
The Lure of the Private Sector¹

— CAREER PROSPECTS AFFECT SELECTION OUT OF THE SENATE

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Abstract

While it is often conjectured that private sector salaries lure politicians out of public service, no quantitative evidence exists on this proposition. I argue that legislators gauge their private sector career prospects by observing how successful their former colleagues are in lobbying jobs. I document that when career prospects improve, so does the probability that senators become lobbyists. An array of tests suggests that the effect arises, because senators react to opportunity costs associated with being in office: There is no effect immediately before a senator's pension scheme improves, nor among those who retire from working life after Congress or are elected to a safe seat. Senators, who have sponsored the least bills and are absent for floor votes, are affected the most. These results show that the electorate competes with private sector actors for the labor of politicians. This sheds light on the motivations of elected officials, and the dynamics of political selection.

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1 Introduction

Recent decades have seen a surge in the number of legislators, who voluntarily leave office for a job in the interest industry (Lazarus et al. 2016) – the so-called revolving door between business and politics. This has fueled widespread public attention as well as a growing research interest in the labor market for politicians (e.g. Diermeier et al. 2005; Eggers and Hainmueller 2009; Fisman et al. 2015; LaPira and Thomas 2017; Mattozzi and Merlo 2008; Palmer and Schneer 2016).

A common – but untested – assumption in both the theoretical and empirical research into the post-elective labor market for politicians is that the lucrateness of private sector employment motivates legislators to walk through the revolving door. If private sector salaries, indeed, can lure elected officials out of public service after they have taken office, it would establish private actors as rivals to the electorate in the demand for the labor of highly skilled politicians – even while those legislators are in the public’s service. Empirically establishing such a finding would shed important light onto the motivations of elected officials, and have implications for the quality of representation as such.

In this paper, I focus on the US Senate, and show that career prospects in the private sector, indeed, do affect senators’ decision to leave public service. Senators continuously keep track of the opportunity costs associated with holding office and leave, when it is most lucrative for them to do so.

My identification strategy builds on a novel idea about how legislators assess their career prospects. I argue that Members of Congress (MCs) gauge how well they would do, if they were to walk through the revolving door, by observing the success enjoyed by their former colleagues, who currently work in the private sector. In this way, they continuously take stock of the opportunity costs to holding office. To do this, however, a senator has to find former colleagues, who are comparable in some sense, and use their experiences to update expectations regarding her own career prospects. I argue that employment histories prior to serving in the Senate and committee assignments during their tenure provide good points of comparison. Thus, I

propose that senators will gauge their career prospects by observing senators-turned-lobbyists, who worked in similar jobs before the Senate, or served in a comparable mix of committees during their tenure. When they become more successful, the currently serving senator becomes more likely to leave office for a private sector job as well. Importantly, because the private sector performance of revolving door senators is shaped outside the reach of their former colleagues, this provides me with a proxy for career prospects, which is plausibly exogenous to the time-varying characteristics of currently serving senators.

To investigate this claim, I have collected data on the pre- and post-Senate career trajectories of senators serving between the 102nd and the 113th Congress. I use hierarchical cluster analysis to group senators together based on how similar a) pre-Senate career trajectories they have followed, and b) the portfolio of committees they have served in. For each of these career groups, I then predict the average size of the lobbying contracts that senators-turned-lobbyists work on. This allows me to capture how successful revolving door senators are in their post-elective careers, and mimics the comparison I expect MCs to make: they gauge their own career prospects by observing their former colleagues.

My results show that when career prospects improve by one standard deviation, the probability that a senator walks through the revolving door increases by approximately 3 percentage points. Compared to the size and turnover in the Senate this is a large effect. It corresponds to three additional senators walking through the revolving door, which is an increase of more than fifty percent over the baseline probability of selecting out in the average Congress, and compares to one-third of the effect of ending a Senate term. These results are robust to a wide range of different specifications. To investigate the mechanism, I show that when senators gain the most from staying in office, they are unaffected by outside career options: senators, who leave working life after Congress, or are elected to a safe seat are unaffected by private sector career prospects. Additionally, senators do not react to signals about private sector salaries, when they are about to experience an improvement in their pension scheme. Finally, I show that the senators, who have been the least active legislatively are affected the most. Both trails of evidence strongly suggest that legislators react to the opportunity costs associated with being

in office.

1.1 Contribution to the Literature

The selection of politicians is widely regarded as one of the most important determinants of good government (Besley 2006; Caselli and Morelli 2004; Fearon 1999; Przeworski et al. 1999). This has placed especially the monetary motivations of political candidates center stage (Ferraz and Finan 2009; Messner and Polborn 2004). But while the extant literature has focused heavily on how monetary incentives structure the selection *into* office (Besley 2006; Caselli and Morelli 2004; Dal Bó et al. 2013; Fearon 1999; Ferraz and Finan 2009; Messner and Polborn 2004; Przeworski et al. 1999) far less empirical attention has been devoted to how the same structures can motivate the selection *out of* office. An important exception is the strand of research, which focuses on the labor market of former politicians. This literature, however, has been predominantly theoretical (e.g. Mattozzi and Merlo 2008), or has investigated either the impact public service has on post-elective wages (Diermeier et al. 2005; Eggers and Hainmueller 2009; Palmer and Schneer 2016), or the effect of politicians' salaries on the career decisions of legislators (e.g. Diermeier et al. 2005; Fisman et al. 2015; Keane and Merlo 2010).

I add to this literature by providing the first quantitative evidence that private sector actors can use the promise of lucrative benefits to draw legislators away from public service. I build on the existing literature by showing that MCs do not only respond to public sources of financial remuneration, but also to private sector career prospects during their career in public service. This is not unimportant: if the voting public selected intrinsically motivated candidates, it would be entirely reasonable to expect that legislators would not be responsive to promises of lucrative private sector jobs (e.g. Besley 2006; Mattozzi and Merlo 2008).

Second, this paper is closely related to the literature on voluntary retirement from Congress. Using interview data Hibbing (1982a) presented evidence indicating that foregone private sector salaries were a factor in retirement decisions among his 24 interviewees. Relatedly, financial remuneration from congressional service has been found to be important determinants of volun-

tary retirement (Diermeier et al. 2005; Groseclose and Krehbiel 1994; Hall and Van Houweling 1995; Hibbing 1982b). Monetary remuneration can be seen as a special case of expected benefits from holding elected office, which more generally have been found to impact retirement decisions (Stone et al. 2010; Theriault 1998; Wolak 2007).

This literature, however, has treated all forms of retirement as equal, and has only focused on remuneration for public service – not the impact of private sector career prospects. Additionally, to the extent that the existing literature has operated with an identification strategy, it has focused on a single Congress. By including data from a longer period, my results not only show that the public has to compete with private sector actors for the labor of their elected officials, but also that this competition extends through time.

Third, 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 (Diermeier et al. 2005; Eggers and Hainmueller 2009; Palmer and Schneer 2016). Importantly, however, it is not only the legislators, who profit. Gaining political connections through the revolving door is highly profitable for companies as well (Acemoglu et al. 2016; Blanes i Vidal et al. 2012; Fisman et al. 2012; Luechinger and Moser 2014). These insight have fueled research investigating how the potential for lucrative post-elective employment may shape political decisions in the period leading up to the legislator leaving office for a private sector job (Adolph 2013, 2018; Egerod 2017; Shepherd and You 2018). I provide a missing piece in this literature by showing that legislators actually do respond to the potential for lucrative private sector employment, which has been an untested assumption until now.

2 How Legislators Assess Career Prospects

It is hardly a controversial statement that monetary rewards structure selection into elected office (Dal Bó et al. 2013; Ferraz and Finan 2009; Messner and Polborn 2004). Additionally, Mattozzi and Merlo (2008) have formalized the intuition behind how private sector salaries induces politicians to leave office. What remains unclear, however, is how currently serving

politicians obtain information about their private sector career prospects. When making career choices, the office holder does not observe the actual distribution of rewards that are available to her in the private sector. Instead she makes her career decisions based on expectations (Levin and Stephan 1991; Oyer 2008; Ryoo and Rosen 2004), which are updated continuously as new information about the actual distribution is revealed (Margolis and Okatenko 2008). Politicians glean this information 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. In this way, they use their colleagues' post-elective performance as a (noisy) signal about their own career prospects. Through this signal, legislators are continuously confronted with new information about the opportunity costs associated with holding political office. When the expected reward approaches their reservation price, the probability that they leave office for private sector employment increases.

Importantly, however, opportunity structures vary between senators and over the course of the tenure of each individual, which could impact the attractiveness of outside career options. If legislators indeed choose their career paths by discounting gains from staying in office against potential private sector earnings, as I suggest here, legislators should not be equally responsive to these recurrent signals of career prospects at all points in their careers. This implies two additional observable implications. First, there are points in a legislator's tenure, when they would give up certain perks by leaving office. In these periods, the private sector earnings, they forego by choosing to remain in office, are not alluring enough, simply because they would give up much more by leaving office (Hall and Van Houweling 1995). One illuminating example of this is the congressional pension scheme, which becomes significantly more lucrative at three specific points during a legislator's tenure. When an MC draws close to such a point, it stands to reason that outside options do not matter much for them. Second, some types of officeholders may value legislative achievement so much that they are unresponsive to changes in outside career prospects (Diermeier et al. 2005; Keane and Merlo 2010; Mattozzi and Merlo 2008). For this type of legislator, opportunity costs to holding office are simply always low, because the payoff from legislative work is high.

2.1 Finding Comparable Colleagues

US Senators – despite being a relatively homogeneous group – vary to a significant degree in their abilities, educations, and previous careers. As depicted in Figure 1, this is mirrored in an equally large variation in post-elective outcomes on the labor market for lobbyists. Some former senators work on lobbying contracts whose value average to almost nothing, while the typical contracts of the most successful senators are worth several hundred thousand.

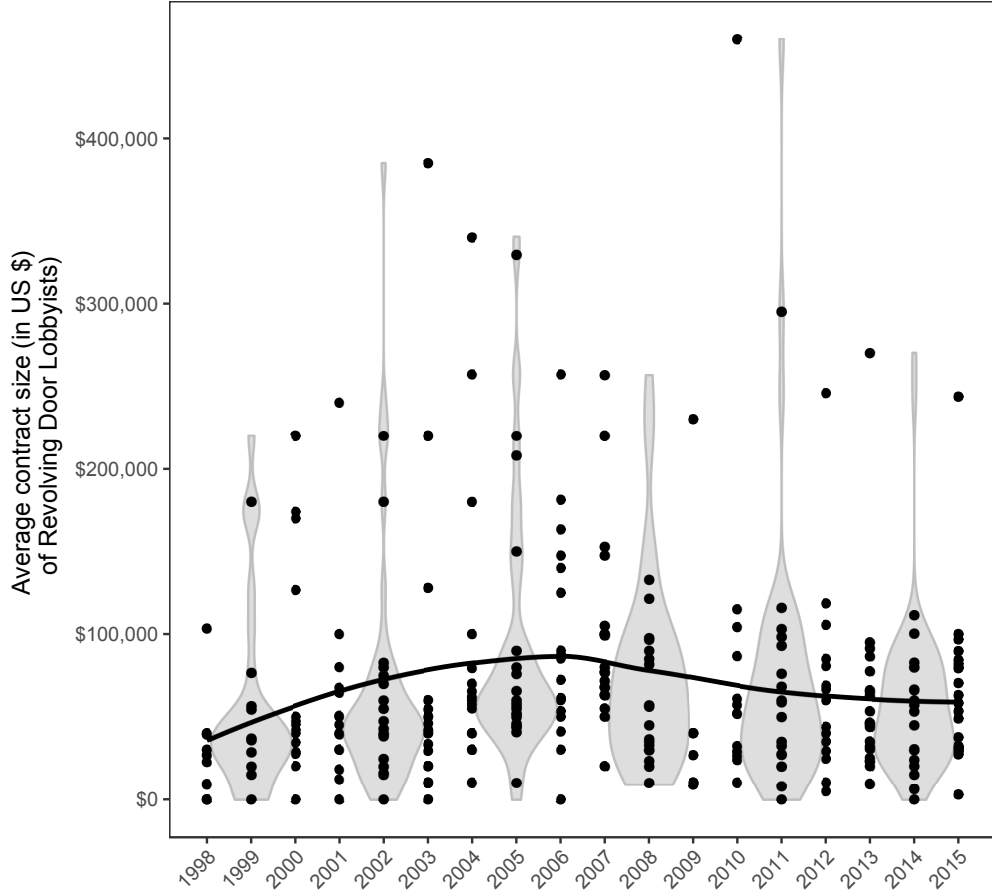


Figure 1: Average Contract Sizes for Senators-Turned-Lobbyists.

Note: For presentational purposes, seven former senators with average Contract Sizes above \$1M are excluded. Solid line is a loess smoother. Violin densities on subsets consisting of three years at the time (1998-2000, 2001-2003 etc.).

On other non-political labor markets, workers, who are about to enter the labor force, are able to predict their future earnings by comparing their individual characteristics to those of

workers, that are already active (Frick and Maihaus 2016). Therefore, I expect senators to look for former colleagues, who have similar individual characteristics, and use their post-Senate success to form expectations as to how well they would do, if they were to walk through the revolving door. This requires them to construct reference groups of senators, who share their individual characteristics. I propose two factors on which senators can construct such reference groups: a) the careers, they followed before entering the Senate, and b) committee assignments during their senatorial tenure.

Adolph (2013) argues that career socialization shapes the decisions public officials make while in office. Specifically concerning MCs, Carnes (2013) has recently shown that the careers legislators followed before being elected to Congress affect their behavior throughout their time in office. Not only do pre-Congress 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, generally speaking (Carnes 2013). This is especially true, when there is congruence between precongressional careers and the focus of the committees MCs are assigned to (Francis and Bramlett 2017).

Similarly, the portfolio of committees that senators have been assigned to during their political careers carry information about their political interests, preferences and post-elective labor market outcomes. A large literature has established the institutional prominence of congressional committees (Shepsle 1978; Shepsle and Weingast 1987). Committee membership affords senators the opportunity to have a political impact, service constituent preferences and attract pork (Berry and Fowler 2016; Endersby and McCurdy 1996; Lazarus 2010; Schiller 1995). Personal interests that predate the political career also play a role in which committees senators seek to be assigned to (Bullock III 1985; Fenno 1973; Schiller 1995).

Importantly, however, because of the prominence of committees in the legislative process, special interests are generally highly interested in targeting them in their influence-seeking (Bertrand et al. 2014; Fourniaies and Hall 2018; Hall and Deardorff 2006). This makes connections to committees an extremely valuable asset for revolving door lobbyists (Cain and Drutman 2014). Consequently, the revenues generated by revolvers (Blanes i Vidal et al. 2012; McCrain

Forthcoming) as well as their personal earnings (Hibbing 1982a) are strongly impacted by the committee connections they have made during their career in public service. Because of this, committee assignment is not only personally important for senators, who wish to see their agenda enacted, it is also a valuable asset for them in their post-elective careers.

In a nutshell, both pre-Senate careers and committee assignment during tenure in Senate carry broad information about politician type. Senators with comparable pre-Senate careers and portfolios of committee memberships are likely to behave similarly during their tenure and experience equally lucrative labor market outcomes afterwards. This makes it natural for senators to use these factors to construct their reference groups.

A senator can look to former colleagues, who are in her reference group and use their success to form expectations about how well she might do herself. When this makes her expect larger rewards from walking through the revolving door, the probability that she will take private sector employment increases.

3 Empirical Strategy

The empirical strategy and the expected results are illustrated in Table 1. First, 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 Senator 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. 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

Table 1: The empirical strategy & expected results

	Reference Group	Expected Post-Elective Success	Career Decision
Former lawyers	$\left. \begin{array}{l} \text{Former Senator}_{1l} \\ \text{Former Senator}_{2l} \\ \vdots \\ \text{Former Senator}_L \end{array} \right\}$	\$150,000	Currently serving senator ₁ selects out
Former military	$\left. \begin{array}{l} \text{Former Senator}_{1m} \\ \text{Former Senator}_{2m} \\ \vdots \\ \text{Former Senator}_M \end{array} \right\}$	\$80,000	Currently serving senator ₂ does not select out
...
Former Career _K	$\left. \begin{array}{l} \text{Former Senator}_{1k} \\ \text{Former Senator}_{2k} \\ \vdots \\ \text{Former Senator}_K \end{array} \right\}$	$E(\text{success}_s)$	Currently serving senator _s selects out with $P(\text{select out} \mid E(\text{success}_s))$

Note: Senators are matched into reference groups using cluster analysis, illustrated here using pre-Senate careers. The predicted size of lobbying contracts that former senators in reference group K work on, is used to gauge the career prospects of currently serving senator s. E.g. currently serving senator₁ selects out due to an expectation of highly valuable Contract Sizes in her reference group of former lawyers, while senator₂ stays in the Senate, because of a relatively low expected payoff in her reference group of former military personnel. More generally, the probability that Senator s selects out increases in her expected post-elective success as a lobbyist – $E(\text{success}_s)$.

in the Senate from 103rd to the 113th Congress collected from Stewart III and Woon (2017). I exclude special committees and leadership positions.

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 a later section, I test the robustness of these baseline choices. The Ward clustering places senators into groups that are most internally coherent, while simultaneously maximizing the differences between groups. This fits the intuition in the theory well. In appendix D, I show diagnostics on the cluster analyses. In appendix A, I describe in more detail the data on careers and committee assignments.

Measuring Post-Senate Career Success. To get an estimate of how successful former senators are in their post-tenure employment, I rely on the average size (in US dollars) of the lobbying contracts they work on. The logic is that the success experienced by senators-turned-lobbyists will be mirrored by value of the lobbying contracts, they work on. Insofar as the most lucrative lobbying 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.

Under the Lobbying Disclosure Act (LDA), lobbying companies are obliged to file separate reports for each of their clients, detailing – among other things – the names of the lobbyists who work on the contract as well as its dollar value. This is made easily available by the Center for Responsive Politics (CRP). I match the names of all former senators who have served in the period 1992-2015 to the names reported on the lobbying contracts. Data under the LDA is available dating back to 1998, and I am able to track the impact of Contract Size on the probability of walking through the revolving door in the period 1998-2015. Since my data on pre-Senate career trajectories includes senators serving since 1992 and on committee assignments since the 103rd Congress, this provides me with data on senators-turned-lobbyists from three Congresses on which to base my initial estimates of Contract Size in 1998. All senators, who have registered as contract lobbyists since then, are included in the measure from

1998 and onwards.

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 a set of year dummies. This prediction is my final explanatory variable, which I will call *Contract Size* in the remainder of the text. This mimics the comparison I expect senators to make: by looking at the pre-Senate careers or portfolio of committee assignments of senators-turned-lobbyists, they will attempt to predict Contract Size within each year.

In Figure 2, I show that both measures indeed do convey important information about private sector career prospects. First, 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. Second, 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 bootstrap this procedure with 1,000 resamples.

This validation exercise illustrates two points: first, that 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. Second, it also shows that it tracks it better than the prediction from other reference groups would have done, and – by extension – that the prediction from the reference groups performs better than a simple average of Contract Size would. Thus, from her reference group, a senator can extract unique information about her private sector career prospects.

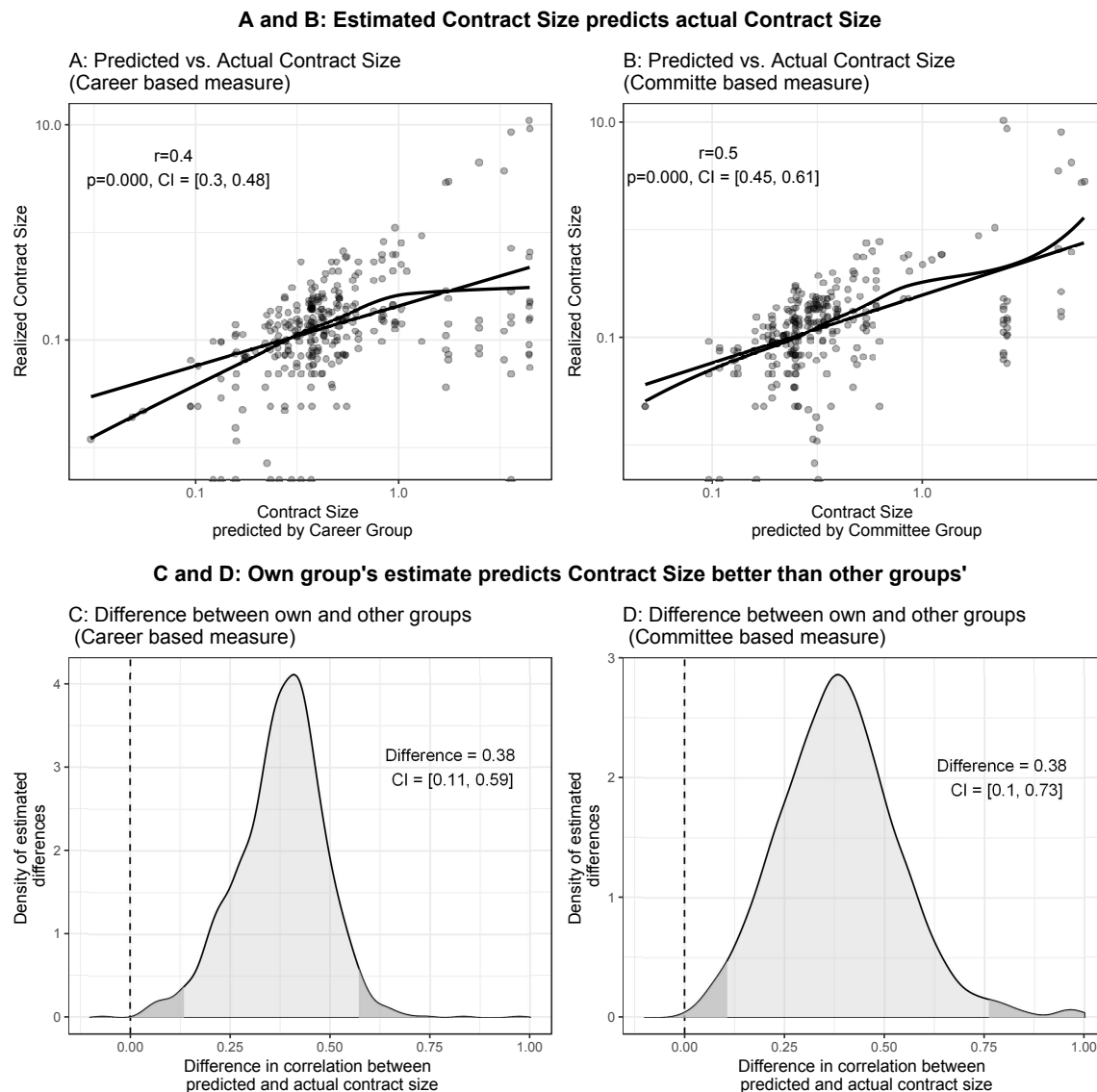


Figure 2: Validating the measure.

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 sharp null of them predicting actual Contract Sizes equally well. Areas below the 2.5th and above the 97.5th percentiles are dark shaded.

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 leaves 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. Previous research has found that former legislators employed with these kinds of special interests work with lobbying and interest representation (Lazarus and McKay 2012) – thus, these are the types of employment that should react to my measure of private sector career prospects.

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. For other kinds of revolving door jobs, the CRP records are lacking, however, and I supplement mainly with searches in the Securities and Exchange Committee’s EDGAR database and the Relationship Science (RS) database. RS is a private company that tracks the careers of important US executives, including MCs. Additionally, I use news stories and press releases.

In total, 122 senators serving in the 105th to the 113th Congress – of whom 44 leave for a revolving door job in the period of investigation – are included in my models. The full specifications rely on 769 and 722 Senator-Congress observations for the models using pre-Senate careers and in-Senate committee assignment, respectively.

3.2 Additional Covariates

I include a number of additional covariates. I proxy political preference by including senator roll call ideal points estimated using the Martin and Quinn (2002) Dynamic Bayesian Item Response Theory (D-IRT) model as its implemented in Armstrong et al. (2014). To measure, whether a senator is in the party’s mainstream or on the fringes, I use the absolute difference between the senator’s own D-IRT score and her party’s median score. 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. Because general vocational behavior has changed markedly throughout my period of investigation, senators with longer tenures could potentially have systematically different professional backgrounds. I use the logged number of years the senator has served in the Senate at time t as a measure of

seniority. Conditional on the time fixed effects, this measure is very highly correlated with the senator’s age. Thus, it should act as a proxy for both, and I do not include age as a control. My results are robust to doing so, but in many models one of the two variables would drop out due to perfect collinearity. Finally, I also include a dummy for whether the senator is up for reelection during the current Congress. This also captures whether the current Congress is the last of the senator’s six year term.

I describe data sources for additional variables, as they are introduced. 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 individual characteristics of currently serving senators. Still, two identification problems remain. First, specific types of senators are likely to both select into more successful vocations earlier in their careers, and a mix of committee assignments that improve their post-elective career prospects. Second, shocks to the political system could influence both strategic retirement from office and the general size of lobbying contracts. Especially salient are reforms of the regulatory regime facing lobbyists. For instance, the Honest Leadership and Open Government Act of 2007 dramatically changed the incentive structures facing both lobbyists and legislators, who consider walking through the revolving door. To deal with both threats to identification, I include twoway fixed effects.

In Figure 3, 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).

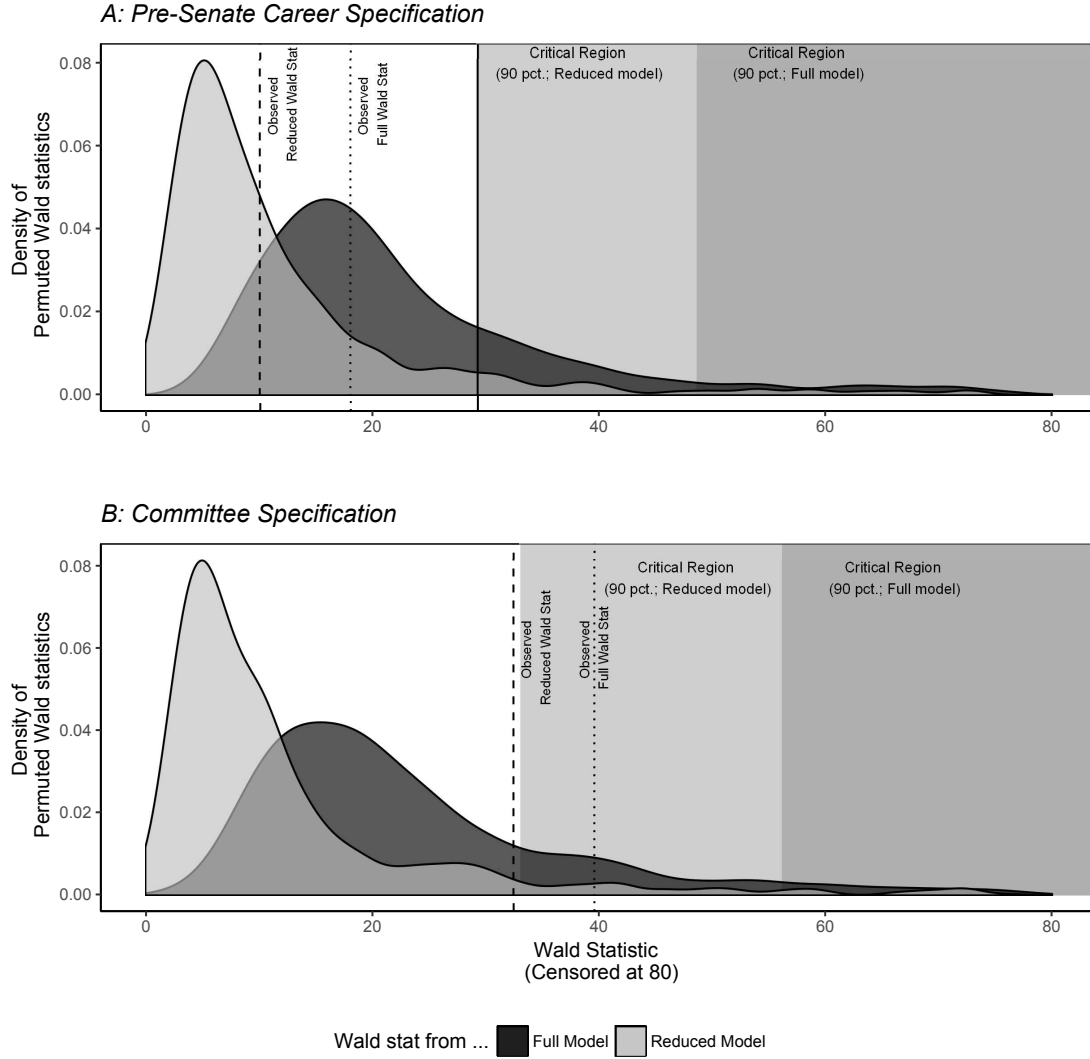


Figure 3: Permutation Test of Covariate Balance.

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 critical regions are the light (reduced model) and dark (full model) areas above the 90th percentile of the permuted distributions. P-values for pre-Senate careers specifications: 0.4 (reduced model); 0.55 (full model). P-values for committee assignment specifications: .1 (reduced model); .18 (full model).

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 is far from being statistically significant, when career prospects are estimated using pre-Senate career trajectories to compare senators on ($p = .4$). It is closer to collective significance (p is just over $.1$), for the models using committee assignments to compare senators.

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. The p -values are $.55$ and $.18$, respectively, for the specifications using pre-Senate careers and in-Senate committee assignments to measure private sector career prospects. Thus, I cannot reject that Contract Size is unrelated to the individual characteristics included here.

4 Main Results

Table 2 presents the results from a number of linear probability models. The first three columns uses pre-Senate careers to measure Contract Size. Column one presents the results without including controls. The coefficient is sizable – I estimate that increasing Contract Size by \$91,000 (which corresponds approximately to a standard deviation) raises the probability of the average senator walking through the revolving door by 3.4 percentage points. This estimate is more than three times the size of its standard error, and the probability that I have found signal in noise is about $.001$. The sharp null can, thus, be rejected at a high level of confidence. In column two, I add controls to the model. The coefficient on Contract Size and its standard error are relatively stable.

While the inclusion of Congress dummies soaks up all shocks to the political system, which have homogeneous effects across senators, a major remaining threat to identification is if these shocks have heterogeneous effects. It would be plausible that reforms as the Honest Leadership

and Open Government Act could have different effects across senators. As an attempt to deal with this, I interact all controls with the time dummies in column three. This allows the effect of Congress-specific shocks to differ depending on the individual characteristics captured by the covariates. As a matter of computational efficiency, I use the Gaure (2013) method to estimate the large number of interactions between fixed effects and the covariates and bootstrap point estimates and standard errors. Once again, the results do not change dramatically.

In the next three columns, I present the results from similar specifications, but use in-Senate committee assignment to estimate the private sector career prospects of currently serving senators. In the fourth column, I run the model without controls. The estimated effect of private sector career prospects in this specification is close to – but smaller than – the one using pre-Senate careers to compare senators. In column five, I include my full range of controls. This does not change the coefficient on Contract Size. Finally, in column six, I interact the controls with the Congress dummies. This does not change the substantive conclusions. The bootstrapped standard error is substantially larger, however.

To gauge effect sizes, it is illuminating to compare this estimate to other coefficients in the model. Depending on the exact model specification, I estimate that the probability of walking through the revolving door is 7 or 8.2 percentage points higher in during election years, i.e. when the senator’s term is ending. Thus, in both of the full specifications, the effect of changing Contract Size by one standard deviation corresponds to more than one-third of the impact of finishing a term. Additionally, looking across Congresses, the average probability of walking through the revolving door is approximately 4.7 – the effect of Contract Size corresponds more than half of this baseline probability.

The identifying assumption is parallel trends absent changes in career prospects. While this is fundamentally untestable, violations will often cause differences in pre-treatment trends. In appendix C1, I test this. While the results based on pre-Senate careers show no trends prior to changes in Contract Size, the results based on committee assignment do. To some extent, this is to be expected. It is easier for currently serving senators to make sure they get seats on the committees that will maximize their post-tenure earnings. This would cause the

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

	<i>DV: SIG Career</i>					
	DiD	Controls	Shock X covariates	DiD	Controls	Shock X covariates
	(1)	(2)	(3)	(4)	(5)	(6)
Contract Size (Career)	0.034 (0.011)	0.032 (0.010)	0.031 (0.014)			
Contract Size (Committee)				0.021 (0.008)	0.021 (0.006)	0.026 (0.015)
Ideal Points		-0.013 (0.005)			-0.014 (0.006)	
Seniority, Logged		-0.012 (0.023)			0.001 (0.023)	
Difference Party		0.025 (0.016)			0.026 (0.015)	
Difference Party ²		-0.004 (0.001)			-0.004 (0.001)	
Election Year		0.070 (0.014)			0.082 (0.012)	
State Liberalism		-0.090 (0.025)			-0.089 (0.029)	
Twoway FE?	Yes	Yes	Yes	Yes	Yes	Yes
Congress X Controls?	No	No	Yes	No	No	Yes
Observations	787	769	769	737	722	722

Note: Dependent variable is SIG Career. Driscoll-Kraay (temporal and cross-sectional) autocorrelation and heteroskedasticity robust standard errors in parentheses in columns 1, 2, 4 and 5. Point estimates in those models are unstandardized OLS coefficients. Bootstrapped estimates (median) and standard errors from 500 resamples at the senator-level in columns 3 and 6. Measures of Contract Size are normalized by their standard deviations.

pre-treatment trends observed here. In a sense, this is both the strength and the weakness of this measure – it sacrifices credible identification for better proxying the manner in which senators actually take stock of their post-elective career prospects. Given that the measure based on pre-Senate careers, which is likely to be exogenous, yields results that are similar to the measure that best proxies behavior, the estimated effect of private sector career prospects should be credible. Additionally, I have tried using the career-based measure as an instrument for the committee-based measure. Since the correlation between them is relatively low, the instrument is somewhat weak, but the results maintain.

One potentially salient concern is that the results could be driven by measurement error induced by the Honest Leadership and Open Government Act in 2007. The reform introduced a two-year cooling off period before senators could lobby after leaving office. This has lead many to simply avoid registration after walking through the revolving door (LaPira 2014), and to some leaving before the new legislation took effect. In appendix C2 I show that congress-by-congress estimates are relatively stable in the periods before and after 2007. This indicates that any bias caused by this data problem is soaked up by the inclusion of time fixed effects.

4.1 Robustness and Further Placebo Tests

In Figure 4, 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.

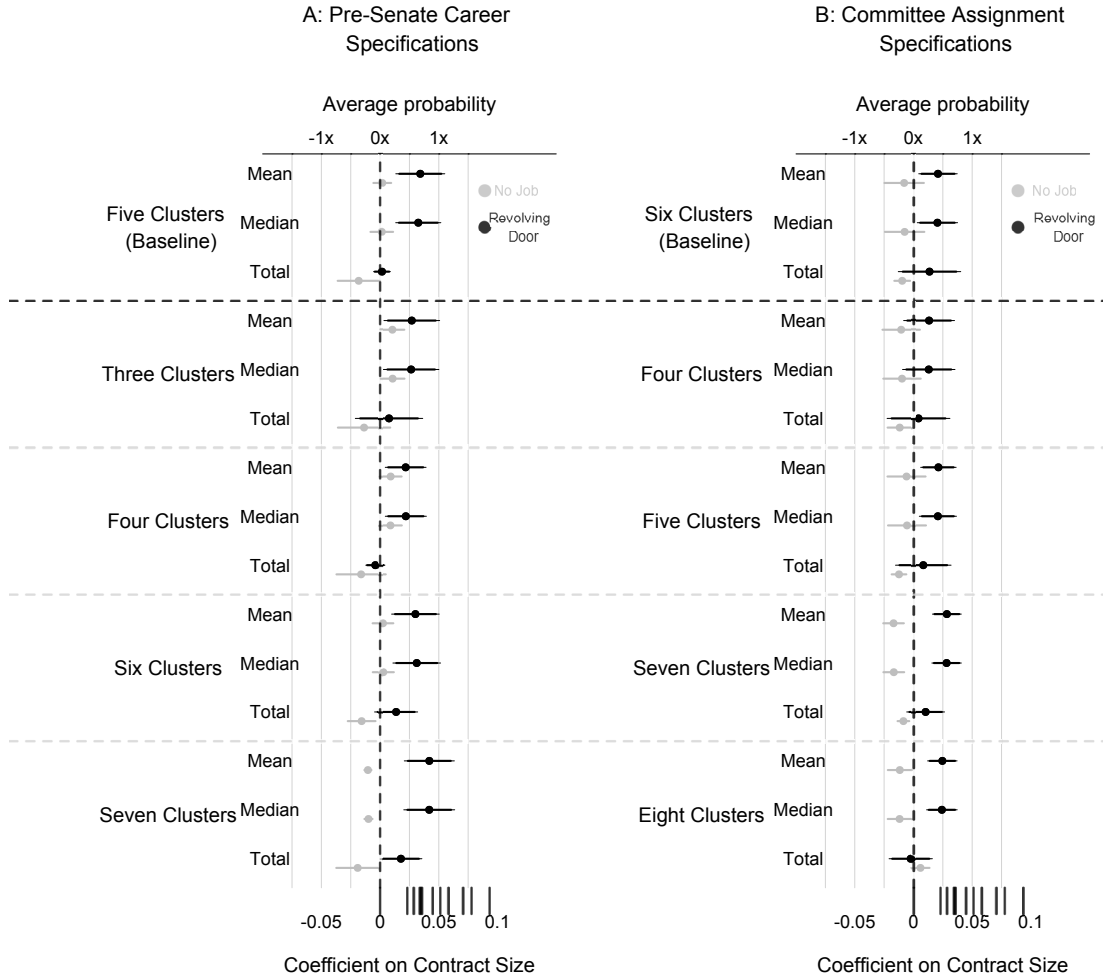


Figure 4: Robustness to Specification Choice and Sanity Checks.

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.

As a third way of measuring career prospects, I use the predicted total Contract Size. The results are not 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 dozen 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.

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.

In appendix C3, I conduct a series of additional robustness checks aimed at dealing with various sources of uncertainty and measurement error. First, and most importantly, because dollar sizes of lobbying contracts vary idiosyncratically, and there is error associated with the

cluster analysis, measuring career prospects in this manner inherently induces measurement error, which can pollute my estimated coefficients and standard errors. The relatively low number of senators, who become lobbyists in each group (I show the distributions in appendix A4), exacerbate this concern. To test the sensitivity of my results to this, I implement the Method of Composition (Tanner 1996; Treier and Jackman 2008) technique outlined in Caughey and Warshaw (2017). The results maintain. Additionally, I conduct a number of robustness checks related to assumptions underlying my uncertainty estimates. 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 are robust to all of these modeling choices.

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 earnings. 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.

I test this mechanism in two ways. First, I investigate the timing of retirements – senators should select out, at the points in their tenure, when they give up relatively little by doing so. Second, I look at different types of senators – the ones, who value legislative work the most, should be the least responsive to outside career options.

5.1 Senators Are Lured When Costs to Holding Office Are High

First, I run subset analyses based on a number of variables that capture opportunity costs associated with holding office at any given point during a senators tenure. To do so as efficiently as possible, I use the Hainmueller et al. (2016) binning estimator and estimate effects within tertiles of these variables. The results are presented in Figure 5. 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.

In the first specification, 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 each senator's pension scheme improves, and estimate local effects within each tertile. This separates a) those who will never see another improvement, from those who will see one within b) three c) and ten years, respectively. 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 there are few years until the senator's pension scheme becomes more lucrative. The point estimates are very small (0.01 and -0.003) and statistically insignificant at conventional levels. Among senators, who will never see another improvement, the effect is considerably larger than the average effect, and the p-value for the difference in effects is .05 and .04 for the specification in Panels A and B, respectively. For those with ten years until their pension scheme improvement, the impact of Contract Size is about the average effect. These results hold even after controlling for age and time spent in the Senate.

As a check on these results, the second specification splits the data based on the time since the last pension scheme improvement. For the specification using pre-Senate careers to form reference groups there is no effect of Contract Size for senators, who have just experienced an increase in the lucrativeness of their pension, while the impact of Contract Size is more than twice as large as the average effect for senators, who have served eleven years since their last improvement. The effect disappears again for senators, who have served for a very long time. The evidence using committee assignments to construct reference groups is not as strong in this case.

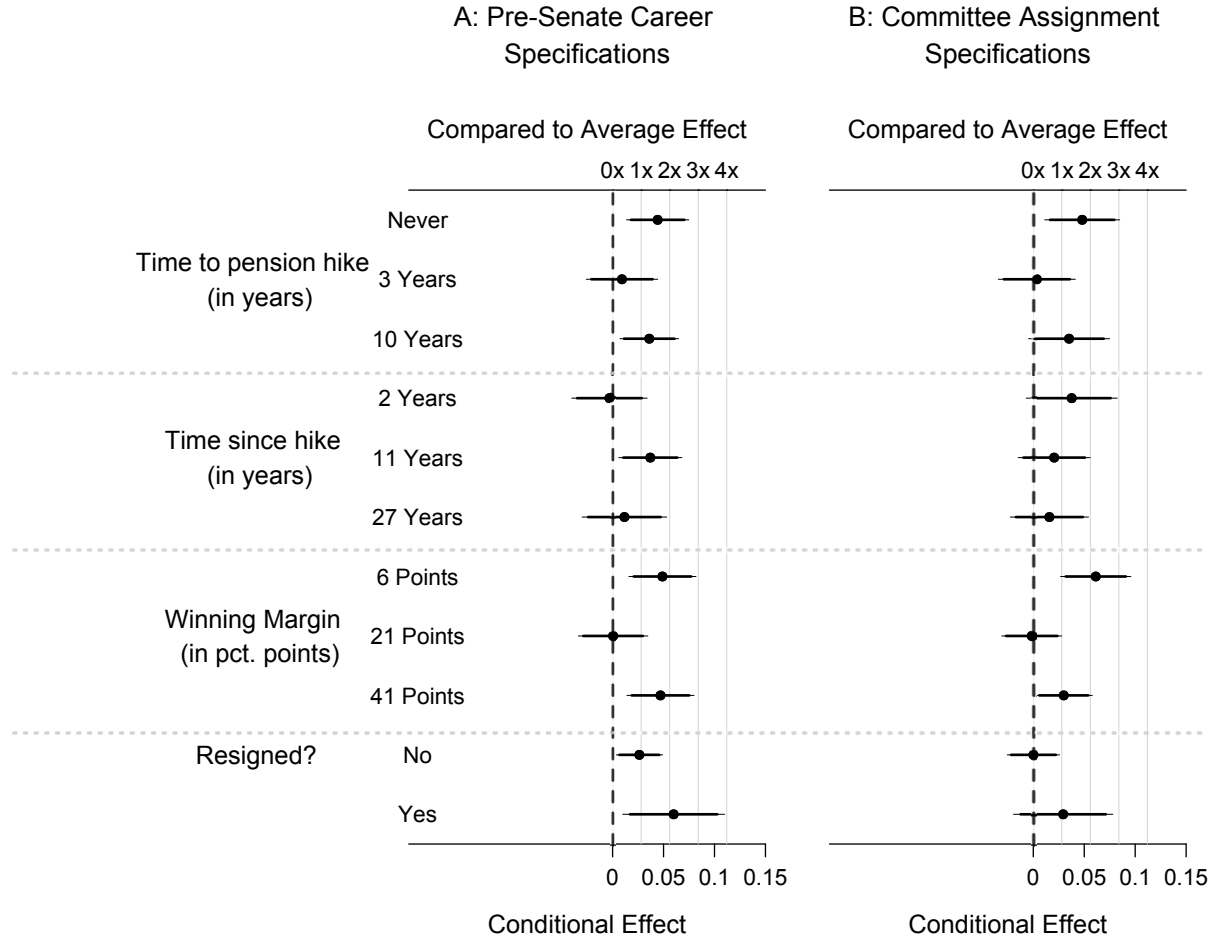


Figure 5: Effects for senators with differing opportunity costs.

Note: Panels A and B show, respectively, results from calculating Contract Size based on pre-Senate Careers and in-Senate committee assignment. In the first three specifications in each panel, the local marginal effects are estimated within tertiles of the variables using the Hainmueller et al. (2016) binning estimator. In the final one, effects are estimated at each level of the binary variables using linear interaction models. Two-way fixed effects are included in all models. Robust confidence intervals are 95 pct. (thin lines) and 90 pct. (thick lines), respectively. Top axes: average effects (columns 3 and 6 in Table 2). Bottom axes: effect of Contract Size.

Uncertainty about a senator’s political future is likely to exacerbate opportunity costs to holding office. In the fourth specification, I use the margin with which the senator won her seat in the previous election. This proxies how certain the senator can be that she will hold on to her seat in the next 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. The results show that for senators, who were elected by a margin below six percentage points, the impact of Contract Size is twice the average effect. These results hold no matter, which reference groups is used to calculate Contract Size, and the p-values for the differences in effects between the first and second bin is .03 and .006 in the two specifications.

Since senators, who choose to resign, 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. 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.

5.2 The Revolving Door Attracts The Least Productive

As argued previously, senators, who put great value on producing legislation, should be least affected by the lure of the private sector (Keane and Merlo 2010). I examine this in Figure 6. Panel A and B show, respectively, how effects vary depending on how many bills the senator has sponsored on average (data collected from GovTrack (2017)), and how many floor votes she has been absent for during her average Congress (data collected from Poole and Rosenthal (2015)). Again, I use the Hainmueller et al. (2016) binning estimator.

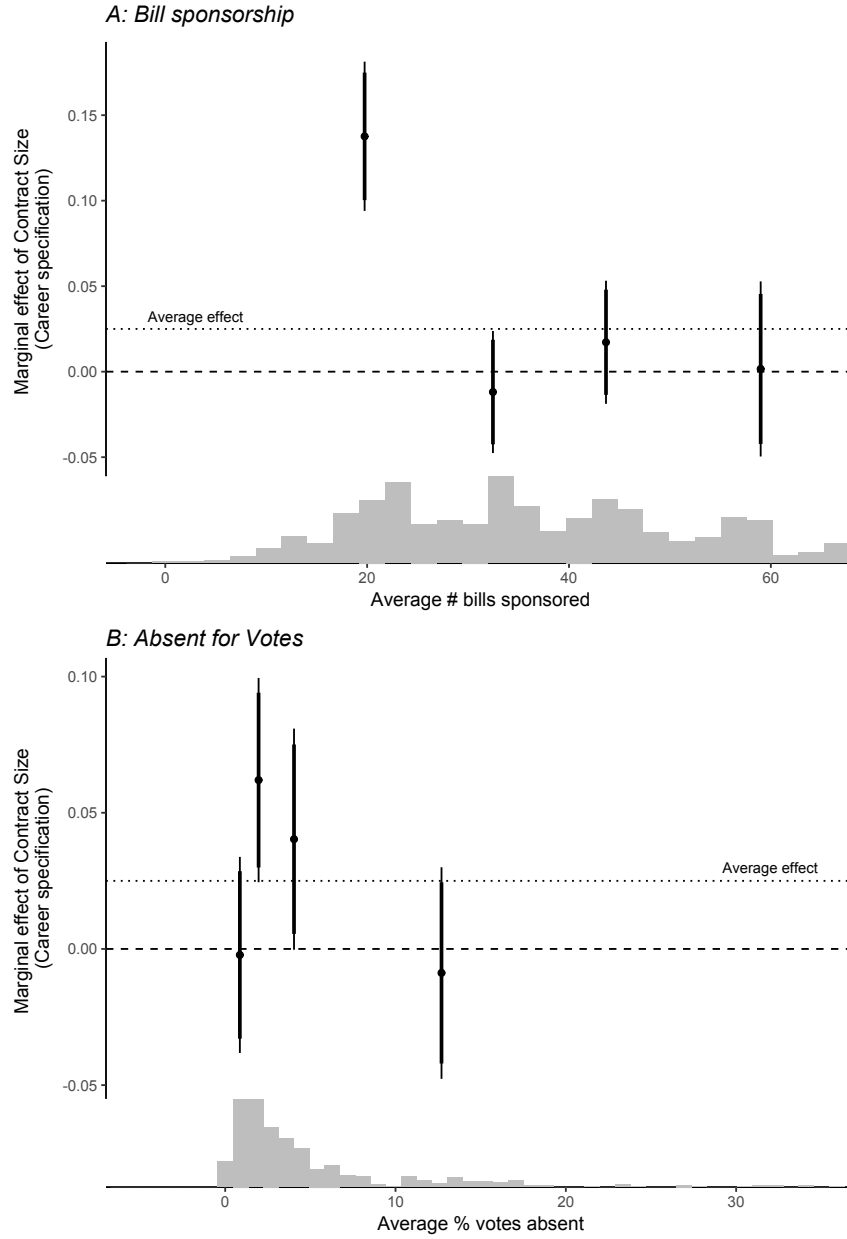


Figure 6: Heterogeneous Effects of Career Prospects.

Note: Local marginal effects are estimated using the Hainmueller et al. (2016) binning estimator. Two-way fixed effects are included in all models. Robust confidence intervals are 95 pct. (thin lines) and 90 pct. (thick lines), respectively. Dotted line shows average effect, dashed line shows sharp null. Histograms show marginal distributions of moderating variables. Results are from specifications using pre-Senate careers – results using committee assignments are similar and can be found in appendix C4.

Looking first at bill sponsorship, it is clear that the senators, who on average have sponsored less than 20 bills per Congress, are affected the most. The average probability of them leaving office for a revolving door job increases by almost 14 percentage points, when career prospects improve by one standard deviation. This is a very strong effect – more than four times the average effect, almost three times the average probability and twice as large as the effect of ending a term in office.

Next, I turn to heterogeneities depending on how many votes a legislator has typically been absent for. Once again, the most diligent senators, who are almost never absent, are close to unaffected by private sector career prospects. The effect spikes for those, who on average have been absent for between two and six percent of votes during their tenure. The effect drops off for senators, who have been absent for more than ten percent of the votes on average. It is difficult what to make of this drop-off, and it might be driven by the fact that few senators have been absent for more than an average of ten percent votes. Otherwise, it could be that senators, who are absent for many floor votes, are of such low quality that they have a difficult time finding a job in the private sector.

6 Conclusion

Motivating good candidates to run for office is a precondition for high quality government, but so is persuading them to stay. My results strongly indicate that US senators are attracted by the potential for lucrative private sector employment and leave office to pursue them. The results suggest that senators update their estimates of the opportunity costs to holding office continuously and react with little delay.

To measure private sector career prospects, I grouped senators, who served between 102nd and the 113th Congress, together based on their pre-Senate careers and the committees they served in. I then computed the expected size of the lobbying contracts for each of these career groups. A desirable by-product of this measurement strategy was that it provided a proxy for career prospects, which was unrelated to the individual characteristics of the senators in my

sample.

The results indicate that when the expected Contract Size increased by \$90,000 (one standard deviation), the probability that the average senator left Congress for a lobbying job rose by between 2 and 3 percentage points, depending on the specification. This translates into between two and three additional senators taking private sector employment, which would be an increase of 50 percent over the baseline probability, and comparable to one-third of the effect of ending a term in Senate. Importantly, senators, who left the work force after leaving Congress, were unaffected.

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 senators experience improved pension schemes. Instead the effect was localized among senators, who would never see another improvement, or who had to run for reelection before one. Similarly, senators, who only narrowly won their seat in the previous election were affected at an above-average rate. Additionally, I found that the senators, who sponsored the fewest bills and often had been absent for floor votes, were affected at rates far exceeding the average effect. These senators put the least value on legislative work and, thus, had less to gain from staying in office.

These results have a number of important implications. First, they show that monetary gains do not only structure the selection into public service – as documented in the extant literature. Their effect persists even after candidates have entered elective office, shaping the timing of resignation. While legislators are intrinsically motivated to serve their constituents and deliver good policies, they respond to material incentives as well. Senators – despite generally being comparatively wealthy – 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 income, they relinquish by holding elected office, becomes large enough, the average senator will leave public service to take private sector employment.

Second, while the literature on selection into elected office has predominantly focused on how incentives can be designed to make good types of politicians choose public service. My

results suggests that equal attention should be paid to making sure they stay in office, after they are elected. The results show that the class of senators, who are the least productive, are most strongly affected by the lure of the private sector. This should remind us that we need to worry more about what kind of legislator it is that chooses to leave public service, than the number of MCs, who do so – if the revolving door weeds out low quality legislators, it might not always be desirable to slow its swing. Before taking this point too far, however, it should be noted that a legislator can be low quality in many other dimensions than productivity (e.g. how they respond to the demands of voters, or how corrupt they are etc.), and that the revolving door may have other negative impacts that could vastly outweigh its potentially positive sides (Adolph 2013; Blanes i Vidal et al. 2012; McCrain Forthcoming).

Finally, the results inform the theoretical literature on how voters should find the optimal candidate (e.g. Besley (2006) and Fearon (1999)). Specifically, when it is a possibility that a particular type of legislator (e.g. the least productive ones) reacts more strongly to career prospects in the private sector than others, the optimal equilibrium strategy for the electorate becomes unclear. Exploring this formally is beyond the scope of this paper, but could be a fruitful venue for future research.

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Online appendix for: The Lure of the Private Sector

September 12, 2018

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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.

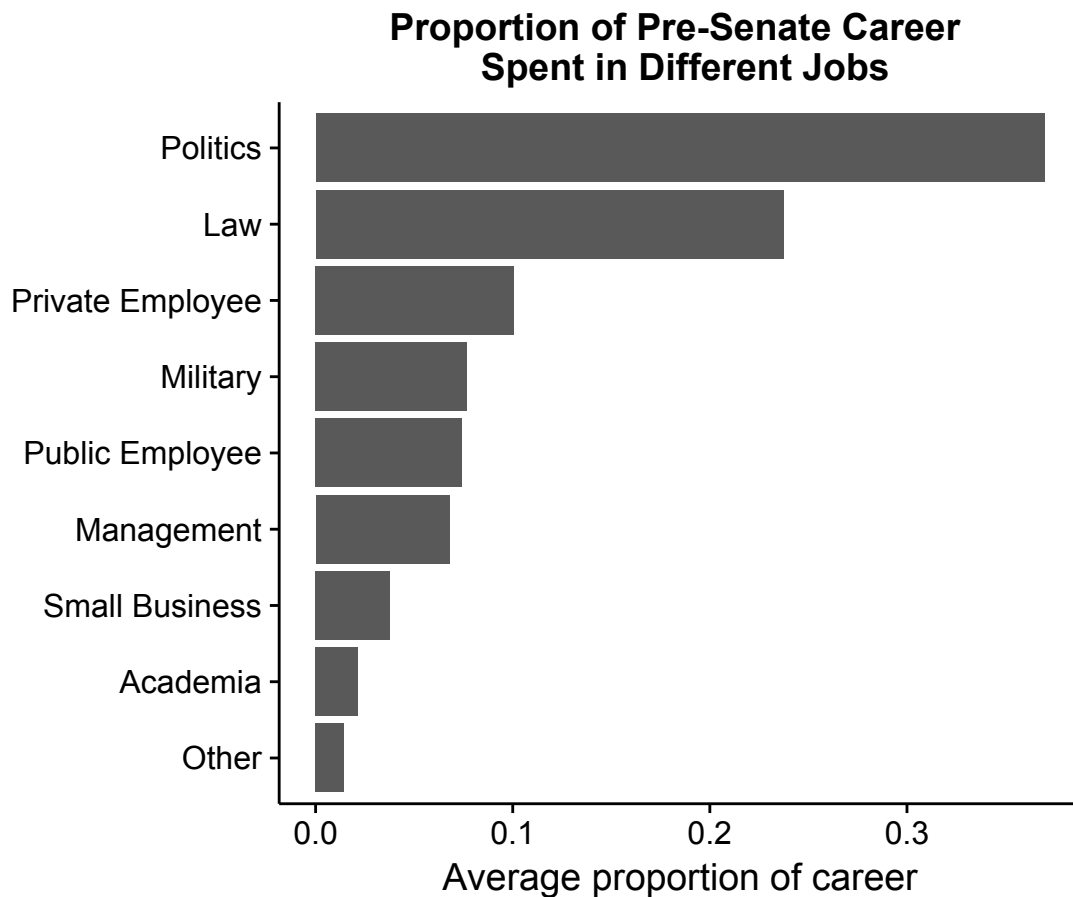


Figure A.1: Average proportion of pre-Senate career spent in various job types.

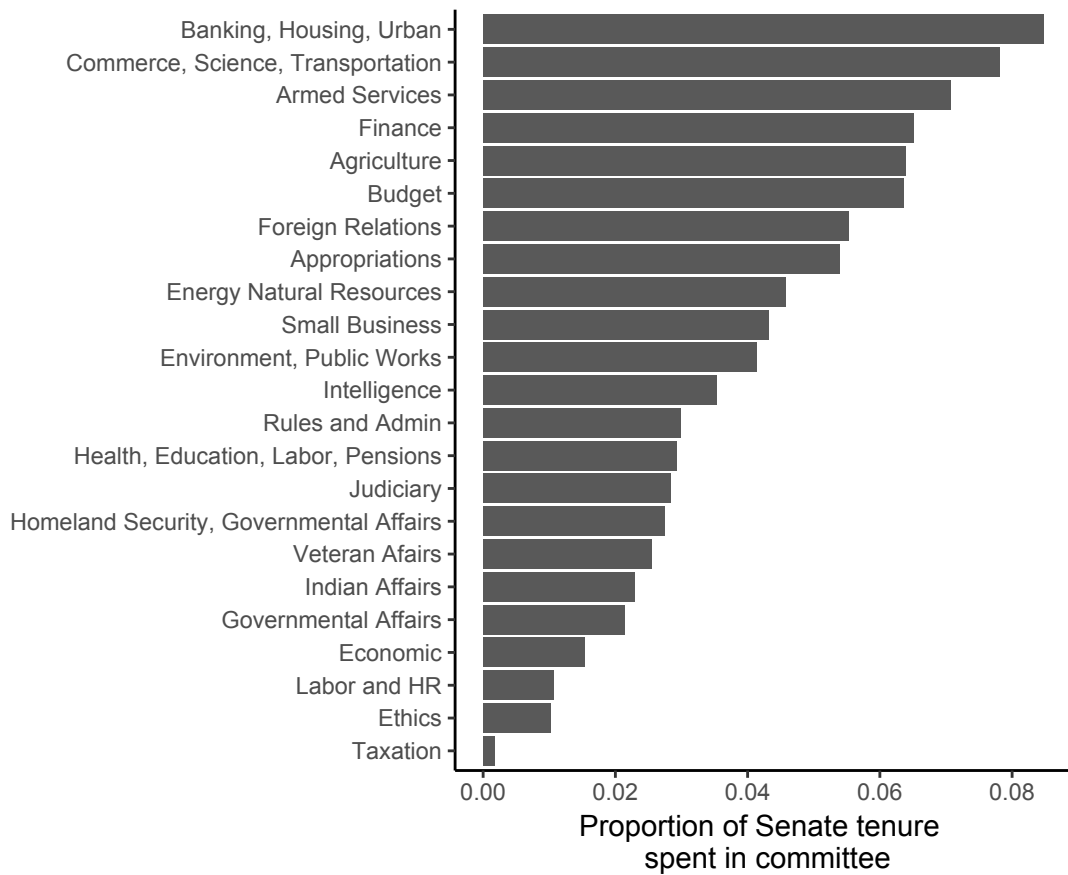


Figure A.2: Average proportion of tenure spent in various Senate Committees.

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) – 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.

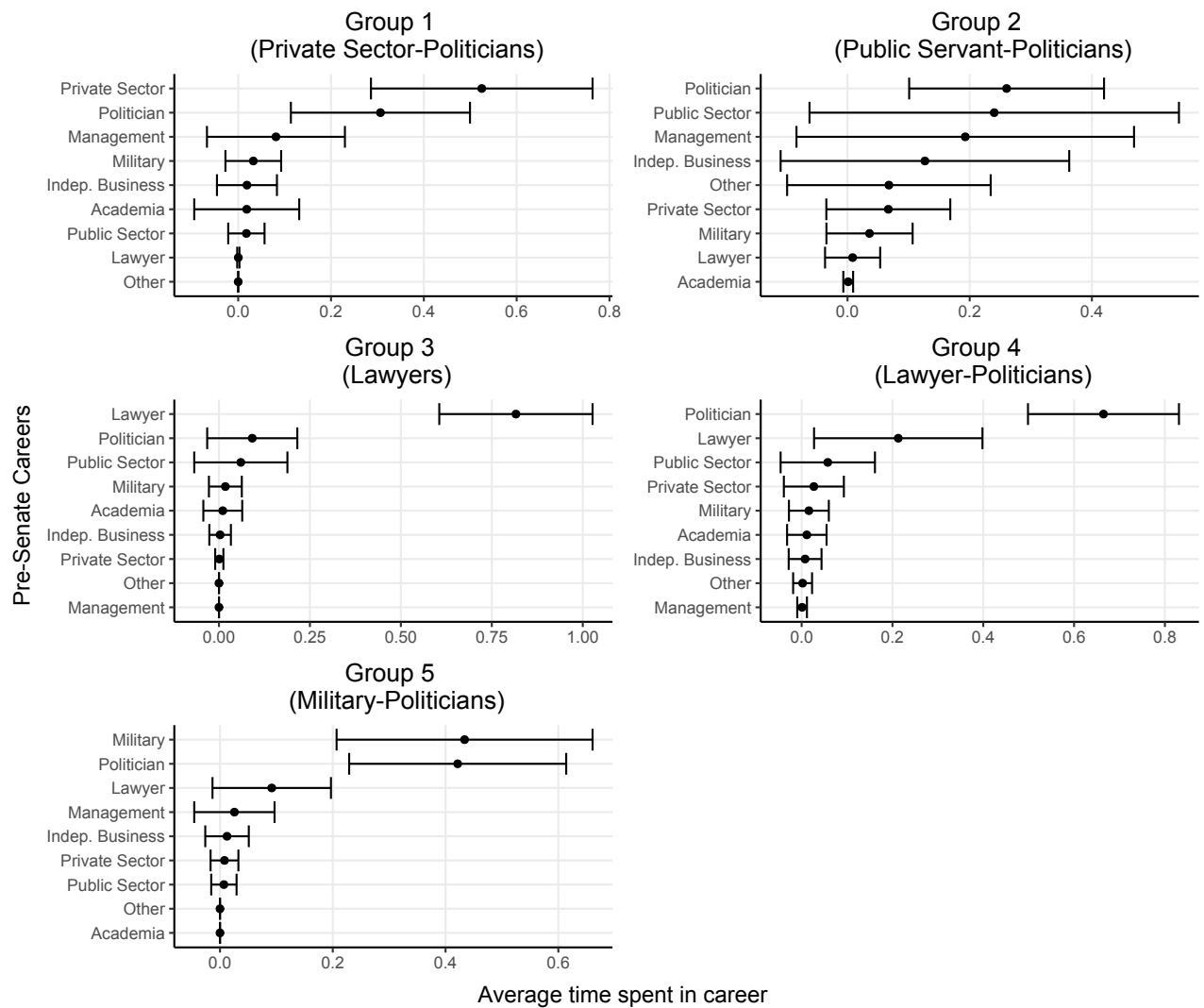


Figure A.3: Mix of pre-Senate careers within five different reference groups.

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.

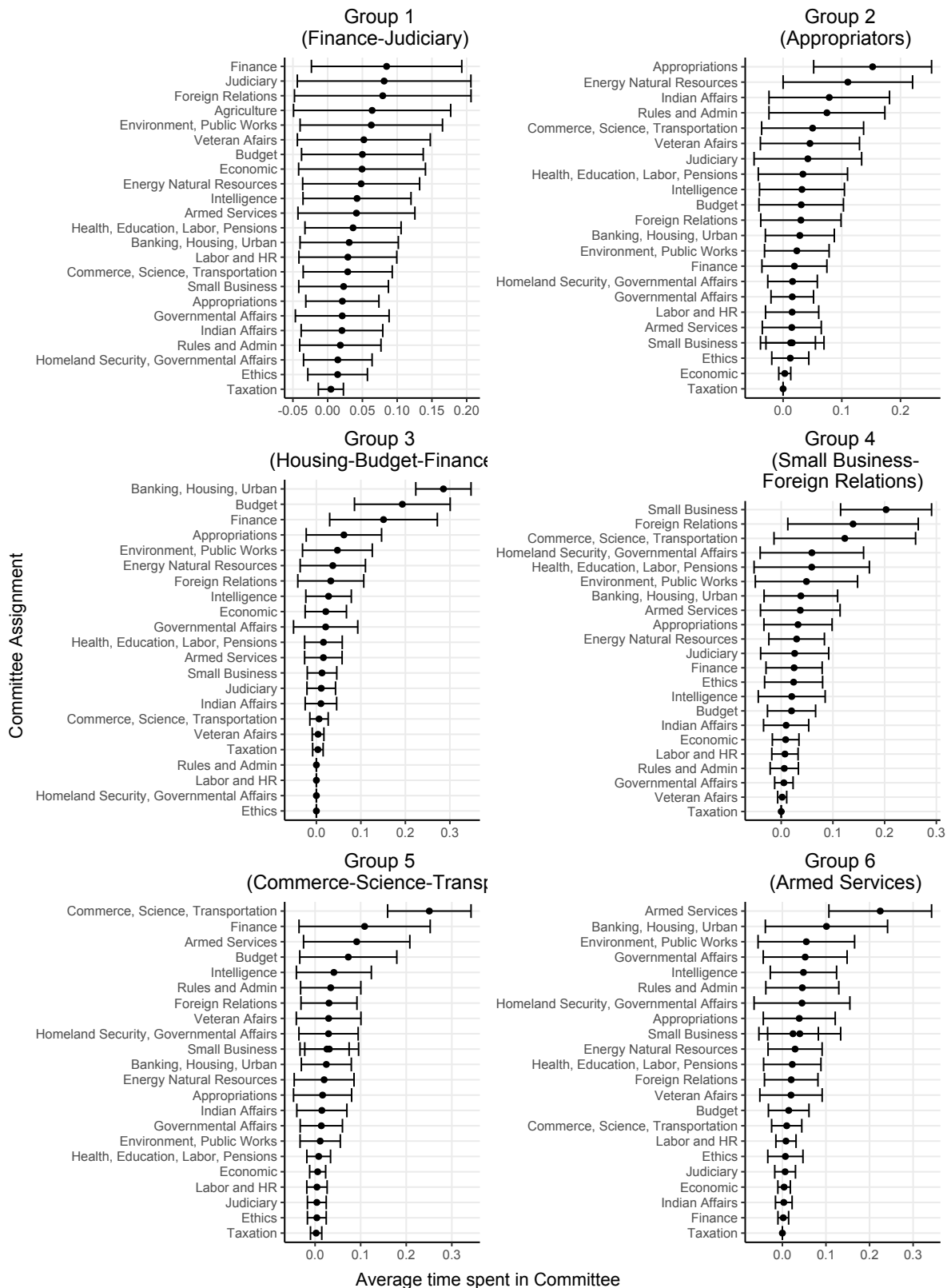


Figure A.4: Mix of committee assignments within six different reference groups.

Note: Points show the average proportion of Senate tenure spent in each committee (special committees and leadership positions excluded) for senators serving between the 102nd 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 performed in the main text, 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					
<i>Panel A: Pre-Senate Careers</i>		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	
<i>Panel B: Committee Assignments</i>		Finance-Judiciary	Appropriators	Housing-Budget	Small Business-Foreign Relations	Commerce-Science	Armed-Services
Total senators from group		90	51	12	21	29	34
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
<i>Dependent variables</i>		
SIG Career	Does the Senator take revolving door job at the end of current Congress?	CRP, RS, 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)
SIG Career _{t-1+h}	The lag of SIG Career of order one through three Congresses (placebo).	
<i>Primary Explanations</i>		
Contract Size (Career)	The predicted dollar size of the lobbying contracts 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 lobbying contracts 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
<i>Covariates</i>		
Ideal Points	D-IRT estimates from roll cal votes.	Poole and Rosenthal (2015) and own estimates.
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.
<i>Mechanism variables</i>		
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 pct. presence	The percentage of votes the senator has been present for in her average Congress.	Poole and Rosenthal (2015))

B.2 Descriptive statistics

Tables B.2 and B.3 show descriptive statistics for two parts of the sample. To estimate reference groups, I rely on a sample of senators serving from the 102nd to the 113th Congress (Table B.2). Data on lobbying contracts under the LDA is as mentioned previously, however, only available from 1998 and onwards. Thus, the models estimating the effect of Contract Size on the probability of walking through the revolving door, relies on a sample of senator serving in the 105th to the 113th Congress (Table B.3). Additionally, I have performed listwise deletion in the second table, keeping only the observations that are used, when estimating the models. The empirical strategy requires that I have a significantly high number of former senators working as lobbyists to estimate Contract Size – the senators, who left the Senate between the 102nd and 107th Congress supplies me with this. The tables show that there are no large discrepancies between the two samples.

Table B.2: Descriptive statistics for full sample (102nd-113th Congress)

Statistic	N	Mean	St. Dev.	Min	Max
SIG Career	1,085	0.046	0.210	0	1
Contract Size (career)	886	0.960	1.000	0.000	4.654
Contract Size (committee)	831	0.767	1.000	0.116	5.338
Ideal points	1,233	1.098	3.272	−6.681	12.550
Seniority	1,232	13.532	10.258	1	52
Difference to party	1,233	2.025	2.003	0.000	10.961
Election year	1,244	0.333	0.473	0	1
State Policy Liberalism	1,213	0.074	1.228	−2.525	2.743
Resigned?	1,244	0.072	0.259	0	1
Dynastic?	1,235	0.080	0.272	0	1

Note: Full period, no deletion of missing observations performed.

Table B.3: Descriptive statistics for estimation sample (105th-113th Congress)

Statistic	N	Mean	St. Dev.	Min	Max
SIG Career	769	0.042	0.200	0	1
Contract Size (career)	769	0.970	1.000	0.000	4.654
Contract Size (committee)	722	0.770	1.000	0.116	5.338
Ideal points	769	1.384	3.600	−6.681	12.550
Seniority	769	13.980	10.991	1	52
Difference to party	769	2.463	2.212	0.000	10.961
Election year	769	0.328	0.470	0	1
State Policy Liberalism	769	0.082	1.292	−2.525	2.743
Resigned?	769	0.059	0.235	0	1
Dynastic?	769	0.074	0.262	0	1

Note: Data on senators serving in the 105th-113th Congress. Listwise deletion of missing observations performed to only describe the data used in estimations.

C Additional analyses of robustness and heterogeneous effects

C.1 Placebo test for 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 and non-treated senators absent the change in career prospects. To substantiate this, Figure C.1 presents a placebo, which tests for pre-treatment trends, by regressing the dependent variable lagged by one, two and three periods, respectively, on Contract Size. Panels A and B show the results for estimated Contract Sizes based on pre-Senate careers and in-Senate committee assignment, respectively.

It is clear that there are no discernible pre-treatment trends in the probability of walking through the revolving door, when the measure of Contract Size is based on pre-Senate careers. While it is, of course, never possible to test the assumption of correct identification, in combination with the balance tests and the stability of coefficients in the face of controls, this test adds to the evidence that the effect of this measure of Contract Size is correctly identified.

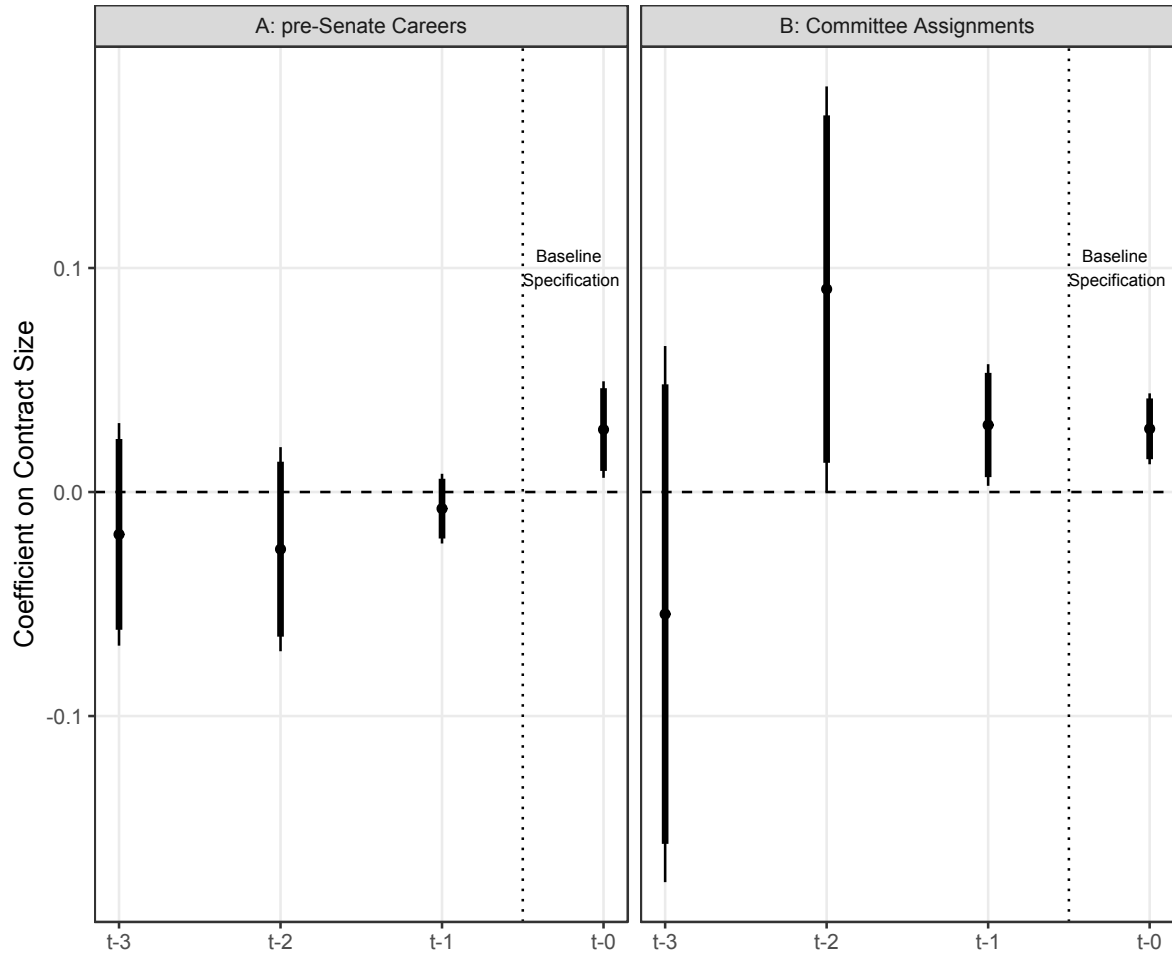


Figure C.1: Test for Pre-Treatment Trends.

Note: The figure presents the correlation between both measures of Contract Size (Panel A: estimates based on pre-Senate careers; Panel B: estimates based on in-Senate committee assignments) and SIG Career in the three Congresses leading up to the final one as well as the baseline estimates. No controls included. Confidence intervals are 90 pct. (thick) and 95 pct. (thin) lines calculated using Driscoll-Kraay autocorrelation and heteroskedasticity robust standard errors. While the results show clear pre-treatment trends in the models, where the measure of Contract Size is based on committee assignment, there is no discernible trend in the models using pre-Senate careers as basis for comparing senators.

C.2 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 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 C.2 estimates Congress-specific effects of Contract Size by introducing random slopes by Congress. I include twoway fixed effects in 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 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.

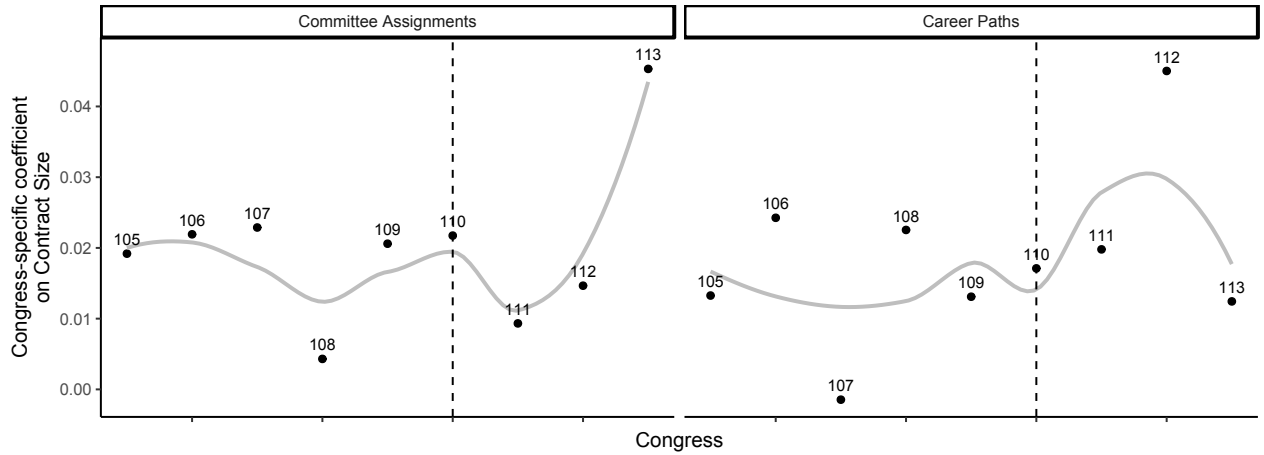


Figure C.2: Congress-specific Coefficient on Contract Size.

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.

C.3 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 C.3. 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.

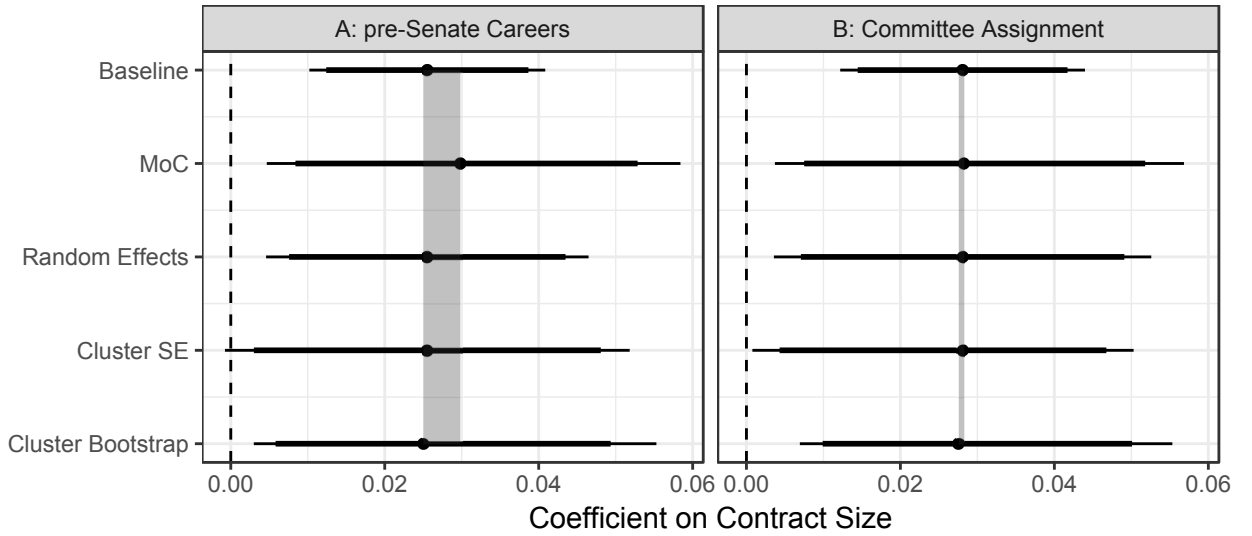


Figure C.3: Robustness to measurement error, clustering and autocorrelation.

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.

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 for arbitrary within-senator temporal autocorrelation, it makes the heroic assumption of no contemporaneous correlation between senator’s career choices. Even so, the results are robust to this choice of standard error. 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, while this widens the confidence intervals, my results remain robust.

C.4 Results for least productive using 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 C.4 I present the same specifications as in the main text, but use committee assignments to estimate Contract Size. They are remarkably similar to the results in the main text.

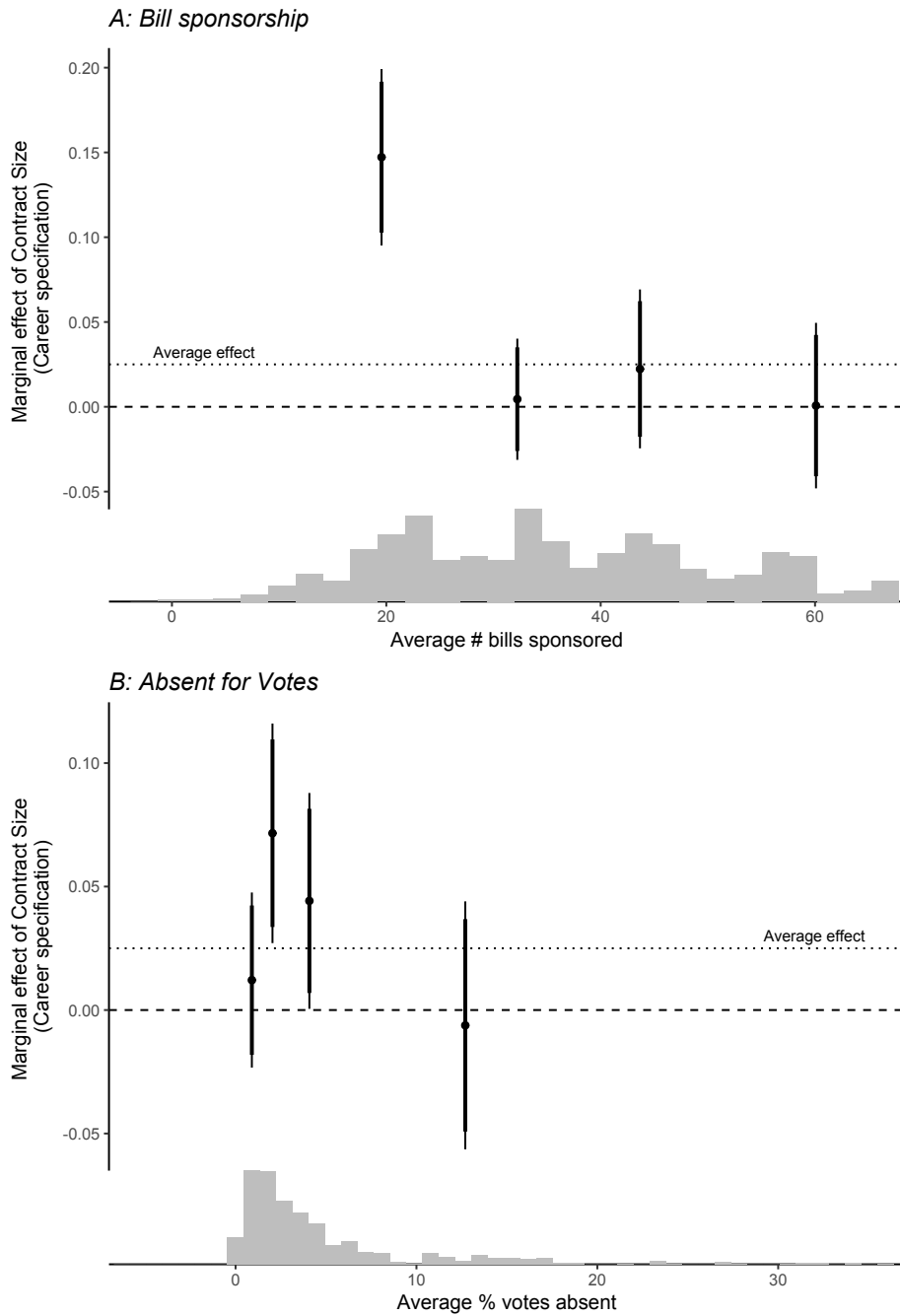


Figure C.4: Committee Specifications: Heterogeneous Effects of Career Prospects.

Note: Local marginal effects are estimated using the Hainmueller et al. (2016) binning estimator. Two-way fixed effects are included in all models. Robust confidence intervals are 95 pct. (thin lines) and 90 pct. (thick lines), respectively. Dotted line shows average effect, dashed line shows sharp null. Histograms show marginal distributions of moderating variables. Results are from specifications using committee assignment. Pre-Senate Career specifications are in the main text.

D 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 D.5 shows two dendograms with the baseline numbers of clusters (five and six) emphasized. For both dendograms, 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 D.7 and D.8 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-

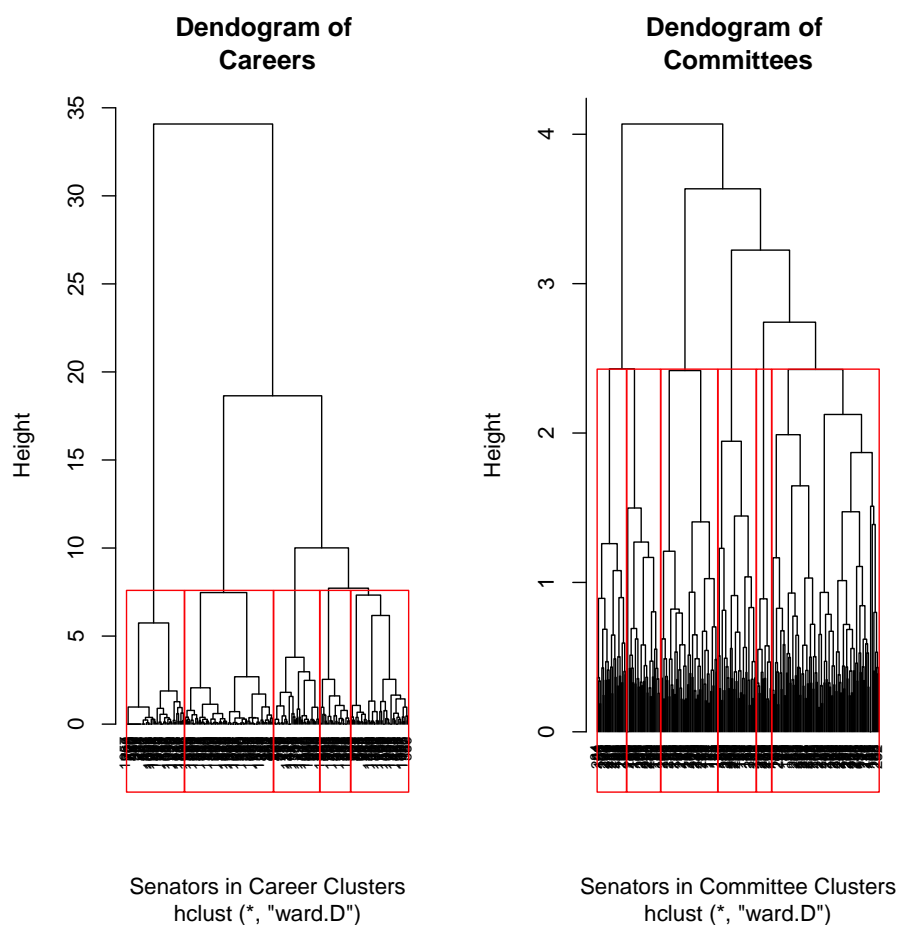


Figure D.5: Dendrogram of different career clusters.

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.

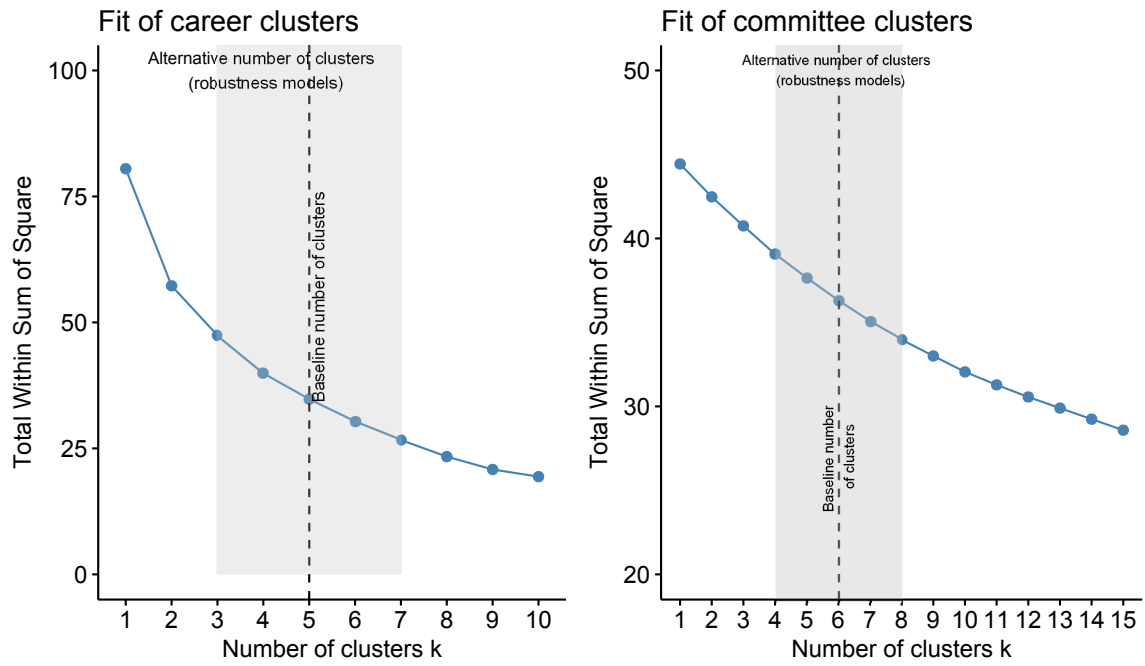


Figure D.6: Fit of different number of clusters.

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).

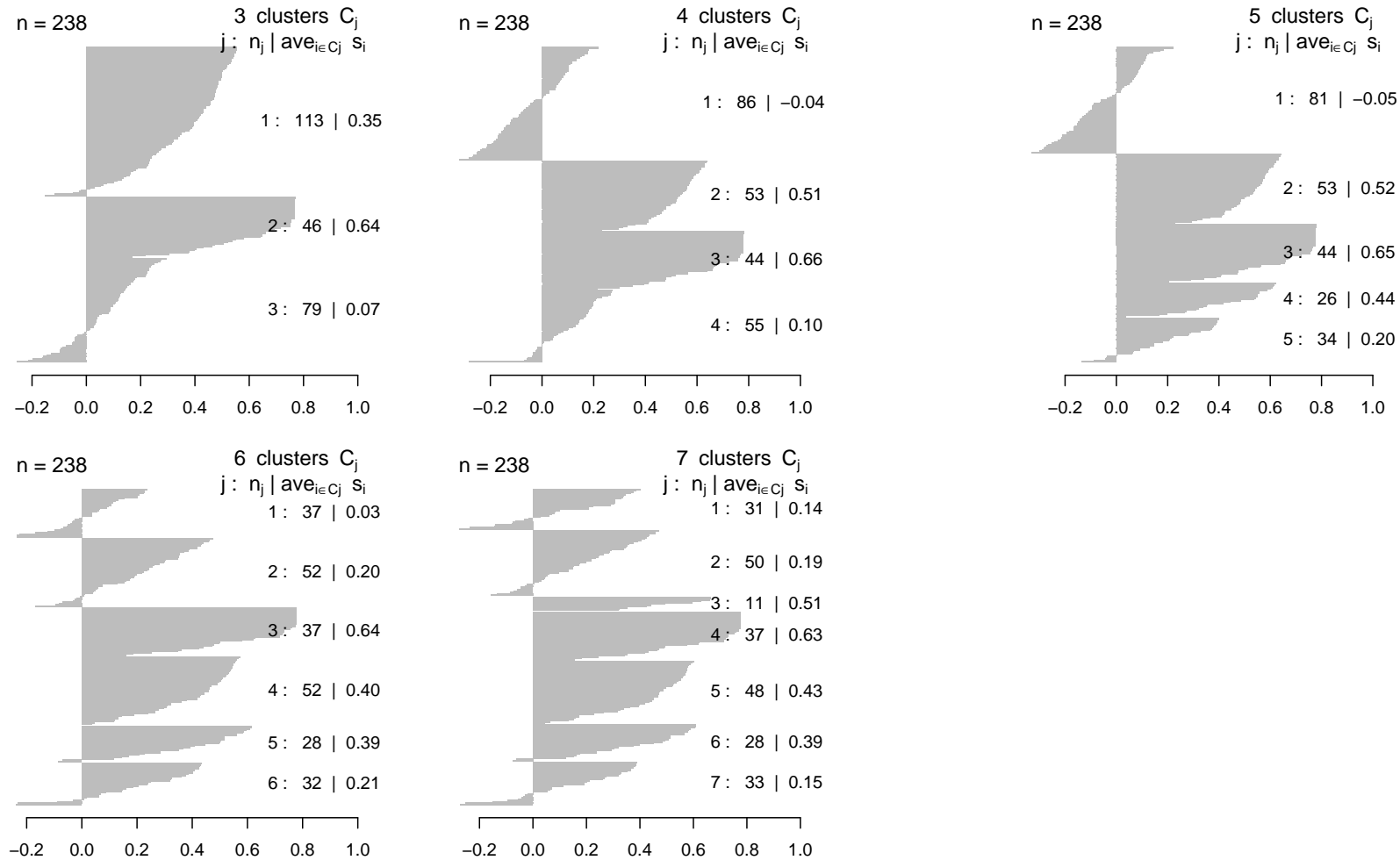


Figure D.7: Cohesion of career clusters.

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.

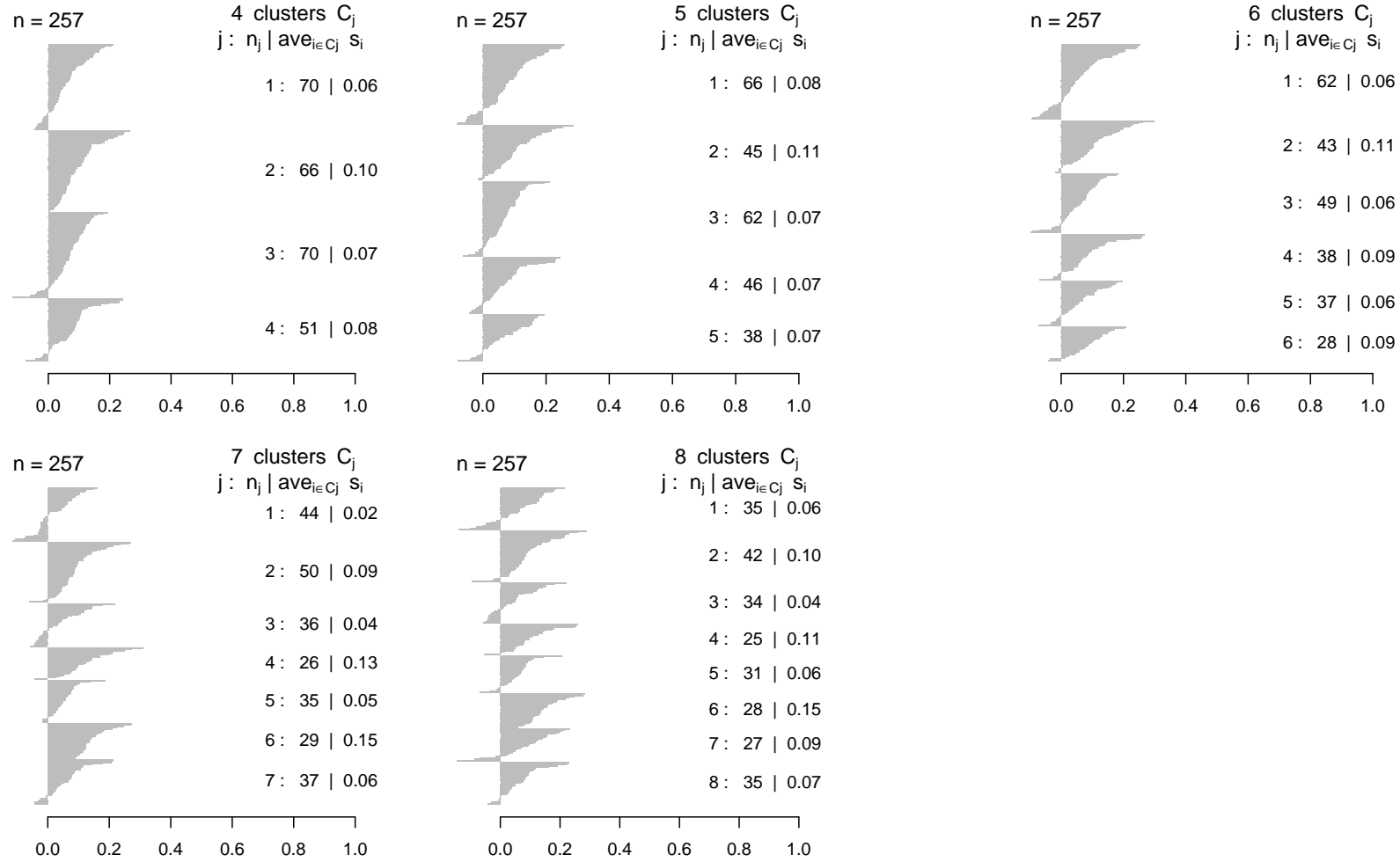


Figure D.8: Cohesion of committee clusters.

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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