The Lure of the Private Sector[1]

– Career prospects affect selection out of Congress

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Abstract

Does the potential for a successful private sector career induce legislators to leave office? How does this affect the representation voters receive? I show that when former US senators—who now work as lobbyists—become more successful, currently serving senators with similar characteristics are more likely to take private sector employment. I replicate all results on data from the House. A number of tests suggest that senators react to the opportunity costs of holding office. Investigating selection effects, I find that legislative specialists are attracted the most in the Senate. Preliminary evidence suggests that the least wealthy respond most strongly in the House. This suggests that the revolving door shapes the skill set of legislators and the representation voters receive.

Keywords: Revolving door politics; US Congress; (Adverse) political selection; The post-elective labor market for politicians.

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1 Introduction

Recent decades have seen a surge in the number of legislators who leave office for a job in the private sector (Lazarus et al. 2016). This has fueled widespread public attention and a growing research interest in the so-called revolving door between business and politics (Adolph 2013; LaPira and Thomas 2017; McCrain 2018).

A common assumption in this research is that lucrative private sector employment motivates legislators to walk through the revolving door. If outside career options, indeed, can lure elected officials out of public service, it would establish private actors as rivals to the electorate in the demand for the labor of politicians—even while those legislators are in the public's service. If the lure of the private sector is stronger for specific types of legislators, the revolving door would change the type of politician that holds office, thereby shaping the representation citizens receive.

In this paper, I show that career prospects in the private sector, indeed, do affect the decision of Members of Congress (MCs) to leave public service, and that this has consequences for the representation voters receive. Any inferences about such a claim is complicated by the fact that career prospects are unobserved: in the best of cases, it would only be possible to observe the offer of revolving door employment that a legislator accepts upon her retirement. Any offer that she has rejected along the way will remain unobserved. However, if it were possible to establish how expectations about post-elective career prospects are formed, this would allow us to measure how those expectations affected career choices.

My identification strategy builds on a novel theory about how legislators are confronted with information about their career prospects. I argue that we can gauge how well an MC would do, if she were to walk through the revolving door, by observing the success enjoyed by her former colleagues, who currently work in the private sector. I argue that to gauge the career prospects of any particular MC, we first need to identify a comparable set of former legislators that now work as lobbyists. The post-elective experiences of this particular group can then be used to capture the career prospects of similar MCs that currently serve. Specifically, I

propose that it is possible to gauge a legislator's career prospects by observing legislators-turned-lobbyists, who worked in similar jobs before Congress, or served in a comparable mix of committees during their tenure. Importantly, because the demand for legislators-turned-lobbyists is shaped outside the reach of their former colleagues, this provides me with a proxy for career prospects which is plausibly exogenous.

To test this claim, I proceed in two steps. First, I collect data on the career trajectories of legislators serving between the 102nd and the 113th Senate. Second, after obtaining all results from the Senate, I provide a complete out-of-sample replication in the House of Representatives. I group MCs together based on a) precongressional career trajectories, and b) the committees they have served in. I use the expected size of the lobbying contracts within these groups to measure the demand for specific types of legislator labor.

My results show that when legislators-turned-lobbyists work on more valuable lobby contracts, the probability that an MC walks through the revolving door increases significantly. I test the mechanism in a variety of ways. Crucially, I show that legislators do not react to outside career prospects when they are about to experience an improvement in their pension scheme. This strongly suggests that legislators react to the opportunity costs associated with being in office

Finally, I show that the lure of the private sector differs strongly between legislators. In the Senate, legislative specialists are attracted most strongly, leaving strategists with a broad legislative focus. In the House, I present preliminary evidence that the career choices of the least wealthy are affected most strongly. Thereby, outside career options can potentially shape representation and policies by attracting expertise from the Senate and those who are least well-off from the House.

1.1 Contribution to the Literature

The extant literature on political selection has focused heavily on how monetary incentives structure the selection of specific types of politicians *into* office (Ferraz and Finan 2009; Hall

2019; Messner and Polborn 2004). Far less attention, however, has been devoted to how the same structures can motivate the selection *out of* office.

The exception is the literature on voluntary retirement from Congress. This has established that financial remuneration from congressional service shapes voluntary retirement (Diermeier et al. 2005; Groseclose and Krehbiel 1994; Hall and Van Houweling 1995; Hibbing 1982a,b; Stone et al. 2010; Theriault 1998; Wolak 2007). Most recently, this literature has been complemented by Weschle (2019) who shows that when returns to office are reduced by limits on campaign contributions, the revolving door becomes more attractive for state legislators. I add to this literature by investigating the pull of private sector career prospects, and through an explicit focus on how it shapes political representation.

The growing literature on revolving door politics has found that the average legislator stands to gain almost extravagantly from leaving office for a private sector job (Eggers and Hainmueller 2009; Palmer and Schneer 2016). Importantly, it is not only the legislators who profit. Political connections shape the value of lobbyists and—ultimately—the firms that employ them (Blanes i Vidal et al. 2012; McCrain 2018; Strickland forthcoming). These insights have fueled research investigating how the potential for lucrative employment may shape decisions before an official leaves public service (Adolph 2013; Shepherd and You 2020). I provide two missing pieces by showing that legislators are motivated by the potential for lucrative private sector employment, and that certain types are attracted the most. Through this selection effect, the revolving door is shapes the skillset of legislators and the representation voters receive.

2 Information About Career Prospects

The main argument in this article is that legislators will leave office when it is most lucrative to do so. Additionally, we can use the labor market experiences of former legislators to estimate the price of hiring certain types of politicians on the post-elective labor market. As the rewards that legislators give up by remaining in office grow larger, they will be more likely to retire

from public service. I will unfold the argument in two steps. First, I discuss how we could expect information about opportunity costs to be communicated to current legislators. Second, I argue that pre-Congressional employment histories and committee service provide valuable information about the legislator's characteristics.

There are two ways information about career prospects can be communicated to incumbents. First, legislators might glean their career prospects by observing how well their former colleagues do in the private sector, which makes them update their expectations about how lucrative it would be for themselves to walk through the revolving door. Second, legislators are likely to be contacted directly by actors on the post-elective labor market. The executive recruitment industry is key in this respect: On K Street, headhunting is big business and provides a whole class of professionals whose only job is to persuade the most valuable legislators to walk through the revolving door.[3] The headhunting industry is likely to make MCs extraordinarily well-informed about their outside career prospects.

2.1 Finding Comparable Colleagues

Despite being a relatively homogeneous group, MCs vary to a significant degree in their abilities. Different types are in high demand on the post-elective labor market at particular points in time. To gauge the price of hiring certain types of legislators, I argue that we can compare MC with similar individual characteristics. Specifically, we can leverage the per contract price of hiring a former MC to form expectations as to how well a similar current legislator would do, if she were to walk through the revolving door. I propose two factors that can be used to construct groups of similar legislators—what I call 'reference groups'. 1) The careers, they followed before running for office, and 2) committee assignments during their congressional tenure.

Carnes (2013) has shown that the careers legislators followed before being elected to Congress affect their behavior throughout their time in office. Not only do pre-Congressional

[3]As an example, the following article in the Washington Post provides an interesting look into the headhunting industry around election-time: `https://wapo.st/2yTIPbi`.

career trajectories impact voting, they also predict the content of the bills MCs propose, how hard they work to see them enacted, and their views of the world (see also Francis and Bramlett 2017).

Similarly, the portfolio of committees that legislators have been assigned to during their political careers carry information about their political interests, preferences and post-elective labor market outcomes. Committee membership affords MCs the opportunity to have a political impact, service constituent preferences and attract pork (Berry and Fowler 2016; Schiller 1995; Shepsle 1978). Personal interests that predate the political career also play a role in which committees legislators seek to be assigned to (Fenno 1973). Special interests are generally highly interested in targeting committees in their influence-seeking (Bertrand et al. 2014; Fouirnaies and Hall 2018). This makes connections to committees a valuable asset for revolvers.

In a nutshell, precongressional careers and committee assignments in Congress carry broad information about the politician's type. Legislators with comparable precongressional careers and portfolios of committee memberships are likely to behave similarly during their tenure. This makes it natural to use these factors to construct reference groups.

3 Empirical Strategy

The empirical strategy is illustrated in Table 1. I obtain the main results on data from the Senate—everything is then replicated in the House. Each senator is placed in two distinct reference groups with similar pre-Senate career paths and in-Senate committee assignments, respectively. I then compute the expected dollar size of the lobbying contracts in each of these groups. This serves as my two measures of career prospects. I now elaborate these steps.

Identifying Reference Groups. I started by computing the proportion of each senator's total career that had been spent working in the following careers: lawyer, independent business owner, politician, academia, management of major company, military, public sector employee, private sector employee. I gathered this data from the Congressional Biographical Database.

Table 1: The empirical strategy & expected results

	Reference Group	**Price Per Unit of Labor**	**Career Decision**
Former lawyers	Former Senator$_{1l}$ Former Senator$_{2l}$ ⋮ Former Senator$_{L}$	\$150,000	Currently serving senator$_1$ selects out
Former military	Former Senator$_{1m}$ Former Senator$_{2m}$ ⋮ Former Senator$_{M}$	\$80,000	Currently serving senator$_2$ does not selects out
⋮	⋮	⋮	⋮
Former Career$_K$	Former Senator$_{1k}$ Former Senator$_{2k}$ ⋮ Former Senator$_{K}$	E(success$_s$)	Currently serving senator$_s$ selects out with P(select out | E(success$_s$))

Similarly, for each senator, I calculated the proportion of her career that had been spent in each of the standing committees in the Senate. I used data on committee assignment in the Senate from 103rd to the 113th Congress collected from Stewart III and Woon (2017).

I then used Ward (1963) hierarchical clustering to group senators into one of five groups based on their pre-Senate careers, and one of six based on their mix of committee assignments. In appendix G, I show diagnostics on the cluster analyses. In appendix A, I describe in more detail the data on careers and committee assignments. To get an idea about what the reference groups capture I describe the types of careers that are located in each cluster in Appendix A2-A3. In Appendix C1, I show that the groups contain senators of very different legislative styles. Let two groups serve as illustrative examples. First, the group of former lawyers tend to be moderates and legislatively effective politicians. For instance, Senator Sheldon Whitehouse (D-RI) spent most of his pre-political career practicing law. Afterwards, he was first appointed the US Attorney and later Attorney General—both of Rhode Island. In the Senate, he was very broadly effective, figuring close to the third quantile of the legislative effectiveness scores. On the other hand, the group of former businesspeople with a background in political work tends to be more extremist and less legislatively effective. Senator Pat Toomey's (R-PA) profile captures this group well. In his pre-political career, he worked as a banker and financial consultant and co-owned a local restaurant with his brothers. Before running for Congress, he served in local government. During his tenure in the Senate, he pushed relatively few bills through the legislative process, and was far to the right on the DW-NOMINATE scale, above the 90th percentile.

Measuring Career Prospects as Equilibrium Price. To measure career prospects, I need an estimate of the exogenous part of the price legislators can get for their labor. On the market for lobbying services, the good that is traded is the lobbying contract. This makes it the natural unit through which to measure the price of senator labor. To estimate this, I rely on the average value of lobbying contracts. The logic is that the success experienced by senators-turned-lobbyists will be mirrored by the price of their contracts. Insofar as the most lucrative

contracts represent the most prestigious, interesting and challenging work assignments, and the most highly paid lobbyists are assigned to work on them, this measure of career prospects will capture a mix of salaries and what we can call ego rents more broadly.

Because contract sizes capture a conjunction of salaries and more intrinsic perks, I also follow Ban et al. (forthcoming) and construct a measure of *Lobbyist Value Added* (LVA). This should more closely reflect the earnings attributable to each individual lobbyist. I do this by first using ridge regression to estimate lobbyist fixed effects, which can be thought of as a measure of each lobbyist's earning history (i.e. LVA). I then weight the value of each contract-lobbyist observation by the ratio of each lobbyist's LVA to the combined LVAs of the other lobbyists on the contract. The intuition is that when a lobbyist with a history of working on high-value contracts work with other lobbyists with histories of lower earnings, the former will be attributed a larger share of the contract value. In Appendix B3, I compare the unweighted and weighted measures and provide more detail on the estimation.

The simple average captures the equilibrium price of the type of contract a legislator typically works on. The weighted average is the price of hiring a senator to work on a contract—i.e. the per unit price of their labor.

I use data on lobbying contracts registered under the Lobbying Disclosure Act (LDA), and made available by the Center for Responsive Politics (CRP). Since expenditure by paying clients and in-house lobbyists include different items, I exclude in-house lobbyists (see Blanes i Vidal et al. 2012). Following Ban et al. (forthcoming) I also exclude *pro-bono work*. I match the names of all former senators who have served in the period 1992-2015 to the names reported on the lobbying contracts.[4]

Finally, I predict the average Contract Size in each career and committee group, respectively, for each Congress in the period under investigation. I use linear regressions with an interaction between group and year dummies. This prediction is my final explanatory variable, which I will call *Contract Size* in the remainder of the text.

[4]Note that LDA data is available back to 1998. Senators before that are included in the measurement of career prospects, but not in the main models.

In Appendix C1-C3, I undertake a number of validation exercises. First, I show that the predicted contract size is highly correlated with the actual value of lobbying contracts revolvers come to work on. Second, the prediction obtained from predicting contract size using the senator's actual reference group outperforms the prediction from using any other reference group the senator could have been placed in. Third, I show that the average contract size correlates strongly with external measures of demand for members of the reference group—most importantly, when senators from a reference group gain more campaign donations, the average contract size of lobbyists from the same group increases. Fourth, in Appendix C3.1-C3.2, I show that the price of a lobbying contract is shaped by shifts in the political environment which drives the demand for different types of lobbyists. This helps the substantive interpretation of Contract Size: The price of hiring a revolver increases, when the political environment increases demand for their skill set.

3.1 Dependent Variable: Walking Through the Door

The dependent variable is a binary indicator, which takes the value one in the last Congress before a senator voluntarily chooses to leave office for a job with some kind of special interest. I count jobs in companies (whether they are lobbying firms or ordinary companies) as well as civil society groups (think tanks, NGOs, universities) as employment with special interest groups.

The measure is relatively broad, and includes other post-elective careers than contract lobbyist. This is to avoid selection problems from shadow lobbying. If there is a component of interest representation in, e.g., board service, then the information about demand for senator-lobbyist is likely to be informative about the demand for senator-directors as well. The choice is based on previous research which has found that former legislators employed with these kinds of special interests work with lobbying and interest representation (Egerod 2019; Lazarus and McKay 2012). Thus, the career prospects of senators, who wish to work as lobbyists for NGOs might be proxied by Contract Sizes among contract lobbyists. In Appendix E3, I investigate how informative the demand for senator-lobbyists is for other types of revolvers.

For contract lobbyists, the information for this variable was mainly collected from the CRP, which collects its information from the Senate Office of Public Records. However, for private sector employment that does not require registration as a lobbyist, the CRP registry is incomplete. I use a number of sources to make up for the blind spots in the CRP data among non-lobbyists. First, many companies put out press releases, when they hire former MCs. Second, Bloomberg tracks the career trajectories of a range of high-profile CEOs and government officials. When it came to memberships on boards of directors, the SECs EDGAR database proved invaluable. When none of these sources gave a picture of post-elective careers, I read their biographies on Encyclopedia Britannica and Wikipedia.

In total, 205 senators serving in the 105th to the 113th Congress—of whom 56 leave for a revolving door job in the period of investigation—are included in my models.

3.2 Controls

To measure, whether a senator's political preference is in the party's mainstream or on the fringes, I use the absolute difference between the senator's own roll call score and her party median. I estimate roll call ideal points using the Martin and Quinn (2002) Dynamic Item Response Theory (D-IRT) model. I include the first and second order polynomials. I use the Caughey and Warshaw (2015) measure of state policy liberalism to capture the ideological leanings of the senator's home state. I also use the logged number of years the senator has served in the Senate at time t. Finally, I also include a dummy for whether the senator is up for reelection during the current Congress.

Variable definitions, descriptive statistics and data sources can be found in appendix B.

3.3 Identification

The allocation of lobbyists to work on specific contracts happens internally in the lobbying companies and is shaped without reference to currently serving senators. Therefore, the dollar size of lobbying contracts is unlikely to be related to time-varying characteristics of currently

serving senators. In Appendix D1, I show that Contract Size does not correlate with a number of observable individual characteristics of senators. This makes it an ideal measure to use in a differences-in-differences model with variation in treatment timing, which I estimate by the inclusion of senator and time fixed effects. This differences-in-differences estimate will be causally identified if trends in senators' probabilities of walking through the revolving door would have evolved in parallel had the change in Contract Size not occurred. Because Contract Size evolves outside of the reach of currently serving senators, they cannot select into changes. This makes the assumption plausible.

The most important threat to the parallel trends assumption shocks to the political system, which influence both strategic retirement from office and the general size of lobbying contracts *heterogeneously across reference groups*. To alleviate this concern, I adopt a series of highly flexible models, allowing for some forms of differential shocks and trends.

4 Senate Results

Table 2 presents the results from a number of linear probability models. The indicator for voluntarily leaving office for a revolving door job is regressed on the senator's predicted Contract Size alongside fixed effects for senator and time. The first four columns use pre-Senate careers to measure Contract Size. Panel A presents results for unweighted Contract Size, which will include both broad intrinsic benefits from working on large projects and the monetary value of them. Panel B uses the LVA weighted Contract Size, which more narrowly reflects monetary earnings of senators-turned-lobbyists. Column one presents the bivariate results. I estimate that when the cost of hiring a the contracts senator-revolvers work on increases by \$91,000 (which corresponds approximately to a standard deviation), the probability that the average senator walks through the revolving door increases by 3.7 percentage points. The null can be rejected at a high level of confidence. The corresponding coefficient on weighted Contract Size is smaller, and I cannot reject the null. This is intuitive: the unweighted measure captures career prospects more broadly than the weighted one, which is intended to be an estimate of

monetary earnings alone. To the extent that senators—who generally are very wealthy—do not only react to potential salaries, but also want interesting jobs, this difference in estimates makes sense.

To alleviate concerns that the parallel trends assumption might be violated, in column two I allow each reference group to follow its own time trend. The results maintain.

A major remaining threat to identification is if shocks have heterogeneous effects across reference groups in a manner not captured by the differential linear trends. It would be plausible that changes in majority status or reforms could have different effects across groups. To deal with this, I first allow the parties to follow their own non-parametric trend (column three), and then include an interaction between all controls and the time dummies (column four). This is a highly flexible specification—again, the results do not change dramatically.

In the next four columns, I present the results from similar specifications, but use in-Senate committee assignment to estimate Contract Size. Coefficients are of similar sizes—but somewhat smaller—than in the first four specifications. Importantly, in these specifications, weighted Contract Size is statistically significant.

Lobbying is thoroughly partisan, and a main reason why former political operatives are valuable is that they retain connections to members of their party (Furnas et al. 2019). In the previous specifications, I deal with potential endogeneity concerns arising from this by allowing for differential trends depending on party. However, in columns 8-11, I include party in the measurement of career prospects. In column eight, I use party to group senators instead of the career and committee-based reference groups. This increases the coefficient. In columns ten and eleven, I include party alongside the reference groups when predicting career prospects. Since this requires a threeway interaction on a relatively small sample, there are power issues in this specification which will add noise to the prediction. This might explain why the coefficients decrease slightly from the baseline.

The effect of Contract Size on taking revolving door employment is substantial. One way of gauging this is by comparing it to the average probability of walking through the revolving door which is approximately 4.7%. Additionally, the probability is between 7 and 8 percentage

Table 2: Pay-off from lobbying and timing of resignation

	Dependent variable: Retire for Private Sector Employment										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Panel A: Unweighted Contract Size										
Contract Size (Career)	0.037***	0.033***	0.035***	0.031**							
	(0.010)	(0.012)	(0.013)	(0.015)							
Contract Size (Committee)					0.025***	0.023**	0.022**	0.025**			
					(0.008)	(0.010)	(0.010)	(0.012)			
Contract Size (Party)									0.046***		
									(0.005)		
Contract Size (Party + Career)										0.029***	
										(0.005)	
Contract Size (Party + Committee)											0.016
											(0.011)
Senator FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Group X time trend?	No	Yes	Yes	Yes	No	Yes	Yes	Yes	No	No	No
Time FE X party?	No	No	Yes	Yes	No	No	Yes	Yes	No	No	No
Time FE X controls?	No	No	No	Yes	No	No	No	Yes	No	No	No
Observations	822	822	822	804	770	770	770	755	793	793	770
	Panel B: Weighted Contract Size										
Contract Size (Career)	0.023	0.022*	0.026**	0.023							
	(0.015)	(0.012)	(0.012)	(0.016)							
Contract Size (Committee)					0.028**	0.033*	0.031*	0.031***			
					(0.014)	(0.018)	(0.017)	(0.011)			
Contract Size (Party)									0.046***		
									(0.004)		
Contract Size (Party + Career)										0.028***	
										(0.005)	
Contract Size (Party + Committee)											0.016
											(0.011)
Observations	806	806	806	788	770	770	770	755	793	793	770
Senator FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Group X time trend?	No	Yes	Yes	Yes	No	Yes	Yes	Yes	No	No	No
Time FE X party?	No	No	Yes	Yes	No	No	Yes	Yes	No	No	No
Time FE X controls?	No	No	No	Yes	No	No	No	Yes	No	No	No

*Note: Dependent variable is SIG Career. Driscoll-Kraay robust standard errors in parentheses. Estimates are OLS coefficients with independent variables standardized. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.*

points higher during election years. Thus, the effect of changing Contract Size by one standard deviation corresponds to between one-third and half of the impact of finishing a term.

4.1 Robustness Checks

In Appendix D2 I run an extensive set of robustness checks. First, I show that the results are robust to using different numbers of career clusters. Second, I test the robustness to different ways of estimating the typical contract size (mean, median and total). Importantly, the results do not hold when using the total contract size. To investigate why this is, in Appendix C3.4, I find that the predicted total value correlates weakly with my external measures of demand or not at all. This indicates that senators base their retirement decisions on the price of hiring revolvers of their type—not what former senators are earning. While this will obviously be related, there are important exceptions. For instance, a high total can be driven by many small contracts, or few extremely large ones combined with many of low value. Crucially, a high expected value per contract will entail a combination of high earnings, intrinsic value and job security, which a large sum will not necessarily do—i.e. a strong demand for high-priced contracts.

I run a set of placebo tests, where I show that senators, who leave the labor market entirely after retiring from Congress are unaffected by career prospects in the private sector. The latter is a very strong test, indicating that it really is career prospects and not other factors, which are behind the results.

In Appendix D3, I test for pre-treatment trends. The results show no trends prior to changes in Contract Size. One potentially salient concern is that the results could be driven by measurement error induced by reforms, which have lead many to avoid registration after walking through the revolving door (LaPira 2014). In Appendix D4 I show that congress-by-congress estimates are relatively stable in the periods before and after the 2007 reform. This indicates that any bias caused by this is soaked up by the time fixed effects.

In Appendix D5, I conduct a series of additional robustness checks. First, and most

importantly, because lobbying contracts vary idiosyncratically, and there is error associated with the cluster analysis, my measure of career prospects contain error. To test the sensitivity of my results to this, I follow two strategies. First, I implement the Method of Composition (Caughey and Warshaw 2017; Treier and Jackman 2008). Second, in Appendix D6, I bootstrap the full procedure to bias-correct the uncertainty estimates. I implement a non-parametric bootstrap, where I resample the input for the cluster analysis and let the additional uncertainty propagate through the remaining steps of the model. Additionally, I use a sequential bootstrap where I first resample the cluster analysis, and then—within each draw from the cluster model—bootstrap the estimates of career prospects. The results maintain.

Finally, I include random effects at the level of the reference group, cluster the standard errors at the senator-level and use the non-parametric bootstrap with resampling at the senator-level. The results from the career specifications are highly robust, while the uncertainty estimates of the committee-based models vary slightly more. Overall, the baseline results are supported.

5 The Mechanism: Opportunity Costs to Holding Office

The turning point in the argument presented here is that elected politicians discount gains from staying in office against potential private sector options (cf Weschle 2019). If this is correct, the senators, who have the most to gain from staying in office, should not be affected by private sector career prospects.

5.1 Senators Are Lured When Costs to Holding Office Are High

First, I exploit the fact that the retirement scheme for MCs becomes significantly more lucrative at specific points in their tenure. After serving five years in Congress, members are eligible to receive full pension, when they reach 62 years of age. When they have served for twenty years, full pension is available at the age of 50, while members serving for more than 25 years can receive a full pension at any age. I compute the number of years until a senator's pension

scheme improves, and use the Hainmueller et al. (2019) binning estimator and estimate effects within bins. I separate a) those who will never see another improvement, from those who will see one b) within this election cycle and c) those who will have to run for reelection at least once. The results are presented in Figure 1. Panel A presents results for pre-Senate career based measure of Contract Size, while Panel B shows the ones for the committee assignment based measure.

The results show that the effect of Contract Size is driven by the senators, who are not about to receive a hike in the lucrativeness of their pension scheme. Both for the pre-Senate career and committee assignment specifications, there is no discernible effect of Contract Size, when the senator's pension scheme will become more lucrative before next election. Among senators, who will never see another improvement, the effect is considerably larger than the average effect. For those with ten years until their pension scheme improvement, the impact of Contract Size is about the average effect. The patterns are comparable between specifications, but there is more noise in the models relying on committee assignments.

Uncertainty about a senator's political future is likely to exacerbate opportunity costs to holding office. In the second specification, I use the margin with which the senator won her seat in the previous election. If it is unlikely that she will be reelected and can continue her political career, gains from holding office go towards zero, and the prospect of lucrative employment in the private sector should be more alluring. There is no clear pattern when using pre-Senate careers. The results for the committee specifications, however, show that for senators, who were elected by a margin below six percentage points, the impact of Contract Size is twice the average effect.

Revolvers make two different decisions: 1) to retire and 2) to go into some form of lobbying. However, senators have different reasons to leave office other than simply deciding to walk through the revolving door. Some are defeated for reelection, others remain in office until they are so old that they leave the labor market after their political career. Importantly, we can leverage these ideas to delve further into the incentives structuring revolving door retirements.

First, since senators who choose to retire are in the best possible position to plan their

future career trajectories, they are most likely to assess the opportunity costs to remaining in office. Senators, who leave Congress, because they lose an election, on the other hand, are in no such position. This would lead us to expect a larger effect for resigning senators.

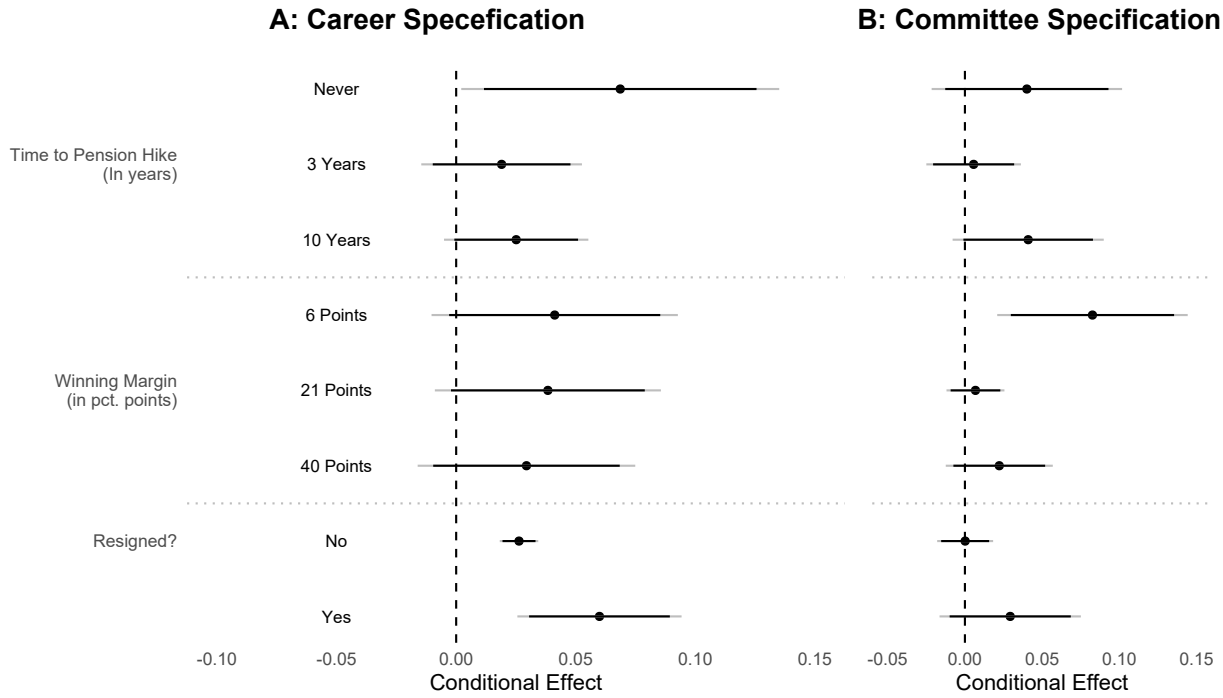

Figure 1: Effects for senators with differing opportunity costs.

Note: Panels A and B show results based on pre-Senate Careers and in-Senate committees. In the first two specifications, effects are estimated within bins using the Hainmueller et al. (2019) binning estimator. In the final one, effects are estimated at each level of the binary variables. Senator and time fixed effects are included in all models. Robust confidence intervals are 95 percent (thin lines) and 90 percent (thick lines).

The figure shows the effect of Contract Size for the subset of senators, who, respectively, did and did not resign of their own volition. The point estimate for senators, who chose to resign, is largest. In the specification using committee assignments, however, it is noisy, and statistically insignificant. Importantly, this difference should be driven by election losers not needing the information on career prospects at this time. They should, however, react to it when it is clear that they will have to leave the Senate. In Appendix E1, I show that this is so: election losers react as strongly to career prospects in lobbying at the time when they leave office. Second, regarding complete retirement, I have already discussed how there is no effect of Contract Size on retirement for leaving the labor market. In addition, in Appendix E2, I show that there is a strong nonlinear age-gradient in this—effects of career prospects on becoming a revolver is concentrated among senators between the late 50s to the late 70s.

6 The Revolving Door Shapes Political Selection

While serving in Congress, legislators face many competing demands on what little time they have. Legislators with certain 'styles' prioritize their time differently (Bernhard and Sulkin 2018). While legislative strategists choose to sponsor bills on a broad array of topics, policy specialists focus on a few areas on which they build expertise. Additionally, all legislators have to raise campaign funds—but some pursue them more vehemently than others (Hall 2019). Crucially, specialists are likely to be in high demand, and fundraisers can have developed ties to future employers. If this results in the revolving door attracting senators differently depending on legislative styles, it can have important selection effects.

I draw on six separate measures to capture legislative styles—three to measure broad legislative engagement, two to capture legislative specialization, and one to measure fundraising. First, I use the average number of bills a senator has sponsored throughout her career. Second, I calculate each senator's eigenvector centrality in the cosponsorship network. To do this, I collect data on each bill's cosponsors (data on sponsorship and cosponsorship are from GovTrack (2017)). I then follow Fowler (2006) and construct directed networks capturing the number of

times each other senator has signed on as a cosponsor supporting the sponsor's bill. Extracting eigenvector centrality captures not only how central each senator is, but also weights this by the centrality of her cosponsors. This captures two important features: how hard the senator has worked to drum up support among well-connected cosponsors, and how well connected she is in the Senate. Third, I use Legislative Effectiveness Scores (LES) (Volden and Wiseman 2018), which capture not only how many bills a senator has sponsored, but also a weighted combination of 15 indicators capturing how far the senator has managed to move those bills through the legislative process.

I use two different measures of specialization. As a first simple one, I calculate the time a senator has spent chairing subcommittees, using data from Volden and Wiseman (2018). Second, I construct a measure of how narrow a set of topics a senator has sponsored bills on. I use data from Adler and Wilkerson (2018) who have categorized bills into Political Agendas Project (PAP) topics. I use the minor topic codes. I then calculate each senator's Herfindahl-Hirschman Index (HHI), measuring their how concentrated their bill sponsoring activity was across topics—higher scores indicate more concentration. To measure fundraising intensity, I calculate each senator's average contribution size from Bonica (2016).

I average all measures over the senator's tenure in office.

6.1 Selection Effects

I examine this in Figure 2. Panel A, B and C show how effects vary depending on the senators broad legislative activity. Panels D and E show results for policy specialization, while panel F presents results on fundraising intensity. Each panel presents estimates from a linear interaction model along with Hainmueller et al. (2019) binning estimator for non-linear effects. The results presented in the main text are for career-based reference groups. Appendix F1 presents the results using committee assignments which show the same pattern.

Looking first at broad legislative engagement, it is clear that the senators, who on average have sponsored the least bills per Congress, are affected the most. The average probability of

selecting out for a senator, who has introduced only 20 bills in her average Congress, increases by almost 7 percentage points, when career prospects improve by one standard deviation. For senators, who have sponsored 40 bills in their average Congress (one standard deviation more), the effect is 4 percentage points lower. After this point, the effect approaches zero and becomes statistically insignificant. The same pattern exists for both cosponsor centrality and LES: The effect is by far strongest for senators with low values on these two moderators.

The results for specialization show the opposite pattern. While less precise, they indicate that the effect of Contract Size are considerably larger for senators who have specialized in a narrower set of topics. The same is the case for fundraising.

This suggests that legislative specialists and fundraisers are attracted away from public service to a much higher degree than broad legislative strategists.

7 Career Prospects and Retirement in the House

In this section, I present the results of a full out-of-sample replication in the House of Representatives. The theory presented in this paper is broad: career prospects should matter in the House as well. Replicating the main results is, therefore, both theoretically and normatively important.

7.1 Data

In order to replicate the results, I have gathered data from a wide range of sources. To construct career-based reference groups in the House, I rely on the Carnes (2016) *CLASS* dataset, which tracks pre-political employment histories of MCs between the 106th and 110th Congress. Once again, I rely on the Stewart III and Woon (2017) data on committee membership. The replication is as exact as possible, and I use the same specifications as I did in the Senate. Therefore, I extract five and six clusters, respectively. Again, I rely on CRP data to measure contract sizes, and identify MCs that register as lobbyists in their post-elective lives. The dependent variable is slightly more narrow in this situation, as I—due to data constraints—only record MCs that

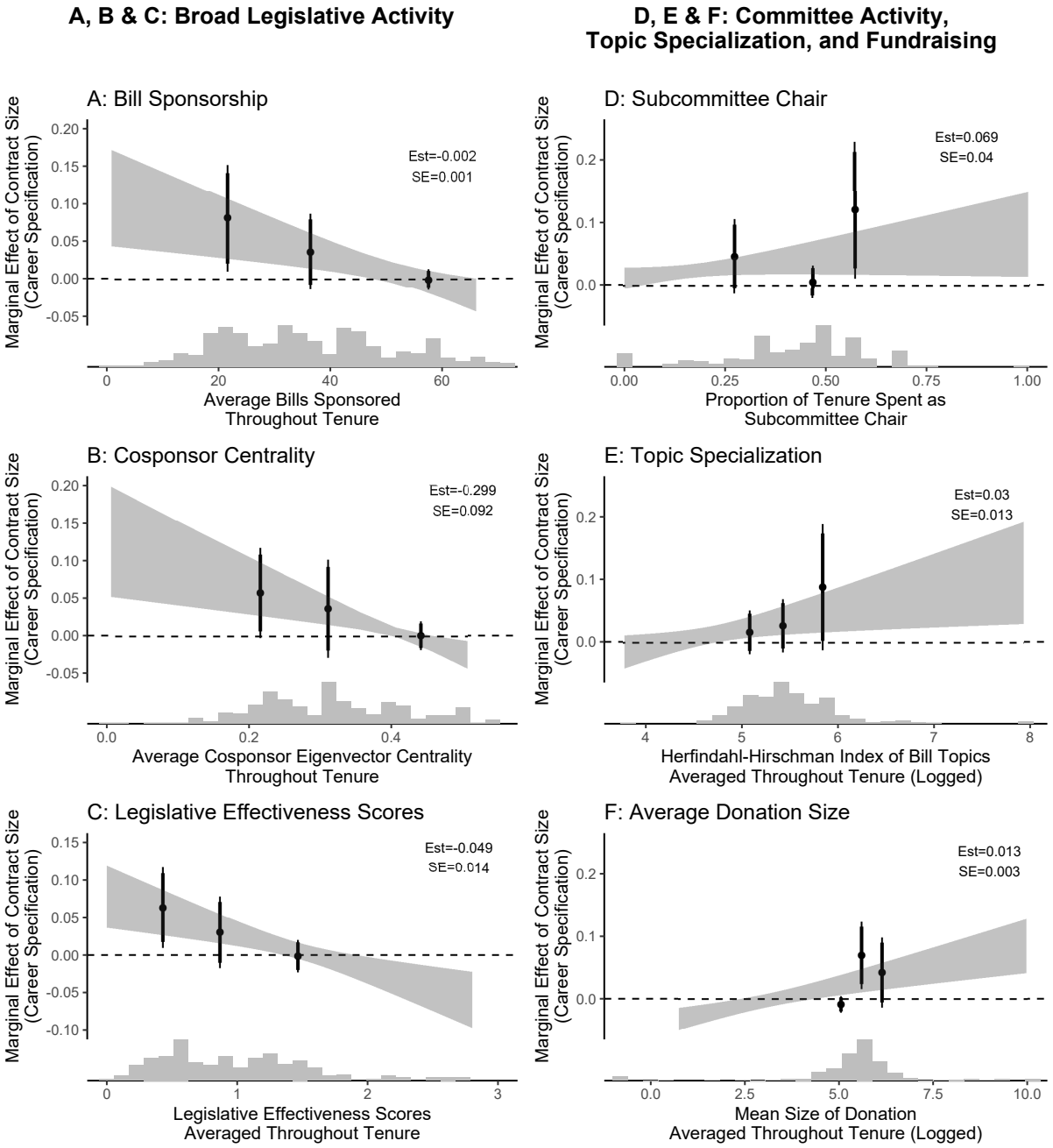

Figure 2: Selection Effects of Career Prospects.

Note: Senator and time fixed effects are included in all models. Shaded areas are 95 percent robust confidence intervals. Thick and thin lines are 90 and 95 percent confidence intervals of the binning estimator. Results using committee assignments can be found in appendix F1.

register as lobbyists.

To investigate selection effects, I use data from Volden and Wiseman (2014) to measure LES, bill sponsorship, chairing subcommittees, and time remaining until an MC's next improvement in her Congressional pension. I use data from Adler and Wilkerson (2018) to compute HHI indices of how concentrated the MC's bill sponsorship is within PAP-topic codes. Finally, I use GovTrack (2017) to construct cosponsorship networks for the House, and calculate the centrality of MCs. This provides data on the House of Representatives covering a shorter time period than available for the Senate, but for more unique MCs. In total, I have data on 657 members.

7.2 The Lure in the House of Representatives

Table 3 presents the results from the replication. Since the coefficients imply percentage point changes, they will have different implications because the two chambers differ in size. I facilitate comparison with the Senate results in two ways. First, I normalize Contract Size in the House by the Senate standard deviation—$91.000. Additionally, I present the number of MCs we would expect to select out of the House following such a change along with the coefficient and its standard error.

The results suggest that an improvement of $91.000 in outside career prospects increases the average probability of selecting out in the House by 1 and 3.5 percentage point, respectively, depending on how we estimate Contract Size. This corresponds to, respectively, four and fifteen additional revolvers. Thus, while the former estimate is approximately the same as in the Senate, the latter is significantly larger. The results are robust to the inclusion of time by party interacted fixed effects, allowing for differential trends between the two parties.

In the Appendix F2, I also replicate the retirement effects that I uncovered in Figure 1. While the results suggests that the same mechanism is at play in the House, they are also more noisy than in the Senate.

Finally, in Appendix F3, I investigate the differential effects across the same variables as I

Table 3: Career Prospects and Revolving out of the House

	Dependent variable:			
	Register as a Lobbyist			
	(1)	(2)	(3)	(4)
Contract Size (Career)	0.010**	0.014***		
	(0.004)	(0.004)		
Contract Size (Committee)			0.035*	0.038***
			(0.020)	(0.013)
Additional Revolvers	≈ 4	≈ 6	≈ 15	≈ 17
MC FE?	Yes	Yes	Yes	Yes
Time FE?	Yes	Yes	Yes	Yes
Time X Party?	No	Yes	No	Yes
Observations	2,103	2,103	2,029	2,029

*Note: Dependent variable is an indicator for the final Congress before leaving for a job as registered lobbyist. Driscoll-Kraay robust standard errors in parentheses. Contract Size is normalized by $90.000. Coefficients are from OLS regressions. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.*

used in the Senate. Interestingly, we observe no strong patterns of moderation, indicating that the revolving door will impact representation differently in the House.

To investigate why, in F4, I draw on data from Carnes (2016) to show the distributions of net wealth in the two chambers. Whereas the median Senator controls upwards of $1,8 million in personal wealth, the median Representative only owns approximately $630,000. In Figure 3 I show that wealth strongly moderates the effect of outside career prospects in the House. The effect is concentrated among the least wealthy and drops off quickly as wealth increases. In the Senate, however, the trend is reversed and the moderation is not statistically significant. The results should be taken with the caveats that the Carnes data only covers the 108th to the 110th Congress, and the results are less clear for the committee-based models in Appendix F4. Still, these results provide a strong—if preliminary—indication that differences in personal wealth is key to understanding the different selection effects between the two chambers.

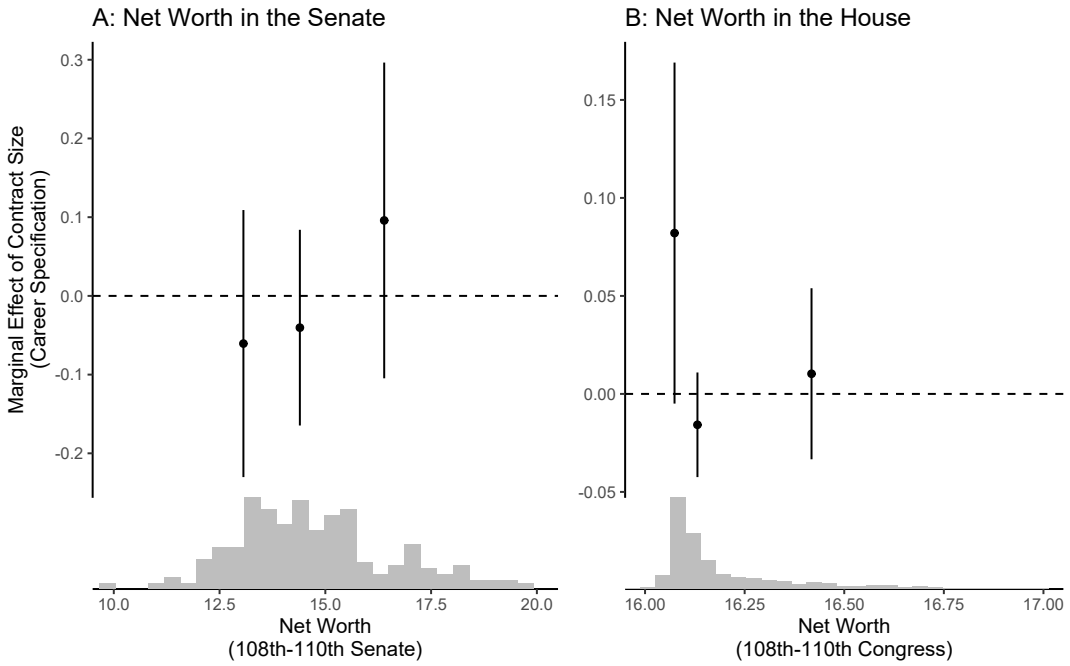

Figure 3: Net worth Moderates the Effect of Outside Career Options.

Note: Effects estimated within tertiles of log net worth using Hainmueller et al. (2019). Lines are 95 percent robust confidence intervals.

8 Conclusion

Motivating good candidates to run for office is a precondition for high quality government, but so is persuading them to stay. My results show that Members of Congress are attracted by the potential for private sector employment opportunities and leave office to pursue them when prospects are good.

The results were originally produced on Senate data and then replicated in the House of Representatives. The results indicate that when the price of hiring a legislator to work on a lobbying contract increased by $91,000 (one standard deviation), the probability that the average legislator left Congress for a lobbying job rose markedly. It caused between two and three additional senators and between four and fifteen Representatives to become revolvers.

I provided evidence that it, indeed, is opportunity costs associated with holding elected office, that drives the effect. Specifically, there was no effect of career prospects immediately before and after MCs experience improved pension schemes. Instead the effect was localized among legislators, who would never see another improvement, or who had to run for reelection before one. Similarly, legislators, who only narrowly won their seat in the previous election were affected at an above-average rate.

These results have at least two important implications. First, they show that monetary gains do not only structure the selection into public service. Their effect persists even after candidates have entered elective office, shaping the timing of resignation. Legislators take stock of the opportunity costs associated with being in politics by gauging the career prospects available to them outside of public service. When the rewards they relinquish by holding elected office are lucrative enough, the average legislator will leave public service to take private sector employment.

Second, the results clearly show that the outside career options exert a different pull depending on the type of legislator. Thereby, the revolving door shapes the type of legislator that remains in office. The patterns of selection, however, differ by chamber. In the Senate, the revolving door attracts legislative specialists, leaving strategists with a broader focus. In

the House, there were no strong patterns across different legislative styles. Instead, there was preliminary evidence that least wealthy MCs reacted most strongly to the lure of the private sector. Both selection effects are likely to shape the quality of representation voters receive. Private career prospects attract legislators with specific policy capabilities out of the Senate which might contribute to a deficit of policymaking expertise. The moderating effect of personal wealth in the House suggests that the revolving door could play a part in skewing representation towards wealthy interests.

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Online appendix for: The Lure of the Private Sector

Contents

A Describing data on committees, careers and reference groups

In this appendix, I briefly describe the raw data on committee assignments pre-Senate careers as well as the mixes of, respectively, pre-Senate careers and in-Senate committee assignments, that distinguishes the clusters from each other. I also show how many senators there are in each group.

A.1 Descriptives on committee and career data

Figures A.1 and A.2 show the average time spent in each type of pre-Senate career and Senate committee.

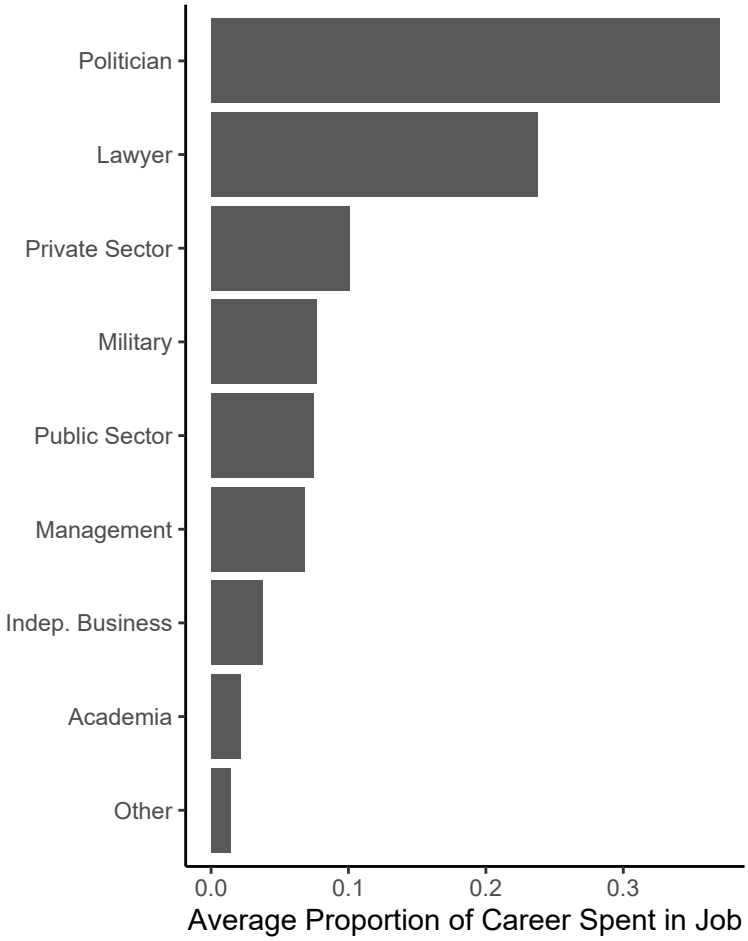

Figure A.1: Average proprotion of pre-Senate career spent in various job types.

A.2 Describing reference groups based on pre-Senate careers

The distributions presented in Figure A.3 indicate that the cluster analysis has successfully separated five distinct types of pre-Senate careers. If we define the groups by their one or two most prominent careers, we can call them the Private Sector-Politicians, the Public Servant-Politicians, the Lawyers, the Lawyer-Politicians and the Military-Politicians, respectively. While holding some other political office than the Senate represents an important component in four of the five pre-Senate career trajectories, each group captures a specific mix of political to non-political career paths. As noted previously, these different combinations of employment histories, that make up the career types measured here, are likely to be associated with world-views (either due to career socialization or selection)

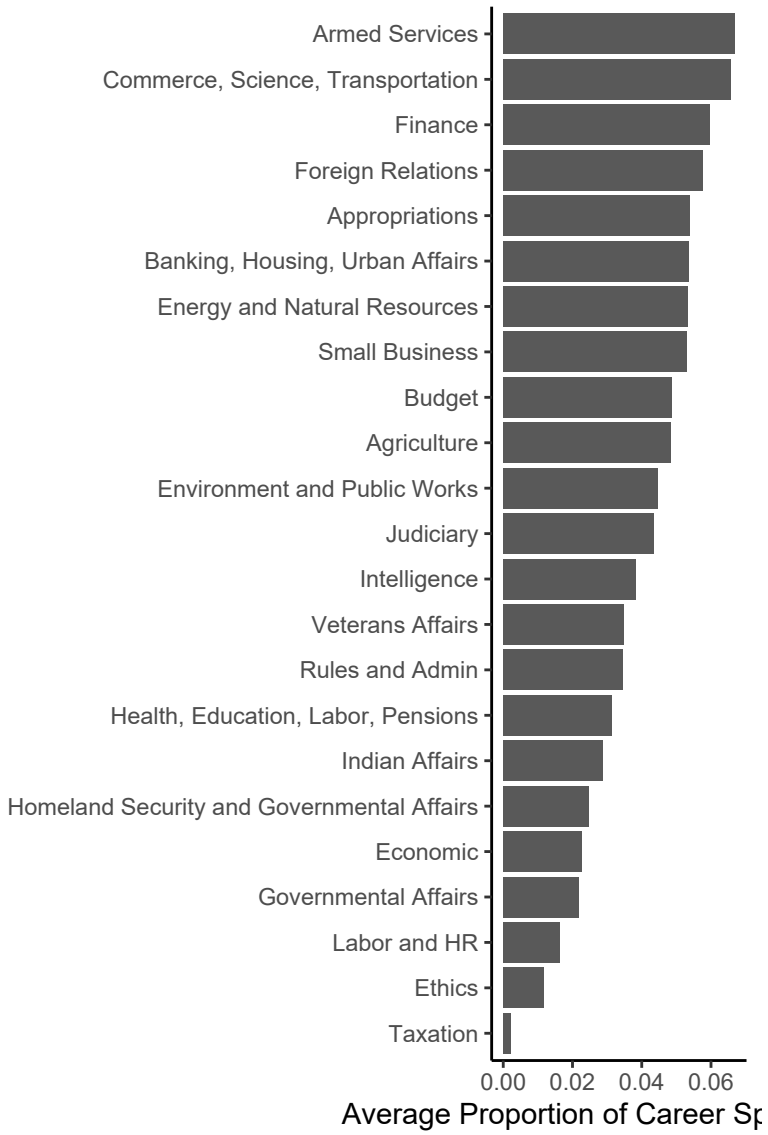

Figure A.2: Average proportion of tenure spent in various Senate Committees.

– it hardly seems like a foolish expectation that senators, who have spent most of their career working in the public sector before entering politics, would behave differently from senators, who have a military background.

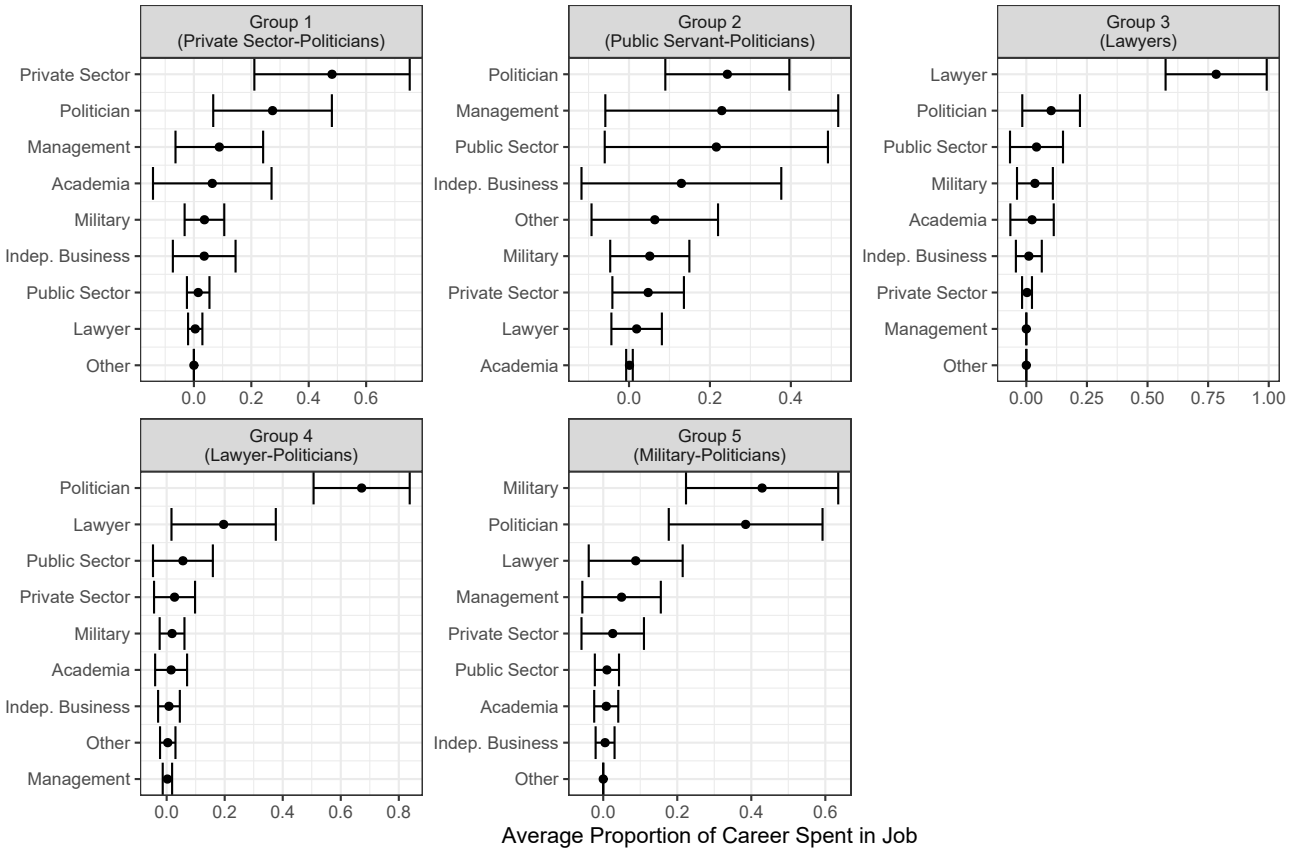

Figure A.3: Mix of pre-Senate careers within five different reference groups.

Note: Points show the average time of pre-Senate careers spent in each type of career. Whiskers show one standard deviation below and above the mean, respectively. These results are based on all senators serving between the 102nd and the 113th Congress.

A.3 Describing reference groups based on committee assignment

Similarly, for each of the six clusters based on committee assignment, Figure A.4 shows the average proportion of a senator's career that has been spent in a given committee. Again, if we name each cluster after their one or two most prevalent committees, we can see that the cluster analysis provides a number of distinct mixes of committee assignments.

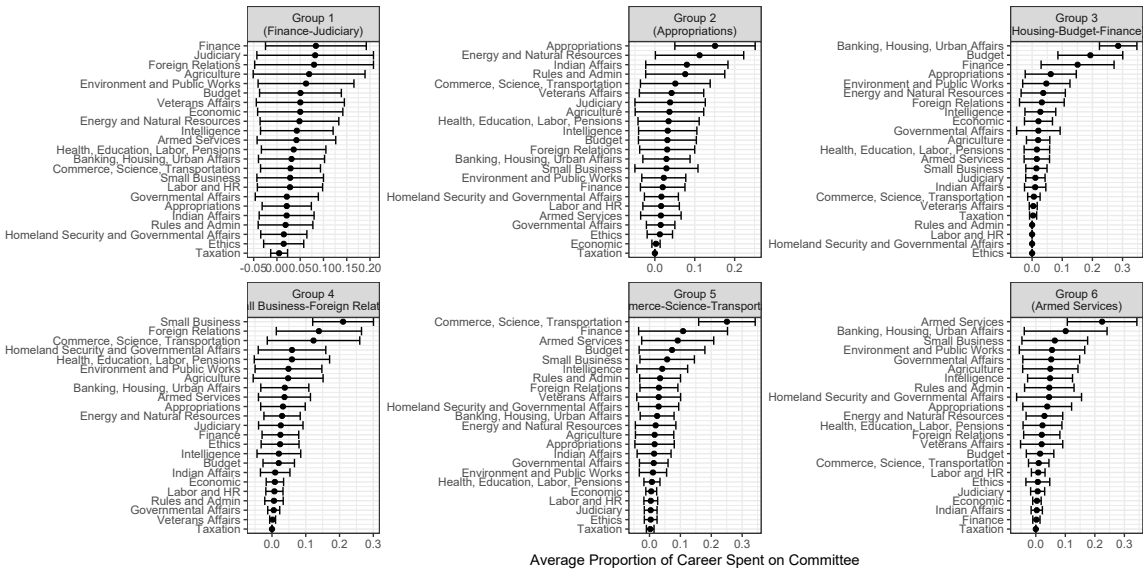

Figure A.4: Mix of committee assignments within six different reference groups.

Note: Points show the average proportion of Senate tenure spent in each committee (special committees and leadership positions excluded) for senators serving between the 103nd and the 113th Congress. Whiskers show one standard deviation below and above the mean, respectively.

A.4 Counts of senators and senators-turned-lobbyists in each group

Table A.1 shows how many senators there are in each reference group overall and how many of them, who leave office to take lobbying jobs. The latter are used to estimate Contract Size – the measure of career prospect used in this paper. As we can see, predicted Contract Size rely on relatively few observations in each group. The low number precludes me from only using senators, who recently became lobbyists. Instead, I have to rely on the yearly contract sizes of all former senators, who at a specific point in time work as lobbyists and are in my data set.

The relatively low number of senators-turned-lobbyists in each group could cast doubt on the validity of the measure. However, the convergent validation exercises, which I perform in Appendix C, suggests that the measure is valid despite the relatively low

frequencies. Additionally, the results are not sensitive towards extracting fewer (or more) reference groups from the cluster analyses, which would change the frequencies in each cell.

Both these concerns (the low N and noise in the cluster analyses) would give rise to (random) measurement error. In appendix C.3, I use Method of Composition (Tanner 1996; Treier and Jackman 2008) to alleviate concerns that the results could be driven by measurement error. In combination, the convergent validation, the stability of the results and the robustness towards random measurement error should provide reassurance that the results are not statistical artifacts of the measure being used.

Table A.1: Distributions of senators across reference groups

	Reference group					
Panel A: Pre-Senate Careers	Private Sector	Public Servant	Lawyers	Lawyer-Politician	Military-Politician	
Total senators from group	39	49	49	75	26	
Total lobbyists from group	9	10	8	15	7	
Panel B: Committee Assignments						
	Finance-Judiciary	Appropriators	Housing-Budget	Small Business-Foreign Relations	Commerce-Science	Armed-Services
Total senators from group	82	83	35	6	57	20
Total lobbyists from group	18	10	4	2	5	5

Note: Panel A shows how senators are distributed across career based reference groups, and how many of them register as lobbyists after elective tenure. Panel B show the parallel distributions for reference groups constructed using committee assignment.

B Variable definitions and descriptive statistics

B.1 Variable definitions and data sources

In Table B.1, I present definitions and sources of all variables included in this study.

Table B.1: Definitions of variables included in the models

Variables	Description	Data source
Dependent variables		
SIG Career	Does the Senator take revolving door job at the end of current Congress?	CRP, 10-K reports, press releases, Wikipedia.
Leave labor market	Does the Senator leaves the labor market at the end of the current Congress (placebo)?	Congressional Biographical Database (CBD)
Primary Explanations		
Contract Size (Career)	The predicted dollar size of the average lobbying contract of senators-turned-lobbyists with pre-Senate labor market experiences that are similar to the currently serving senator's.	CBD and CRP.
Contract Size (Committee)	The predicted dollar size of the average lobbying contract of senators-turned-lobbyists with mixes of committee assignments that are similar to the currently serving senator's.	Stewart III and Woon (2017) and CRP
Contract Size (LVA weighted)	Similar to the above, but where each report-lobbyist observation is weighted by the ratio of the lobbyist's LVA to the combined LVA of the other lobbyists on the contract. Each lobbyist's LVA is estimated through ridge regression (see below).	See above.
Covariates		
Difference to party	Difference between senator's own D-IRT and party median D-IRT.	Own calculations.
Seniority	The number of years the senator has served in the Senate at time t.	CBD and own calculations.
State Policy Liberalism	IRT estimates of liberalism of state policy.	Caughey and Warshaw (2015)
Election Year	Dummy for whether the senator is up for reelection in the current Congress.	Ballotpedia.
Mechanism variables		
Pension hike	Time until/since the senator receives her next improvement of her pension scheme	Own calculations based on time spent in both chambers of the US Congress.
Winning margin	Difference between the senator's and the runner up's vote share in the previous election.	Ballotpedia.
Resigned?	Did the senator leave Congress of her own will or lose an election?	CBD
Average Bills Sponsored	The number of bills that the senator has sponsored in her average Congress over her tenure.	GovTrack (2017)
Average Cosponsor Centrality	The senator's eigenvector centrality, averaged over her tenure.	Cosponsor data: GovTrack (2017)
Average Legislative Effectiveness Scores	Weighted combination of the number of bills sponsored and how for they each get in the legislative process. Measure averaged over the senators time in the Senate.	Volden and Wiseman (2018).
Sub-Committee Chair	Proportion of career spent as chair of sub-committee	Volden and Wiseman (2018)
Topic Specialization	The Herfindahl- Hirschman Index measure of how concentrated a senator's bill sponsorship is within PAP topic codes from. Averaged over time in Senate.	Own calculation based on Adler and Wilkerson (2018)'s minor topic codes.
Fundraising Intensity	Average donation size throughout tenure.	Own calculation based on data from Bonica (2016).

B.2 Descriptive statistics

Tables B.2 shows descriptive statistics on key variables in the study.

Table B.2: Descriptive Statistics

Statistic	N	Mean	St. Dev.	Min	Max
SIG Career	1,134	0.049	0.217	0.000	1.000
Contract Size (Career)	886	0.960	1.000	−0.000	4.654
Contract Size (Committee)	831	0.767	1.000	0.116	5.338
LVA Contract Size (Career)	867	2.034	1.000	0.318	3.945
LVA Contract Size (Committee)	831	2.075	1.000	0.370	5.246
Ideal Points	1,233	1.098	3.272	−6.681	12.550
Seniority	1,232	13.532	10.258	1.000	52.000
Difference to Party	1,233	2.025	2.003	0.000	10.961
Election Year	1,244	0.334	0.472	0	1
State Policy Liberalism	1,213	0.074	1.228	−2.525	2.743
Resigned?	1,244	0.072	0.259	0	1

B.3 Estimating LVA Weighted Contract Size

As mentioned in the main text, I follow Ban et al. (forthcoming) and construct a measure of average Contract Size that is weighted after each lobbyist's long-run value added—the LVA. I do so by estimating each lobbyist's fixed effect. The data has a very high level of dimensionality and fixed effects are highly correlated. To deal with this, I follow Ban et al. (forthcoming) and only estimate lobbyist fixed effects for those, who appear on at least 12 reports throughout their career. Lobbyists, who have worked on fewer reports—'supporting lobbyists'—are pooled in a joint fixed effect. Second, I use ridge regression, which is a particularly effective way to stabilize the estimation in the presence of high multicollinearity. This is done by minimizing the function:

$$y_{ijr} = \sum_{ijr}(y_{ijr} - \gamma_i - \delta_j)^2 + \lambda \sum_{ij} ||\gamma_i^2 + \delta_j^2||_2^2.$$

Where y is the dollar-value of contract, r, which lobbyist, i, and supporting lobbyist, j, work on. γ and δ are ficed effects for each lobbyist and supporting lobbyist, respectively. This is similar to the least squares estimator, but adds a shrinkage term, which penalizes

the fixed effects by a squared function of their size. Importantly, λ—the regularization penalty—decides the size of the shrinkage. Because the estimation is highly demanding, I divide the total LDA data in subsets consisting of 1,000 randomly sampled lobbyists and estimate the fixed effects of them. Between 1998 and 2015, the LDA data includes roughley 18,000 unique lobbyists, meaning that I run the ridge regressions in 18 iterations. Because the samples are randomly drawn, this should not influence the estimates themselves, but it does provide considerable computational efficiency. Within each sample, I apply 5-fold cross validation to find the λ that minimizes out-of-sample prediction error. I then use the resulting LVAs to construct a weighted measure Contract Size, which should capture lobbyist earnings, separate from other perks of working on large contracts.

In Figure B.1 I compare the unweighted and LVA weighted measures of yearly Contract Size for senators-turned-lobbyists. Similarly to Ban et al. (forthcoming), low levels of the unweighted measure yields significantly higher levels of the weighted measure. This stems from the fact that senators generally have histories of working on more lucrative contracts, and are attributed a larger share of each reports dollar-value. As discussed in the main text, this does not capture the fact that there are other benefits associated with working on valuable contracts, which might attract the senator. These are important, when investigating what draws legislators into the lobbying profession, while the weighted measure is likely better, when focus is on estimating lobbyist revenue or earnings (Ban et al. forthcoming; Blanes i Vidal et al. 2012; McCrain 2018).

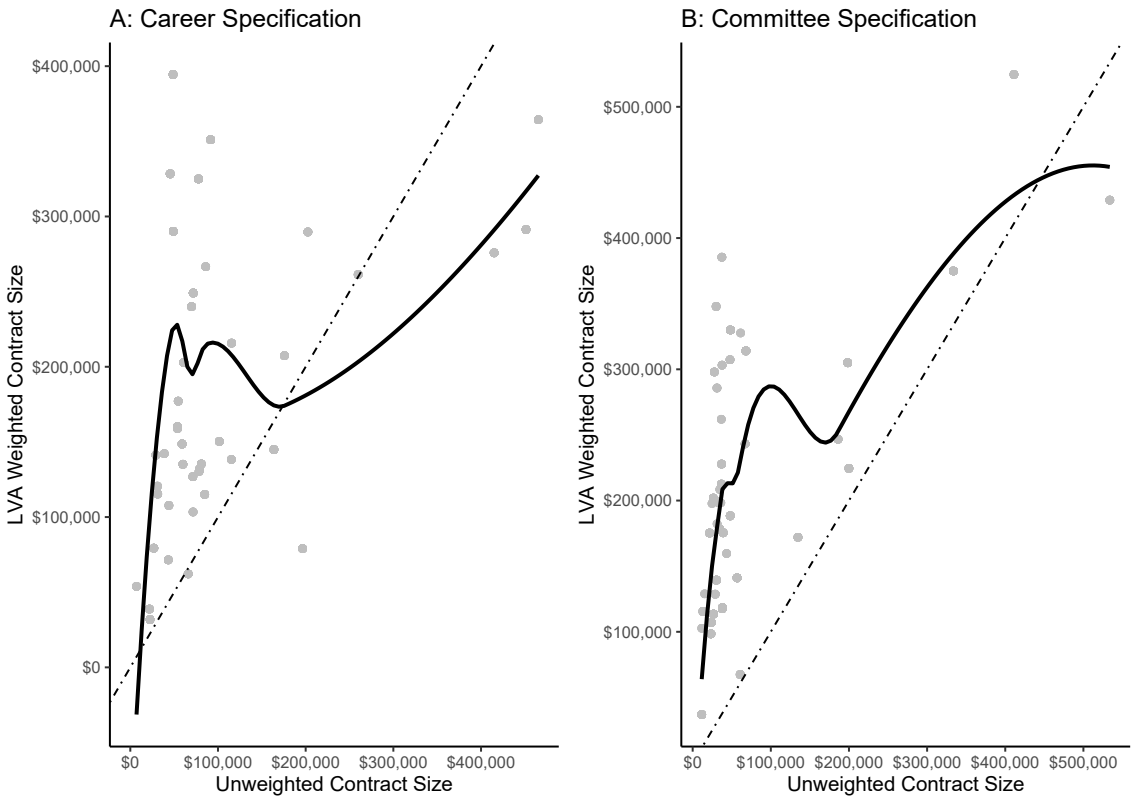

Figure B.1: Comparing Unweighted and Weighted Contract Size.

Note: The figure shows a comparison of the unweighted and weighted Contract Size for both the career and committee specifications. Solid line shows loess estimates, dash-dotted line shows perfect correspondance.

C Validating the Measure of Career Prospects

In this section, I validate my measure of career prospects. I do so in two ways. First, I focus on the reference (or affinity) groups and show that they contain senators with different legislative styles. Second, I show that estimated contract size correspond closely to the contract sizes that the senators, who choose to walk through the revolving door would work on. Additionally – and perhaps most importantly – I also show that contract sizes predicted using a senator's own reference group corresponds much more closely to realized contract sizes than the predictions that arise from using other reference groups.

After validating the measure of career prospects, I investigate what drives it. This helps in providing a substantive interpretation of what changes in career prospects represent. Additionally, it provides a test of the measure's construct (Adcock and Collier 2001) validity by showing that it correlates with factors we would expect it to correlate with—most importantly, other measures of the demand for certain political actors.

C.1 Can Affinity Groups Distinguish Senator Styles?

An important first step in validating the measure of career prospects lies in investigating whether the reference groups at the heart of it are sensible. They are theoretically reasonable (they have high construct validity (Adcock and Collier 2001)), but do they represent meaningful groupings of senators?

In this section, I show that both the reference groups based on pre-Senate careers and in-Senate committee assignment include distinct types of senator styles. Specifically, I show that the groups are very different in terms of ideological extremity (moderation), time spent engaging with broad legislative activity, and fundraising. These are important elements (but not a comprehensive list) of different legislative styles (Bernhard and Sulkin 2018). The results are presented in Figures C.2 and C.3. For instance, we observe that senators who have spent more time in politics are relatively less moderate in their voting, while those with a background in law are more moderate. On the other hand, lawyers spend more time sponsoring bills and raising money.

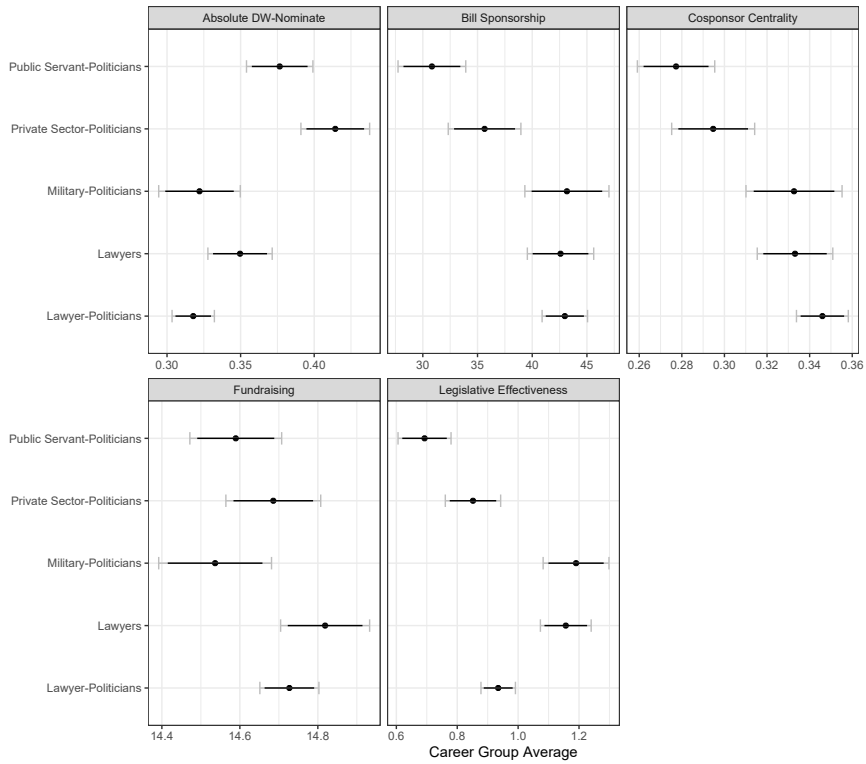

Figure C.2: Career Groups and Senator Legislative Style.

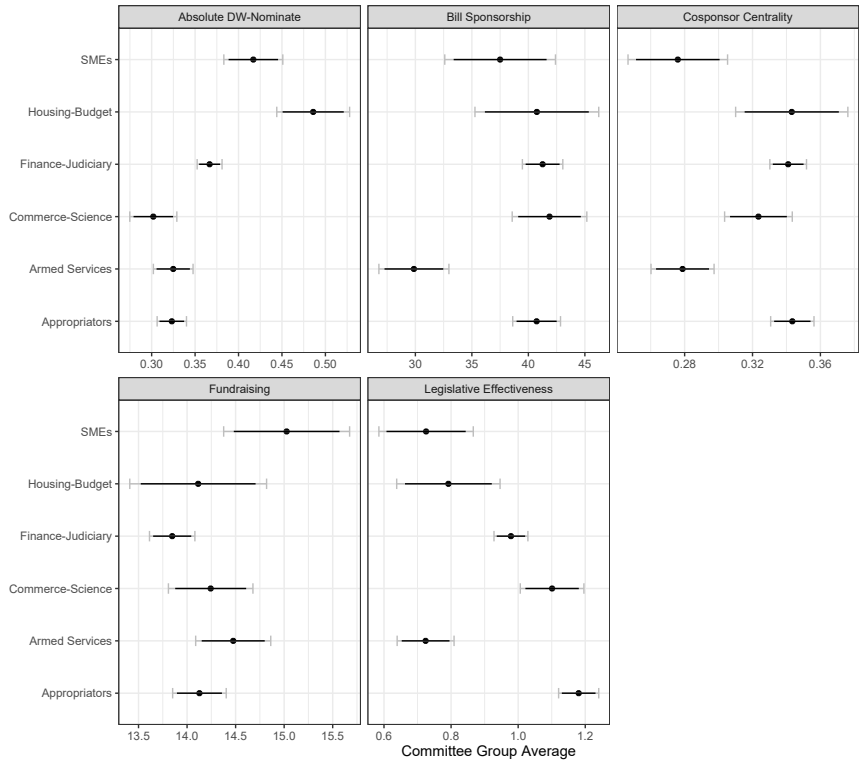

Figure C.3: Committee Groups and Senator Legislative Style.

Note: Points are each committee group's average on one of the five variables. Lines are 90 percent (black) and 95 percent (grey) confidence intervals.

C.2 Validating the Measure

In Figure C.4, I show that both measures indeed do convey important information about private sector career prospects. I plot Contract Sizes predicted using the senator's reference group (Panels A and B show pre-Senate career and committee measures, respectively) against realized Contract Sizes. The correlations are substantial – approximataly .4 and .5 – and estimated with very high precision. There is evidence of heteroskedasticity, in that there is more variance for high levels of predicted contract size.

In Panels C and D, I show that the prediction of Contract Size using a Senator's own reference group outperforms the prediction from some other group. I do this, by first computing the correlation between a senator's realized Contract Size and the Contract Size predicted for some other group chosen at random. I take the difference between this and the predictive power of a Senator's own group. I repeat this process 1,000 times each time choosing a different reference group, thereby creating a bootstrapped sampling distribution. The results show that both the career-based and the committee based reference groups correlate by approximately .41 and .25 more strongly with realized contract sizes than another reference group, chosen at random. Since this is slightly less than the correlation between predicted and actual contract sizes, this suggests that the other reference groups do weakly predict career prospects. A senator can, however, gain much less information by looking at other groups compared to her own.

This validation exercise illustrates two important points: first, the predicted Contract Size tracks the value of the actual contracts, which senators can expect to work on, if they were to walk through the revolving door. This shows that reference groups not only provide information about a senator's legislative style (as shown in the previous section), the group also information about how valuable lobbying contracts a senator can expect to work in, should she leave office to become a lobbyist. Second, it also shows that it tracks it better than the prediction from other reference groups would have done. Thus, from her reference group, a senator can extract unique information about her private sector career prospects, which she would not have been able to collect by looking at senators from other groups.

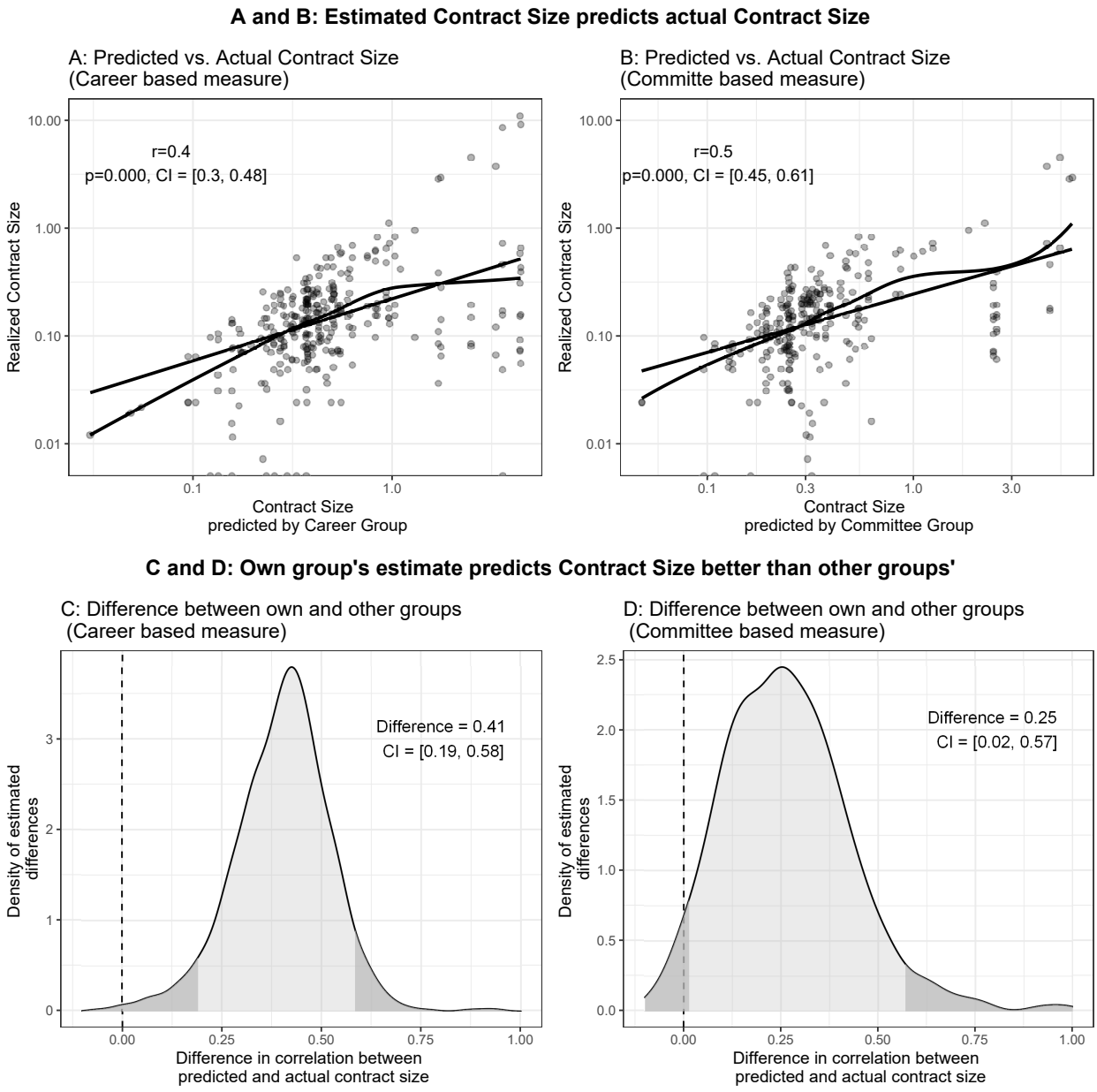

Figure C.4: Validating the measure.

Note: Panels A and B show the relationship between Contract Size and realized sizes of lobbying contracts. Panels C and D plot the bootstrapped (1,000 trials) difference in correlation between realized Contract Size and Contract Size predicted using the Senator's own reference group and a random group. The dashed vertical line shows the null of them predicting actual Contract Sizes equally well. Areas below the 5th and above the 95th percentiles are dark shaded.

C.3 Making Sense of Career Prospects

The preceding analyses suggest that this measure of career prospect is, indeed, valid. As with any new measure, however, the question of where its variation stems from arises. The answer to this question is important for at least wo reasons. First, it will endow the measure with a more intuitive interpretation. Second, understanding what drives variation in career prospects allows us to think more clearly about questions of causal identification and the potential inclusion of controls.

The analysis proceeds in two steps. In the first exploratory step, I investigate the correlates of individual contract size, and I provide evidence on which legislative characteristics that correlate with being a revolving door lobbyist in high demand. I also look at a particularly important political shock to earnings of politically connected lobbyists—the party that controls the majority in the Senate.

Second, to substantiate that average Contract Size captures the price per contract for a reference group's lobbyists, I show that it correlates with two important other measures of demand—the number of revolvers from and the amount of campaign donations to that particular reference group. Importantly, however, total Contract Size is much less strongly related to these other measures of demand. This provides a partial explanation for why results using the mean and total contract size diverge (see Appendix D2).

C.3.1 Which Political Backgrounds are in Demand?

In this section, I investigate which political characteristics that make a former legislator a successful lobbyist. I explore the following factors:

1. Having been a moderate, measured through the absolute value of the DW-NOMINATE score.

2. Having been a broadly effective bill sponsor, measured through legislative effectiveness scores.

3. Having sponsored many bills broadly across topics.

4. Having been an efficient fundraiser,

5. Having been central in the cosponsor network.

6. Having specialized in writing bills within a narrow set of topics. This is measured by computing each senator's HHI score across bill topics as classified by the Political Agendas Project (PAP).

Because of the partisan nature of lobbying, I allow all of these factors to differ between parties. I include year fixed effects to deal with trends arising from inflation. Figure C.5 shows the results—particularly the partisan differences are striking. Among Democrats, moderates and broad legislative strategists are very clearly the most successful lobbyists. Among Republican lobbyists, however, the most successful ones were relatively extremist, less broadly effective and less broadly active. Additionally, efficient fundraisers make more successful lobbyists among Republicans, but not Democrats. All of these slope differences are statistically significant. Interestingly, cosponsor centrality does not seem to matter for either party. Finally, being a specialist within a relatively narrow set bill topics (as measured through the PAP topic HHI) is rewarded very significantly for both parties (note that the results hold without the strong Democratic outlier). This final result helps us interpret the results in Panels A through C—it suggests that those partisan difference do not arise because Republicans are not rewarded for doing legislative work. Instead, it shows that Republican legislative strategists are not in high demand in K Street, while among Democrat lobbyists both strategists and specialists are in high demand.

It is important to note that these are stylized facts about which types of revolvers that are in high demand on K Street—they are not causal estimates, but descriptive patterns. Besides being interesting in their own right, they help me endow changes in Contract Size with a substantive interpretation. It allows us to think of it in terms of increased demand arising from, for instance, legislative specialists being in higher demand. As we shall see in the next section, this happens at certain points in time, when Congress produced more legislation.

Additionally, the strong partisan differences highlight the importance of incorporating party into the main analysis. I have done this in two ways. First, I incorporate political party into the measure of career prospects itself. The analysis in this section, however,

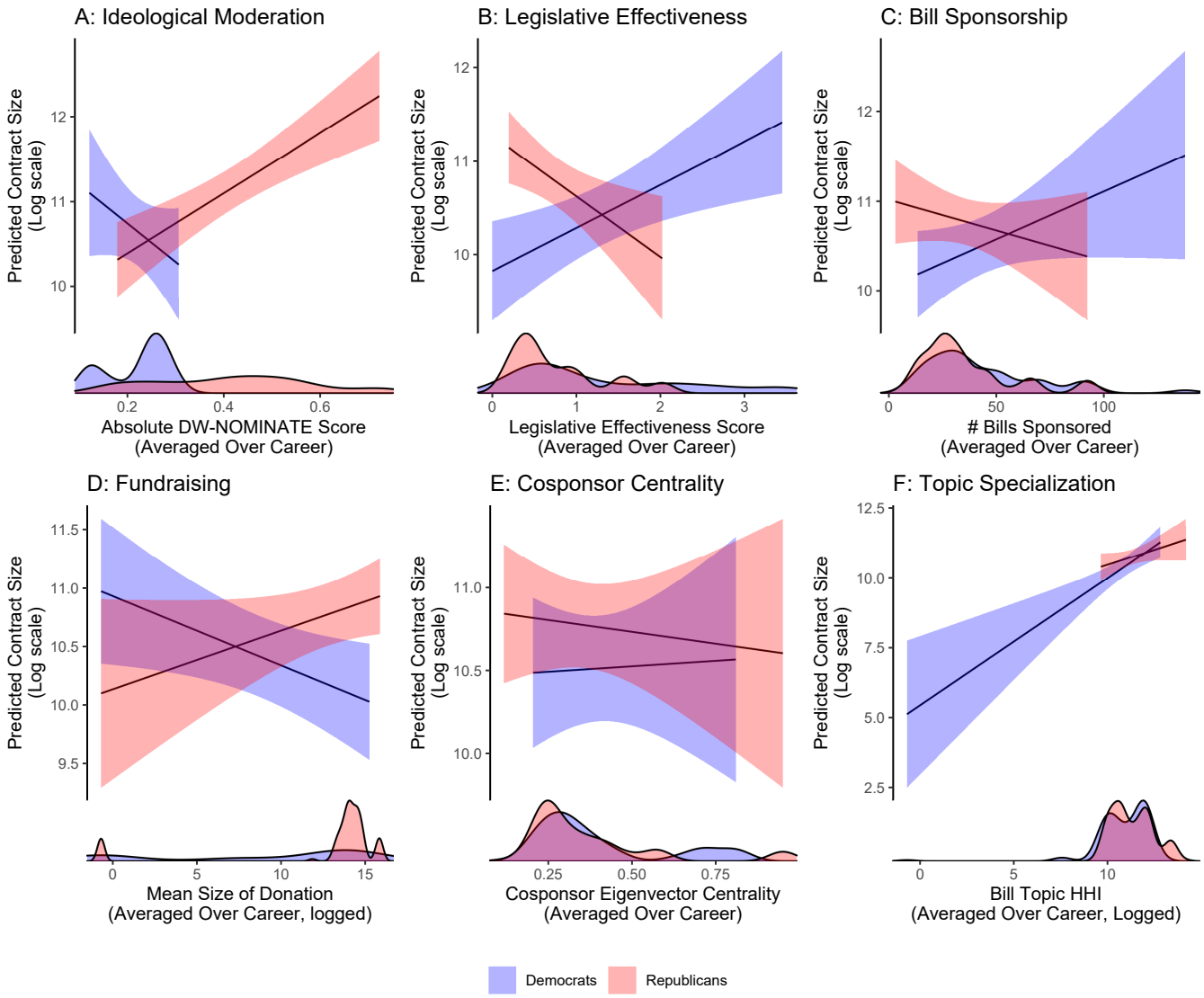

Figure C.5: Drivers of Individual Contract Sizs.

Note: Each plots shows marginal predictions from a regression of individual average contract size on one of the five independent variables and an interaction with the revolver's former party. All models include fixed effects for year. 90 percent pointwise confidence intervals are autocorrelation and heteroskedasticity robust.

highlights how this strategy will make the estimates vulnerable to political shocks. Therefore, my preferred strategy is to allow for heterogeneous shocks by party.

C.3.2 The Conditional Value of the Senate Majority

Previous research has shown that connections to the House majority significantly benefit firms (Furnas et al. 2019). It is an open question, however, whether this result can be replicated at the level of individual lobbyists. In this section, I investigate whether a former senator's contract size increases (decreases), when her former party gains (loses) the majority.

First, Table C.3 shows the result from a simple generalized differences-in-differences estimated by including revolver and year fixed effects. While the price of hiring a revolver on a contract does increase when their former party wins the Senate majority, the increase is very small compared to the baseline individual differences uncovered in the previous section. Additionally, they are extremely noisy.

Table C.3: Senate Majority of Former Party and Revolver Contract Size

	Dependent variable:
	Log Average Contract Size
Former Party Gains Majority	0.063
	(0.230)
Revolver FE?	Yes
Time FE?	Yes
Observations	319

Note: Differences-in-differences estimate comparing contract sizes of revolvers whose former party gains the Senate majority to revolvers whose former party loses it. Estimated through two-way fixed effects regressions. Revolver clustered standard errors in parentheses.

To delve more deeply into this, Figure C.6 shows how this association is highly conditional on how legislatively productive Congress is in. When measuring legislative activity, it is important to note that the Senate majority is likely to behave strategically. This would make legislative activity in the Senate post-treatment to whoever is in the majority. To reduce these concerns, I use the number of substantive bills given committee consideration in the House (data from Volden and Wiseman (2014)) to measure legislative activity in Congress. While this obviously does not solve all selection problems, it will

avoid some of them.

The results show that gaining the majority does increase the contract size of the party's revolvers—but only when the legislative activity in Congress is above average. The caveat is, of course, that the uncertainty from the estimates in Table C.3 carries over to these estimates, and the marginal effect of gaining the majority does not become statistically significant until legislative activity is very high. Still, this is informs us about the conditional value of certain types of political connections.

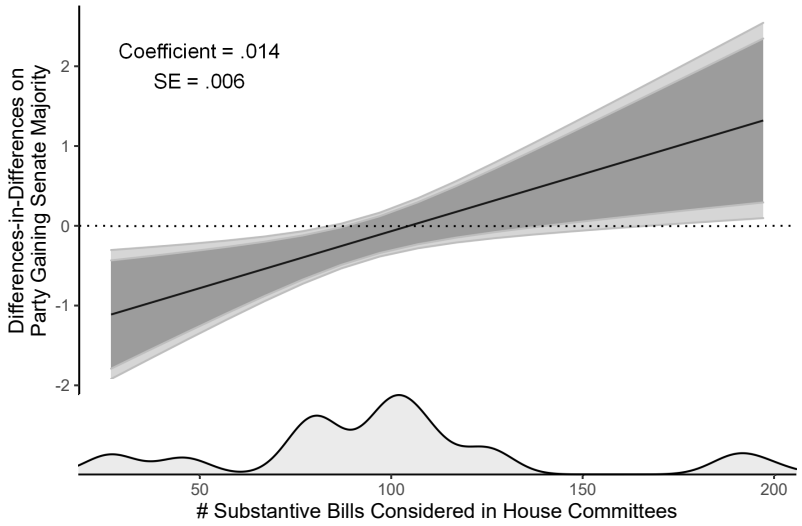

Figure C.6: Senate Majority, Legislative Activity, and Contract Size.

Note: The figure shows the result from regressing average contract size (logged) on an interaction between the revolver's former party winning the Senate majority and how many substantive bills are considered in committees in the House of Representatives. Revolver and year fixed effects included. Gray shaded areas are 90 percent (dark) and 95 percent (light) confidence intervals, respectively, computed from autocorrelation and heteroskedasticity robust standard errors.

C.3.3 Average Contract Size Measures the Demand for Certain Actors

In the two preceding sections, I set out to gain a substantive understanding of the variation in Contract Size in an exploratory fashion. In this section, I investigate whether the average contract size, indeed, captures the underlying theoretical construct that I expect—demand for certain types of political actors, and the equilibrium price of hiring them.

An external measure of demand: In order to validate this idea, we need a measure of demand that is conceptually unrelated to the price of lobbying contracts.

I believe that campaign donations to incumbents in the reference group provides such a measure. We have seen in Appendix C1 that the reference groups successfully gather senators with similar legislative behavior. Furthermore, Appendix C3.1 and C3.2 showed that different legislative styles make revolvers valuable during certain political contexts. Crucially, interest groups can hire lobbyists with certain characteristics—and they can gain access to incumbent senators of the same type through campaign donations (Kalla and Broockman 2016). Therefore, if average Contract Size in a reference group indeed measures equilibrium price per unit of senator labor, it should correlate with campaign donations to senators in that same group. The reason is that both should be driven by the underlying demand for certain political assets. In a nutshell, if political spending depends on which political assets are valuable, then spending on lobbyists should correlate with donations to senators that control the same political assets.

As an additional measure, I use the number of senators from a group that become revolvers at a certain point in time. While this measures a combination of supply and demand, it captures that legislators walk through the revolving door when it is most lucrative to do so. This provides a sanity check on the donation-based measure.

To get a better understanding for which political dynamics the groups capture, I also include the group's average distance in DW-NOMINATE scores to the party's median ,and the group's average state policy conservatism (Caughey and Warshaw 2015). Since these latter two variables correlate very strongly, I include them in separate regression models.

Table C.4 presents the results from a series of regressions of average contract size on these group characteristics. Panel A and B show results from career and committee-based groups, respectively. With an elasticity of approximately .5, the correlation between donations to career-based reference groups and average contract size is very strong—when donations to the group increase by one percent, the contract size of the group's lobbyist increases by 0.5 percent. Importantly, I also find that revolvers time their retirement to

situations when there are high average contract sizes—an important sanity check.

Table C.4: Affinity Group Characteristics and Revolver Contract Size

	Dependent variable:			
	Log Average Contract Size			
	(1)	(2)	(3)	(4)
	Panel A: Career Specifications			
Donations to Group	0.528**	0.463***	0.576	0.498
	(0.233)	(0.169)	(0.277)	(0.259)
New Revolvers from Group	0.154***	0.133**	0.180	0.204
	(0.052)	(0.063)	(0.272)	(0.285)
Group Distance to Party Median Ideal	−0.230**		−0.196	
	(0.105)		(0.169)	
Group Policy Conservatism		−0.864**		−0.440
		(0.337)		(0.259)
Revolver FE?	No	No	Yes	Yes
Time FE?	Yes	Yes	Yes	Yes
Observations	271	271	271	271
	Panel B: Committee Specifications			
Donations to Group	0.221	0.261	−0.047	−0.032
	(0.365)	(0.336)	(0.339)	(0.344)
New Revolvers from Group	0.224	0.245	0.004	−0.014
	(0.174)	(0.203)	(0.227)	(0.213)
Group Distance to Party Median Ideal	0.102		0.320	
	(0.147)		(0.174)	
Group Policy Conservatism		0.085		0.530**
		(0.333)		(0.199)
Revolver FE?	No	No	Yes	Yes
Time FE?	Yes	Yes	Yes	Yes
Observations	234	234	234	234

*Note: Revolver-clustered robust standard errors in parentheses. Estimates are unstandardized OLS coefficients. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively. In Panel A, reference groups are estimated through pre-Senate careers. In Panel B, they are estimated through Senate committee portfolio.*

The results for the committee-based groups generally show the same patterns, but the coefficients are estimated with more noise and are smaller. This suggests that the committee-based measure captures demand to a smaller extent. An important reason for this might be the strategic element in group formation: senators choose which committees to serve on contemporaneously. After all of the former validation exercises—and given

the large coefficients—this does not invalidate the committee-based measure, but could explain the smaller coefficients, I generally uncover in the main text.

A problem with comparing coefficients across specifications, is that the measurement—most importantly the reference group members—changes. This can drive differences in the results. The non-demand factors provide check on this. As we can see, they change markedly between measurements, while the coefficients on the demand factors change much less. This lends credence to the demand results.

C.3.4 The Sum and the Mean Do Not Measure the Same Concept

In this section, I discuss how the total and the mean contract size differ in what they measure.

The basic goal of this paper is to estimate the price elasticity of the supply of senator labor. Do senators walk through the revolving door when the price per unit of their labor is high? The unit that is traded on the market for lobbying services is the contract. This makes it most natural to use the lobbying contract as the unit through which to estimate the price of hiring a senator. The average contract size captures exactly this: The average price of a lobbying contract a senator works on.

The sum, on the other hand, yields the total amount spent on a certain type of senator-lobbyist. While this is obviously related to demand at any particular price per contract, it is closer to the firm's revenue (see Blanes i Vidal et al. 2012). If we can estimate each lobbyist's contribution to a contract (as in Ban et al. forthcoming), the LVA weighted sum captures the firm's revenue per (senator-)lobbyist. By extension—if and only if the market is efficient—the simplest microeconomic models predict that revenue will translate directly into (senator-)lobbyist salary. If the market is inefficient, salary will be some unknown function of total contracts.

This short conceptual discussion is important—it illustrates theoretically the differences in what the average and the total contract size capture. To substantiate this empirically, I repeat the validation exercise in Table C.4, but investigate the relation between group-level donations and *total* contract size. For comparability, I also include the

results from column 1 in Panels A and B in Table C.4. As we can see, total contract size is much more weakly related to donations—in some cases, the sign is even negative.

Table C.5: Reference Group-Level Demand: Average and Total Contract Size

	Dependent variable:			
	Log Total	Log Average	Log Total	Log Average
	(1)	(2)	(3)	(4)
Donations to Career Group	−0.363	0.528**		
	(0.563)	(0.233)		
New Revolvers from Career Group	−0.316	0.154***		
	(0.317)	(0.052)		
Difference to Party Median (Career)	−0.401	−0.230**		
	(0.370)	(0.105)		
Donations to Committee Group			0.603	0.221
			(0.854)	(0.365)
New Revolvers from Committee Group			−0.445	0.224
			(0.528)	(0.174)
Difference to Party Median (Committee)			0.599***	0.102
			(0.206)	(0.147)
Time FE?	Yes	Yes	Yes	Yes
Observations	234	271	234	234

*Note: Revolver-clustered robust standard errors in parentheses. Estimates are unstandardized OLS coefficients. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.*

It is still important to investigate when the mean and the sum diverge. Mathematically, this will happen when there are many, high-variance contracts. I.e. when a lobbyist experiences large differences in contract sizes within a year. This is illustrated in Figure C.7, where I plot the total contract size against the average. Points are colored after the logged variance in contract size within a year. The dash-dotted line shows what would be perfect correspondence between the two, while the solid line shows the actual fit. As we can see, the two measures diverge as contract sizes increase, because the yearly variance increases. If we were to predict the sum of contracts with the average of contracts, the error would increase by one-fifth of a percent for each percent increase in variance.

The analysis in this section informs the finding in Appendix D2 showing that senators do not react to predicted total contract size—only mean and median contract size. Importantly, this happens, because a high average or median suggests a high price per

contract, while a large total can be driven by many contracts, each of low value. This suggests that senators base their retirement decisions on the demand for lobbyists of their type—particularly, when the price of the contracts they work on is high. They do not react to the revenue generated by similar revolvers, or however this revenue translates into salary. This—along with the finding in the main text that the coefficients on the LVA weighted contract size are smaller—suggests that revolvers do not react so much to what can be earned in a narrow sense. Rather, they react to career prospects in a broader sense—a compound of earnings per contract as well as the prestige and challenges involved in working high-value contracts.

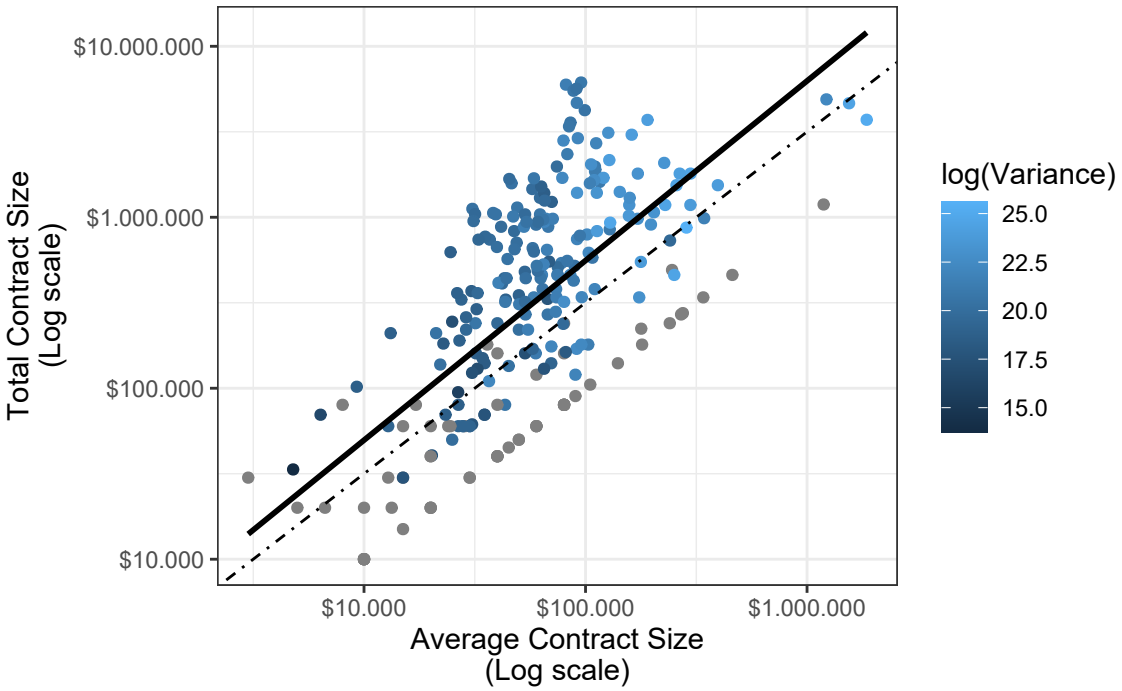

Figure C.7: When Does the Total and the Average Diverge?

Note: Points colored by the size of within-year variance in contract size. Solid line is the least squares fit—dot-dashed line is the perfect fit.

D Additional Analyses of Robustness

D.1 Tests for Balance in Covariates

In Figure D.1, I show that Contract Sizes are balanced across time-varying individual characteristics of the senators in my sample. The figure presents the Wald statistic from permutation tests (Gerber and Green 2012) regressing Contract Size on a host of predictors as well as twoway fixed effects. I do this for both ways of measuring career prospects and for two model specifications (grey distributions are the reduced models, black distributions add interaction effects).

In the reduced model, I assume the covariates to be homogeneously related to Contract Size across the senator's tenure. As we can see, the observed Wald statistics are far from the critical values that is printed in the figure.

If covariates were differently related to Contract Size during the final Congress in which revolving door senators serve, however, this could be masked by a non-existing relationship during the rest of the tenure. To alleviate this concern, the full specifications include interactions between a final-term dummy and all independent variables. I still cannot reject that career prospects are unrelated to the individual characteristics of currently serving senators. Thus, I cannot reject that Contract Size is unrelated to the individual characteristics included here.

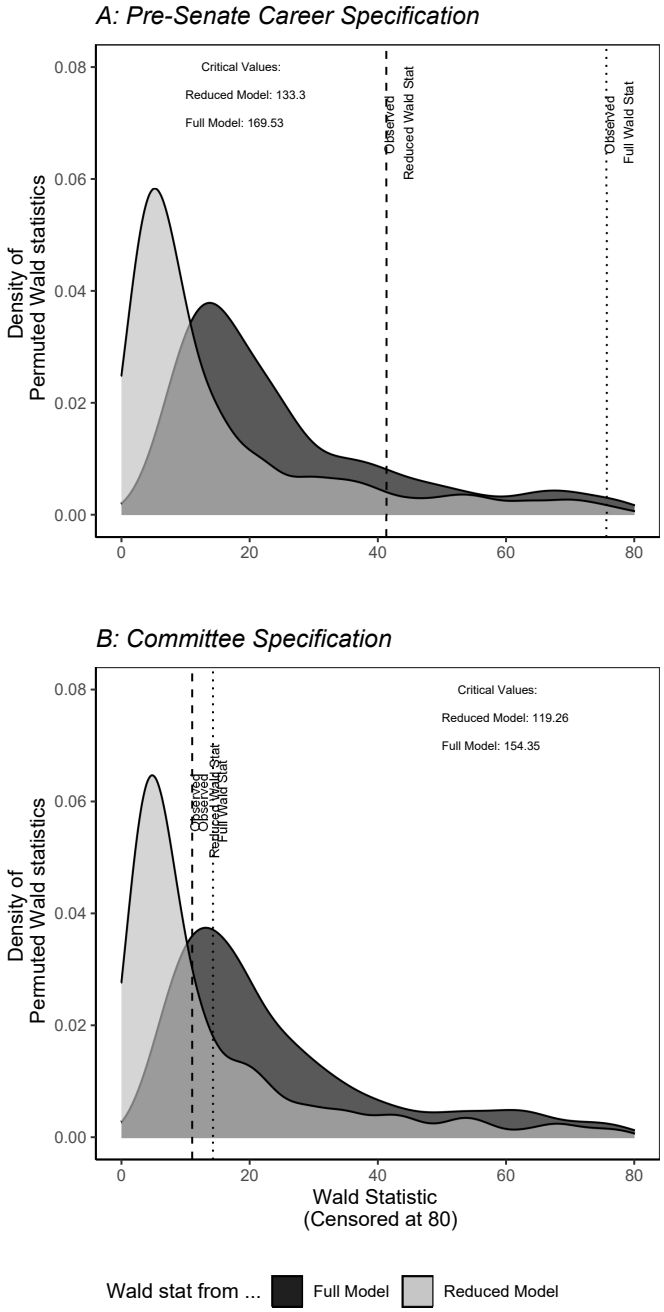

Figure D.1: Permutation Test of Covariate Balance.

Note: Two distributions of Wald statistics (1,000 permutations) under the null of no relation between Contract Size and any covariate. Dashed and dotted lines represent the observed Wald statistics in the reduced and full models, respectively. Contract Size is based on pre-Senate Careers in Panel A, and committee assignment in Panel B. x axes censored at 80 for presentational purposes. All models include twoway fixed effects. All covariates included in the reduced model. Full models include interactions between a final-term dummy and each covariate. The 90% critical values are printed in the plots.

D.2 Robustness and Placebo Tests

In Figure D.2, I show the robustness of my main findings and present a number of placebo tests of the model. Panels A and B show results based on Contract Size estimated using, respectively, pre-Senate careers and in-Senate committee assignment.

First, I test the robustness of using the average Contract Size to measure career prospects – as in the main specifications. One way of doing so, is to use the median Contract Size, which puts less weight on the few extremely large Contract Sizes. In this way, I deal with the potential problem with outliers, caused by few senators experiencing very large Contract Sizes. This yields remarkably similar results.

As a third way of measuring career prospects, I use the predicted total Contract Size. The results are not completely robust to this, which might indicate that senators respond to the value of the *typical* lobbying contracts, when gauging career prospects, not the sum of all contracts. This is most likely due to the large differences in the number of contracts that senators-turned-lobbyists work on. If a revolving door senator's full number of lobbying contracts sum up to a large amount, but she had to lobby for more than a dusin clients in order to put it together, while another former senator only worked on a few highly lucrative contracts to make the same amount, the sum does not contain all information about their respective career prospects. Instead, the mean or median size of their lobbying contracts will provide the best proxy for how successful they are in their post-elective careers, as it takes into account both the total revenue and the number of contracts worked on to obtain that revenue.

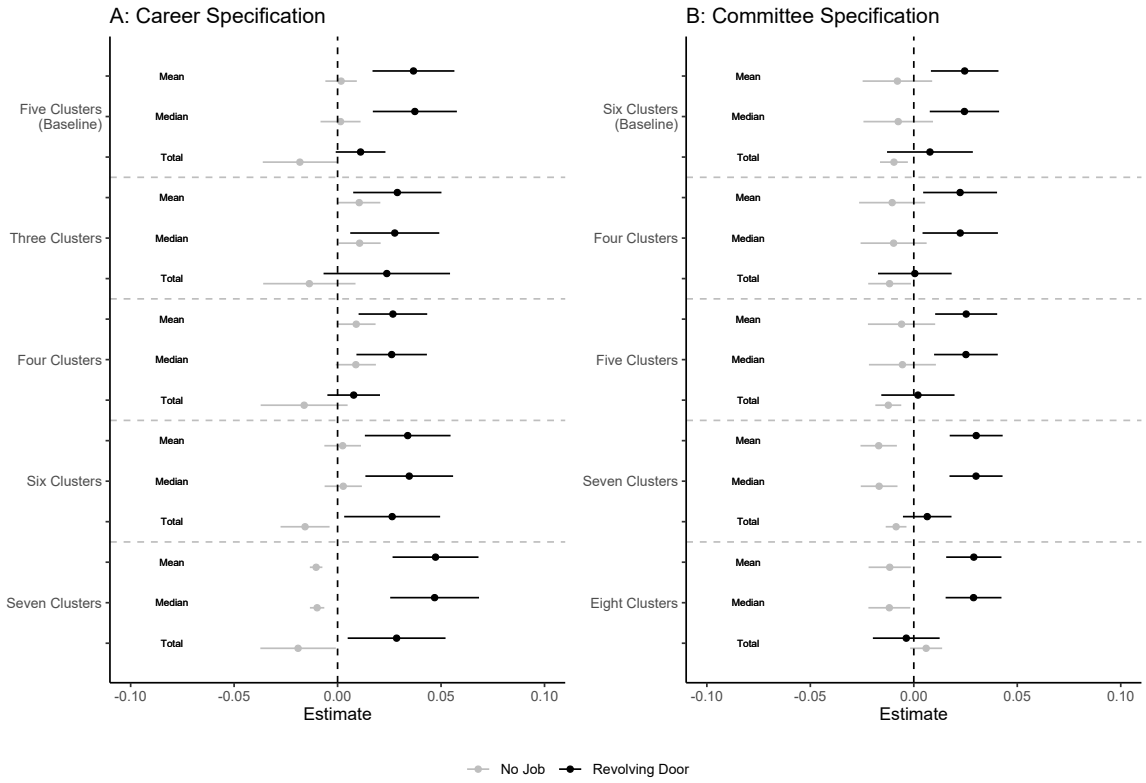

Figure D.2: Robustness to Specification Choice and Sanity Checks.

Note: Sensitivity to a) varying the number of career groups, and b) estimating the typical Contract Size using the mean, median and sum total. Black and grey points are from models with SIG Career and retirement as dependent variables, respectively. Top axes show average probability of walking through the revolving door. Bottom axes show coefficient on Contract Size. The rugs show Congress-specific proportion of senators walking through the revolving door. Estimates are from two-way fixed effects LPMs. Confidence intervals are 95 pct. (thin lines) and 90 pct. (thick lines), computed using Driscoll-Kraay robust standard errors.

Using cluster analysis to group senators with similar careers or committee portfolios together implies making a somewhat arbitrary decision about the number of clusters to extract. To check the sensitivity of the results towards my baseline choices of five and six clusters, I vary the number of groups to retrieve from the cluster analysis. For the career based measure, I vary the number of clusters from three to seven, and for the committee based measure, I vary it from four to eight. The results are remarkably stable across these

different specifications.

Furthermore, for all specifications, I present the results from a placebo model, where I regress a dummy for leaving the labor market after retiring on Contract Size. If potential private sector success had the same effect on the probability of leaving the labor market, as it had on the likelihood of taking a private sector employment, it would indicate a problem with the model. Comparing the results from the models with these two different dependent variables is striking. In all specifications, the results from modeling the probability of leaving the labor market are substantively very small, lining up closely around zero, sometimes entering with a negative sign. Additionally, it is mostly insignificant statistically speaking.

D.3 Further Placebo Tests: Pre-Treatment Trends

As remarked upon in the main text, the identifying assumption in the main specifications is that the probability of leaving office for a revolving door job would have evolved similarly for treated an non-treated senators absent the change in career prospects. To substantiate this, Table D.1 presents placebos, which tests for pre-treatment trends, by regressing the dependent variable on both measures of Contract Size with a one year lead on them. If there is a statistically significant pre-treatment trend, future Contract Size should not predict current career decisions.

It is clear that there are no discernible pre-treatment trends in the probability of walking through the revolving door for either measure.

D.4 Congress-Specific Estimates and the Impact of Reform

A potential problem, which I have raised a couple of times in the main text, is that reforms to the regulatory regime facing lobbyists also change incentive structures. In this regard, the Honest Leadership and Open Government Act of 2007 is an especially salient concern, since this reform both changed reporting requirements and introduced a two year cooling-off period for senators, before they could register as lobbyists. This shock to the system could both cause senators to leave Congress and to reporting behavior to

Table D.1: Testing for Pre-Trends

	Dependent variable:	
	Active Future Career	
	(1)	(2)
Contract Size$_{t+1}$ (Career)	−0.009	
	(0.013)	
Contract Size$_{t+1}$ (Commitee)		0.005
		(0.008)
Senator FE?	Yes	Yes
Congress FE?	Yes	Yes
Group X Time Trend?	Yes	Yes
Observations	701	764

Note: Dependent variable is SIG Career. Driscoll-Kraay robust standard errors in parentheses. Estimates are unstandardized OLS coefficients.

change. While the former could potentially confound Contract Size, the latter would cause systematic measurement error in the independent variable, and both would bias my results. In the main paper, I deal with this by including time fixed effects and time fixed effects interacted with all covariates, respectively.

To investigate whether this effectively deals with the potential confounding, Figure D.3 estimates Congress-specific effects of Contract Size by introducing random slopes by Congress. I include twoway fixed effectsin the model. Two points should be noted about this strategy. First, it is likely to be an overspecified model. However, it also provided the least restrictive test of the effect of regulatory reform on the coefficient on Contract Size. Because of this overspecification, confidence intervals would be extremely large, which is why, I do not show them – no differences are statistically significant. Second, since relatively few senators leave in each Congress, we could expect the estimates for a few Congresses to diverge.

The trend in coefficients across time is relatively flat. For some periods before 2007 (the 110th Congress), the coefficient on Contract Size is larger than after, for others it is smaller, and there is no clear trend. In each specification, there are two outlying Congresses, which – as mentioned above – was to be expected. These results are robust

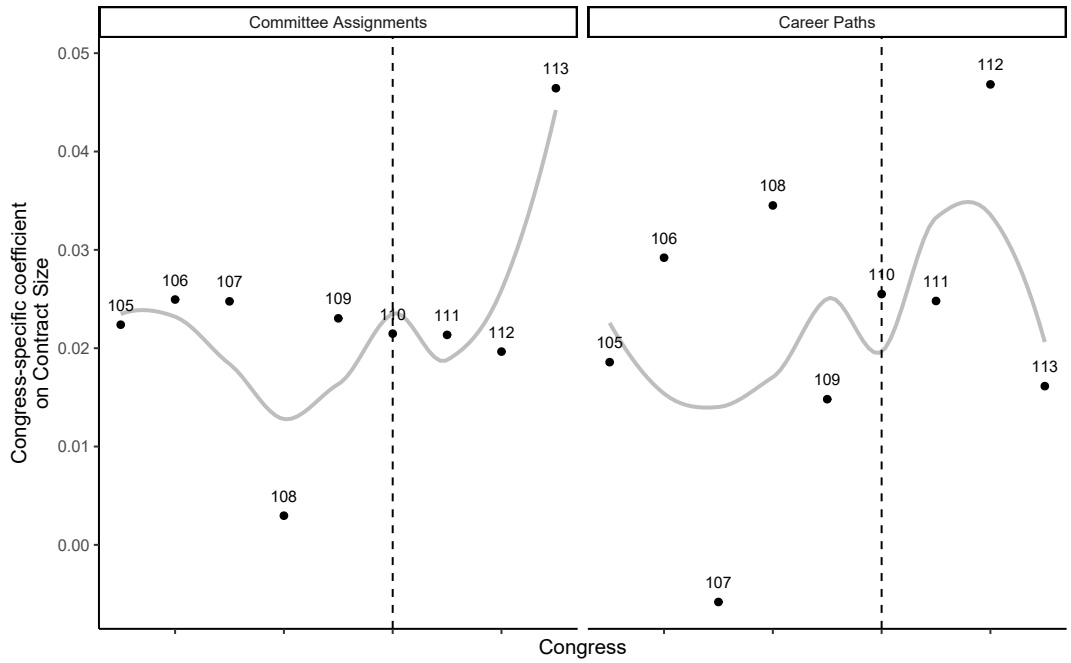

Figure D.3: Congress-specific Coefficient on Contract Size.

Note: Congress-specific coefficients estimated by adding random slopes on Contract Size for each Congress. Loess smoother shows the trend over time. The dashed line shows the Congress in which the Honest Leadership and Open Government Act was implemented.

to interacting the time fixed effect with Contract Size, thus estimated fixed instead of random slopes. This strategy, however, induces extremely large estimates for all years due to multicollinearity.

D.5 Robustness to Sources of Uncertainty

In this appendix, I test the robustness of my results towards different ways of dealing with measurement error and autocorrelation in the error term. Results are presented in Figure D.4. Panels A and B show results for reference groups based on pre-Senate careers and committee assignment, respectively. The first rows show the baseline results for comparison.

There are two potentially large sources of measurement error in Contract Size. First, given that reference groups are correctly identified, random variation in the sizes of lobbying contracts would cause any given estimate of career prospects to be idiosyncratically

off. Second, and relatedly, as we have seen, estimates of references groups are noisy – especially for those based on committee assignment. Because any given division of senators into groups will be associated with some error as well, this is likely to increase the noise associated with the idiosyncratic variation in sizes of lobbying contracts. Luckily, however, the entire distribution of the error laden Contract Size is observed, and I can therefore use the Method of Composition (MoC) (Tanner 1996; Treier and Jackman 2008) to correct the bias induced in my estimates. I use the procedure outlined in Caughey and Warshaw (2017): First, I sample from the error laden data, I then use this to draw a parameter estimate from a multivariate normal distribution. The process is repeated 500 times, where – given that error is random – each draw of parameter estimates will be from the marginal distribution of the true estimate. Finally, I integrate over the sampled parameter distribution to get a corrected estimate.

The results from this procedure is shown in the second rows in Panels A and B. As we can see, the point estimate is slightly larger, when using pre-Senate careers to estimate reference groups, and unchanged when using committee assignment. In both cases, standard errors are considerably larger. This is no surprise given that the relatively low cohesion in the reference groups (especially when using committee assignment to proxy reference groups). It does also indicates that measurement error is no (significant) source of bias in the estimated effect of Contract Size.

Second, Contract Size is estimated at the level of the senator's reference group, this could cause clustering. Tests show that less than one pct. of the variation in career choices is at the level of reference groups. To be certain that my results are not driven by clustering, however, I rerun the models including random effects for the senator's reference group. This increases the confidence intervals slightly, but the substantive results remain unchanged.

Third, the Driscoll-Kraay standard errors used in the main specifications impose assumptions about the structure of temporal autocorrelation, but are robust in the presence of correlation in career choices between senators. I test the robustness of this choice in two ways. In the fourth rows, I cluster standard errors at the senator level. While this allows

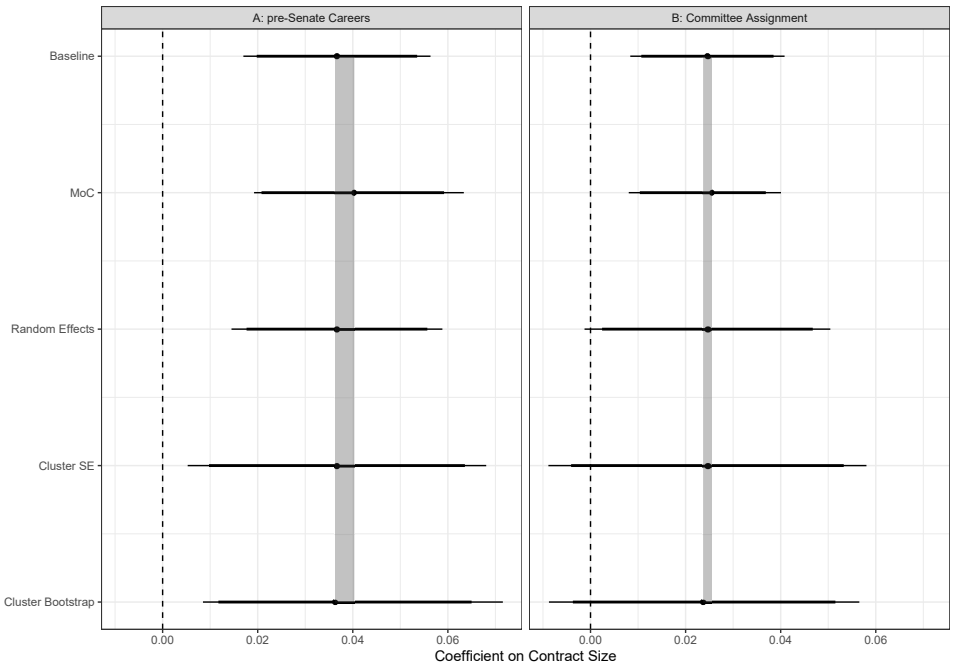

Figure D.4: Robustness to measurement error, clustering and autocorrelation.

Note: Confidence intervals are 95 pct. (thin) and 90 pct. (thick), respectively. Specifications using MoC and the non-parametric cluster bootstrap are based on 500 (re-)samples, and their point estimates and CIs are the relevant percentiles of the simulated distributions. The gray shaded areas show the variation in point estimates.

for arbitrary within-senator temporal autocorrelation, it makes the heroic assumption of no contemporaneous correlation between senator's career choices. Even so, the results for the career specification are robust to this choice of standard error, while the results for the committee specification lose statistical significance. In the fifth and final rows, I use the non-parametric bootstrap procedure with clustering at the senator-level to compute confidence intervals. I draw 500 samples with replacement, reestimating the model within each of them. This imposes the least possible structure on the uncertainty estimates, thus, allowing for arbitrary autocorrelation structures and flexible distributions. Again, the career specification are robust to this choice of standard error, while the results for the committee specification lose statistical significance.

D.6 Probabilistic Cluster Assignment and Noisy Estimation of Career Prospects

The model in the main text is estimated in three stages: the reference groups are estimated using cluster analysis, contract size is predicted using those groups, and finally revolving door retirements are modeled in a differences-in-differences setting. In the baseline model, the uncertainty arising from the first two steps are not accounted for. In the previous section of the appendix, I treat this as a problem of measurement error which allows for a parametric solution using the Method of Composition. However, all of the additional uncertainty might not be captured in this way. In this section, I use the non-parametric bootstrap to treat 1) the cluster assignment as probabilistic, and 2) the prediction of career prospects as noisy.

There are essentially two sources of uncertainty in the cluster analyses of careers and committee assignments, and I develop two bootstrap procedures to model each. First, there is within-senator uncertainty, where any single senator could have chosen to spend more or less time in the different career trajectories she has followed. To account for this, in the first bootstrap strategy, for each senator, I resample the columns of containing the variables describing their careers and committee assignments, respectively. Second, there is between-senator variance. To capture this more conventional way of thinking about uncertainty, I resample senators and their entire career or committee trajectory. In both cases, I draw 500 samples with replacement, reestimating all stages of the model—the cluster analysis, the prediction of contract sizes, and the final differences-in-differences estimate—within each draw.

Both of these approaches will, however, leave the autocorrelation and heteroskedasticity in the dependent variable unaccounted for. Therefore, I present both standard errors bootstrapped in this way, and use the bootstrap to bias-correct the baseline robust standard errors. In the latter approach, I use the bootstrap to estimate the unmodeled uncertainty in the cluster analysis, and then add it into the baseline robust standard errors.

The results from these two procedures are presented in columns one, two, four and

five. As we can see, there is a substantial amount of unmodeled variance in the career specifications. Despite this, the baseline results maintain in most of the specifications.

The second source of unmodeled variance stems from the prediction of contract sizes based on reference groups. To deal with both sources of additional uncertainty simultaneously, I use the sequential bootstrap (Rao et al. 1997). Within each of the 500 draws from the distribution of potential reference groups, I conduct an additional set of bootstraps, and resample the prediction of contract sizes 200 times within each chain. This is a very computationally intensive model, yielding a total of 100.000 samples sequentially. Results are presented in columns three and six. The career specifications are highly robust to this choice of standard error, while the results for the committee specification lose statistical significance, when using the bootstrap to bias-correct. The latter, however, are also robust to simply using the bootstrapped standard errors.

Table D.2: Bootstrap Correcting the Model

	Dependent variable:					
	Active Future Career					
	(1)	(2)	(3)	(4)	(5)	(6)
Contract Size (Career)	0.037	0.037	0.037			
	(0.010)	(0.010)	(0.010)			
Contract Size (Committee)				0.025	0.025	0.025
				(0.008)	(0.008)	(0.008)
SE Bias	0.01	0.01	0.01	0.008	0.008	0.008
Bootstrap SE	0.002	0.002	0	0.001	0.001	0
Bias-Corrected SE	0.01	0.009	0.01	0.012	0.022	0.021
Senator FE?	Yes	Yes	Yes	Yes	Yes	Yes
Congress FE?	Yes	Yes	Yes	Yes	Yes	Yes
Observations	822	822	822	770	770	770
Bootstrap	Within Senator	Across Senator	Within + Sequential	Within Senator	Across Senator	Within + Sequential

Note: Dependent variable is SIG Career. Baseline standard errors in parentheses, bias and alternative standard error estimators reported below. Estimates are OLS coefficients with independent variables standardized.

E Testing Additional Observable Implications

In this section, I discuss and test to additional observable implications of the theory of how career prospects shape selection out of office. First, I provide more nuance to the finding in the main text that election losers do not react to information from the post-elective labor market. Second, I investigate how effects change as senators grow older.

E.1 Election Losers React to Career Prospects After They Lose

In Figure 2 of the main text, I show that the effects are driven by senators who retire voluntarily. This is intuitive: as they make the decision to walk through the revolving door themselves, they can process and react to the market's signal of their post-political career prospects. Election losers, on the other hand, do not necessarily react to the information available before Election Day. After they lose their reelection bid, however, they should react to the information available at that time. This is an important nuance to the results presented in the main text. Furthermore, this line of reasoning presents an additional testable implication, which I could not delve into in the main text because of space constraints.

Most election losers who take revolving door employment land their first post-elective job during their first year out of office. In Tabel E.3, I re-estimate the same specification as in the main text. That is, I interact the measure of career prospects with an indicator of voluntary retirement, and use any event of revolving door employment as the dependent variable. However, I use the estimated career prospects during the year *after* the current Congress – this corresponds to the election losers's first year out of office. Importantly, the results show that when career prospects in lobbying improve, so does the probablity that the recently unemployed senator takes a revolving door position. Additionally, the estimate using the career-based reference groups is of the same magnitude as among voluntary revolvers. While the committee-based estimate is somewhat smaller than the corresponding estimate in the main text, this shows an important pattern: election losers process information about career prospects similarly to voluntary revolvers. Only, they react to the information that is available at a later date, during the time when they are

forced to find a new position.

Table E.3: Late Information and Careers of Election Losers

	Dependent variable:	
	Retire for Private Sector Employment	
	(1)	(2)
Contract Size (Career)	0.027***	
	(0.005)	
Contract Size (Committee)		0.016***
		(0.003)
Voluntary Revolver	−0.012	
	(0.019)	
Contract Career X Voluntary		−0.025
		(0.032)
Senator FE?	Yes	Yes
Time FE?	Yes	Yes
Observations	754	725

*Note: Dependent variable is an indicator for the final Congress before leaving for a job as registered lobbyist. Driscoll-Kraay robust standard errors in parentheses. Estimates are unstandardized OLS coefficients. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively. The other main term in the interaction (being a voluntary revolver) is included in the regression but not shown for presentational purposes.*

E.2 Conditioning on Senator Age

One of the benefits of the measure of career prospects is that it is unrelated to the senator's age. Howver, labor market opportunities change significantly as a person grows older. Therefore, while age cannot act as a confounder, investigating how effects vary between senators of different ages allows us to test which age-groups react most strongly to outside career options.

In Figure E.5, I use the Hainmueller et al. (2019) kernel estimator[1] to allow for the effect of career prospects to be moderated by age in a non-linear fashion. The results show that effects change very smoothly as concave a function of age. The effect of ca-

[1]There is a good density across all levels of ages which makes it reasonable to use this estimator instead of the binning estimator.

reer prospects on the probability of taking a revolving door job are concentrated among senators who are between their late 50s and mid 70s.

This result is relatively intuitive: young senators are likely to prioritize political results, and are in a position where they still have most of their careers in front of them. Therefore, they can still expect to have a long post-elective career where they can make money. As they grow older, however, the trade-off between political results and private earnings changes. The expected length of the older senator's post-elective career is shorter, and this increases the opportunity cost to holding office.

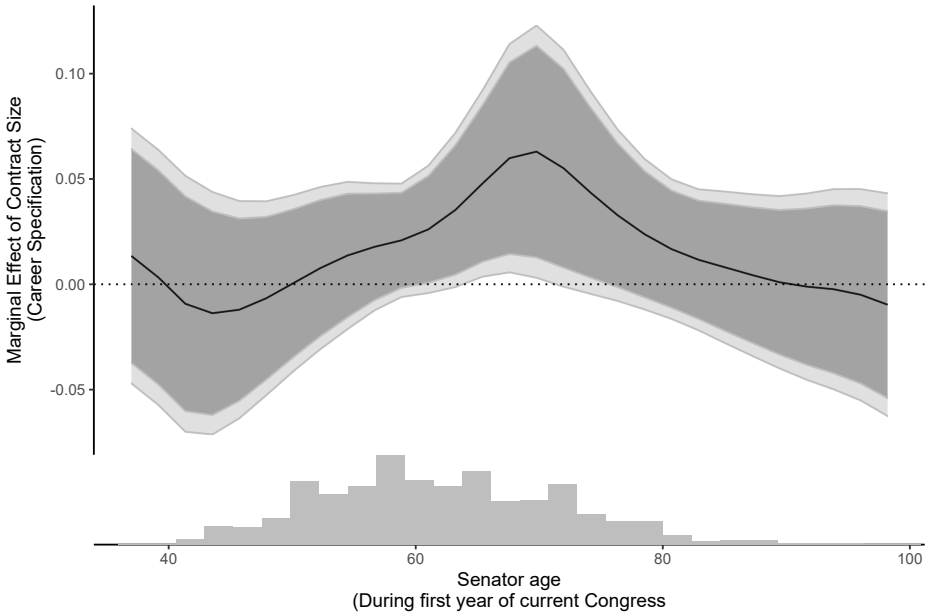

Figure E.5: Heterogeneous Effects of Career Prospects Depdending on Senator Age

Note: The figure shows estimates of how career prospects are moderated by age from the Hainmueller et al. (2019) kernel estimator, which allows for non-linear heterogeneous effects. Senator and time fixed effects are included. Robust confidence intervals are 95 percent (thin lines) and 90 percent (thick lines).

E.3 Different Revolving Door Careers

In the main models, I combine all revolving door careers into a single group. This deals with important sources of selection problems, because some senators avoid registering as lobbyists while still working with interest representation. Previous research has found

that former MCs work in jobs with lobbying components in universities (Lazarus and McKay 2012), listed companies (Egerod 2019), and even lobbying firms (LaPira 2014). It does, however, rest on the assumption that the information on demand for senator-lobbyists is transferable to other types of jobs that have lobbying components—i.e. that senators can learn about the demand for their type among non-lobbying firms by looking at the demand among lobbying firms. In this section, I investigate how transferable the information is.

To do so, I split the variable capturing revolving door retirements in the main analysis into three different dependent variables: retiring for work, respectively, in (1) lobbying firms, (2) non-lobbying companies, and (3) NGOs, think tanks and universities. Because modeling them separately would induce problems of multiple comparisons, I use a Bayesian model for multiple outcomes (Thurston et al. 2009). The model works by modeling the dependent variables together as a function of Contract Size, and adding a random slope for each revolving door career. As in the baseline models, I add fixed effects for senator and time and do so separately for each of the three outcome variables. Full details on the model specification and estimation can be found in the next subsection.[2]

Figure E.6 plots the posterior densities of the differences-in-differences for each of the dependent variables. Results for career-based reference groups are in Panel A, while committee-based groups are in Panel B. There are two points of interest: (1) the general size of the coefficients, and (2) how they vary between careers. I will comment on each in turn.

First, it is worth noting that all estimates are smaller than the baseline ones. Since these three dependent variables are simply disaggregated versions of the combined indicator of leaving office for a revolving door career, this indicates that something is gained by combining them. There is some interaction which is not captured by modeling them separately. Second, private sector career prospects clearly exert an influence on other forms of revolving door careers than lobbying. In the pre-Senate career specifications,

[2]Coefficients are very similar in separate regression models, but have smaller standard errors, indicating that the uncertainty is underestimated.

lobbying is clearly affected the most, but careers in non-lobbying firms are affected as well. In the committee specifications, lobbying careers do not seem to be affected at all, but the distribution is relatively wide.

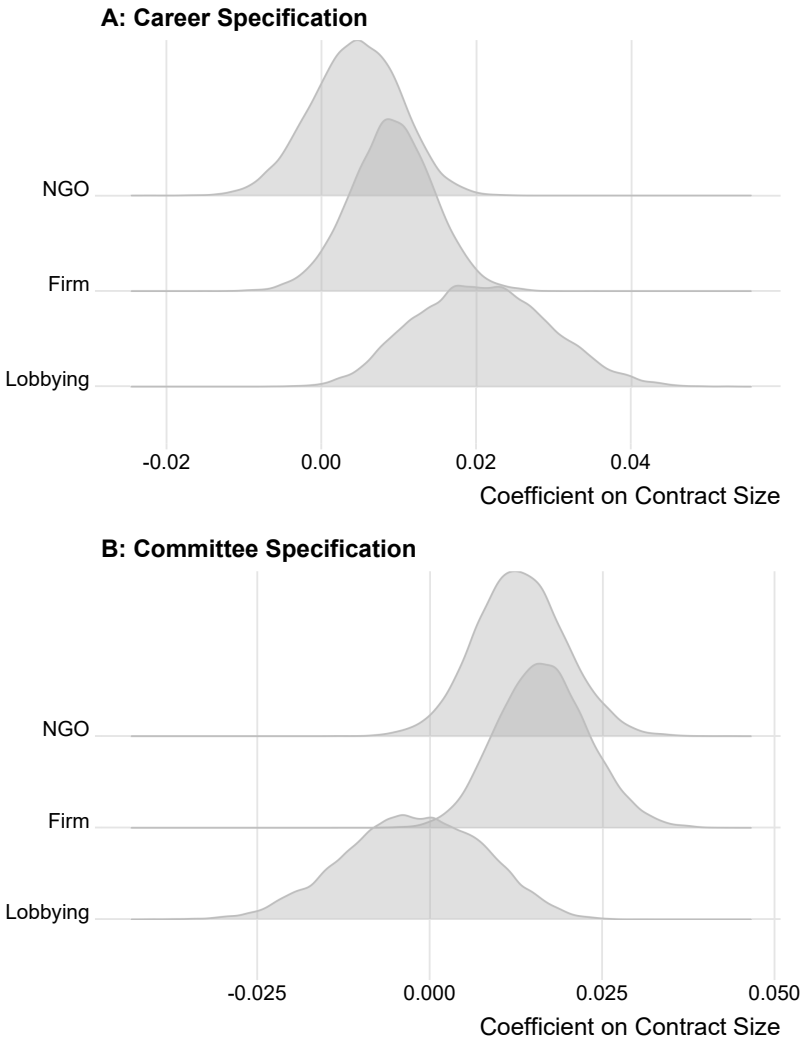

Figure E.6: Effects of Contract Size on Different Revolving Door Careers

Note: The figure shows the posterior distributions from Bayesian regressions of different types of revolving door careers on Contract Size. I run 41,000 iterations of the model in three chains. 3,500 iterations used for burn-in, and a thinning interval of five is used for the remaining simulations. Final sample size is 7,500.

E.3.1 Model Details

In the model below, *career* represents a senator's, i, revolving door retirement decision at time, t, along three different types of post-elective employment, j. I assume that decisions

are drawn from a normal distribution with parameters μ and σ.

$$career_{itj} \sim N(\mu_{itj}, \sigma)$$

$$\mu_{itj} = \beta_j \cdot demand_{it} + \gamma_{ij} + \alpha_{tj}$$

The systematic component is given by the estimated demand for reference group senators and full sets of dummies for senators (γ) and time (α). The important addition to the baseline model is that the parameter of interest, β, is allowed to vary between outcome variables. I place uninformative priors on all parameters of interest—a standard normal density on the hyperparamter for β, and a uniform density (0,100) on the innovation parameter. Thus, I allow the extent of pooling to be estimated from the data.

I use Gibbs sampling to estimate the model, whereby I simulate a random walk across the parameter space. I run a total of 41,000 iterations of the model in three chains. 3,500 of those iterations are used for burn-in, and I apply a thinning interval of five to the remaining simulations. The final sample size is 7,500. I simulate separate models for the career and committee based reference groups.

F Further Results on the Revolving Door and Selection

F.1 Results for the Committee Assignment Measure

In the main text, I presented results showing how the lure of the private sector was heterogeneous across different levels of engagement with legislative productivity. Those results were based on Contract Size estimated using pre-Senate careers. In Figure F.7 I present the same specifications as in the main text, but use committee assignments to estimate Contract Size. They are similar to the results in the main text with the exception that the coefficients on topic specialization are less precisely estimated.

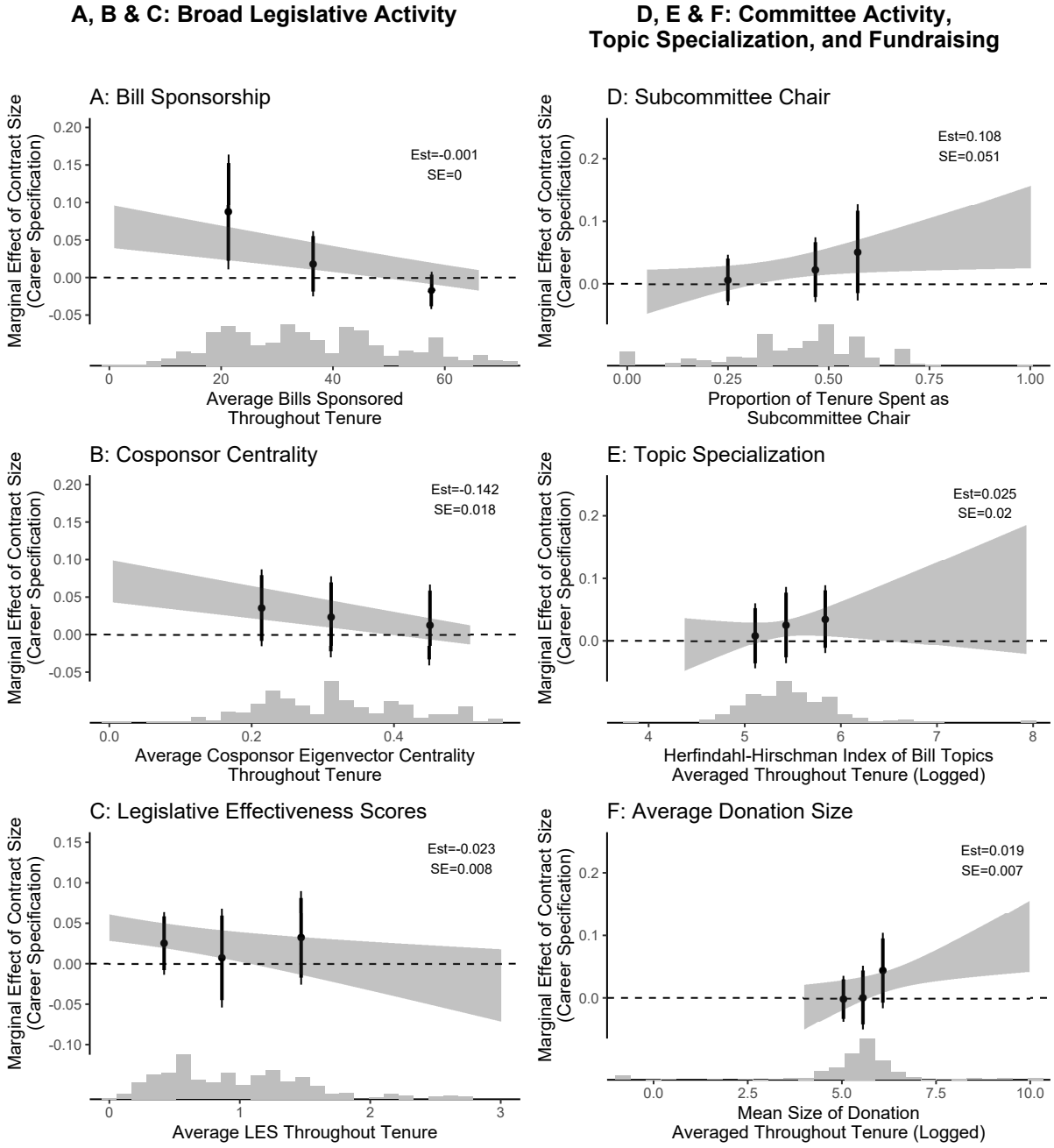

Figure F.7: Committee Specifications: Selection Effects of Career Prospects.

Note: Senator and time fixed effects are included in all models. Robust confidence intervals are 95 percent (thin lines) and 90 percent (thick lines). The Hainmueller et al. (2019) binning estimator shows plausibility of linearity assumption.

F.2 The Revolving Door and Congressional Pension in the House

For the results to fully replicate we need to investigate whether the same mechanism is driving the results: Do Representatives react to the opportunity costs to holding office as well, or is something else driving the results?

The analysis of heterogeneities depending on time remaining until the pension scheme improves is complicated by Members of the House being up for reelection in every cycle. We can, however, leverage that there are more observations in the House. This allows us to at smaller bins. Importantly, we can estimate the effect among MCs that will gain an improved pension this year or the next year, and compare them to the ones that would have to run for re-election once or several times.

The results show no effect for the MCs who will gain their pension improvement within this electoral cycle. Instead, the effect is concentrated among MCs who will have to run for reelection before seeing any improvement. The results are estimated with a great deal of noise, however.

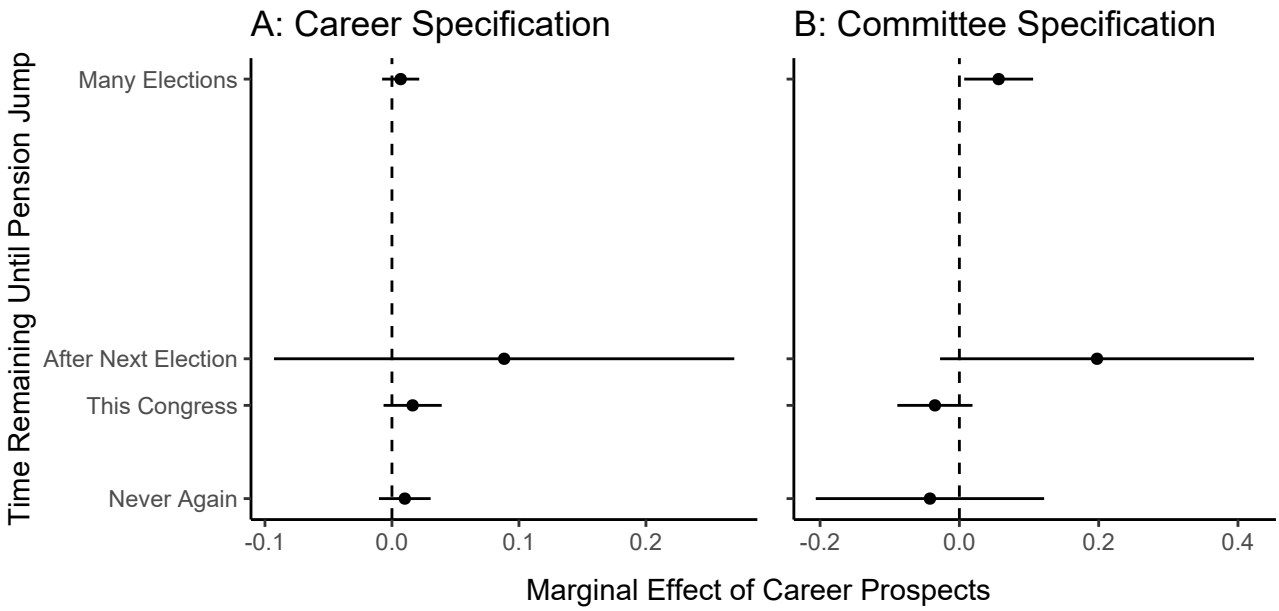

Figure F.8: Opportunity Costs in the House of Representatives.

Note: Senator and time fixed effects included in all models. Estimates are from Hainmueller et al. (2019) binning estimator. Effects estimated within two small groups: MCs 1) gaining improvement within this cycle, and 2) those that would gain it after next election. And two large groups: those 1) with more than one election before seeing improvement, and 2) who will never see another improvement.

F.3 Selection Results for the House

To investigate political selection effects in the House, Figure F.9 shows heterogeneous effects depending on the same factors as was investigated for the Senate. While the results were very clear in the Senate, they are less so here. As was the case in the Senate, MCs that sponsored few bills and had low LES are more likely to select out. The moderation is non-linear, however, and MCs that score high on these variables also tend to react more strongly to outside options. Cosponsor centrality and legislative specialization, however, do not seem to exert a moderating effect. The results for fundraising show that the effect is concentrated among MCs that have generally raised *few* dollars which is the opposite of the Senate results. The results for the committee-based reference groups can be found in Appendix F3. While these interactions (and lack thereof) are individually interesting, the broader picture is more fuzzy than in the Senate.

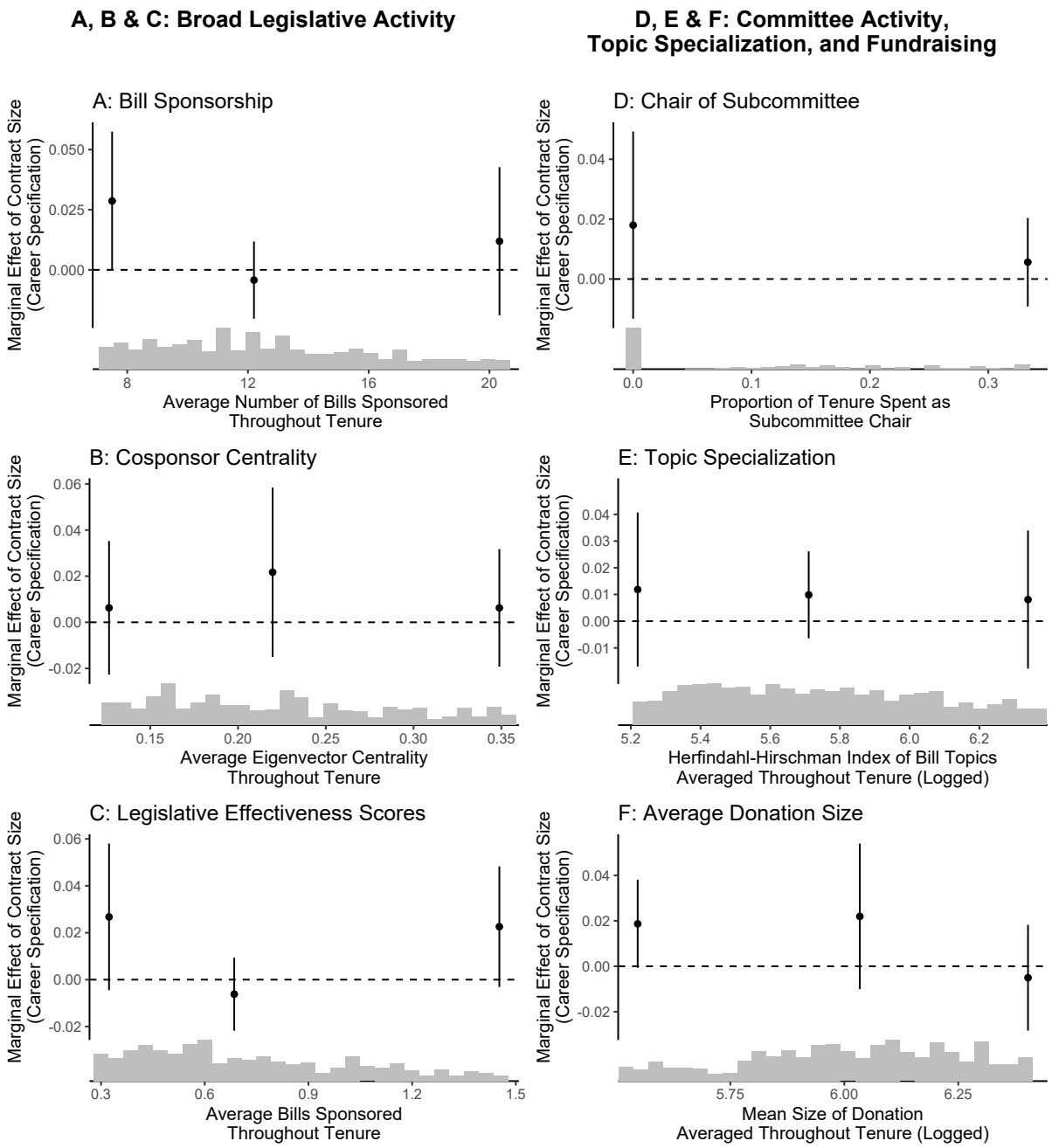

Figure F.9: Selection Effects in the House.

Note: MC and time fixed effects are included in all models. Robust confidence intervals are 95 percent (thin lines) and 90 percent (thick lines). The Hainmueller et al. (2019) binning estimator used to estimate non-linear interaction effects. Results using committee assignments can be found in Appendix F3

F.4 Results for Committee Assignment Measure in the House

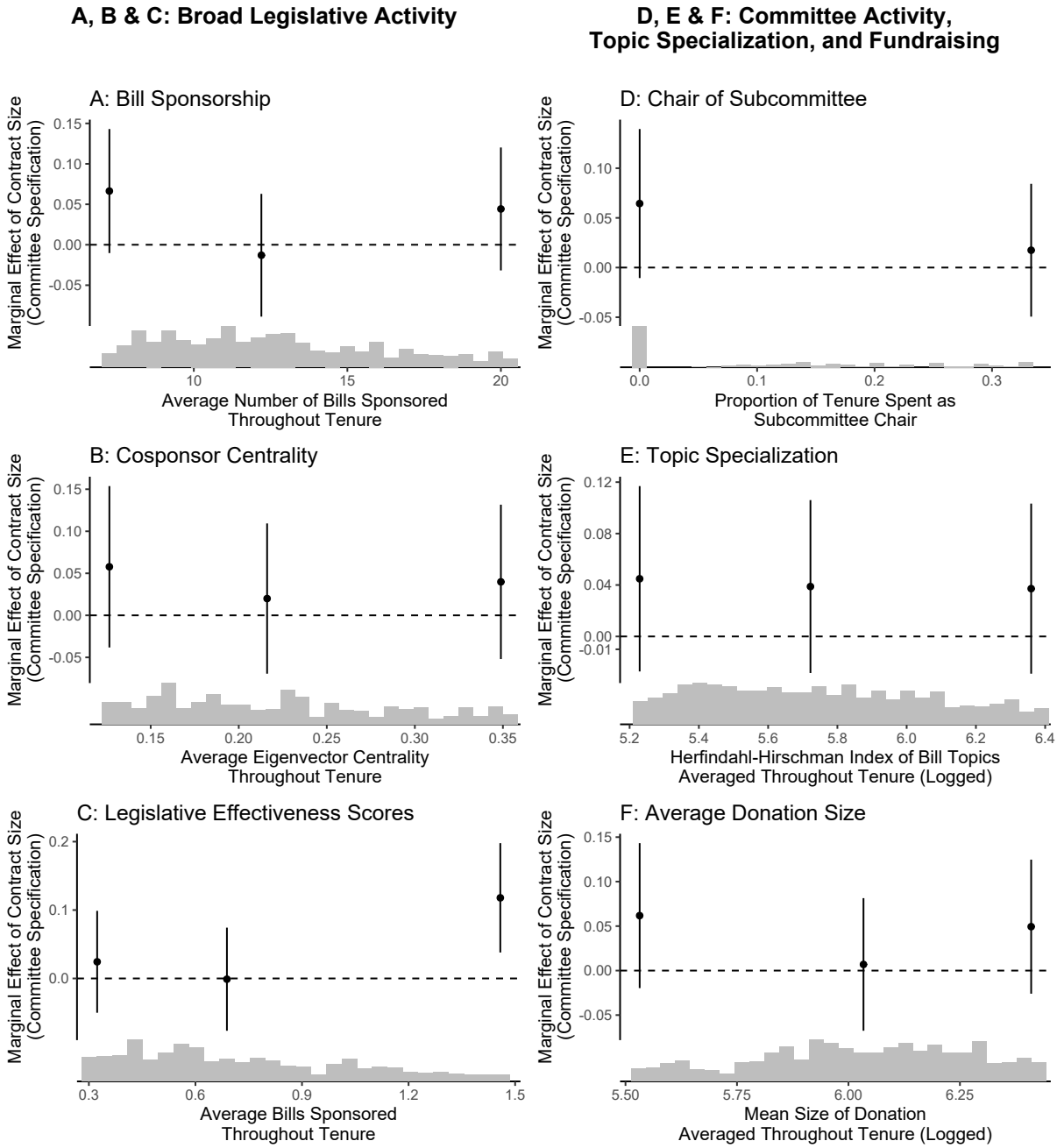

Figure F.10: House Committee Specifications: Selection Effects of Career Prospects

Note: Senator and time fixed effects included in all models. Effects estimated at the sample tertiles of moderator variables using the Hainmueller et al. (2019) binning estimator. Robust confidence intervals are 95 percent.

F.5 Networth and the Lure of the Private Sector

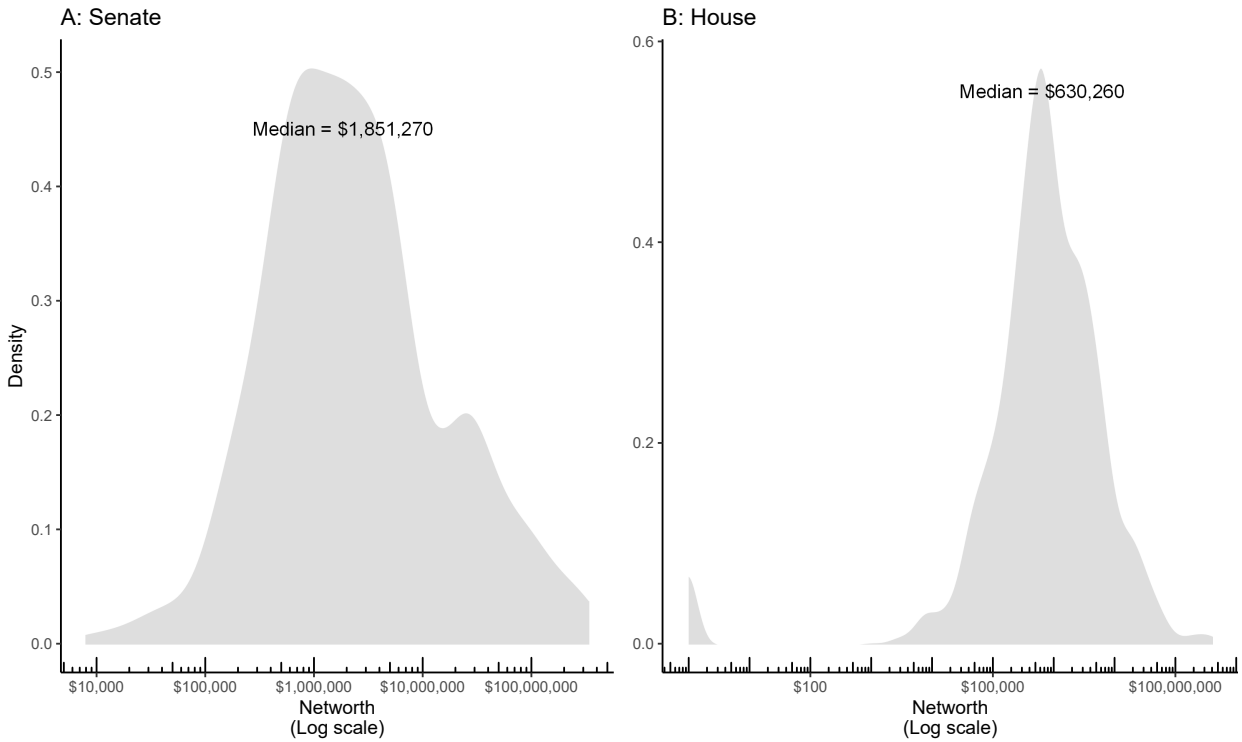

Figure F.11: Distributions of Net worth in the Two Chambers of Congress.

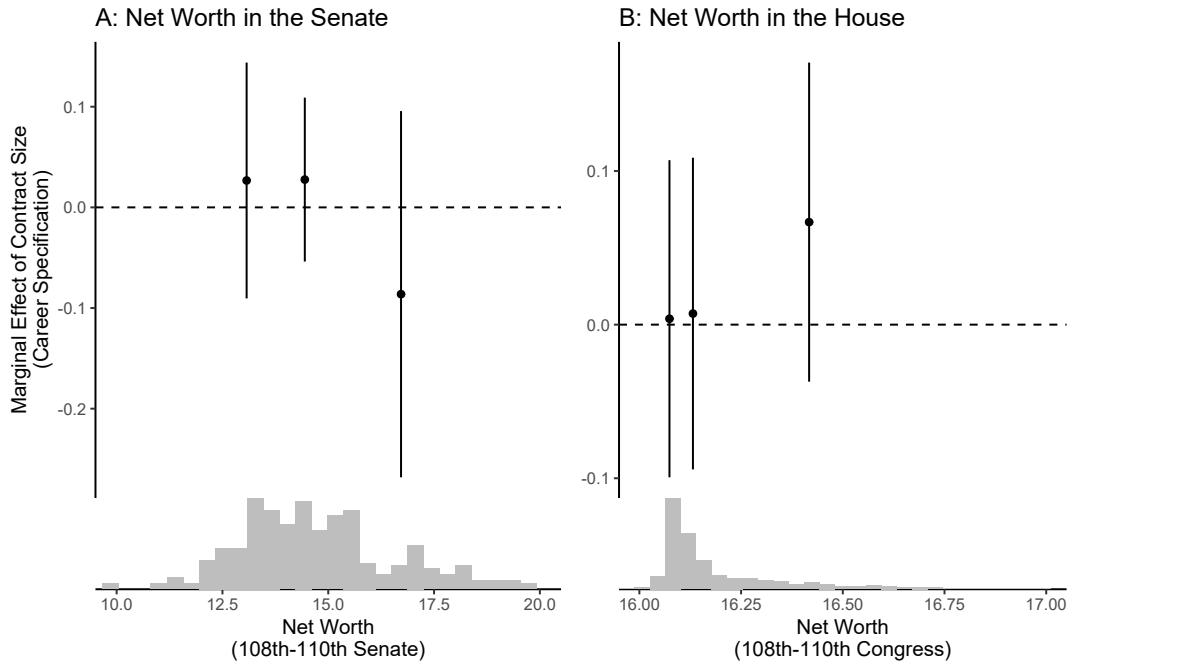

Figure F.12: Moderating Effect of Net Worth in Committee Specifications.

Note: Effects estimated within tertiles using the Hainmueller et al. (2019) binning estimator. Robust confidence intervals are 95 percent.

G Diagnostics of cluster analyses

In this appendix, I consider some diagnostics of the different specifications of the Ward's hierarchical cluster analyses, I use in my main results. While the exact specification of the number of clusters to extract can be debated, the diagnoses clearly illustrates that senators are clustered in the pre-Senate career trajectories, and distinct types of career paths, thus, can be measured by applying cluster analysis in this way. Thus, the overall approach is validated. Given that the results are highly robust to the exact specification, the number of clusters that is used is of less concern.

Figure G.13 shows two dendograms with the baseline numbers of clusters (five and six) emphasized. For both denodograms, it seems clear that the first two clusters are well-fitted and cohesive. While it is clear that the three final groups should be broken up in some way, it is less clear, whether the best fit is provided by five clusters, or – alternatively – the fourth group should be integrated in one of the other two groups.

Next, I show the model fit of a number of different cluster specifications. For the cluster analysis of pre-Senate careers, the marginal improvement in total within-cluster sum of squares decreases markedly between the specifications with three and seven clusters. A specification somewhere between them (e.g. the baseline of five clusters), thus, seems appropriate. For the cluster analysis on committee assignments, the marginal improvement is large over the range of different clusters, but there is not single cluster specification, which alone yields a very large improvement over the former. The speed of improvement does seem to level off after including eight clusters, which is why I limit the number of clusters based on committee assignment used in my various specifications to be between four and eight.

Finally, I plot the within cluster cohesion for all the different specifications. Figures G.15 and G.16 show the silhouette for, respectively, the career and committee based cluster analyses. For the cluster analyses of pre-Senate careers, we can see that cohesion is far from perfect in any single specification, but reasonable levels of cohesion are reached for most groups in different specifications. It seems clear that the specification extracting three clusters has too large within-group differences. This is improved upon in the four-

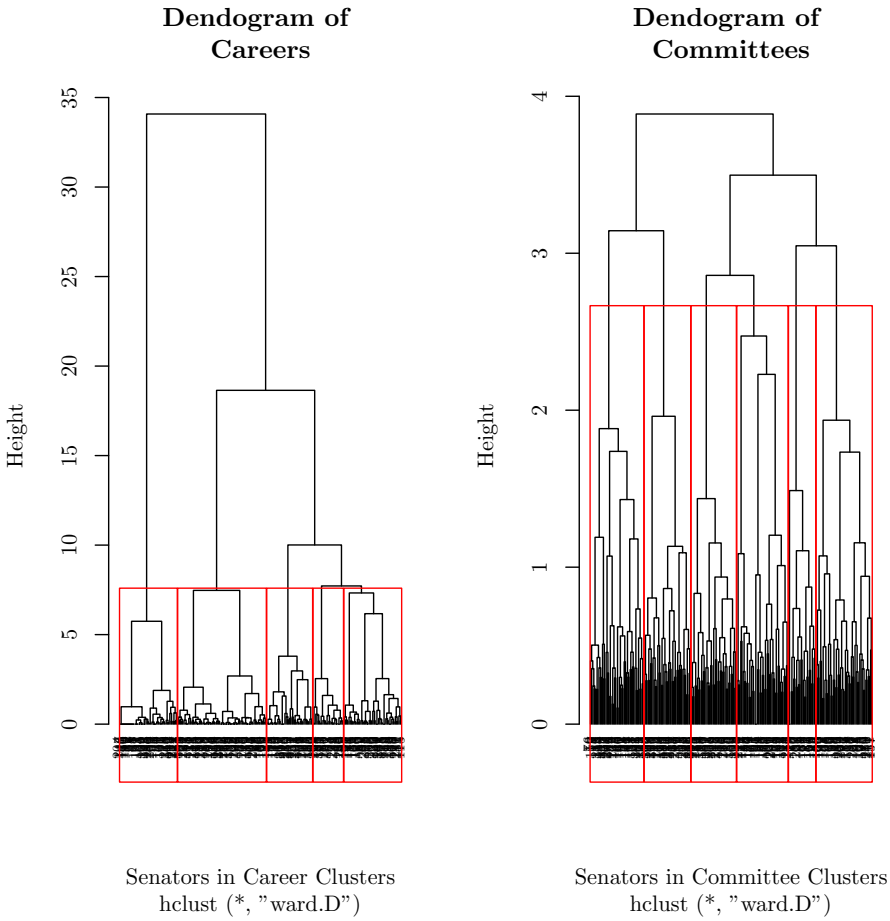

Figure G.13: Dendogram of different career clusters.

Note: The five baseline career clusters are highlighted by red rectangles.

cluster specification, but at the cost of decreasing cohesion in the first group. Using five clusters improves cohesion in the final group. Including more (six and seven) clusters improves somewhat on the poor cohesion in the first couple of groups, but decreases cohesion in the best fitted groups.

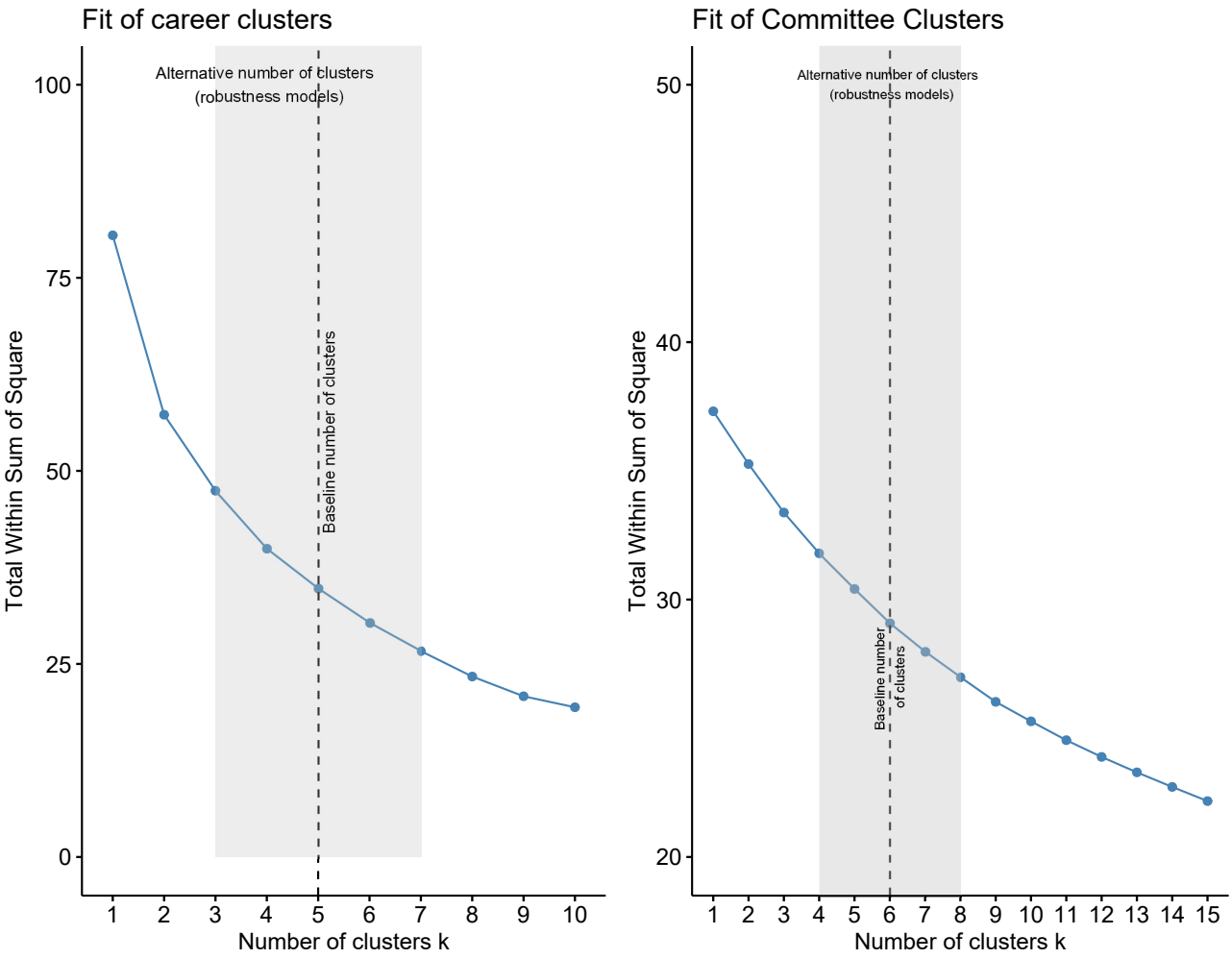

Figure G.14: Fit of different number of clusters.

Note: The vertical dashed line is for the baseline specification of clusters (five and six, respectively). The gray-shaded areas show the alternative specifications that are used to test the robustness of the main results (3-7, and 4-8, respectively).

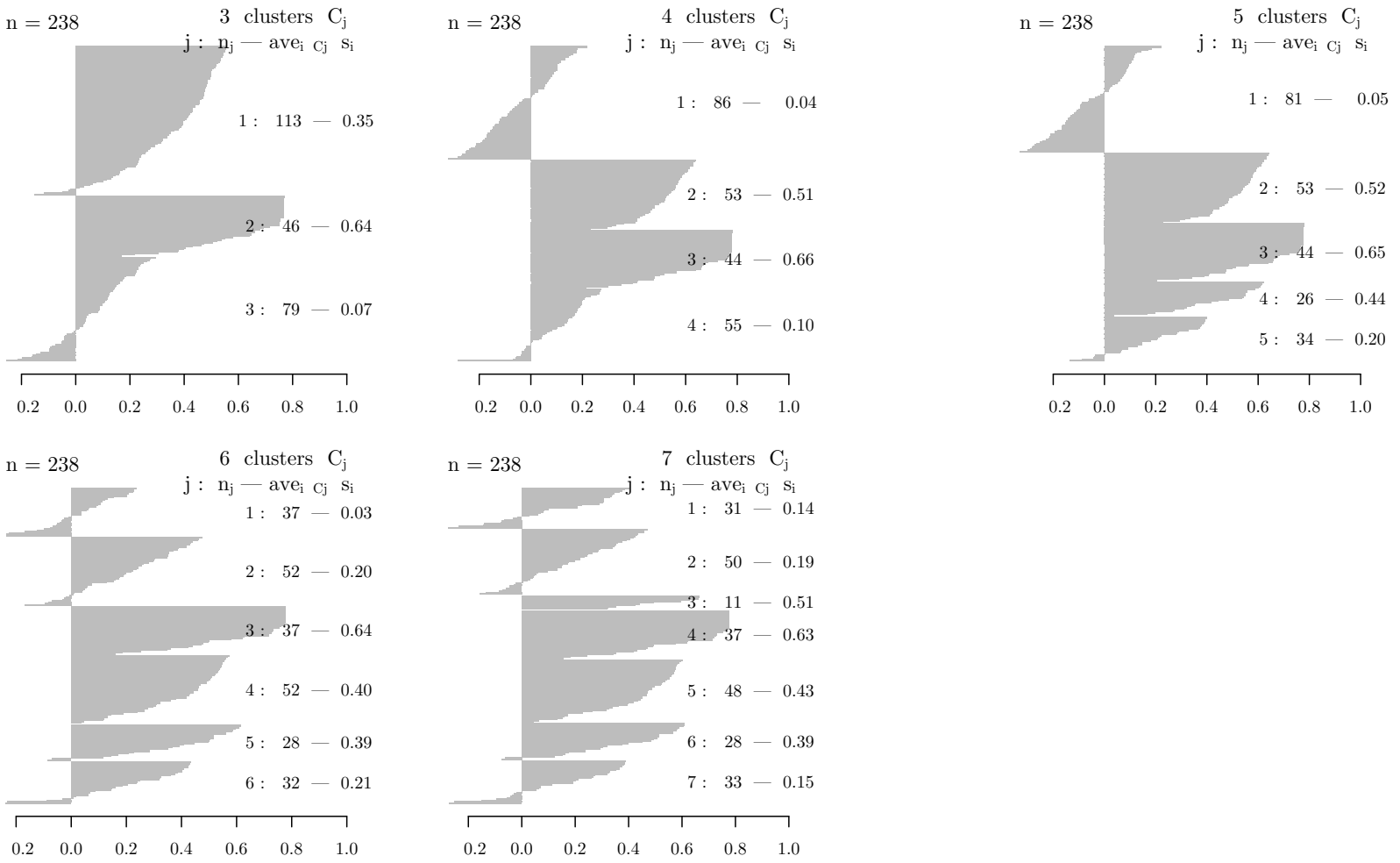

Figure G.15: Cohesion of career clusters.

Note: This figure shows the cohesion – as measured through the silhouette score – of the different number of career groups that can be extracted from the cluster analysis. While we can see that cohesion is far from perfect in any single specification, reasonable levels of cohesion are reached for most groups. I am further reassured by the fact that the same substantive results are obtained using any of these alternative number of clusters.

Again, for most senators in my sample, the specification with five clusters seems like a reasonable one. Again it should be noted that, I am further reassured by the fact that the same substantive results are obtained using any of these alternative number of clusters – thus, the results are not an artefact that comes by because of poorly fitted cluster analyses.

Turning to the cohesion in the committee based clusters, the picture is more messy. No solution obtains high levels of cohesion, which suggests that there is considerable amounts of noise in the assignments of senators to committees. Since the results are the same across specifications and when using the clusters based on pre-Senate careers, the lack of cohesion should not be of too much concern. Similarly, adjusting for measurement error in various ways in Appendix E does not change the results.

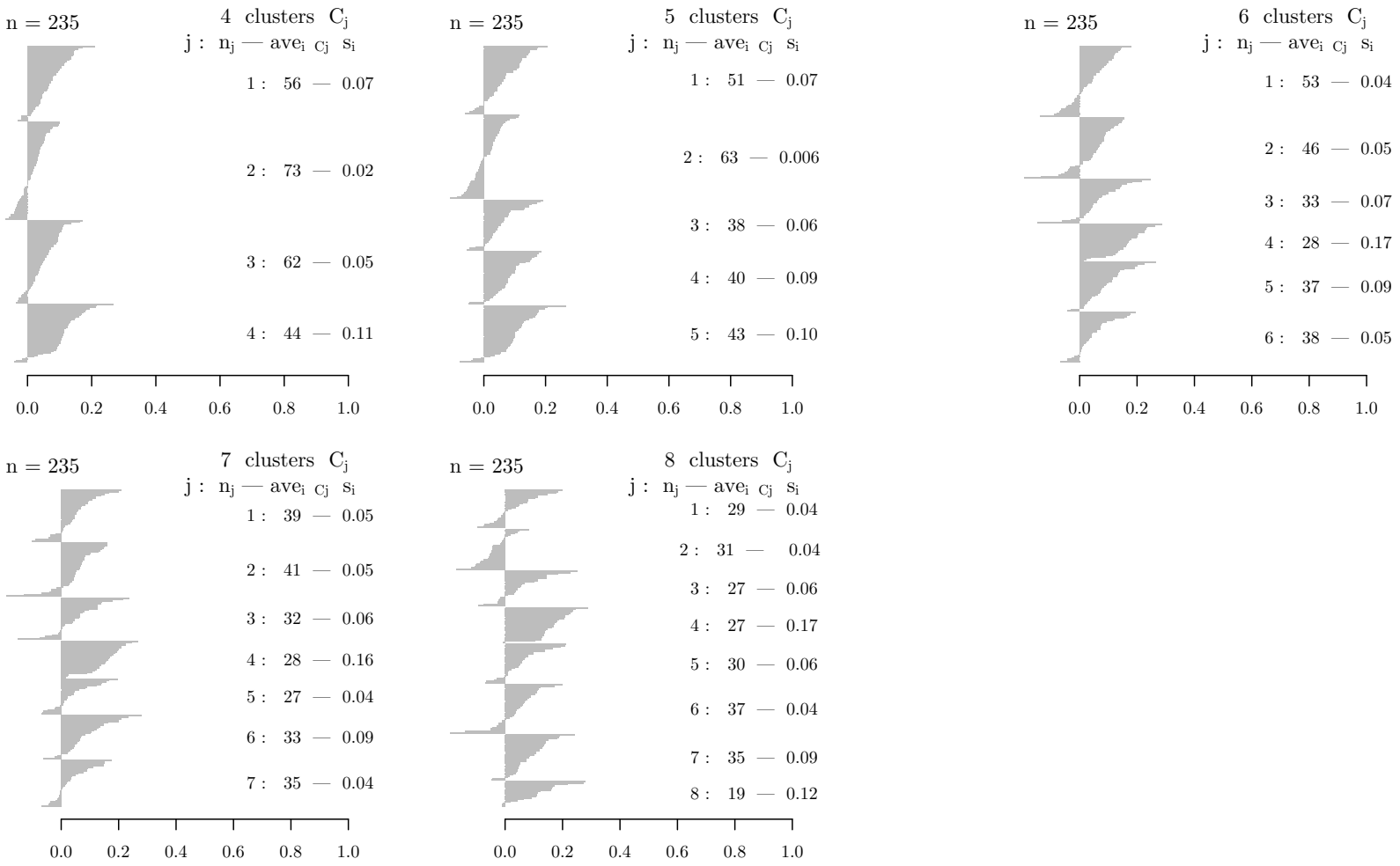

Figure G.16: Cohesion of committee clusters.

Note: This figure shows the cohesion – as measured through the silhouette score – of the different number of reference groups based on committee assignment that can be extracted from the cluster analysis. While cohesion is generally low, the results are highly robust, indicating that low cohesion should not be of too much concern.

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