass the connections and trust needed for access without hiring revolvers.

A third explanation for the overall null finding is that the existing US-based literature showing that clients are willing to pay more to hire revolvers (Bertrand, Bombardini, and Trebbi 2014; Blanes i Vidal, Draca, and Fons-Rosen 2012; McCrain 2018; Strickland 2023) is not driven by actual access. Clients may be willing to pay more to hire politically connected lobbyists, believing that they gain better access to policymakers. However, in reality, this might not be the case. This would be consistent with recent literature emphasizing the principal-agent problem between clients and their lobbyists, which shows that lobbyists sometimes choose to pursue goals that are at odds with the preferences of the groups that retain them (Ellis and Groll 2018; Holyoke 2022).

Turning to the normative implications, it may be a positive conclusion that revolvers do not seem to shape access to the EU Commission overall. This suggests that, in general, it is not possible to “buy” access by hiring former EU officials. However, we do find an effect for commercial lobbying firms – the part of the interest group system where the business model relies on personal relations (Ellis and Groll 2018). This could be cause for concern. These professional lobbying firms, in particular, can be seen as having weaker connections to broader society and lower democratic

legitimacy compared to other organized interests. On the other hand, we also show that their access gains are generally short-lived. While this does not rule out that the most highly connected lobby firms exert disproportionate influence, at least it suggests that hiring a revolver does not buy such influence for a longer period of time.

While we have taken a significant step in assessing the effect of hiring revolvers on access in the EU in this article, there is scope for extending our analysis. Besides delving deeper into the three questions we outline above; this would include testing the generalizability of our results in larger samples with more treatment events and in other political systems. Future datasets would also benefit from tracing the career paths of the revolving door lobbyists over longer time-periods in order to develop more fine-grained measures of how the net balance of revolvers employed at any given point affects access. Finally, additional scrutiny of the practices for employing revolvers could help understand variation in the value of these staffers for obtaining access to different types of meetings.

Conflicts of Interest

On behalf of all authors, the corresponding author states that there is no conflict of interest.

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Online Appendix for: Revolving Door Benefits?

Contents

Appendix A: Coding instructions for classifying meeting topics	2
Appendix B: Hiring Revolvers is Not Associated with Shorter or Less Diverse Subject Descriptions	6
Appendix C: Alternative DiD Estimator.....	7
Appendix D: Testing for Differential Pre-Treatment Trends	9
Appendix E: Temporal Dynamics in Effect of Revolvers on Meetings with Commission.....	9
Appendix F: Does The Revolving Door Complement Traditional Lobbying Strategies?.....	12
Appendix G: Results for Other EU Institutions	13
Appendix H: Linear Group Trends	14
Appendix I: Results from a Quarterly Panel	15
Appendix J: Including Never-Treated Interest Groups.....	16

Appendix A: Coding instructions for classifying meeting topics

Below, we describe our instructions for classifying the subject headers of meetings into four mutually exclusive categories: 1) High specificity, 2) Medium specificity, 3) High abstraction and 4) Not policy relevant/other. Each of these categories represent one indicator variable, and each meeting subject should be categorized as zero or one on each of the four variables.

See the attached spreadsheet for an example of how the classifications should be entered into the dataset.

Coders are advised to use Google searches in uncertain cases.

1. High specificity

Meetings related to specific (sets of) policies. They are recognizable in that they will refer to a specific name of a policy initiative, package, directive, regulation or an act.

Clear-cut cases:

When it is explicitly stated that the meeting is on an initiative, directive, package, regulation, act, a specific regulatory decision (e.g. an antidumping case), a white paper, a green paper, or some other way of describing a formal proposal. This holds in any situation, where a named policy is mentioned.

Less clear cases:

Sometimes the subject line will not include direct indicators that the meeting was related to specific policy. In those cases, it can often be unclear whether the subject of a meeting is very broad or on a specific (set of) proposals that have been given a broad name. When it is difficult to classify the meeting subject for this reason, a quick Google search can typically reveal the correct level.

Example: The APCO Worldwide meetings with the Commission on “Digital Skills” (May 22, 2015) and “Digital Health” (Jan 21, 2017). Quick Google searches on these two subject lines reveal that the Commission has initiatives under both those names. This indicates that the meetings were related to these specific policies.

Another example: “Neighbourhood and Enlargement negotiations” (Avisa Partners meeting, Jan 2, 2018). This is a reference to specific political negotiations and should be classified as “high specificity”.

Sometimes the Commission meets with organized interests about a report, or because they have written a report. These meetings are only highly specific, if the topic of the report is mentioned.

There are some examples of very wide policy events or packages (e.g. Brexit, the EMU, the sovereign debt crisis). These are not examples of specific policies. However, if the meeting mentions a specific policy in relation to these topics (e.g. invocation of Article 50), this is highly specific.

2. Medium specificity

Meetings related to issues concerning a specific economic sector/industry of operation. They are recognizable in that they 1) refer to an issue that is relevant to a clearly delineated economic sector, but 2) do not mention specific policies by name.

By “clearly delineated economic sector” we mean that the subject refers to a (sub-)category of the 2-digit NAICS sector classification, which can be found here:

https://en.wikipedia.org/wiki/North_American_Industry_Classification_System. This 2-digit system is further sub-dividable, and any reference to a lower level than the top 2-digit one also counts as a reference to a sector.

If the meeting makes reference to any specific group in society (e.g. consumers) this counts as medium specific as well.

Clear-cut cases:

Subject lines that directly mention a recognized economic sector or a specific product that is relevant to such a sector.

Example 1: “Transport” (APCO Worlwide meeting, March 23, 2015) is a reference to a specific sector of operation.

On the other hand, the subject line “Trade” (AccelorMittal meeting Feb 18, 2015) is an example of something that could appear “medium specific”. However, since it does not refer to any specific sector or industry, it should be placed in the “high abstraction” category.

Two examples of lower than the 2-digit level: “economic developments in the railway sector”, “economic outlook of the steel industry” both refer to clearly delineated sectors of operation.

Less clear cases:

Some subject headings refer to relatively specific concepts, but without also referring to a particular sector for which the meeting was important. These should be categorized as “high abstraction”, not “medium specificity”.

Two examples: “Data protection, free flow of data, research programmes” (BNP Paribas meeting, Sept. 4, 2018). While data might seem like something that is clearly delineated, the subject makes no reference to a specific sector.

“Pressing challenges for European labour markets and recent government reactions” (Bertelsmann Stiftung meeting, March 27, 2018). Although ‘labour markets’ seems relatively specific, no reference is made to a particular labor market of a sector.

Some subject headings use very general descriptions but the policy matters for a particular sector. All meetings, where the topic is on a particular good that is directly relevant to a specific sector, should be categorized as medium specific.

Example 1: “Financial services agenda” (BNP Paribas meeting, May 17, 2017) seems broad but refers to a particular sector (financial services). Similarly, mentions of “financial services” or “financial policy” refers to that sector.

Example 2: “Illegal content online” (Computer & Communications Industry Association meeting, February 1, 2018), because the meeting explicitly focused on “online” content, it should be categorized as medium specific, because it refers to a good produced by a specific sector.

Example 3: “Securitisation” (BNP Paribas, May 6, 2015) refers to the particular process of securitization within the financial industry. Therefore, it should be classified as medium specific.

3. Low specificity (high abstraction)

Meetings related to general outlook, broad policy questions. They are recognizable in that they will 1) refer to very general policy matters, general policy areas or political visions, that 2) are not directly relatable to a specific sector or a specific policy. (2) are identifiable in that they do not refer to specific sectors of operation nor specific policies.

Clear-cut cases:

“Exchange of views on the EU Tax agenda” (AccelorMittal meeting, April 24, 2019). “Clean economy”, “Future of Europe”, “industrial policy of Europe”.

Less clear cases:

“Better regulation” (mentioned in many meetings) sounds very general, but is a reference to the Commission’s guidelines that aim at securing high quality in their development of new regulations. Make sure to apply Google searches in uncertain cases.

Sometimes the meeting will be on a specific proposal that the organized interest wishes to advance or some form of policy report that the organized interest has authored. These meetings should be classified as “High abstraction”, this is in keeping with one of our goals which is to measure the stage of the policy process that they discuss. The same goes for meetings in roundtables of, e.g., CEOs etc.

4. Non-Policy Relevant or Undefined

Meetings that are “introductory” or concern formalities (e.g. meetings to coordinate or hand over calendars).

Meetings with CEOs, coalitions of organized interests, etc., should be coded as undefined whenever no policy relevant topic for the meeting is mentioned.

Meetings that otherwise do not fall under the described categories.

Appendix B: Hiring Revolvers is Not Associated with Shorter or Less Diverse Subject Descriptions

As noted in the main text, we could expect organized interests that hire revolvers to try to avoid disclosing the content of their meetings. To test whether this drives our finding that organized interests have more meetings about agenda-setting when they hire former Commissioners, we pursue two strategies in Table B1. In column 1, we investigate whether the meetings are described with fewer words by calculating the total number of words in a subject description. In column 2, we estimate a similar model, but use the type-token ratio as the dependent variable, i.e. we divide the number of unique words with the total number of words used in a subject description. The former would capture if descriptions are strictly shorter, while the latter captures whether the descriptions of the meetings are less complex linguistically. To avoid post-treatment bias, we impute zeros for months when no meeting was held. In both cases, we cannot reject the null and estimate small differences-in-differences coefficients. This supports the interpretation of the findings we present in the main text.

Table B1: Hiring Revolvers and Linguistic Diversity of Meeting Subjects

	<i>Dependent variable:</i>	
	Subject Length (1)	Lexical Diversity (2)
Hire Commission Revolver	-1.649 (1.361)	0.015 (0.011)
Organized Interest FE?	Yes	Yes
Month FE?	Yes	Yes
Year X Organized Interest FE?	Yes	Yes
Observations	306	306
Residual Std. Error	4.820	0.043

*Note: Robust standard errors with organized interest-level clustering in parentheses. *, ** and *** indicates statistical significance at the 10, 5 and 1 per cent levels. Dependent variables: in column 1: the number of words in a subject description. In column 2: the type-token ratio.*

Appendix C: Alternative DiD Estimator

The fixed effects estimator of the difference-in-differences may be biased in the presence of staggered treatments and heterogeneous treatment effects over time. To make sure that our results are not driven by this, we estimate the difference-in-differences using the Imai et al. (forthcoming) estimator. This technique estimates a separate difference-in-differences for each treatment event, and selects a clean control group that has not yet been treated. After estimating each difference-in-difference, the technique averages them all into a few estimates that are allowed to vary over the treated period. In Figure C1, we apply this technique. The main concern is that staggered treatment timing will bias the fixed effects estimator. However, the Imai et al. estimator (forthcoming) requires a lot more statistical power, since it estimates separate difference-in-differences for each treatment event. Therefore, if the two techniques produce comparable estimates, one should prefer the fixed effects estimator, since the Imai et al. (forthcoming) estimator will tend to be much less precise.

For comparison with our baseline results, we also present the estimate of the average effect throughout the treated period. In Panel A, we do not adjust for additional features besides matching exactly on year. Thereby, this specification is directly comparable to our baseline fixed effects models in the paper.

As we can see, the estimate is very similar, indicating that the results of the fixed effects regression are not biased by heterogeneous effects over time. The uncertainty in Figure C1 is much larger, however. A feature of the Imai et al (forthcoming) technique is that it allows for flexibly matching on covariates. In Panel B, we use Mahalanobis distance to match on the pre-treatment dependent variable, and we also match exactly on the type of organized interest. This increases the estimates significantly.

Overall, this analysis reassures us that our baseline fixed effects regressions provide reasonable estimates.

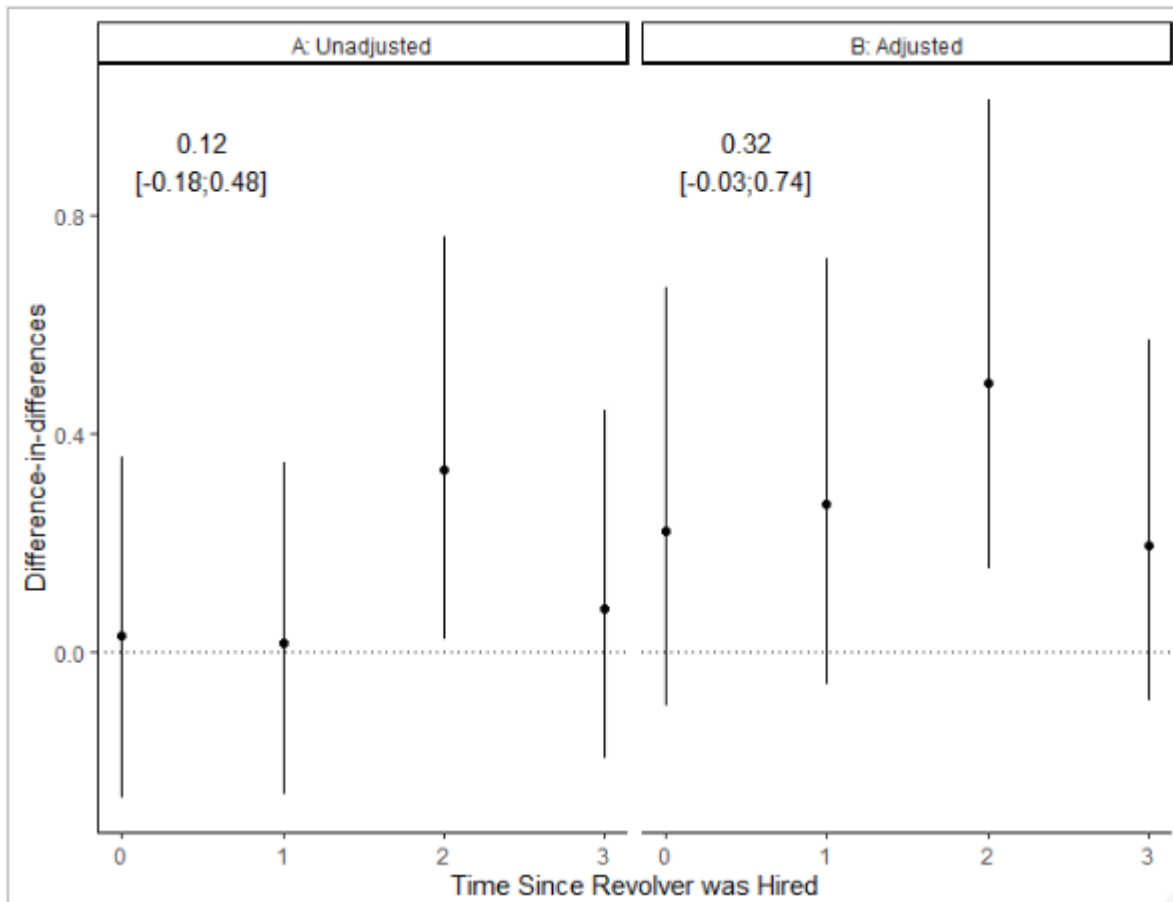


Figure C1: Robustness to Heterogeneous Effects over Time. *Note: The figure shows estimates from the Imai et al (Forthcoming) DiD estimator. The dependent variable is a binary indicator of an agenda-setting meeting that month. Lines are 90% bootstrapped confidence intervals with clustering for organized interest. Both models match exactly on year. The model in Panel B uses Mahalanobis distance matching on the pre-treatment dependent variable and also matches exactly on type of organized interest.*

Appendix D: Testing for Differential Pre-Treatment Trends

The assumption of parallel trends is fundamentally untestable. However, if the trends prior to treatment are informative about the trends after treatment, we can use pre-treatment trends as a placebo. In that case, we want to investigate whether hiring a Commission revolver is related to differential trends in access before the hiring takes place.

To investigate this, in Table D1, we follow Angrist and Pischke (2008) and add a one-year lead to the treatment indicator for Commission revolvers. As we can see in Table D1, we find no strong evidence of a differential pre-trend.

D1: Hiring Former EU Official or Politician and Meeting Content

	<i>Dependent variable:</i>	
	Agenda-Setting? ln Agenda-Setting +.5	
	(1)	(2)
Hire Commission Revolver ₊₁₂	-0.084 (0.105)	-0.095 (0.115)
Organized Interest FE?	Yes	Yes
Month FE?	Yes	Yes
Organized Interest X Year FE?	Yes	Yes
Observations	594	594
Residual Std. Error	0.333	0.436

Note: Differential pre-treatment trends are tested by applying one- and two-year lags to the independent variable, respectively. Robust standard errors with clustering at the organized interest-level in parentheses. Coefficients are from unstandardized OLS regressions.

Appendix E: Temporal Dynamics in Effect of Revolvers on Meetings with Commission

In Figure E1, we present estimates of how effects vary across time. To do so, we add leads of lengths one through twelve to the dependent variable and estimate separate models for each. When comparing the estimates to the baseline specification, this corresponds to estimating which time-periods drives the effect – from one through twelve months into the future. As we can see, the effects on meetings overall remain statistically insignificant. However, we estimate that the effect on agenda-setting meetings is present for some months after the revolver arrives. Overall, it is safe to conclude that it is short-lived.

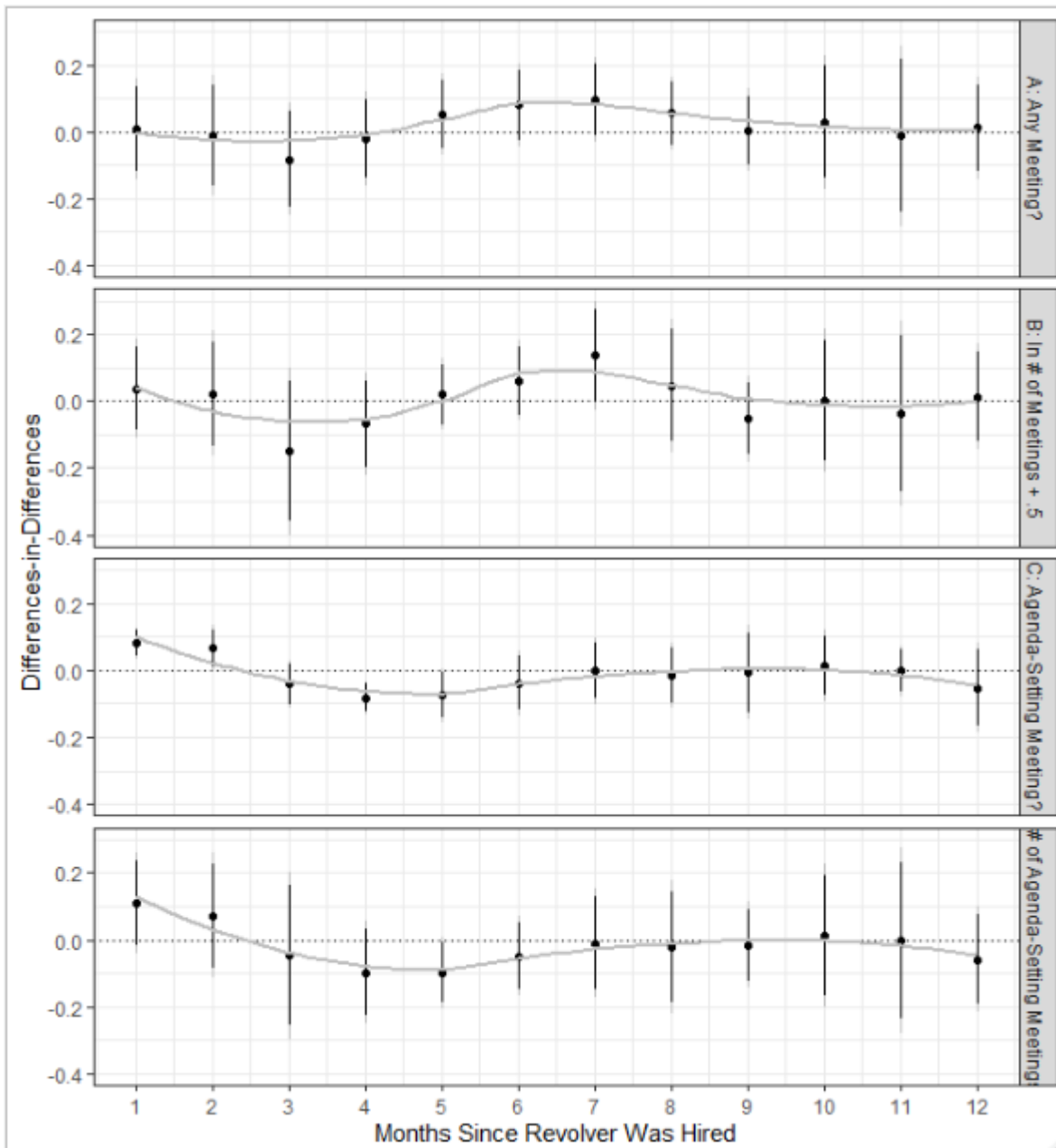


Figure E1: Temporal Dynamics in Effects on General and Agenda-Setting Meetings. *Note:* Each estimate is from a separate model, each applying a one-month lead to the respective dependent variable. Estimates are from regressions with fixed effects for organized interest, month, and an interaction between organized interest and year. Confidence intervals are 95 per cent (grey) and 90 per cent (black) with clustering for organized interest.

Appendix F: Does The Revolving Door Complement Traditional Lobbying Strategies?

As we explain in the main text, a serious threat to identification is if organized interests pursue other influence-seeking strategies simultaneously with hiring a revolver. In Table F1, we leverage data on yearly lobbying expenditures to investigate whether this is the case. In column 1, we investigate whether hiring a revolver is associated with a higher (natural log) of lobbying expenditures in the same year, as the hiring occurs. In column two, we investigate whether hiring a revolver generally succeeds a change in expenditure, by using the lagged expenditure as the dependent variables. In columns three and four, we use the difference in expenditure between the year of hiring and the previous and next year respectively, to investigate whether expenditures change when revolvers are hired. While there is some tendency for organized interests to decrease their spending before they hire Commission officials (but not Commissioners) and increase them afterwards, none of the estimates are statistically significant. This reassures us that our main results are unlikely to be driven by other strategies.

F1: Hiring Revolvers and Lobbying Expenditures

	<i>Dependent variable:</i>			
	In Expenditure (1)	In Expenditure t-1 (2)	FD Expenditure t+1 (3)	FD Expenditure t-1 (4)
Hire Commission Official	-0.023 (0.101)	0.048 (0.070)	0.031 (0.075)	0.045 (0.065)
Organized Interest FE?	Yes	Yes	Yes	Yes
Month FE?	Yes	Yes	Yes	Yes
Observations	847	581	515	581
Residual Std. Error	0.588	0.523	0.616	0.445

Note: Robust standard errors with organized interest-level clustering in parentheses. Estimates are from unstandardized regression models.

Appendix G: Results for Other EU Institutions

Table G1 shows the estimated effect of hiring revolvers from other EU institutions on gaining agenda-setting meetings with the Commission. We estimate sizable coefficients, which are too noisy to be considered statistically significant.

G1: Meetings and Revolvers from the European Parliament and Permanent Representation

	<i>Dependent variable:</i>	
	Agenda-Setting Meeting? (1)	ln # Agenda-Setting + .5 (2)
Hire EU Politician	0.117 (0.167)	0.214 (0.239)
Organized Interest FE?	Yes	Yes
Month FE?	Yes	Yes
Organized Interest X Year FE?	Yes	Yes
Observations	467	467
Residual Std. Error	0.286	0.413

*Note: Coefficients are from unstandardized OLS regressions. Robust standard errors with interest organized interest-level clustering in parentheses. . *, ** and *** indicates statistical significance at the 10, 5 and 1 per cent level.*

Appendix H: Linear Group Trends

In the main results, we interact year and group dummies to control for unobserved differential shocks happening annually to the individual group. A different way confounding can happen is if groups follow different time-trends. This could threaten our results, if confounders affecting both hiring and meetings could trend within a year, rather than at a yearly level. To test whether our results can be driven by this, we use an alternative specification, where we interact the group fixed effects with a linear time trend. The results are presented in H1 – they are very similar to the results presented in the main paper.

H1: Hiring Former Commission Employees and Linear Trends in Commission Meetings

	<i>Dependent variable:</i>	
	Meeting? (1)	ln # Meetings + .5 (2)
Hire EU Official	0.061 (0.048)	0.086 (0.052)
Organized Interest FE?	Yes	Yes
Year-Month FE?	Yes	Yes
Organized Interest X Year-Month Trend?	Yes	Yes
Organized Interest X Year FE?	No	No
Spending X Year-Month FE?	No	No
Observations	849	849
Residual Std. Error	0.420	0.684

*Note: Coefficients are from unstandardized OLS regressions. Robust standard errors with interest organized interest-level clustering in parentheses. . *, ** and *** indicates statistical significance at the 10, 5 and 1 percent level.*

Appendix I: Results from a Quarterly Panel

In Table I1, we aggregate our data into a year-quarter dataset, and present the results from four specifications: each our two dependent variables and with, respectively, an interaction between year and group fixed effects and an interaction between groups and linear time. The results largely resemble the monthly panel, although the specification in column 4 is significant at the 10 per cent level.

I1: Hiring Former Commission Employees and Commission Meetings (Quarterly Panel)

	<i>Dependent variable:</i>			
	Meeting?		ln # Meetings + .5	
	(1)	(2)	(3)	(4)
Hire EU Official	0.058 (0.059)	0.097 (0.057)	0.167 (0.103)	0.191* (0.099)
Organized Interest FE?	Yes	Yes	Yes	Yes
Year-Month FE?	Yes	Yes	Yes	Yes
Organized Interest X Year FE?	Yes	No	Yes	No
Organized Interest X Time Trend	No	Yes	No	Yes
Observations	849	849	849	849
Residual Std. Error	0.378	0.378	0.692	0.692

*Note: Data from a quarterly panel. Coefficients are from unstandardized OLS regressions. Robust standard errors with interest organized interest-level clustering in parentheses. *, ** and *** indicates statistical significance at the 10, 5 and 1 per cent level.*

Appendix J: Including Never-Treated Interest Groups

In the main paper, we constrain our analysis to include only groups that at some point are treated. In Table J1 and Figure J1, we include all groups that never hire revolvers. This does not change the results substantially.

J1: Including Never-Treated Groups

	<i>Dependent variable:</i>	
	Meeting? (1)	ln # Meetings + .5 (2)
Hire EU Official	0.048 (0.053)	0.122 (0.083)
Organized Interest FE?	Yes	Yes
Year-Month FE?	Yes	Yes
Organized Interest X Year FE?	Yes	Yes
Spending X Year-Month FE?	No	No
Observations	305,156	305,156
Residual Std. Error	0.202	0.255

*Note: Never-treated groups included. Coefficients are from unstandardized OLS regressions. Robust standard errors with interest organized interest-level clustering in parentheses. *, ** and *** indicates statistical significance at the 10, 5 and 1 per cent level.*

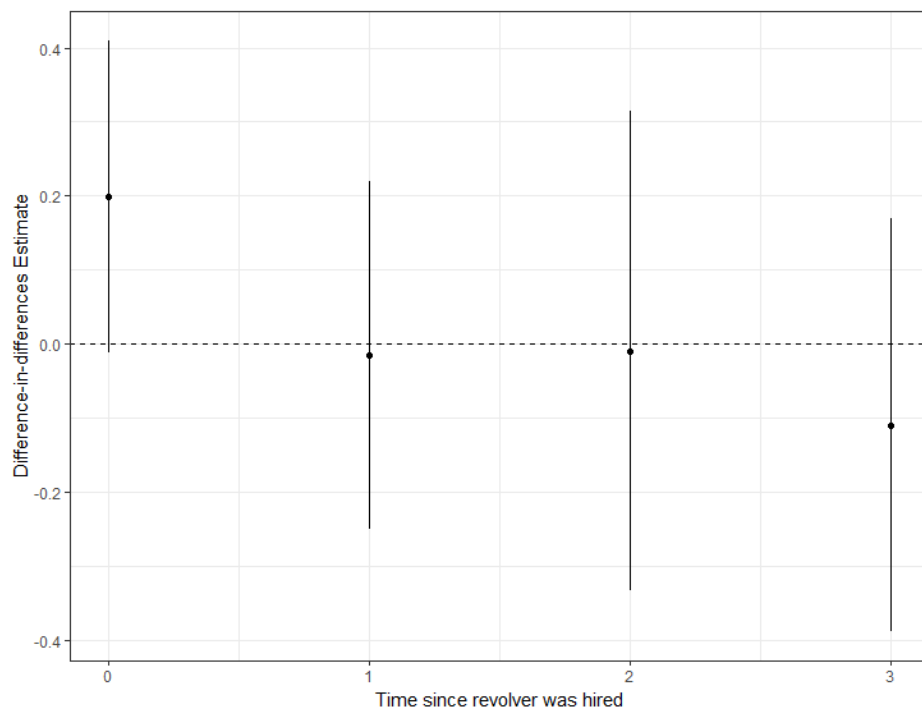


Figure J1: Including Never-Treated in the Imai et al Estimation.

References

- Angrist, J. D., & Pischke, J. S. (2008). *Mostly harmless econometrics: An empiricist's companion*. Princeton: Princeton University Press.
- Imai, K., Kim, I. S., & Wang, E. H. (forthcoming). Matching methods for causal inference with time-series cross-sectional data. *American Journal of Political Science*.