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.

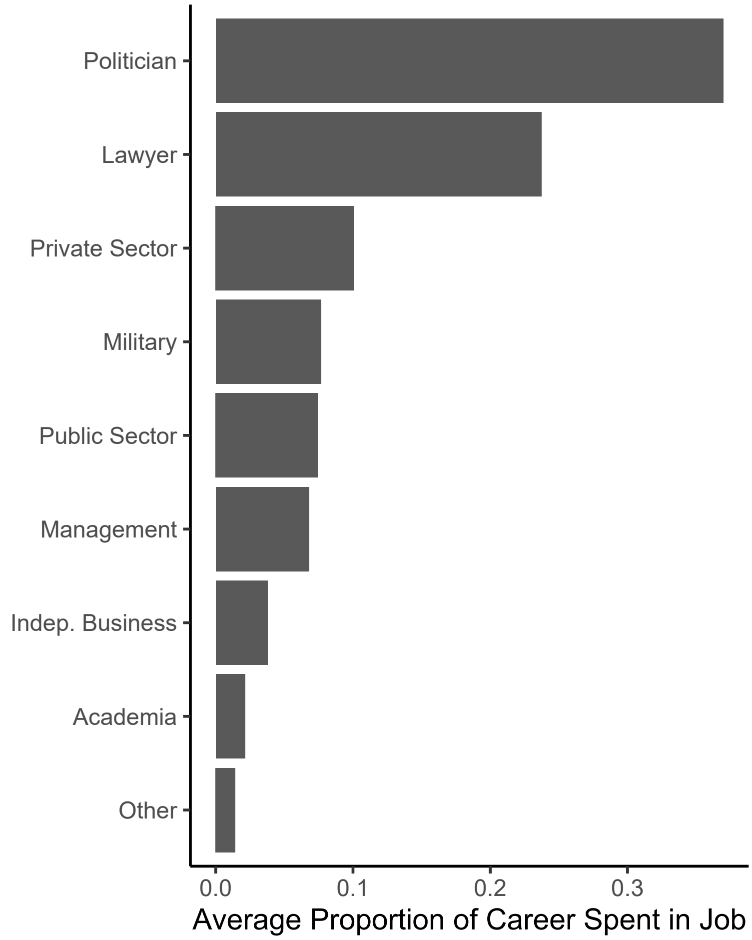

Figure A.1: Average proprotion of pre-Senate career spent in various job types.

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)

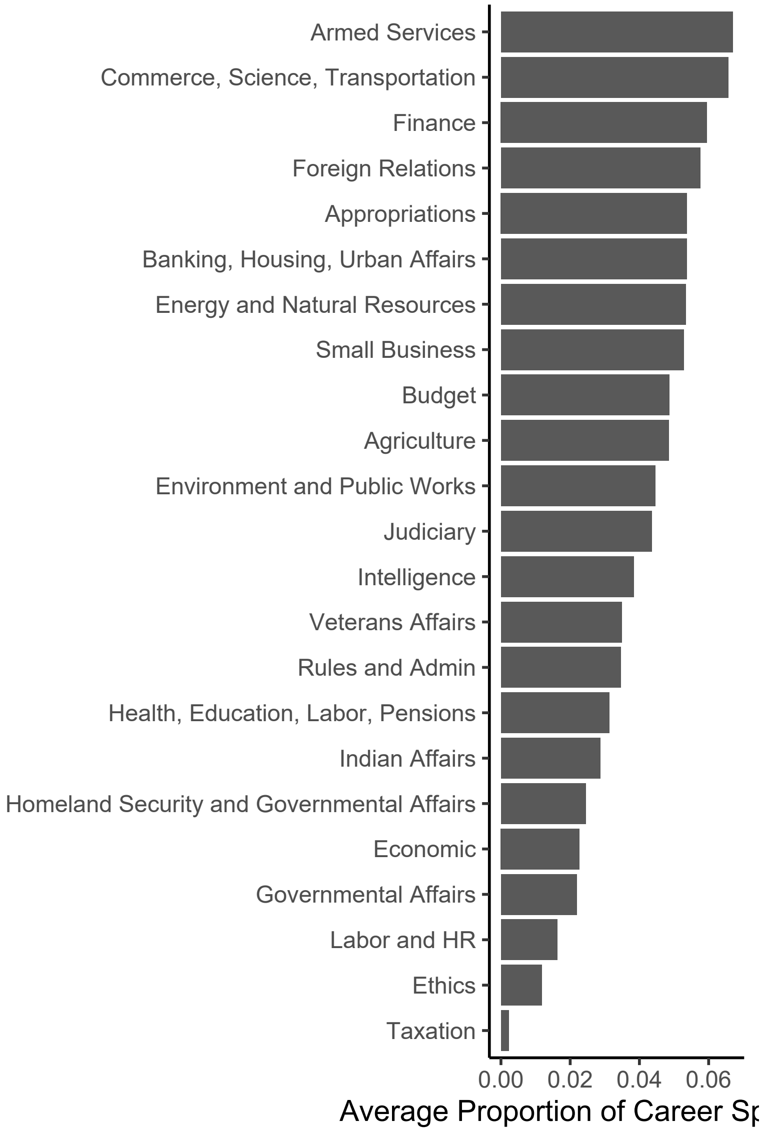

Figure A.2: Average proportion of tenure spent in various Senate Committees.

– 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.

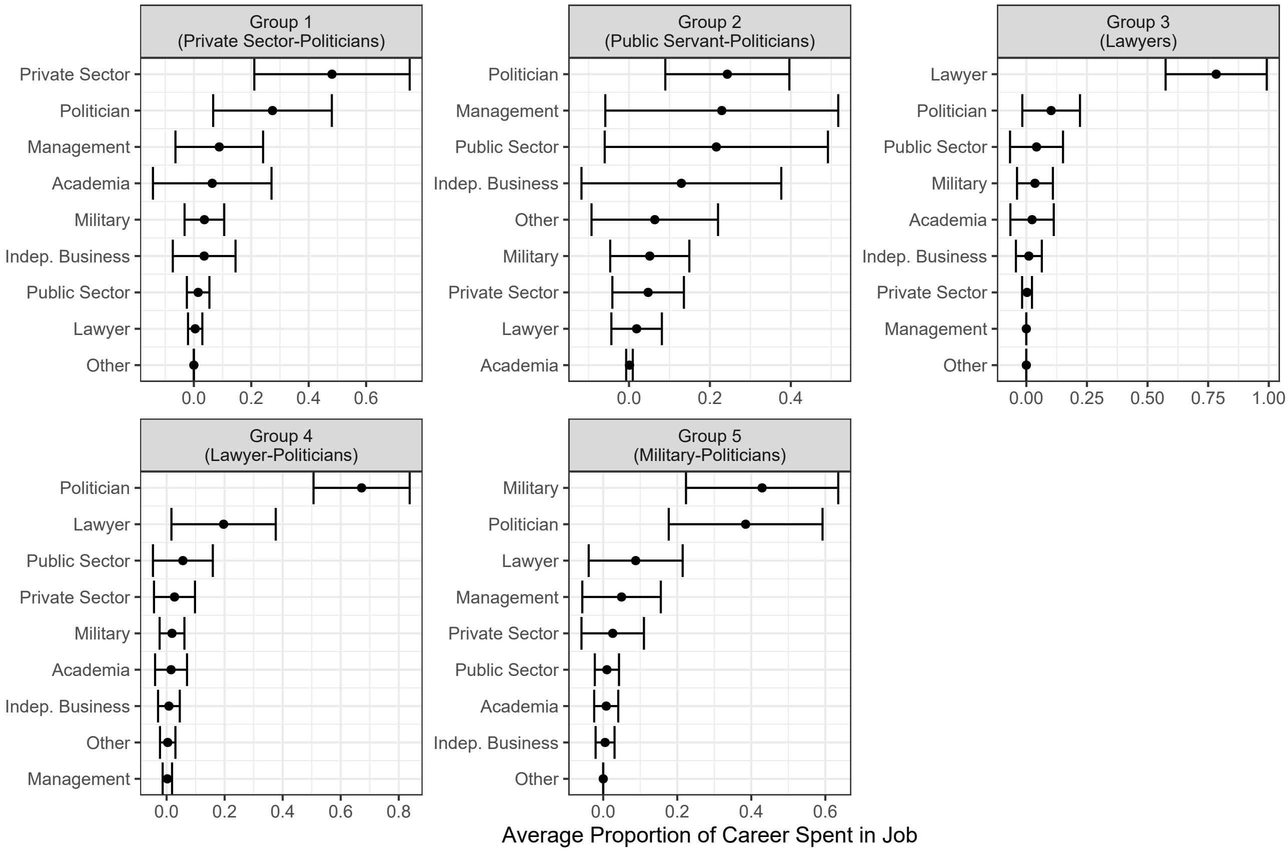

Figure A.3: Mix of pre-Senate careers within five different reference groups.

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.

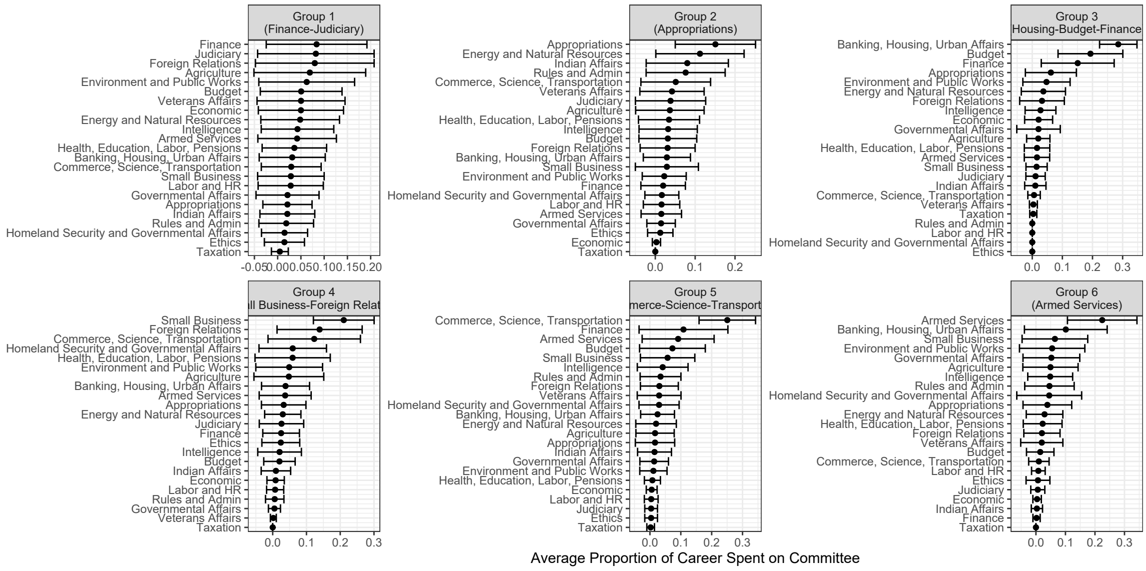

Figure A.4: Mix of committee assignments within six different reference groups.

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).

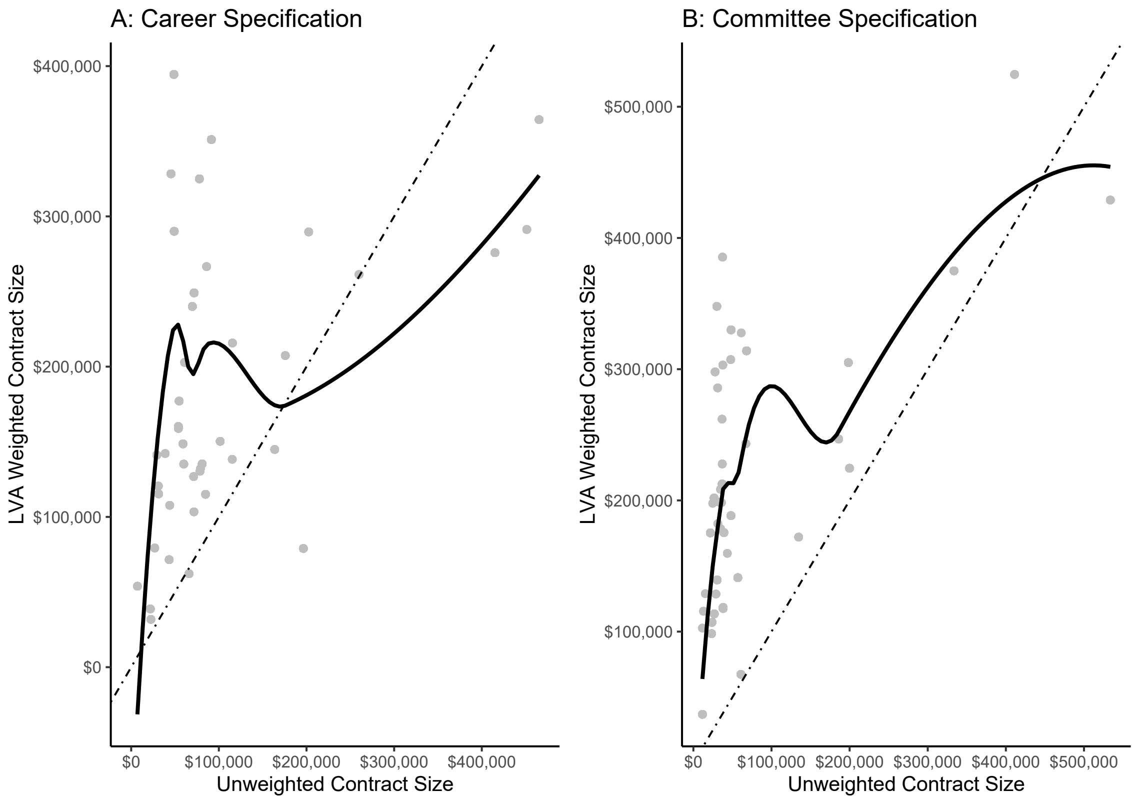

Figure B.1: Comparing Unweighted and Weighted Contract Size.

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.

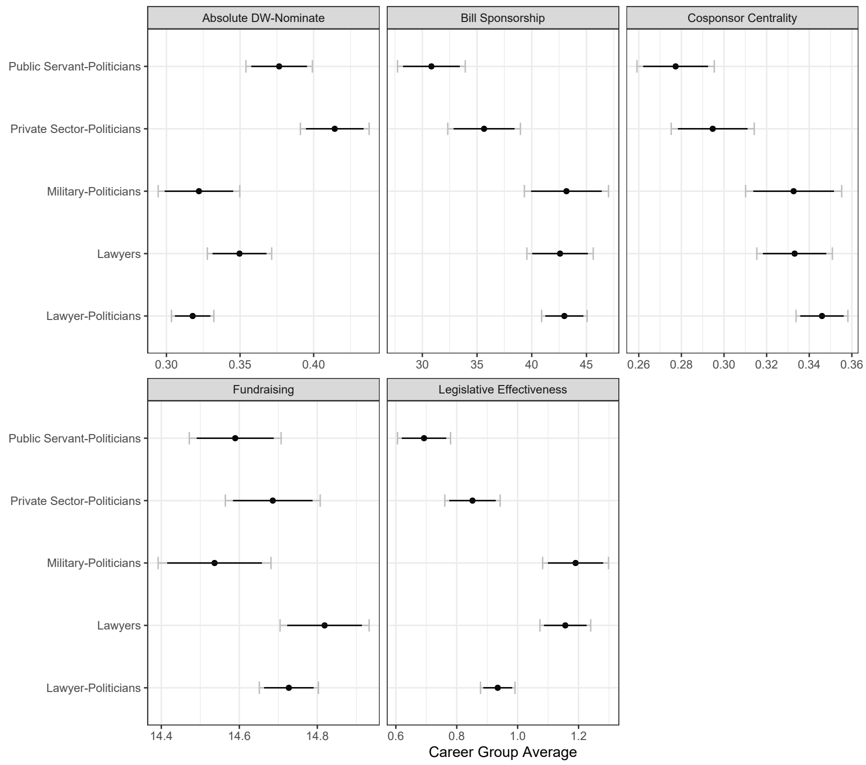

Figure C.2: Career Groups and Senator Legislative Style.

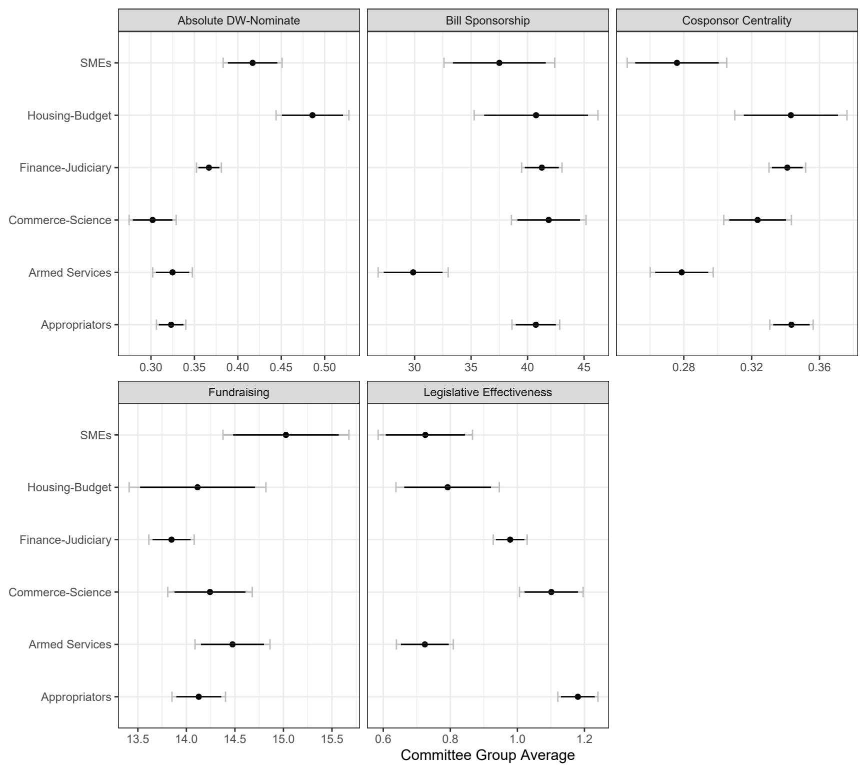

Figure C.3: Committee Groups and Senator Legislative Style.

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.

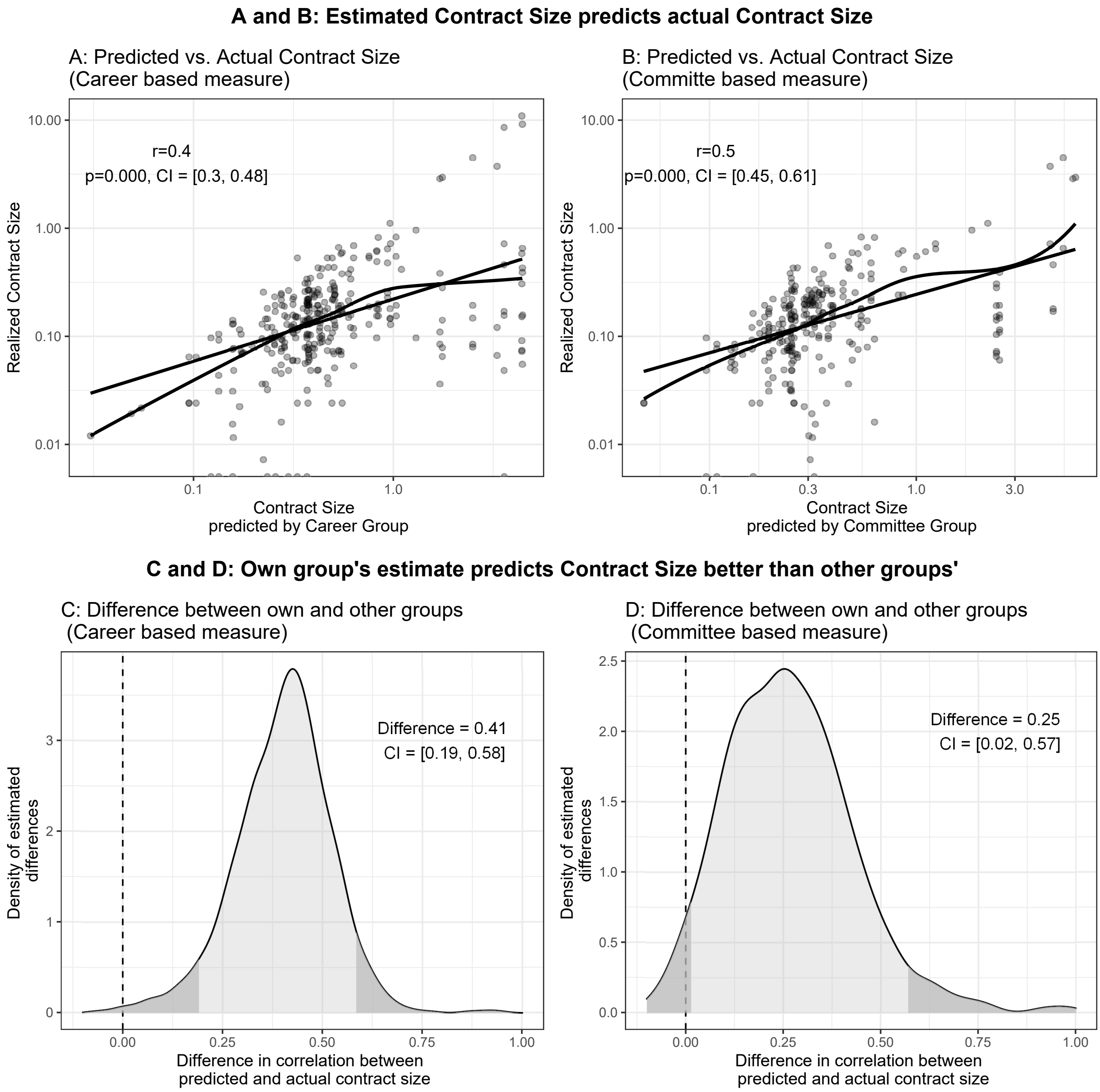

Figure C.4: Validating the measure.

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,

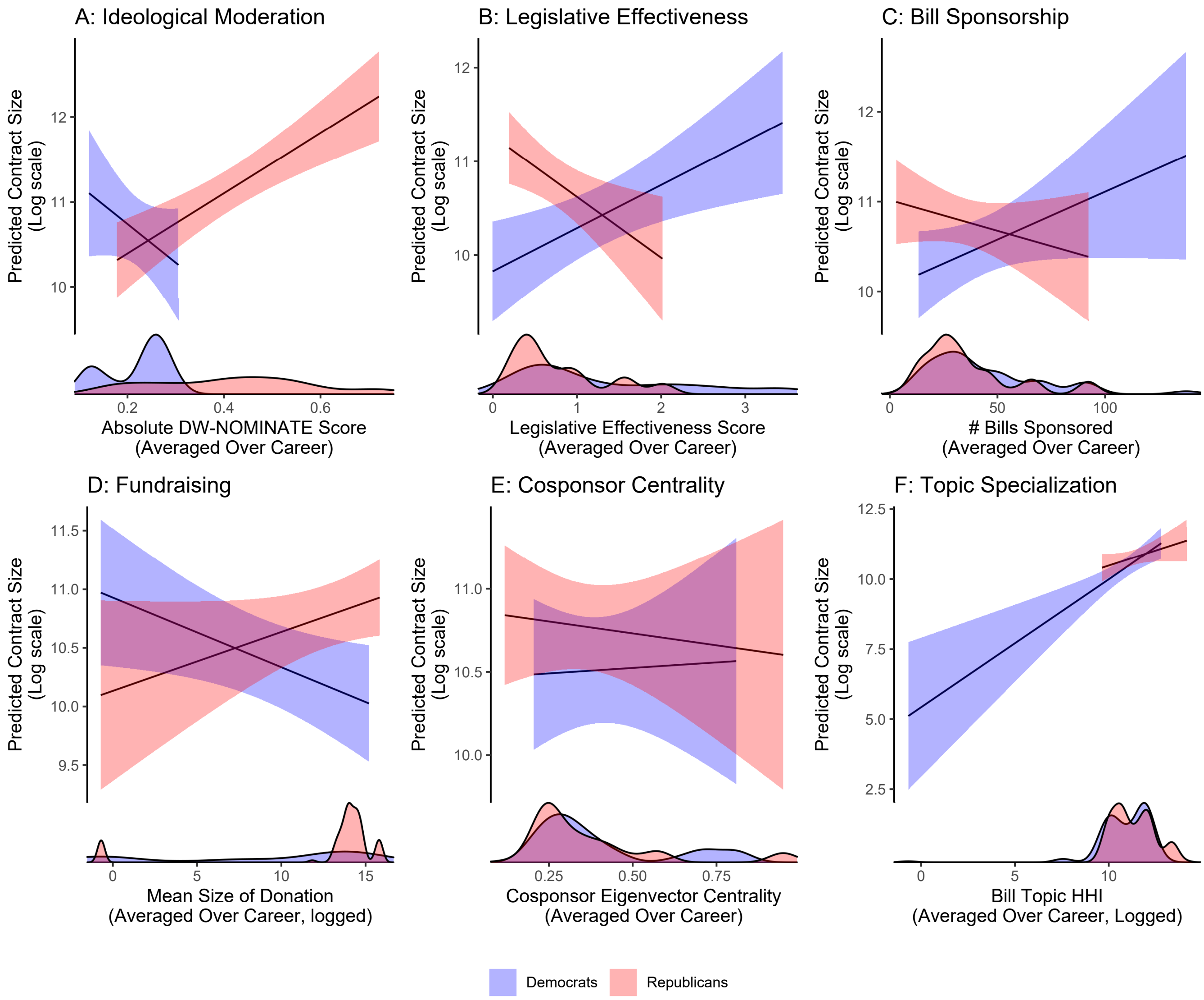

Figure C.5: Drivers of Individual Contract Sizs.

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.

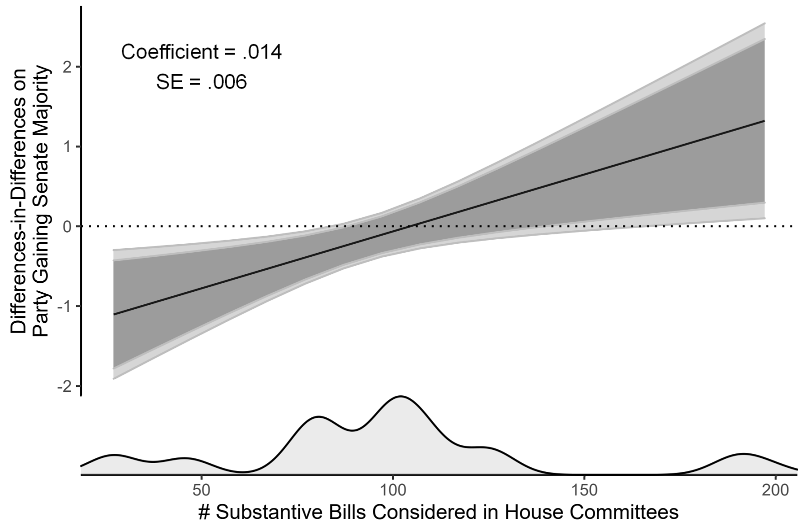

Figure C.6: Senate Majority, Legislative Activity, and Contract Size.

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.

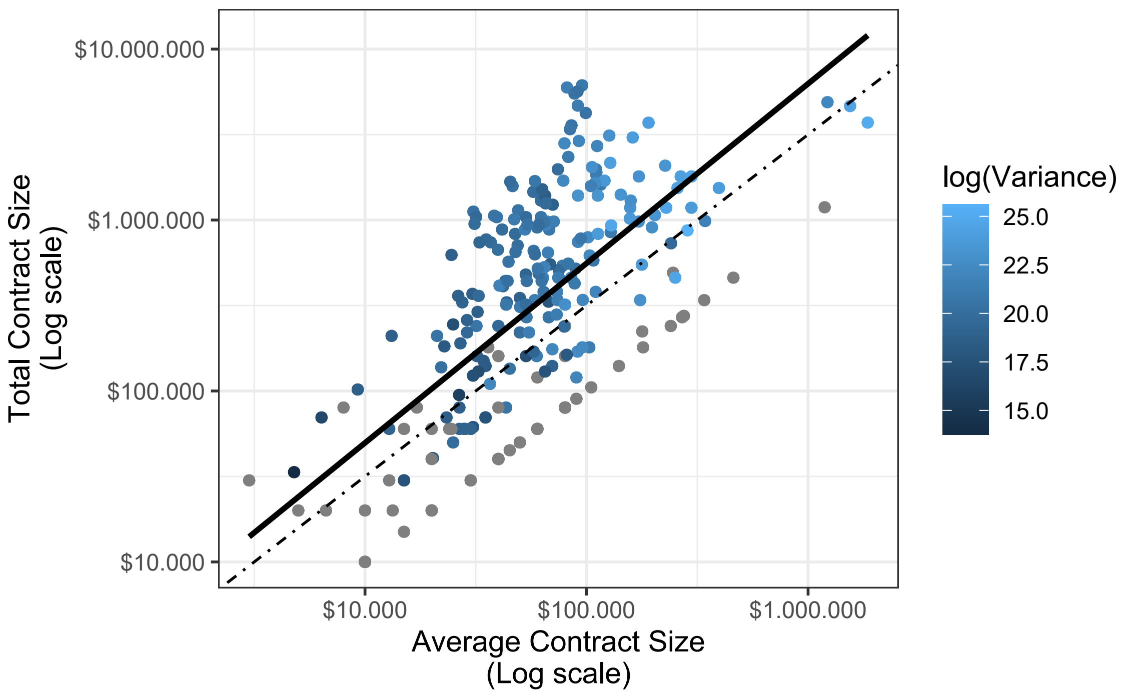

Figure C.7: When Does the Total and the Average Diverge?

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.

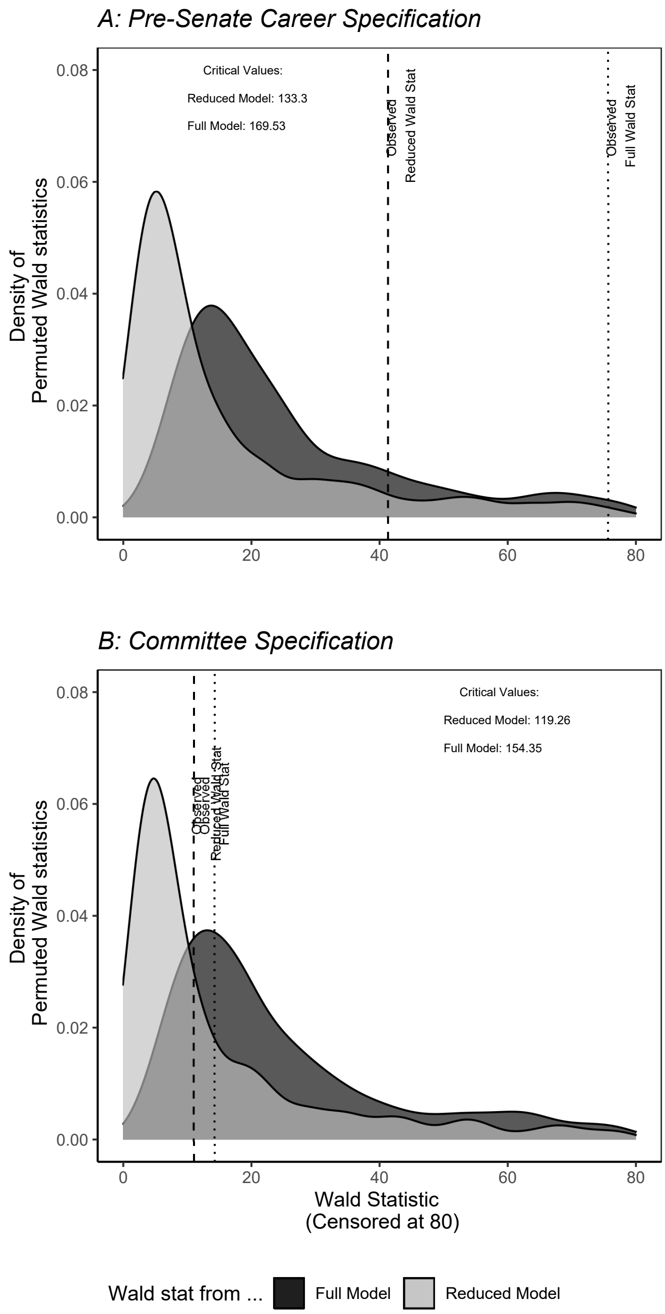

Figure D.1: Permutation Test of Covariate Balance.

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.

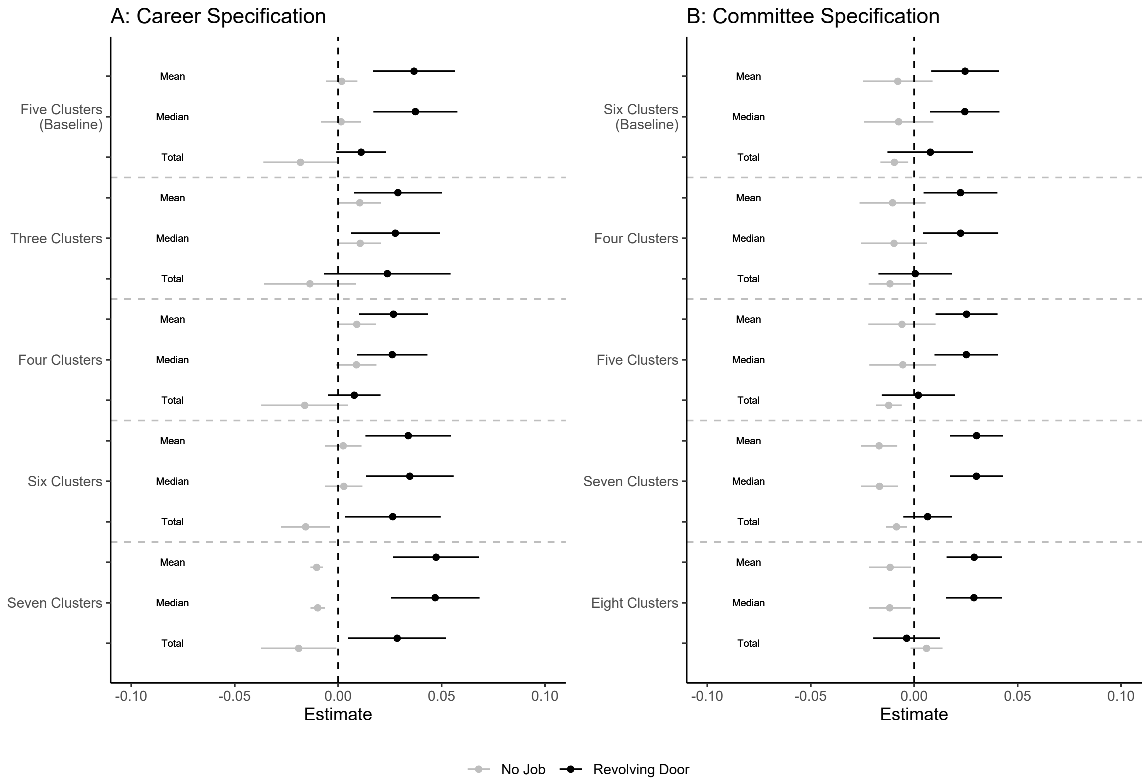

Figure D.2: Robustness to Specification Choice and Sanity Checks.

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

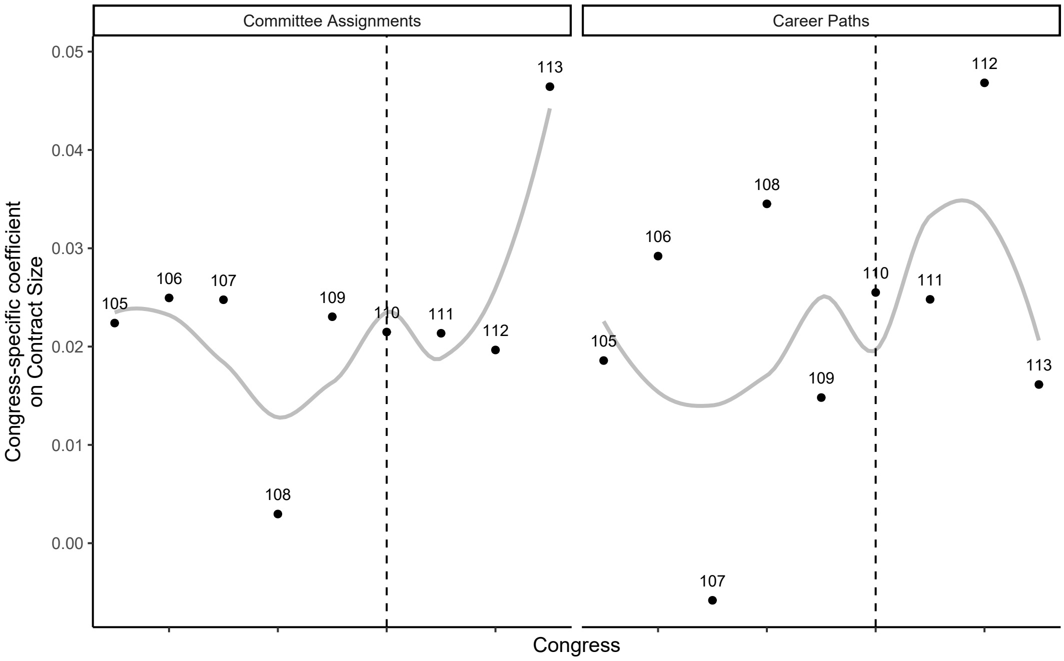

Figure D.3: Congress-specific Coefficient on Contract Size.

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

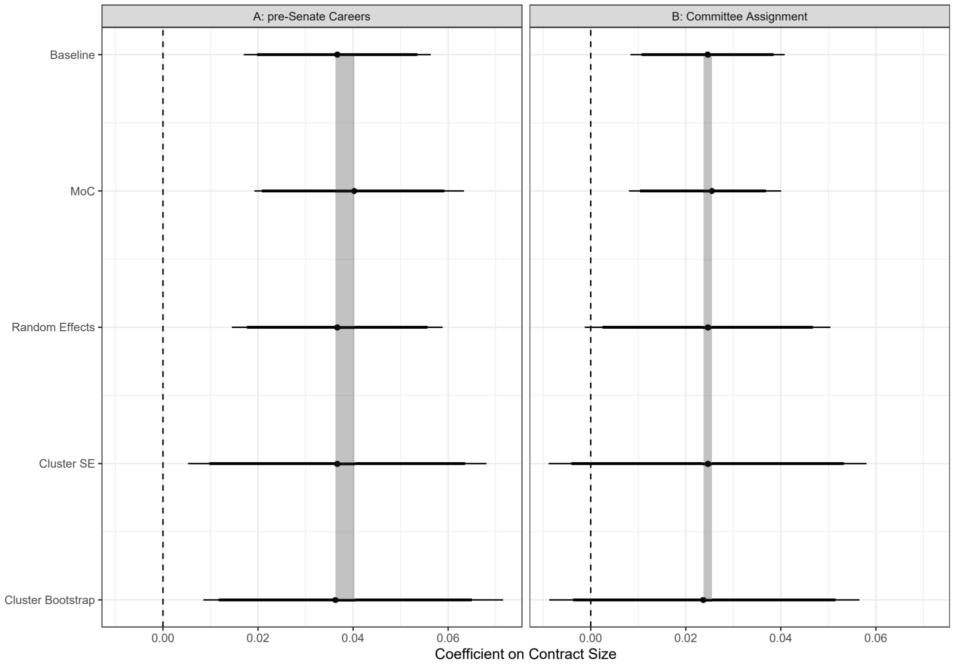

Figure D.4: Robustness to measurement error, clustering and autocorrelation.

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.

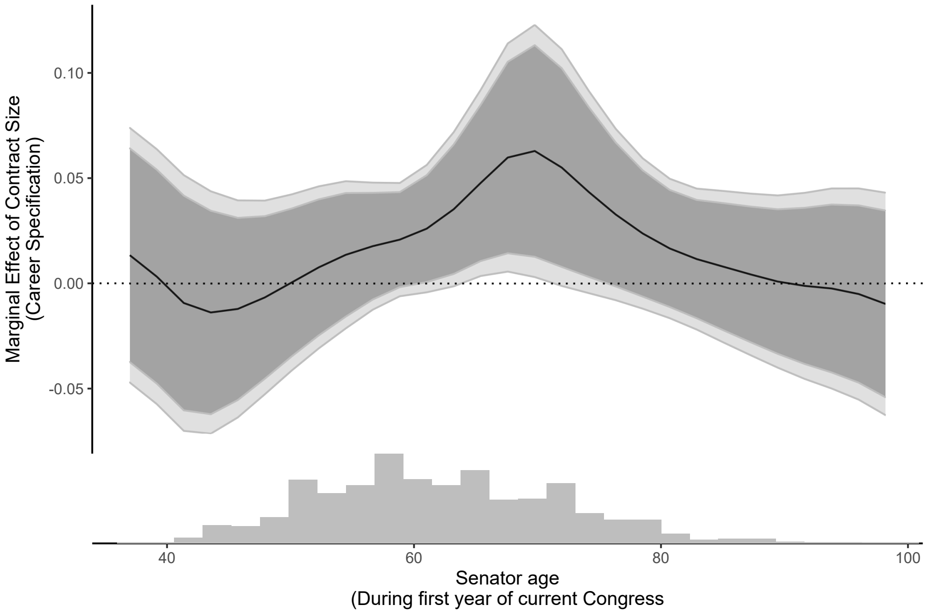

Figure E.5: Heterogeneous Effects of Career Prospects Depdending on Senator Age

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.

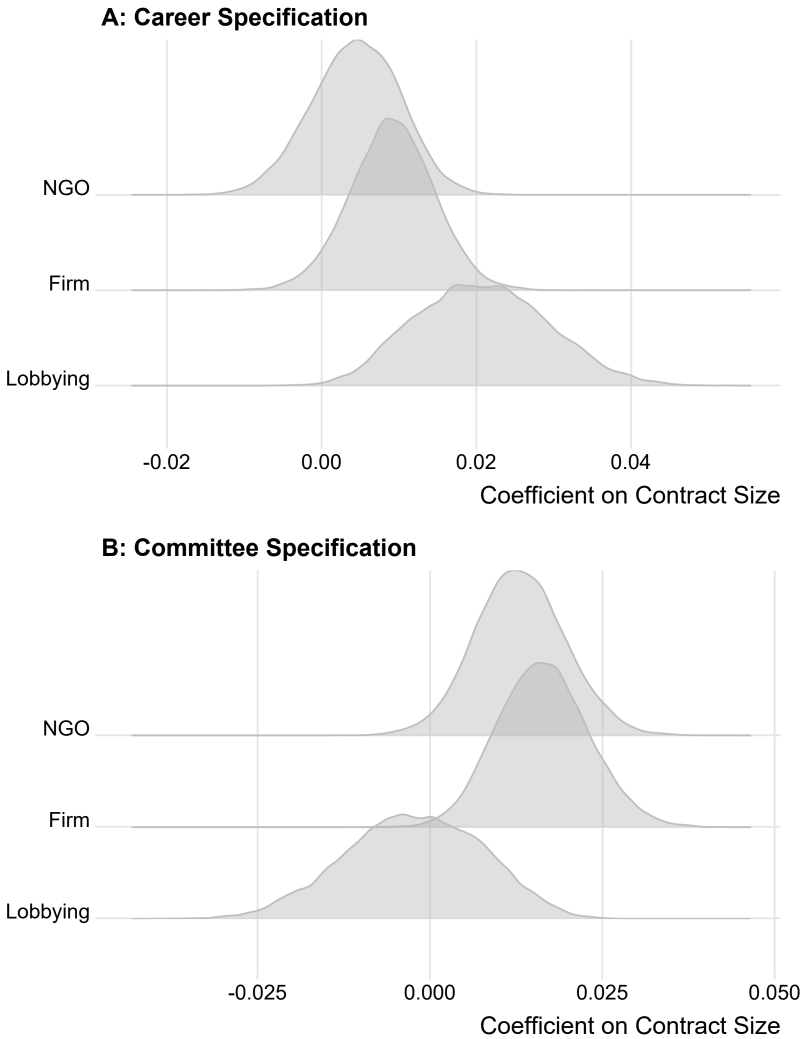

Figure E.6: Effects of Contract Size on Different Revolving Door Careers

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.

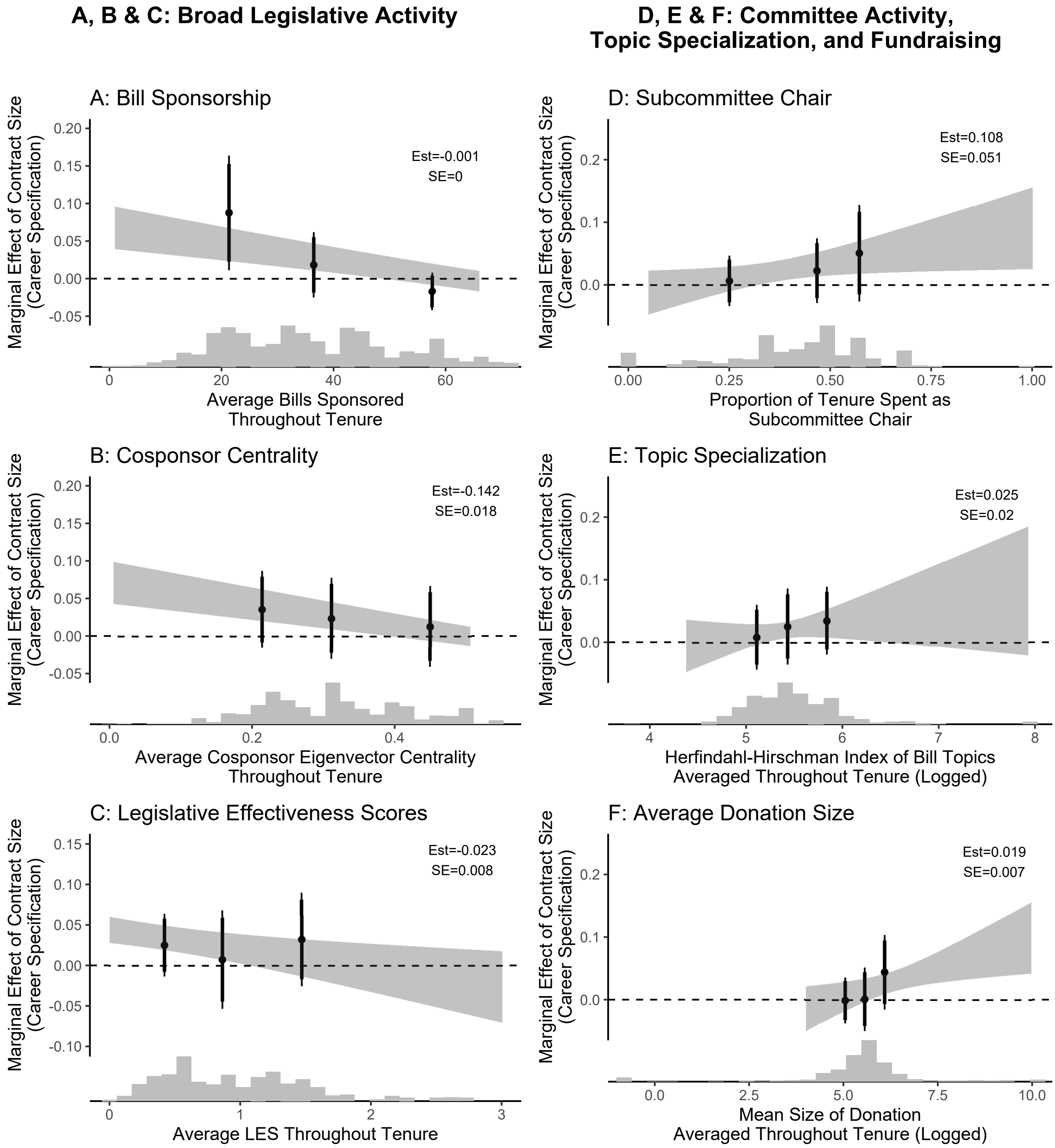

Figure F.7: Committee Specifications: Selection Effects of Career Prospects.

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.

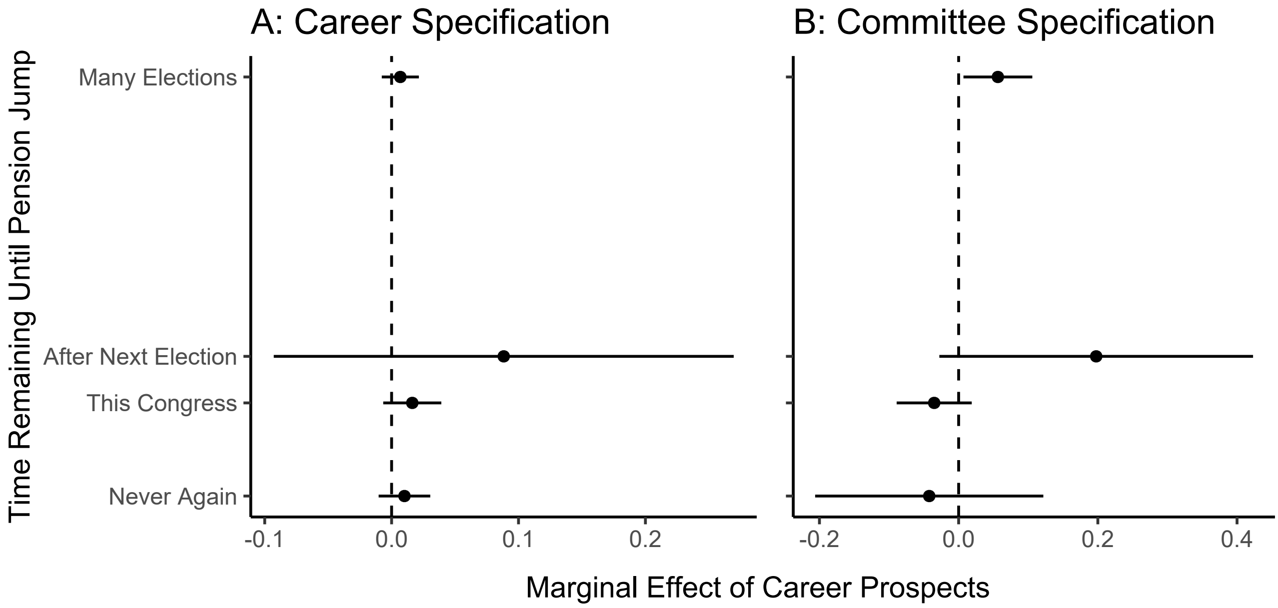

Figure F.8: Opportunity Costs in the House of Representatives.

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.

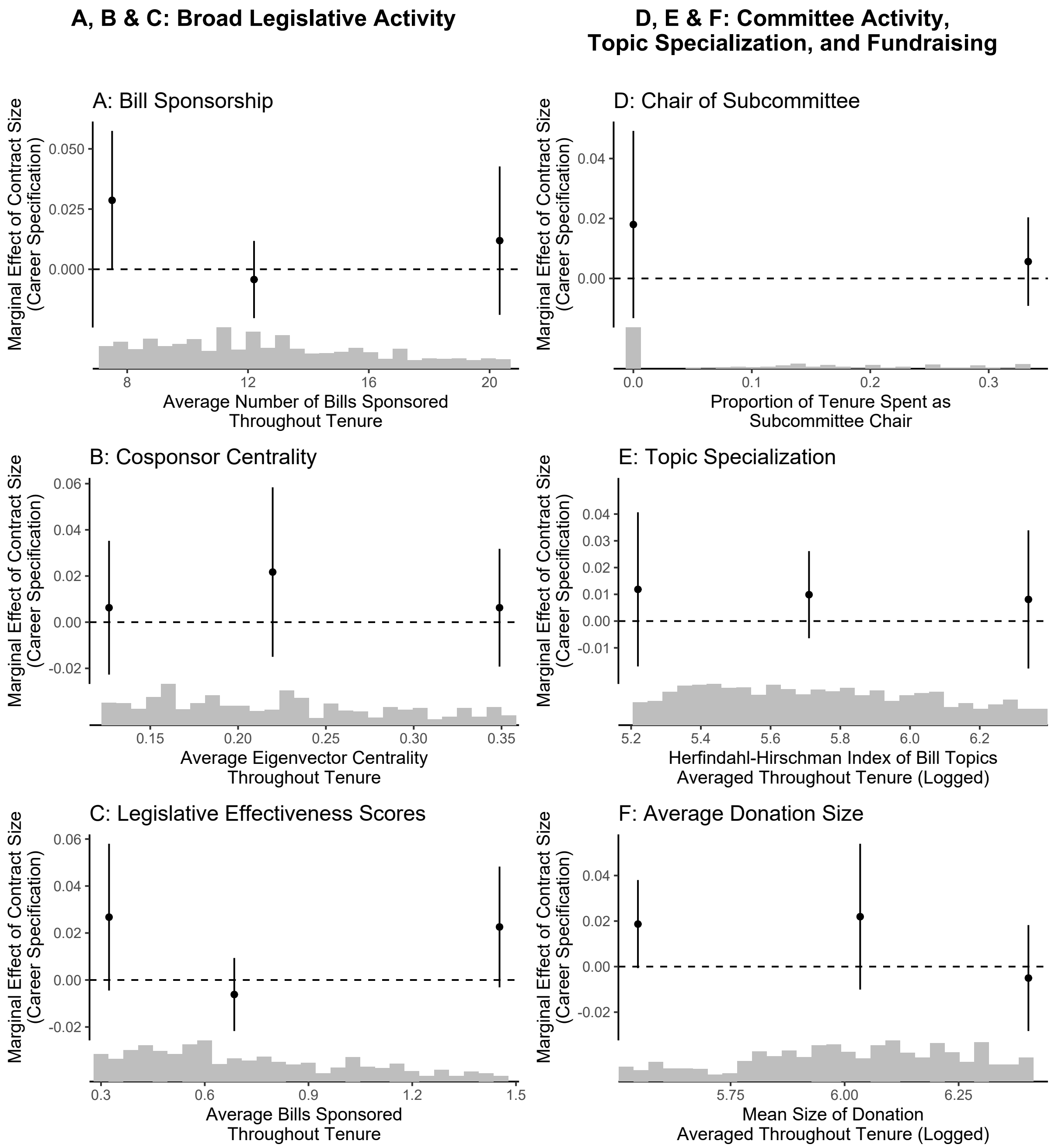

Figure F.9: Selection Effects in the House.

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

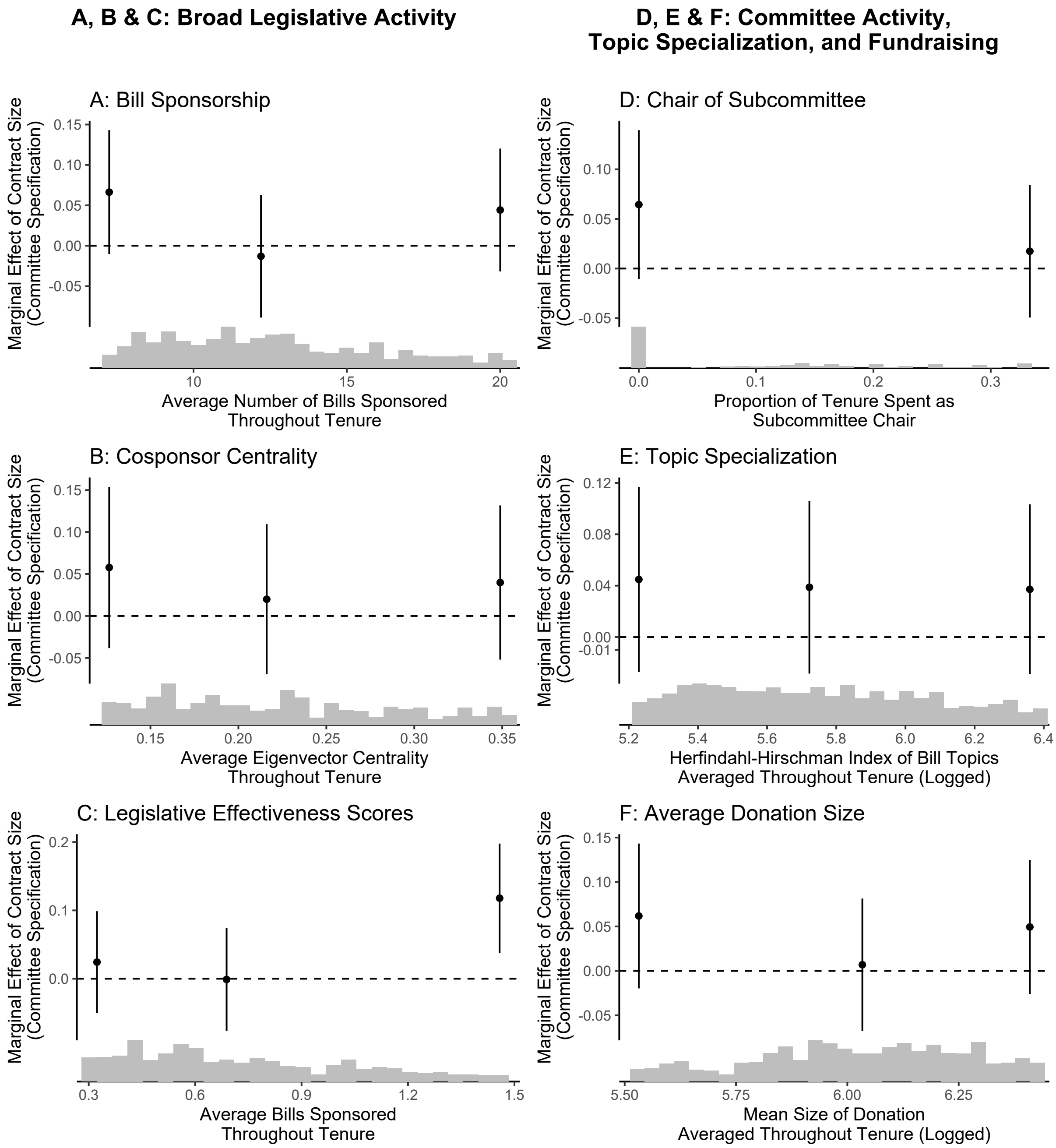

Figure F.10: House Committee Specifications: Selection Effects of Career Prospects

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

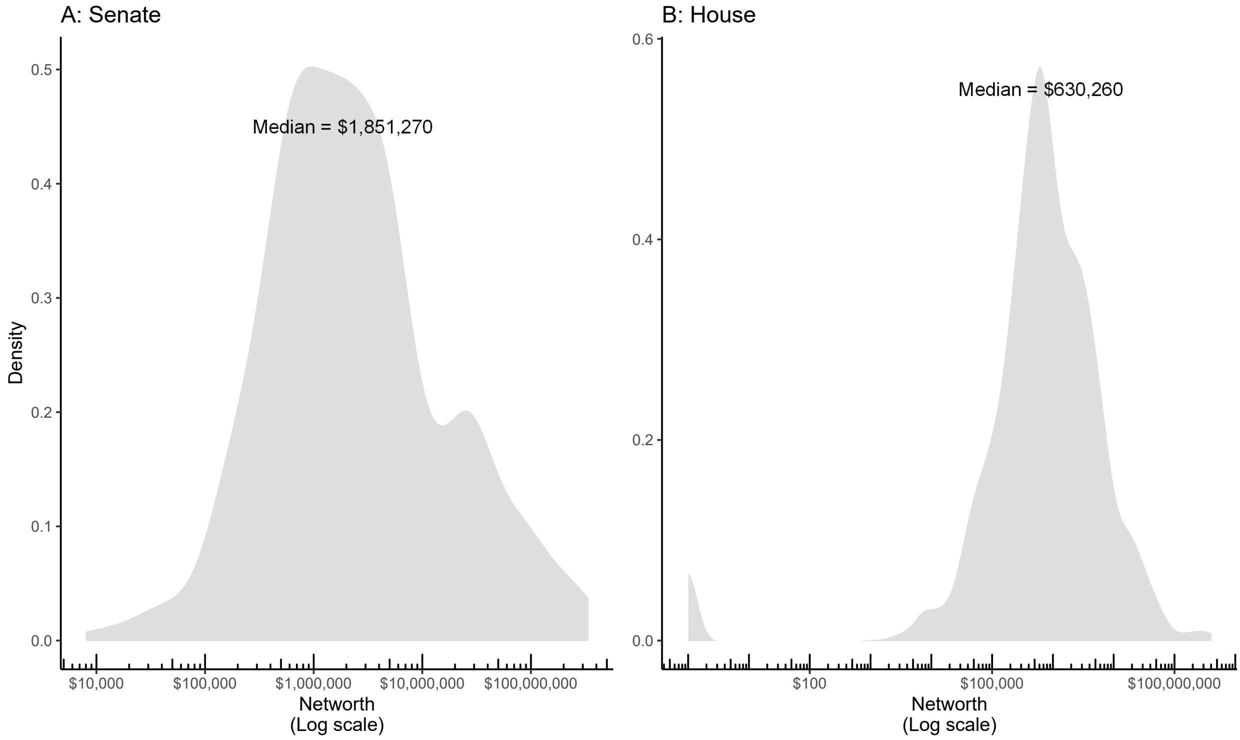

Figure F.11: Distributions of Net worth in the Two Chambers of Congress.

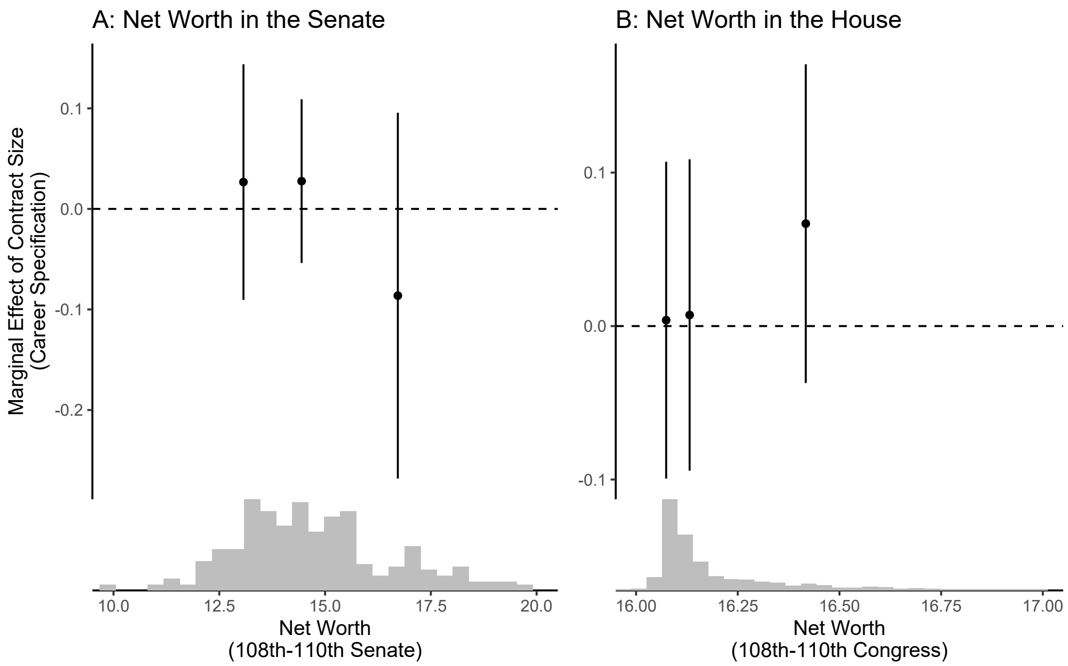

Figure F.12: Moderating Effect of Net Worth in Committee Specifications.

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-

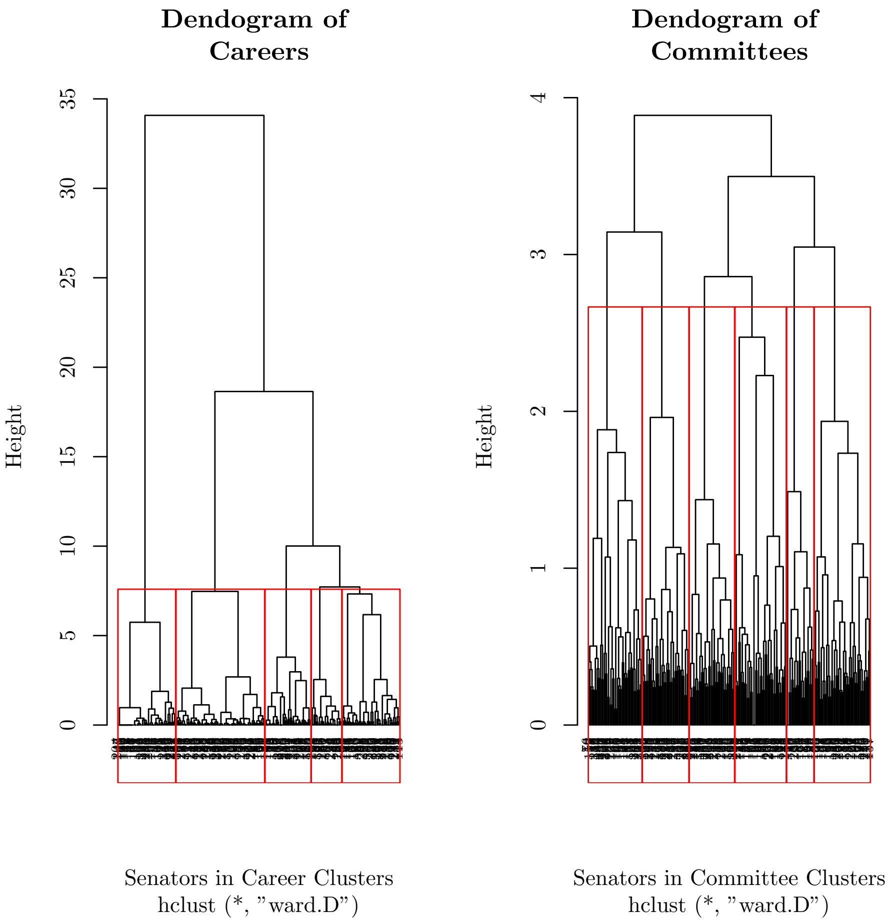

Figure G.13: Dendogram of different career clusters.

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.

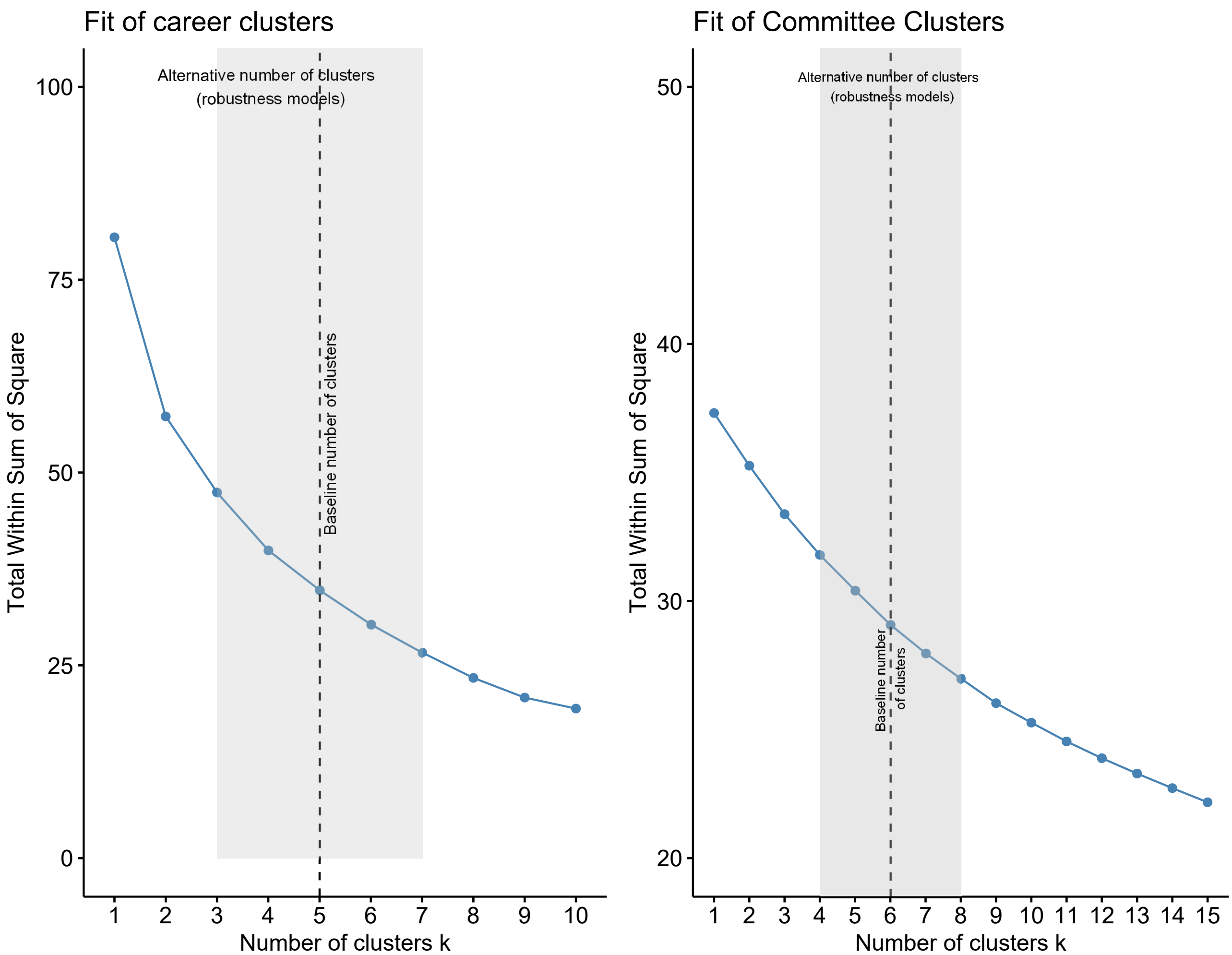

Figure G.14: Fit of different number of clusters.

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).

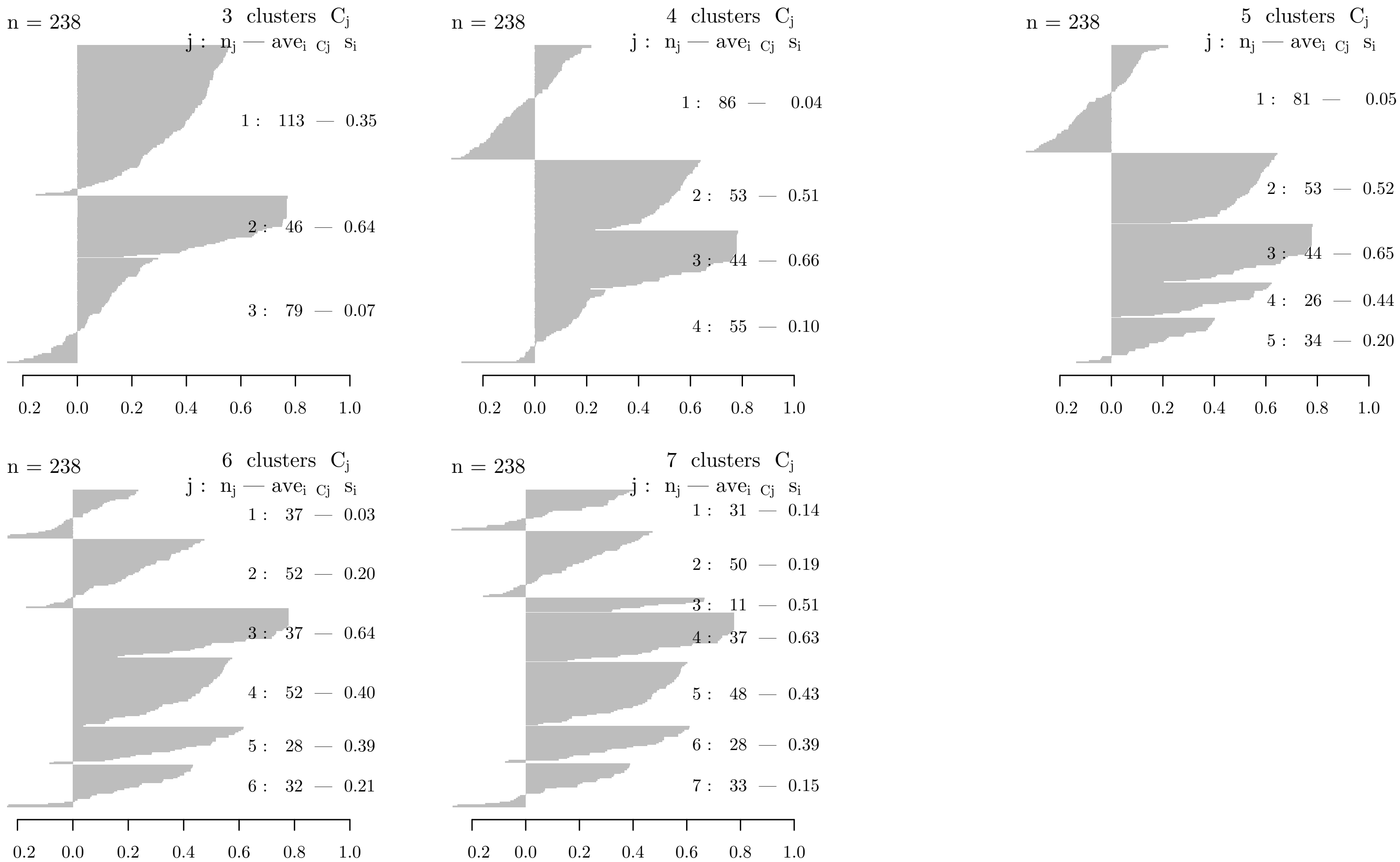

Figure G.15: Cohesion of career clusters.

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.

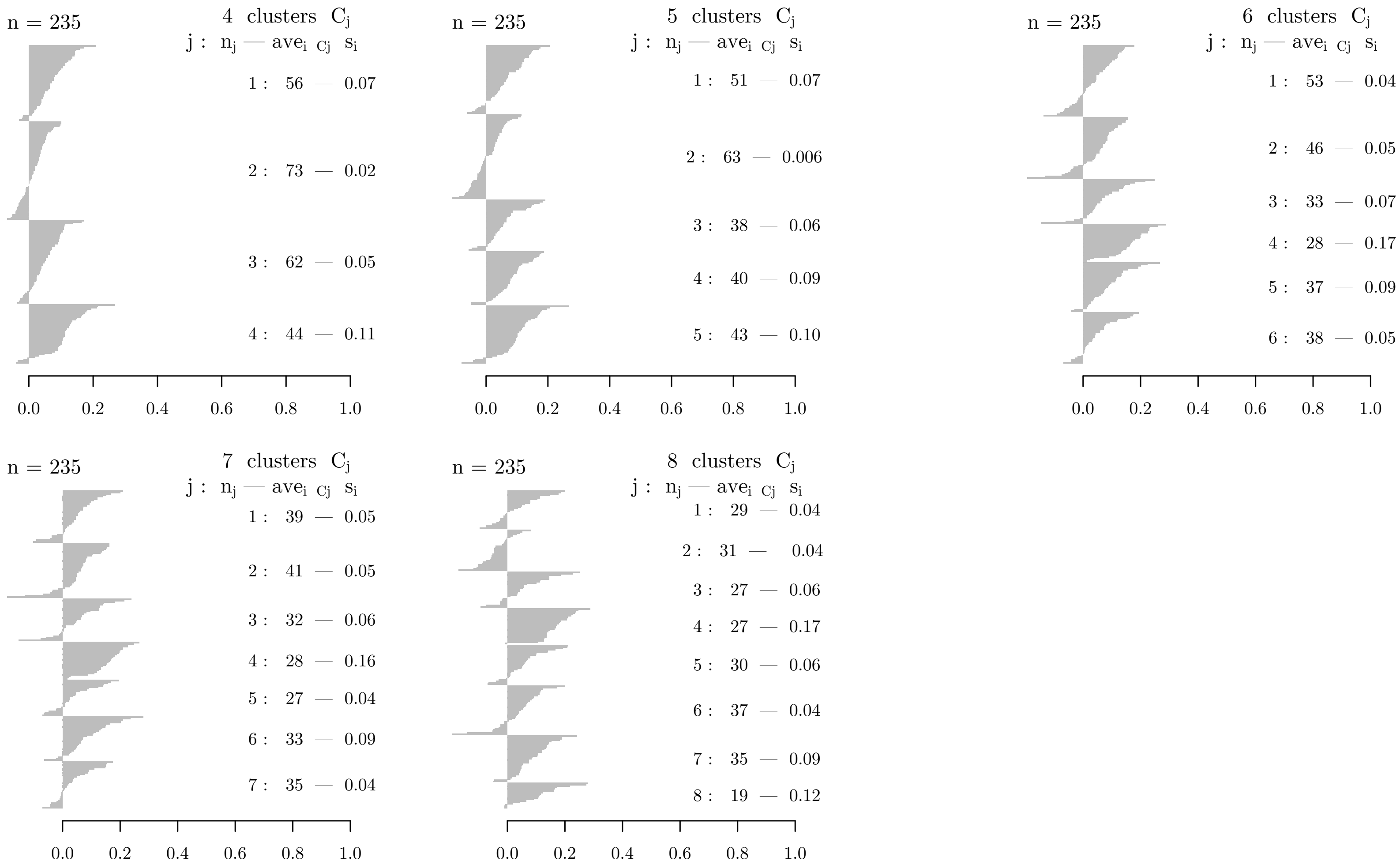

Figure G.16: Cohesion of committee clusters.

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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