Political Interactions and Voter Responses

Political Interactions and Voter Responses

Luc Tucker

A thesis submitted in fulfillment of the requirements for the degree of Doctor of Philosophy in Economics.

University of Warwick, Department of Economics

May 2014

Contents

List of Figures

Acknowledgments

I would like to thank first and foremost Professor Andrew Oswald for his supervision throughout the PhD studies.

I would also like to thank the Paris School of Economics for their hospitality during the past two years while accommodating me as a Visiting Researcher. Particular thanks go to Professor Andrew Clark for his role in that arrangement as well as his help regarding the content of this thesis. Dr. Margherita Comola also offered many extremely useful comments for which I am very grateful. Professor John Gastil was always helpful throughout the process of producing Chapter 1 and I am thankful to him for that. Particular thanks are also owed to Seyhun Orcan Sakalli, Marie Boltz, Paola Villar, Ricardo Estrada, Alejandro Del Valle Suarez, Carolina Alban and Andrea Locatelli for their always useful insights.

Paul Youdell played a crucial role in developing the direction of this thesis in its early stages and I am extremely grateful for his help during that period.

Declaration

This thesis is submitted to the University of Warwick in support of my application for the degree of Doctor of Philosophy. It has been composed by myself and has not been submitted in any previous application for any degree. A version of Chapter 1 has been published as a chapter in the edited book *The Australian Citizens' Parliament and the future of deliberative democracy*, co-authored with one of the editors; Professor John Gastil. The analysis and results included in Chapter 1 are all my own work (see the 'Statement of Originality' on page 39 for details).

Luc Tucker

30 September 2013

Overview

This thesis investigates empirically the way in which agents in political bodies can influence their peers, as well as the ways in which voters respond to the behavior of legislators in their electoral choices. These relationships are fundamental in trying to comprehend the way in which political decisions are made. Economists should take particular interest in these topics, given their importance in understanding the incentives faced by legislators.

Questions as to the possibility of peer effects between political agents are of huge importance in democratic governments. Political debates play a central role in many legislative bodies, where the assumption is implicitly made that opinions can be influenced by the other debate participants. This fundamental assumption is tested in Chapter 1, which is the first to measure the extent of peer influence regarding reported political opinions in an explicitly political environment. This has previously not been possible, given that discussions and debates in legislative chambers take place between participants with particular characteristics and political interests, making it hard to separate the role of peer effects in determining their preferences. This thesis makes use of experimental data, which offers a unique opportunity to distinguish these effects and quantify the degree to which peer effects can influence political preferences.

In particular, Chapter 1 uses data from an experiment conducted in Australia in 2009 to consider whether participants showed evidence of having influenced one-another during political discussions. Each of the models used exploits the fact that table allocations were randomized in this experiment and controls for agents' characteristics, which were also recorded. The key finding of this chapter is that when asked to assign weights to eleven criteria for an effective political system, agents who sat on the same table during the experiment reported preferences that were more similar than those who did not share a table. The effect is small at 4.8% of a standard deviation but is statistically significant and of larger magnitude than other pairing characteristics which could have been expected to influence the differences between weighting choices, such as whether the two players were of the same gender. One year after the Citizens' Parliament, participants were asked to report their political positioning on the 'left-right' scale. It is not found to be the case that the table allocations influenced these reported positions.

Having demonstrated that participants in legislative bodies can influence one-another's reported political preferences, this thesis goes on to analyze the relationship between legislators and the constituents they represent, by considering the question of whether politicians who are more active in parliament are rewarded with a higher probability of being reelected. The particular parliamentary behavior analyzed is the asking of parliamentary questions. The UK House of Commons uses a ballot system to determine which members are selected to ask a question from those who expressed an interest in doing so. This chapter is the first in the literature to exploit this randomization to show that the asking of such questions increases a member's chances of being reelected by their constituents. It is shown that while the ordering of parliamentary questions is determined at random, the practicalities of conducting debates introduce a potentially endogenous element to the determination of which questions receive oral answers (particularly the speed at which questions are answered). This chapter uses a matched sampling approach to cope with such non-random cases, but also includes alternative results, to show that the findings are not reliant on the use of this technique.

Chapter 2 exploits a natural experiment to show that Members of Parliament who are selected to ask parliamentary questions are more likely to be reelected in forthcoming elections. It was necessary in this study, however, to drop certain observations as a result of the fact that the Speaker in the House of Commons, who chairs debates, has some influence over the number of questions reached in each debate, which could undermine the randomization in these cases. Chapter 3 of this thesis goes on to consider this process in more detail. This chapter shows that in fact questions posed by older and more experienced members, as well as those from opposition parties, are more likely to receive oral answers than should be expected under a true randomization.

Chapter 3 offers the first opportunity to consider the Speaker's role in parliamentary debates under the conditions of a 'natural experiment'. Results presented here point to the role of the Speaker in controlling the speed at which debates progress as contributing significantly to the findings listed above, for example by acquiescing to pressure from more senior members by allowing them to ask their questions in debates where time constraints would otherwise prevent them from doing so. The finding is also an important consideration for future studies which aim to exploit such randomizations as natural experiments relating to parliamentary activity.

Such a finding is potentially significant in the context of the UK political system, where the ballot system is in place precisely to ensure that all members of the House of Commons have an equal opportunity to ask questions, regardless of their levels of seniority.

The final chapter of this thesis continues to examine the link between legislators and the citizens they represent. In particular, Chapter 4 makes use of the large Behavioral Risk Factor Surveillance System (BRFSS) dataset from the USA. While this dataset has been extensively used to study health outcomes, this chapter represents the first attempt to use the dataset to study the link between political outcomes and the economic prosperity of constituents. This is achieved by matching survey respondents to their representatives in Congress and restricting attention to cases where members of the lower house seek election to the upper house.

Members of the US Senate (the 'upper house' in Congress) are elected to serve a state as a whole, whereas members of the House of Representatives (the 'lower house') serve a district within one of those states. This chapter shows that members of the House of Representatives who seek election to the Senate (without necessarily being successful) tend to have previously served in districts with permanently higher incomes. Furthermore, incomes are found to be temporarily higher in districts where the representatives are successfully elected to the Senate than those where the representatives were unsuccessful in their attempt to be elected. This is interpreted as showing that in Senate elections, voters reward legislators who served districts where average incomes were seen to increase under their tenure.

These chapters use a diverse range of datasets to consider the impacts of political behavior. It is shown that the behavior of agents in political environments not only influences their peers, but is also recognized and rewarded by the voters they represent. Voters are found to respond to political behavior by both reelecting legislators who are more active (by asking more parliamentary questions) and by electing those legislators who have previously served districts where average incomes increased under their tenure.

Chapter 1

Peer Effects in an Experimental Parliamentary Setting

1.1 Introduction

Many group decisions rely on debates and discussions. In some cases these debates form a negotiation process, with those involved required to agree on a particular course of action. In other cases, including many parliamentary systems, debates are held as a precursor to a vote, with each participant in the debate casting one vote to either accept or reject the proposal being discussed.

It is therefore of interest to economists and political scientists whether policymakers are able to influence one another's voting choices in such political settings. For economists this is particularly relevant because it leads to the possibility of strategic behavior among those taking part in the debate. A vast literature on peer effects in networks spans many disciplines across the social sciences. Notable examples include the study of college roommates (Sacerdote (2001)), where the random assignment of students to shared college accommodation was found to influence academic achievement. Restricting attention to the transmission of political views within networks, Campos and Fernanda (2012) and Dey (1997) consider how political ideologies are transferred between college students. This study represents the first study to test for the transmission of political ideas between agents in an explicitly political setting.

While an increasing amount of data has become available on the 'networks' that exist between agents, a common problem in such studies is that researchers are unable to distinguish those effects that are transmitted between players who are linked by the network from those that led to its formation in the first place. In particular, in many settings agents have some influence over the players with whom they are 'linked', however such links are defined. In social settings where a link denotes 'friendship', for example, it might be expected that agents sharing similar characteristics are more likely to choose to link with one-another. Under such 'homophily', if two linked players are found to behave in similar ways, it is not possible to determine whether that effect comes from players influencing each other via the network, or from the fact that players with such similar characteristics would always behave in similar ways, regardless of the presence of the friendship link between them.

The Australian Citizens' Parliament (ACP) was an experiment conducted in 2009, where Australian citizens were selected at random to take part in a four-day process, with the task of answering the question: *"How can Australia's political system be strengthened to serve us better?"* Each participant in the ACP was randomly selected as the representative for one of the 150 electorates across Australia. Members were asked to collaboratively generate proposals for improving the Australian democratic system, with each member then weighting these proposals according to their perceived importance.

Following the four day deliberation process, these prioritized proposals were put forward to the Australian government, thus offering participants a genuine sense of worth in this experimental parliamentary process. This dataset makes it possible to analyze whether these weighting choices were influenced by other factors and particularly other participants. Finally, the background characteristics recorded for each player can be included in regressions as controls.

Much analysis has been carried out on the recorded findings from the ACP, some examples of which can be found in *The Australian Citizens' Parliament and the Future of Deliberative Democracy*. A key part of this analysis has been to determine the effectiveness of democratic procedures. Within this objective, this chapter will build on the findings reported in Tucker and Gastil (2013) in the above edition, by looking for evidence of whether ACP participants were able to influence other players through their debates. The purpose of this study will be to offer a more detailed explanation of the testing process which provided these results, as well as considering some proposed alternative estimation techniques.

The key component of the ACP organization is that players were allocated at random to different tables at the start of each of the four days. This design feature of the ACP experiment can be exploited by comparing the behavior of players who shared a table with that of players who did not share a table. Under these conditions, if two ACP participants who shared a table during the debating process are on average more likely to report similar preferences at the end of the debates, this can therefore be attributed to the table allocation itself. By contrast, without the aid of the randomized table allocations, in the event that 'table contacts' reported more similar preferences than those who did not share a table, it would be possible to argue, for example, that participants sharing similar characteristics were simply more likely to choose to sit together, and also then more likely to report similar preferences. This would explain the correlation between table allocations and reported preferences, even in the absence of any direct effect between the players themselves during the debate.

An important motivation for political and other debates is the idea that through exchanging opinions, a somehow more desirable outcome will be reached. Such a hypothesis relies on the assumption that opinions are malleable and can therefore be influenced by the information or perspective provided by others during the discussions. Without this possibility, the debate itself simply offers participants the opportunity to air their views, with no prospect of affecting the decision-making itself. This study considers whether this basic pre-requisite for effective debate is itself evident in the data described above.

The first part of the chapter supplements the findings of Tucker and Gastil (2013), by using similar dyadic regression techniques to infer whether the allocation to tables affected the preferences reported by the subjects in the experiment. This chapter, however, adopts the method proposed by Fafchamps and Gubert (2007) for calculating standard errors in dyadic regressions. The second part of the chapter goes on to consider alternative estimation techniques. The first such method instead treats individuals as separate observations and uses instrumental variables to determine whether agents reported similar preferences to those with whom they shared a table during the experiment, following the method of Bramoulle et al. (2009). The final results will revert to dyadic regression techniques, but with political leaning of participants on the 'left-right' scale as reported in surveys conducted one year after the ACP took place as the dependent variable.

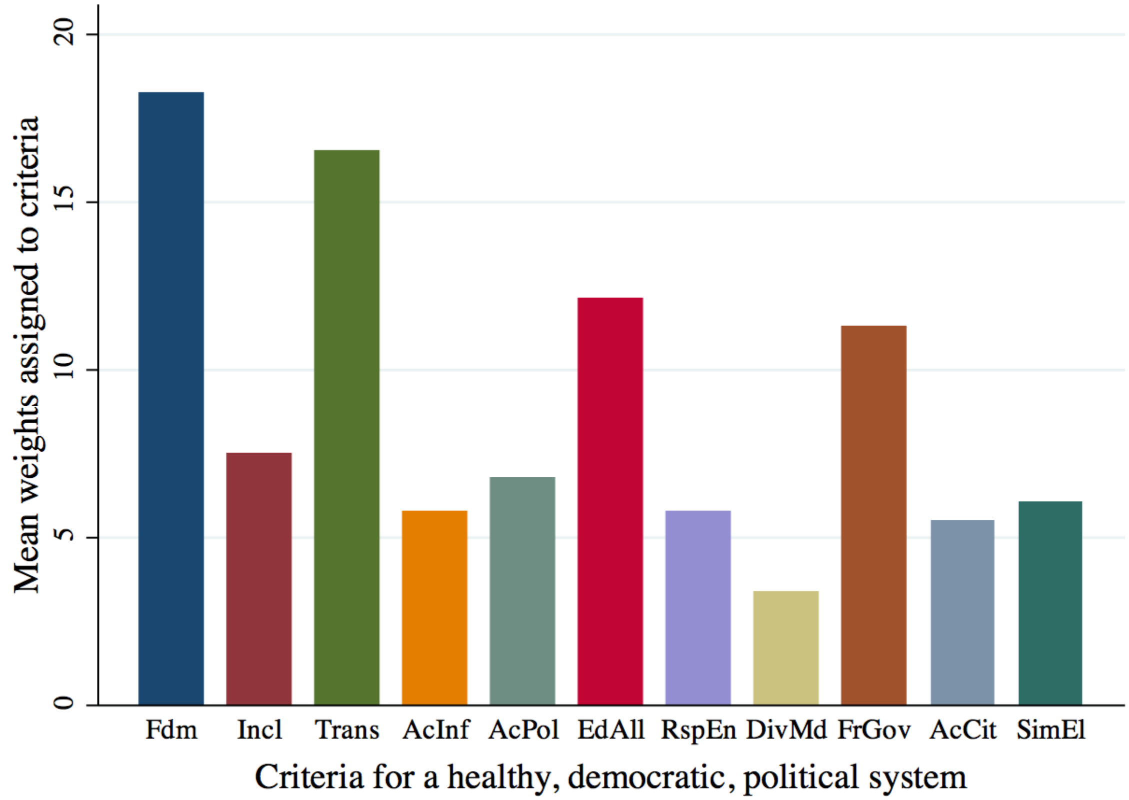
1.2 Criteria to be Weighted

On the third day of the ACP deliberations, players were asked to prioritize various criteria which they deemed to be important in an effective political system. To achieve this, players were given a list of eleven criteria and asked to assign weights to each of these criteria, with a total weighting allocation of 100 and a maximum weight of 30 available to be allocated to each criterion. The eleven criteria to be weighted were:

1. Freedom ('Fdm')
2. Inclusiveness ('Incl') - especially multiculturalism and minority groups
3. Transparency ('Trans') - open and honest political system
4. Access to information ('AcInf')
5. Access for all to the political system ('AcPol')
6. Guaranteed education for all ('EdAll')
7. Respect for the environment ('RspEn')
8. Diverse media ('DivMd')
9. Justice and fairness in government ('FrGov')
10. Active Citizenship ('AcCit')
11. Simplified electoral system with accessible government ('SimEl')

Each of the ACP participants therefore allocated 100 weighting points across each of these criteria, to reflect their perceived relative importance. The average weights assigned to each of the criteria across participants are reported in Figure 1.1. The weighting choices across these criteria will be used to form the dependent variable in the regressions that

Figure 1.1: Bar chart showing the mean weights assigned to each of the eleven criteria for a 'healthy, democratic, political system'.



follow in Section 1.4. The objective will be to analyze whether players who shared tables allocated weights which were on average closer together than other players who did not share a table during the ACP process.

1.3 The ACP Network

ACP participants held discussions within the tables to which they were randomly assigned on each day of the ACP. The definition of a link used in this chapter says that any two players i and j are linked as 'neighbors' if they were allocated to the same table on any of the first three days of the ACP. On day 1, players were assigned their tables, which results in a number of individual fully-connected networks, one for each table. The first-day network is illustrated in Figure 1.2(a), with each player represented by a node, and a line drawn between any two players who were allocated to the same table during the first day of the ACP. The table allocations were re-randomized at the start of the second day. Linking all players who shared a table on days 1 or 2 results in the network shown in

Figure 1.2(b). Similarly, table allocations were re-randomized at the start of the third day. Figure 1.2(c) therefore illustrates the network where any two players who shared a table on any of the first three days of the ACP are joined by a link. Given that weights were assigned to the criteria listed in Section 1.2 on the third day of the ACP, this third-day network is the one used for the analysis that follows.

To give some further idea of the characteristics of the network defined in this way, the average number of links per player is 15.9, with a standard deviation of 1.4. The minimum number of table contacts for any player is 11, while the maximum is 18. The fact that some players saw more repetition among their table contacts than others leads to this variation in the total number of contacts, as two players assigned to the same table on more than one occasion are treated in the same way as players who shared a table only once.

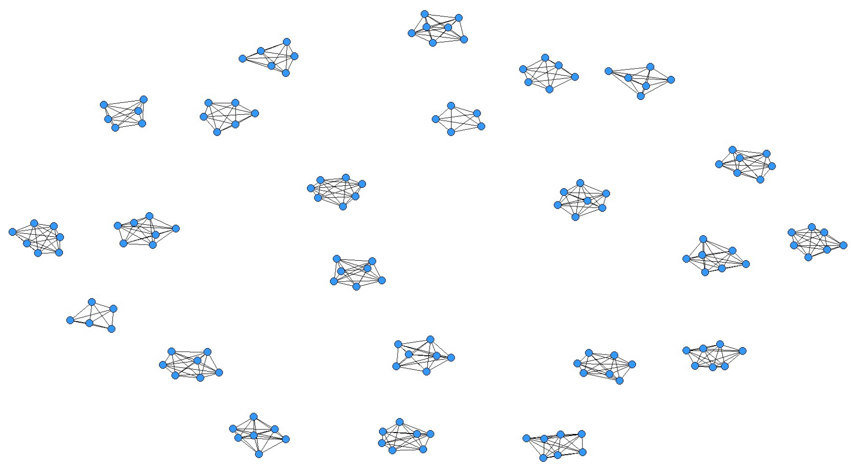
1.4 Dyadic Regressions

This section considers each possible pairing of ACP participants, and asks whether those pairings who were randomly assigned to the same table over the course of the first three days of the ACP were more likely to report similar weighting choices in regard to the criteria for an effective political system. The results reported in Tucker and Gastil (2013) were based on dyadic regressions, very similar to the ones reported here, but where standard errors were calculated using the 'quadratic assignment procedure' (Hubert and Schultz (1976); Krackardt (1987)). Conversely, standard errors reported here in Tables 1.1 to 1.4 are based on the method proposed by Fafchamps and Gubert (2007), where the variance-covariance matrix is calculated taking into account the fact that the error terms associated with dyads which share individuals cannot be assumed to be uncorrelated.

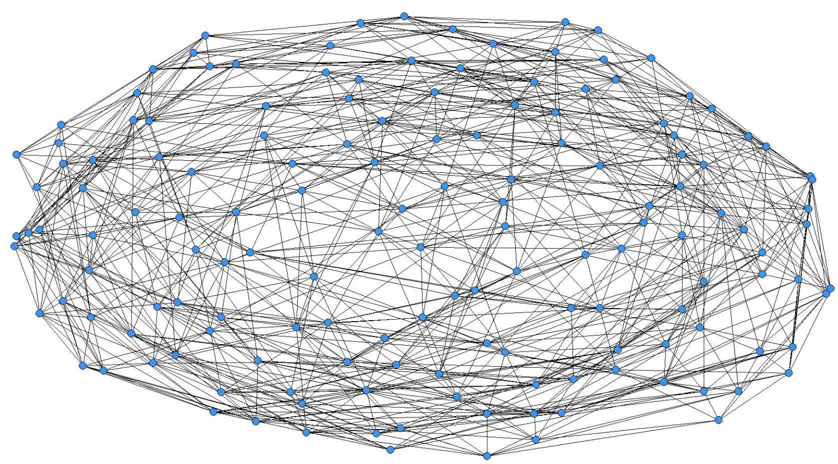
1.4.1 The Dyadic Regression Model

The dyadic regression model considers each possible pairing of participants as a separate observation in the data. For any such pairing ij, the dependent variable (y_{ij}) in these regressions is the total difference between the weighting choices of players i and j across all 11 criteria. Letting w_i^c represent the weighting choice of player i for criterion c, this

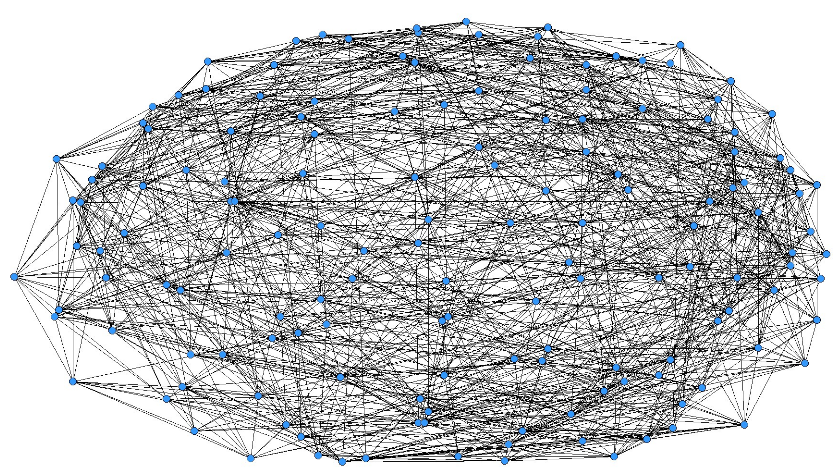
Figure 1.2: Illustrations of the ACP network with links added cumulatively over the first three days of discussions



(a) The ACP with players matched to the same table on day 1 joined together by a link



(b) The ACP with players matched to the same table on days 1 or 2 joined together by a link



(c) The ACP with players matched to the same table on days 1, 2 or 3 joined together by a link dependent variable can be expressed as:

$$y_{ij} = \sum_{c=1}^{11} \left| w_i^c - w_j^c \right|$$

This dependent variable is designed to provide a measure of the difference between the weighting choices of the two participants that make up each pairing. Lower values indicate that a pairing is comprised of two participants whose weighting choices are more similar.

The explanatory variable of interest in these regressions will be a dummy variable indicating whether the two players involved in the pairing were assigned to the same table on any of the first three days of the ACP discussions. Given that this variable is a characteristic of the pairing itself, it can be denoted x_{ij}. Letting T_i^d represent the table allocation of player i on day d, you have:

$$x_{ij} = \begin{cases} 1 & \text{if } T_i^1 = T_j^1,\ T_i^2 = T_j^2, \text{ or } T_i^3 = T_j^3 \\ 0 & \text{otherwise} \end{cases}$$

By regressing the dependent variable described previously on this explanatory variable indicating whether each pairing of players shared a table, it is possible to compare the average total weighting difference among those who shared a table with the corresponding value among those who did not share a table. A negative coefficient would then imply that those pairings who shared a table chose weights that were closer together. Furthermore, under the experimental conditions enjoyed here, such a result could be attributed to the table allocation itself.

It is possible to control for the characteristics of each pairing using data collected on ACP participants. For any individual characteristics of players i and j, denoted z_i and z_j respectively, it is possible to control for the absolute difference between these values, as well as the sum. Some of these control variables will take the form of ordered categorical variables. These variables are centered around zero to aid the interpretation of binary variables in the regressions which include interaction terms.

Including categorical variables as continuous controls imposes a linearity assumption on the correlation between these variables and the dependent variable. While this is undesirable, the inclusion of many dummy variables is not possible in paired regressions of this kind. The approach used here is justified because as a result of the randomized nature of table allocations, those allocations should not be correlated with any of these additional control variables. Their inclusion is therefore primarily intended to improve the precision of the findings, as opposed to addressing omitted variables bias.

This gives the following regression equation explaining the absolute difference between the reported political attitudes of all possible player pairings:

$$y_{ij} = \alpha + \beta x_{ij} + \gamma_1 |z_i - z_j| + \gamma_2 (z_i + z_j) + \delta_1 x_{ij} |z_i - z_j| + \delta_2 x_{ij} (z_i + z_j) + \varepsilon_{ij}$$

In a simple version of the model with no interaction terms and one binary control variable for each player capturing their gender ($z_i = 1$ if player i is female), the average effect of an increase in the number of females in a pairing from 0 to 1 on the dependent variable is captured by $\gamma_1 + \gamma_2$. Representing the dependent variable for given values of the controls as $y_{ij}(x_{ij}, z_i, z_j)$, you have:

$$\gamma_1 + \gamma_2 = y_{ij}(x, 0, 1) - y_{ij}(x, 0, 0)$$

Conversely, the difference between the dependent variable among all-male pairings compared with all female pairings is given by $2\gamma_2$, so you have:

$$2\gamma_2 = y_{ij}(x, 1, 1) - y_{ij}(x, 0, 0)$$

After introducing interaction terms, the value $\delta_1 + \delta_2$ captures the difference between the effect of being matched to the same table between all-male pairings and pairings comprised of one female and one male participant:

$$\delta_1 + \delta_2 = (y_{ij}(1, 0, 1) - y_{ij}(0, 0, 1)) - (y_{ij}(1, 0, 0) - y_{ij}(0, 0, 0))$$

Similarly, $2\delta_2$ captures the difference between the effect of sharing a table on the dependent variable for pairings who consist of two male participants and those consisting of two female participants:

$$2\delta_2 = (y_{ij}(1,\,1,\,1) - y_{ij}(0,\,1,\,1)) - (y_{ij}(1,\,0,\,0) - y_{ij}(0,\,0,\,0))$$

All the tables that follow will report the coefficients themselves, with the analysis explaining their interpretation in more detail where necessary. Given that these controls do not benefit from the same randomization as the explanatory variable of interest β, discussion of their interpretation will in general be kept to a minimum, except in providing some context for the coefficient of interest.

1.4.2 Calculating Standard Errors

Calculating standard errors in the usual way would ignore the dependence that exists between pairings who share one of their participants. In particular, it is not possible to assume that ε_{ij} is uncorrelated with ε_{ik}, because both are dependent on player i. For this reason, Tucker and Gastil (2013) used the 'quadratic assignment procedure' to calculate p-values directly. This procedure involves re-randomizing the network many times, so that in each case there can be no correlation between table allocations and the dependent variable. By recording the model coefficients under each new randomization, it is possible to estimate the distribution of coefficients directly. The point at which the unrandomized coefficients then fall within this estimated distribution can then be used to infer the significance of the coefficients.

Fafchamps and Gubert (2007) provide an alternative method, which is claimed to be more efficient, and involves calculating standard errors using a variance-covariance matrix which accounts for the cross-observation correlation described above.

To explain this difference, introduce the variable:

$$m_{ijkl} = \begin{cases} 1 & \text{if } i = k,\ i = l,\ j = k, \text{ or } j = l \\ 0 & \text{otherwise} \end{cases}$$

So, the variable m_{ijkl} indicates whether either player in the dyad ij is also a member of the dyad kl. Using this definition, and letting N and K represent the number of dyadic observations and number of regressors respectively, the variance-covariance matrix for calculating network-corrected standard errors takes the form:

$$\text{AVar}\left(\hat{\beta}\right) = \frac{1}{N-K}\left(X'X\right)^{-1}\left(\sum_{i=1}^{N}\sum_{j=1}^{N}\sum_{k=1}^{N}\sum_{l=1}^{N}\frac{m_{ijkl}}{2N}X_{ij}\varepsilon_{ij}\varepsilon'_{kl}X_{kl}\right)\left(X'X\right)^{-1}$$

In particular, when calculating standard errors in the usual way, off-diagonal elements in the matrix $E\left[\varepsilon\varepsilon' \mid X\right]$ are zero, because individual error terms are unrelated. This is not the case for paired data. In this case, if any two error terms ε_{ij} and ε_{kl} share one of their players (if $i = k$, $i = l$, $j = k$, or $j = l$), then you will have $m_{ijkl} = 1$. This in turn means that the corresponding off-diagonal element in the matrix $E\left[\varepsilon\varepsilon' \mid X\right]$ can be non-zero.

1.4.3 Dyadic Regression Results

The first column of Table 1.1 reports the results of the most basic dyadic regression, where the total weighting difference between each pairing (y_{ij}) is regressed on the 'Pairing shared a table' dummy variable (x_{ij}), without any controls for participant characteristics. The negative coefficient reported here implies that pairings assigned to the same table during the ACP on average choose weights that are closer together by 1.4 weighting points. Given that there are 11 criteria, this implies that pairings who shared a table choose weights that are on average 0.13 weighting points closer together on the 0-30 scale for any one criterion.[1] This might seem to be a small effect, but based on network-corrected standard errors described above, this effect is found to be statistically significant at the 5% level.

The second column in Table 1.1 reports the results of a similar regression, but this time controlling for some basic characteristics of participants, including gender, age and education levels. The 'Age' and 'Education' variables are both categorical variables, and are included as such in regressions. This in turn means that when interpreting the corresponding coefficients, it should be noted that the 'Difference between age-groups of pairing' and 'Difference between education groups of pairing' in these cases correspond to differences between the category numbers. Category numbers range from 1 to 5 in the case of the 'Age' variable, which generally put the participants into ten-year age brackets, and 1 to 4 for the 'Education' variable. None of these control variables are found to have a significant

[1]The mean difference between weighting choices for any single criterion is 8.82 and the standard deviation is 2.64.

Table 1.1: ACP participants were asked to assign weights to eleven criteria for an effective political system. For each possible pairing of participants, it is possible to calculate the difference between their weighting choices for each criterion. Summing these differences across all the criteria offers a measure of the difference between the political attitudes of the pairing. The following table reports the results of dyadic regressions using this total weighting difference ($y_{ij} = \sum_{c=1}^{11} \left| w_i^c - w_j^c \right|$) as the dependent variable (mean 97.1, standard deviation 29.1), based on the entire dataset. The aim is to determine whether pairings who shared a table reported preferences across the criteria that were more similar than those pairings who did not share a table.

	(1)	(2)	(3)
Pairing shared a table	-1.403*	-1.435*	-1.425*
	(-1.97)	(-1.99)	(-1.99)
Different-gender pairing		0.414	0.415
		(0.78)	(0.78)
No. of females in pairing		-1.071	-1.092
		(-0.50)	(-0.51)
Difference between age-groups of pairing		0.236	0.228
		(0.37)	(0.36)
Sum of age-groups in pairing		0.307	0.334
		(0.42)	(0.46)
Difference between education groups of pairing		0.639	0.642
		(0.94)	(0.95)
Sum of education groups in pairing		-0.560	-0.586
		(-0.67)	(-0.70)
Different internet access pairing			0.0966
			(0.21)
No. of people with internet access in pairing			0.930
			(0.38)
Observations	21462	21462	21462

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1.2: Table with simply-defined control variables to aid interpretation of the 'Table match' coefficients. Again, this table reports the results of dyadic regressions using the total weighting difference ($y_{ij} = \sum_{c=1}^{11} \left| w_i^c - w_j^c \right|$) as the dependent variable (mean 97.1, standard deviation 29.1), based on the entire dataset, with the aim of determining whether pairings who shared a table reported preferences across the criteria that were more similar than those pairings who did not share a table. Defining control variables in this way makes it possible to directly assess the size of the coefficient associated with the explanatory variable of interest.

	(1)	(2)	(3)	(4)
Pairing shared a table	-1.403*	-1.432*	-1.438*	-1.433*
	(-1.97)	(-2.03)	(-2.03)	(-2.02)
Same gender pairing		-0.436	-0.433	-0.417
		(-0.78)	(-0.78)	(-0.75)
Same age category pairing			-0.883	-0.837
			(-0.86)	(-0.81)
Same education category pairing				-0.781
				(-0.73)
Observations	21462	21462	21462	21462

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

effect on the dependent variable, however it remains the case that those pairings who were randomly assigned to the same table during the course of the ACP chose weights that were closer together than those pairings who did not share a table. The size of this coefficient is of similar magnitude to the first column, and remains significant at the 5% level.

The final column in Table 1.1 controls for one extra characteristic, namely the internet access available to the participants who made up the pairings. With no survey questions relating to participant income, this is intended as a form of (very imperfect) proxy. Again, none of the control variables are statistically significant, but the coefficient of interest remains significant at the 5% level, suggesting that those pairings who shared tables chose weights that were on average closer together.

To assess the magnitudes of these coefficients, comparison with the size of the other coefficients in Table 1.1 is not straightforward because of the way in which these coefficients must be interpreted. Table 1.2 reports the results of regressions with some simply-defined control variables included to allow a straightforward comparison.

The first thing to note is that the magnitude of the coefficient of interest is largely unchanged from Table 1.1 in these regressions, at around -1.4. This shows that while control variables defined in this way control for less of the variation in their respective variables than in Table 1.1, the effect on the 'Pairing shared a table' variable is largely unchanged.

The second column of this table reports the results of a dyadic regression of the total weighting difference on a dummy variable indicating whether the two players in each pairing shared a table in the ACP, with a single additional control variable which indicates whether the two participants that make up each pairing were of the same gender (0 if they were of different gender, 1 if they were the same gender). Table 1.2 shows that same-gender pairings were found to choose weights that were just 0.4 weighting points closer together than pairings made up of different-gender participants. This translates to an effect of 0.04 weighting points per criterion. The effect of having shared a table at the ACP on the closeness of a pairing's weighting choices is therefore found to be more than three times larger than the estimated effect of a pairing being made up of two players of the same gender.

The third column in Table 1.2 introduces an additional dummy variable, which takes the value 1 if a pairing consists of two participants from the same age category and 0 otherwise. Results in this case show that players from the same age category choose weights that are 0.9 weighting points closer together across the 11 criteria, or 0.08 per criterion. Similarly therefore, the effect of having shared a table during the ACP is found to be larger than the effect of two players being of similar age.

Finally, the fourth column in Table 1.2 introduces a similar dummy variable which is equal to 1 if the two players in a pairing came from the same education category and 0 otherwise. Pairings made up of players from the same education category are found to choose weights that are around 0.8 weighting points closer together across all the criteria, which in this case translates to such pairings choosing weights that are 0.07 weighting points closer together on the 0-30 scale for a single criterion. Again, this effect is found to be smaller than the effect of the two players having shared a table at the ACP in bringing their weighting choices closer together.

A year after the conclusion of the ACP process, participants were also asked to report their position on the 'left-right' political scale. In particular, an ACP questionnaire contained the question *"In political matters, people talk about the 'left' and the 'right' in Australia. Please use the response scale provided to say if you are to the left, the centre, or to the right?"* Participants were then arranged on a five-point scale according to these responses, with those choosing 'Strongly left' assigned a value 1, and those choosing 'Strongly right' assigned a value 5.

This variable can be included as an additional control variable. Not all participants offered responses to this question, however, so the dataset is substantially reduced by doing so. The results in Table 1.3 are therefore based on pairings between 107 individual participants, as opposed to 147 previously. To aid comparison, the first three columns in this table report the results of similar regressions to Table 1.1, but based only on the sub-sample of participants who disclosed their political leaning on the 'left-right' scale.

As was the case with the whole sample, those pairings randomly assigned to the same table during the ACP were found to assign weights that were closer together than those pairings who were never assigned to the same table. The coefficients are now larger in absolute terms, at -3.0 in each of the first three regressions, and also now significant at the 1% level, despite the smaller sample size. Once again, as there are 11 criteria, this implies that for any one criterion, pairings who shared a table chose weights that were 0.27 weighting points closer together than pairings who did not share a table. None of the other controls are found to have a significant impact on the dependent variable.

The fourth column of Table 1.3 then introduces the 'left-right' variable as an additional control, but its inclusion has very little impact on the explanatory variable of interest, with the coefficient barely changing at -3.0 and remaining significant at the 1% level.

Finally, the fifth column of Table 1.3 reports the results of similar regressions, but with the inclusion of additional interaction terms between variables related to self-reported political positioning and the 'Pairing shared a table' dummy variable. To aid the interpretation of the coefficient of interest, the 'Left-right' variable is rescaled to ensure it has zero mean. Under these conditions, the explanatory variable of interest remains close to -3, and significant at the 1% level, so the finding that pairings who sat on the same table choose more similar weights persists.

The inclusion of the interaction terms implies that for pairings with political positions

Table 1.3: For each possible pairing of participants, it is possible to calculate the difference between their weighting choices for each criterion. Summing these differences across all the criteria offers a measure of the difference between the political attitudes of the pairing. The following table reports results from dyadic regressions with this total weighting difference ($y_{ij} = \sum_{c=1}^{11} \left|w_i^c - w_j^c\right|$) as the dependent variable (mean 95.5, standard deviation 28.2). The aim is again to determine whether pairings who shared a table reported preferences across the criteria that were more similar than those pairings who did not share a table. Results in all columns are based only on those observations for which data on self-reported political leaning is available.

	(1)	(2)	(3)	(4)	(5)
Pairing shared a table	-3.034**	-3.036**	-3.017**	-2.966**	-3.087**
	(-2.80)	(-2.70)	(-2.73)	(-2.65)	(-2.74)
Different-gender pairing		1.407	1.406	1.417	1.413
		(1.28)	(1.27)	(1.29)	(1.28)
No. of females in pairing		-0.0868	-0.00461	-0.00725	-0.00720
		(-0.03)	(-0.00)	(-0.00)	(-0.00)
Difference between age-groups of pairing		-0.450	-0.410	-0.415	-0.440
		(-0.56)	(-0.52)	(-0.51)	(-0.54)
Sum of age-groups in pairing		0.975	1.074	1.000	0.994
		(1.08)	(1.21)	(1.08)	(1.07)
Difference between education groups of pairing		0.615	0.626	0.591	0.588
		(0.94)	(0.96)	(0.90)	(0.90)
Sum of education groups in pairing		-0.717	-0.787	-0.917	-0.919
		(-0.82)	(-0.90)	(-1.07)	(-1.07)
Different internet access pairing			-0.0510	-0.0348	-0.0521
			(-0.16)	(-0.11)	(-0.17)
No. of people with internet access in pairing			2.336	2.303	2.273
			(0.87)	(0.86)	(0.85)
Difference between political positons of pairing				1.071	1.437
				(0.94)	(1.31)
Combined political position pairing				-0.225	-0.117
				(-0.16)	(-0.08)
Diff. btw. political pos. * Table match dummy					-3.255*
					(-2.07)
Combined political pos. * Table match dummy					-0.928
					(-1.42)
Observations	11342	11342	11342	11342	11342

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

one category further apart, holding their combined political position constant, the coefficient associated with being matched to the same table is reduced by 3.26 weighting points. Given that the effect of being assigned to the same table is to reduce the difference between the weighting choices of pairings, this implies that the negative effect will be much larger among those pairings whose political positions are more different. This intriguing result therefore suggests that the weighting choices of players are more likely to be pushed closer together by being allocated to the same table if those players hold very different political beliefs.

In light of this result, Table 1.4 reports the results of similar regressions, but where rather than using participants' own assessment of their political leaning as a control variable, an alternative measure of their political preference is inferred from their answers to a different survey question. In particular, participants were asked to respond to the following statement: *"Our society would be better off if the distribution of wealth was more equal"*. Those who report that they "Strongly agree" to this statement are placed at one end of the five-point scale, and are assumed to correspond with those participants who should consider themselves 'left-wing'. Conversely, those who "Strongly disagree" are placed at the other end of the scale, forming a group assumed to contain 'right-wing' participants. The use of this alternative measure also serves to counter the suggestion that participants may misreport their own political preferences.

Table 1.4 works exactly as Table 1.3, in that all regressions are based on the same subset of the sample for whom data on political leaning (now inferred from the alternative question) are available.

The coefficient of interest remains negative, and significantly so, throughout all five regressions considered. This suggests that among those participants who responded to the distribution of income question, those pairings who were assigned to the same table chose weights that were cumulatively 2.8 weighting points closer together than those who never shared a table. Once again, no other control variables have coefficients significantly different from zero at the 5% level. This translates to pairings who shared a table assigning weights to each criterion that are 0.26 weighting points closer together on the 0-30 scale.

The interaction terms between the political positioning variables and the dummy vari-

Table 1.4: For each possible pairing of participants, it is possible to calculate the difference between their weighting choices for each criterion. Summing these differences across all the criteria offers a measure of the difference between the political attitudes of the pairing. The following table reports results from dyadic regressions with this total weighting difference ($y_{ij} = \sum_{c=1}^{11} \left|w_i^c - w_j^c\right|$) as the dependent variable (mean 95.8, standard deviation 28.4). The aim is again to determine whether pairings who shared a table reported preferences across the criteria that were more similar than those pairings who did not share a table. Results in all columns are based only on those observations for which data is available on 'inferred' political leaning, i.e. based on responses to the statement that *"Our society would be better off if the distribution of wealth was more equal"*.

	(1)	(2)	(3)	(4)	(5)
Pairing shared a table	-2.873**	-2.866*	-2.832**	-2.838**	-2.856**
	(-2.63)	(-2.54)	(-2.58)	(-2.59)	(-2.67)
Different-gender pairing		1.040	1.040	1.050	1.040
		(1.12)	(1.11)	(1.12)	(1.11)
No. of females in pairing		-0.524	-0.283	-0.357	-0.356
		(-0.21)	(-0.12)	(-0.15)	(-0.15)
Difference between age-groups of pairing		-0.0524	-0.0142	0.00976	0.0134
		(-0.06)	(-0.02)	(0.01)	(0.02)
Sum of age-groups in pairing		1.218	1.324	1.473	1.474
		(1.36)	(1.51)	(1.71)	(1.71)
Difference between education groups of pairing		0.864	0.843	0.802	0.798
		(1.22)	(1.20)	(1.13)	(1.12)
Sum of education groups in pairing		-0.593	-0.746	-0.455	-0.454
		(-0.67)	(-0.83)	(-0.50)	(-0.49)
Different internet access pairing			0.388	0.395	0.397
			(0.62)	(0.63)	(0.62)
No. of people with internet access in pairing			3.797	3.694	3.702
			(1.35)	(1.31)	(1.32)
Difference between political positons of pairing				0.400	0.347
				(0.45)	(0.40)
Combined political position pairing				0.766	0.819
				(0.76)	(0.82)
Diff. btw. political pos. * Table match dummy					0.515
					(0.42)
Combined political pos. * Table match dummy					-0.481
					(-0.85)
Observations	11990	11990	11990	11990	11990

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

able of interest indicating whether players shared a table, which were previously negative, are now close to zero using this alternative measure of political leaning. The correlation between the two alternative measures of political leaning is 0.20, which helps to explain why the patterns previously observed in Table 1.3 are not repeated.

What does persist throughout these regressions, however, is the finding that those pairings who were randomly allocated to the same table at some point during the ACP on average chose weights that were closer together than those pairings who were not assigned to the same table. When considering the entire dataset, those pairings allocated to the same table chose weights that were around 1.4 weighting points closer together on average than those who were not allocated to the same table. This effect remains significant at the 5% level, even when controlling for various personal characteristics. Furthermore, when also controlling for political leaning, based on responses to two separate questions (and reducing the sample size accordingly in either case), those pairings who shared a table chose weights that were generally around 3 weighting points closer together than those assigned to different tables. Even with the smaller sample size, and including all the various controls, the effect in these cases remains significant at the 1% level throughout.

1.5 Alternative Estimation Methods

1.5.1 Instrumental Variables Estimation

Rather than considering possible pairings of observations (as in Section 1.4), an alternative method could be proposed, following Bramoulle et al. (2009), which takes individuals as separate observations and asks whether players are influenced by those with whom they shared a table, by taking the mean of all the weights chosen by these 'table contacts', and including this average as a regressor.

It would be necessary in this case to use an instrumental variables (IV) specification as opposed to ordinary least squares, because if it was assumed that participants allocated to the same table during the ACP were influenced by one-another, then it would be possible that the player in question also influenced the responses of each of his neighbors, meaning that their average response would be subject to reverse causality.

Manski (1993) distinguished between three paths of possible influence across such networks. 'Correlated' effects refer to the case where players who are directly linked in a network are more likely to behave in similar ways. Aside from this effect, it is also possible to distinguish between 'exogenous' effects, where the characteristics of each player have an effect on the behavior of those other players to whom the player is directly linked, and 'endogenous' effects, where the behavior of each player has an influence on those around them.

While correlated effects can be ruled out in this case, thanks to the randomized nature of the ACP seating assignment, Manski (1993) highlighted potential problems in distinguishing between the exogenous and endogenous effects. Bramoulle et al. (2009) showed, however, that in the absence of correlation effects, it is possible to separately identify the exogenous and endogenous effects by using an instrumental variables approach under certain conditions on the network. One such condition is that the network contains 'intransitive triads'. The presence of an intransitive triad implies that you can find a player i, who is linked to another player j, who in turn is linked to some other player k, but where no direct link exists between players i and k. In such cases, any influence between players i and k must come through their mutual links with player j. This makes it possible to use the characteristics of player k as instruments for the actions of player j in explaining the behavior of player i.

One potential advantage of such an IV specification over the dyadic models reported in Section 1.4 is that the direction of causality is explicitly accounted for. This method therefore identifies the effect of a one-point increase in the average weight assigned to a criterion among those players with whom each player shared a table on their own weighting choice. This could be considered more interpretable than the effect on a pairing that is reported in dyadic regressions. Furthermore, in this case, the weights associated with each criterion are treated in separate regressions. This offers the prospect of a more detailed breakdown of which criteria could be driving the results from the dyadic regressions, where the dependent variable was calculated by aggregating across all 11 criteria.

A key requirement for using this estimation procedure, however, is that in such a model the characteristics of players with whom any player i never shared a table (player k in the illustration above) must have a strong enough impact on the behavior of player j to be considered a valid instrument. In this experiment, this is not found to be the case, so the results are merely summarized here for the sake of brevity.

Table 1.5 in the Appendix shows the results of IV regressions of the form described above, where the characteristics used to instrument the endogenous regressor (the average weighting choice among each player's direct table contacts) are the gender, age and education status of those players who could have influenced the player in question via some intransitive triad. Coefficients are generally found to be positive, which could be interpreted as suggesting that if a player shares tables with other participants who on average assign a higher weight to the criterion under consideration, then that player too will assign a higher weight on average to the same criterion. In most cases, however, results are found to be insignificant at the 5% level. More importantly, as alluded to above, the F-statistics associated with the first-stage regressions range from 0.322 to 3.943, which suggests that using intransitive triads in this way does not provide instruments with a sufficient influence over the weighting choices of other players to provide meaningful estimates.

It is possible to conduct similar regressions, with the inclusion of additional controls for 'cultural variables' (as were used in Tucker and Gastil (2013)) and political leaning (both self-reported and inferred from other survey questions). While the coefficients associated with the average weighting choice of neighbors are generally positive, which would support the findings of the dyadic regressions reported in Section 1.4, these results are generally not found to be statistically significant and are again based on instruments that are found to be too weak to provide meaningful results. Detailed tables reporting these findings are therefore omitted from this chapter.

1.5.2 Political Leaning as the Dependent Variable

This section reports the results of further regressions, whereby instead of being included as a control variable as in the model described above, political leaning on the five-point 'left-right' scale is now included as the dependent variable. The objective in such specifications is to determine whether players' political leaning itself was influenced by those with whom they shared a table during the course of the ACP.

The weighting choices that have been used as dependent variables in the regressions up to this point were recorded on the third day of the ACP, so table allocations on the final day of the ACP have been ignored up to now, given that they took place after weighting choices were made. The dependent variable used in this section, however, comes from survey responses that were given one year after the ACP. For this reason, players who shared a table on the last day of the ACP are taken as being directly linked in the network used here.

To state this more formally, letting x_{ij} again represent a dummy variable indicating whether players i and j shared a table, you have:

$$x_{ij} = \begin{cases} 1 & \text{if } T_i^1 = T_j^1,\ T_i^2 = T_j^2,\ T_i^3 = T_j^3, \text{ or } T_i^4 = T_j^4 \\ 0 & \text{otherwise} \end{cases}$$

By adjusting the definition of x_{ij} in this way, the average number of direct table contacts per player rises to 20.9 (from 15.9 using only the first 3 days), with the standard deviation rising to 1.7 (from 1.4 previously). The minimum number of direct table contacts is now 16, while the maximum is 24 (from 11 and 18 respectively).

Given that the problem of weak instruments continues to prevent the estimation of meaningful instrumental variables regressions, attention here is once again focused on dyadic regressions, which take each pairing as a separate observation.

The results themselves are only summarized here for the sake of brevity. Starting with regressions using differences between self-reported political leaning as the dependent variable, the coefficient indicating the effect of having shared a table is -0.03 in a regression model with no controls included. The coefficient also remains close to this level when gender, age, education, and internet access are controlled for in subsequent regressions. None of these results are found to be statistically significant, with t-statistics of around 0.9 in all cases.

Turning to dyadic regressions with differences between the inferred measure of political leaning as the dependent variable, the coefficient of interest is now around -0.04 both with and without the usual controls. Again, none of these coefficients are found to be significant, with t-statistics of around 1.1.

Dyadic regressions with differences in either measure of political leaning as the dependent variable therefore suggest that those pairings who shared a table are associated with political positions on the 'left-right' scale that were slightly closer together than those pairings who were not assigned to the same table on any of the four days of the ACP. However, regardless of which measure of political leaning is used, none of these effects are found to be significantly significant. On this basis, it is not possible to reject the null hypothesis that table allocations have no effect on either measure of political leaning.

The nature of these results is perhaps unsurprising, given the fact that ACP participants offered responses to both survey questions on political leaning a year after the completion of the ACP meetings. For this reason, any effect on political leaning coming from table contacts would have to persist for a year to show up in these findings. It is for this reason that political leaning as reported by either of the two measures offers more practical use as a control variable than as a dependent variable as it is being used here.

Beyond this measurement issue, it is also very plausible that while it is possible for players to influence one-another's decisions during discussions and debates (as the dyadic regressions on weighting choices above might suggest), political leaning is somehow beyond influence from others in such forums.

1.6 Conclusion

This chapter has used the randomized table allocations from the Australian Citizens' Parliament (ACP) to determine whether participants were influenced by those with whom they shared a table during the discussions. Participants were asked to assign weights to reflect the relative importance they attach to 11 criteria for an effective political system. By taking each possible pairing of participants and summing the differences between the weighting choices they assigned to each of the criteria, lower values are associated with player pairings whose weighting choices were more similar.

Results from dyadic regressions with the total difference in the weighting choices of participants as the dependent variable suggested that participants were indeed influenced by those with whom they shared a table. Those pairings who were randomly allocated to the same table during any of the first three days of the ACP were found to choose weights that were closer together than those assigned to different tables. The size of the effect in estimations based on the entire sample is around 1.4 weighting points across all criteria.

This implies that pairings who shared a table chose weights that were 0.13 weighting points closer together for each individual criterion. The size of this effect is found to be larger than those associated with other pairing characteristics.

Further regressions were also conducted which used the characteristics of players who could influence others as an instrument for their weighting choices, but instruments defined in this way were found to be too weak to offer meaningful results.

Finally, regressions which used the political leaning of participants as the dependent variable were also estimated. Findings were inconclusive and in no case was it possible to reject the null hypothesis that the ACP debates had no effect on participants' political positions. Given that the data on political leaning was collected a year after the ACP, however, it is also possible that debates did in fact influence members' political positions on the 'left-right' scale, but that this effect diminishes over time.

These results show that ACP participants were able to affect the opinions of others through their debates in the ACP. This finding is therefore supportive of the role of debate in political contexts, as well as in other social and professional contexts. The results reported here benefit from the fact that the ACP participants were chosen at random using stratified sampling to generate a group that is representative of the Australian population. A group of politicians may not necessarily behave in a similar way, however, as such a group may not be well represented by a sample of the wider population. Further work which considers politicians themselves would therefore provide an interesting comparison to these findings.

References

Borgatti, S. P., M. G. Everett, and L. C. Freeman (2002). UCINET for windows: Software for social network analysis.

Bramoulle, Y., H. Djebbari, and B. Fortin (2009). Identification of peer effects through social networks. *Journal of Econometrics*, 41–55.

Campos, C. and L. L. d. L. Fernanda (2012). Peers' influence on political choices: Evidence from random classroom assignments in college. *University of East Anglia Working Paper Series*.

Conley, T. (1999). GMM estimation with cross sectional dependence. *Journal of Econometrics*, 1–45.

Dey, E. L. (1997). Undergraduate political attitudes: Peer influence in changing social contexts. *Journal of Higher Education*, 398–413.

Fafchamps, M. and F. Gubert (2007). The formation of risk sharing networks. *Journal of Development Economics*, 326–350.

Fafchamps, M., A. Vaz, and P. C. Vicente (2013). Voting and peer effects: Experimental evidence from Mozambique. *Working Paper*.

Hubert, L. and J. Schultz (1976). Quadratic assignment as a general data analysis strategy. *British Journal of Mathematical and Statistical Psychology*, 190–241.

Kelejian, H. and I. Prucha (1998). A generalized spatial two-stage least squares procedure for estimating a spatial autoregressive model with autoregressive disturbances. *The Journal of Real Estate Finance and Economics*.

Krackardt, D. (1987). QAP partialling as a test of spuriousness. *Social Networks*, 171–186.

Lee, L.-f. (2003). Best spatial two-stage least squares estimators for a spatial autoregressive model with autoregressive disturbances. *Econometric Reviews*, 307–335.

Manski, C. F. (1993). Identification of endogenous social effects: The reflection problem. *Review of Economic Studies*, 531–542.

Moffitt, Robert, A. (2001). Policy interventions, low-level equilibria, and social interactions. In *Social Dynamics*. MIT Press.

New Democracy Foundation (2009a). Australia's first Citizen's Parliament.

New Democracy Foundation (2009b). Citizen's Parliament final report.

Sacerdote, B. (2001). Peer effects with random assignment: Results for Dartmouth roommates. *The Quarterly Journal of Economics*, 681–704.

Tucker, L. and J. Gastil (2013). Evidence of peer influence in the Citizen's Parliament. In *The Australian Citizens' Parliament and the future of deliberative democracy*. Pennsylvania State University Press.

Appendix

Table 1.5: IV regressions with the dependent variable in each column being the weights assigned to a certain criterion, controlling for gender, age and education. The explanatory variable of interest is the average weight assigned to the criterion in question by the players with whom each player shared a table. Instruments for the average weighting choice among player i's neighbors in these regressions are the characteristics of players who shared a table with one of the neighbors of player i, but not player i themself, forming an 'intransitive triad'.

	Fdm	Incl	Trans	AcInf	AcPol	EdAll	RspEn	DivMd	FrGov	ActCit	SimEl
Dependent variable avg. among neighbors	2.763*	2.586	0.605	3.158	2.581	2.063	2.841	-6.480	-0.0303	1.211	-1.589
	(2.01)	(0.91)	(0.50)	(1.10)	(1.21)	(1.06)	(1.05)	(-0.91)	(-0.01)	(0.75)	(-0.66)
Female respondent	-1.538	1.222	-0.682	2.370	-2.394	1.679	0.562	-2.634	-1.679	1.862	2.832
	(-0.62)	(0.32)	(-0.35)	(0.96)	(-1.09)	(0.53)	(0.29)	(-0.89)	(-0.66)	(1.07)	(1.07)
Age of respondent	-0.617	-0.0936	0.402	-0.535	0.308	-0.161	0.957	-0.897	1.257	-0.511	0.927
	(-0.91)	(-0.14)	(0.75)	(-0.64)	(0.59)	(-0.22)	(1.01)	(-1.30)	(1.05)	(-1.06)	(1.70)
Education of respondent	0.971	0.804	-0.274	0.101	0.871	-0.746	0.0286	1.005	0.176	-0.567	-0.447
	(1.28)	(1.26)	(-0.44)	(0.14)	(1.52)	(-1.07)	(0.04)	(0.90)	(0.28)	(-1.04)	(-0.62)
Proportion of female neighbors	-10.72	-9.733	4.474	7.998	2.018	-13.51	-3.983	-18.53	-0.448	8.513	16.04
	(-0.83)	(-0.34)	(0.45)	(0.61)	(0.15)	(-0.97)	(-0.39)	(-0.94)	(-0.02)	(0.94)	(1.08)
Avg. age of neighbors	2.227	1.654	3.145	1.559	-1.548	0.227	-3.394	-6.097	3.882	1.306	1.483
	(0.69)	(0.37)	(1.25)	(0.27)	(-0.61)	(0.06)	(-0.98)	(-0.93)	(0.39)	(0.47)	(0.48)
Avg. education of neighbors	-0.225	-1.568	-3.754	-0.307	-0.0661	1.067	0.208	3.125	0.210	-1.254	2.942
	(-0.07)	(-0.64)	(-1.32)	(-0.11)	(-0.03)	(0.38)	(0.05)	(1.07)	(0.04)	(-0.51)	(1.17)
Observations	147	147	147	147	147	147	147	147	147	147	147

t-statistics in parentheses, * implies $p < 0.05$

(1) The Fdm dependent variable refers to the Freedom criterion, with mean 18.2 and standard deviation 10.4

(2) The Incl dependent variable refers to the Inclusiveness criterion, with mean 7.4 and standard deviation 8.6

(3) The Trans dependent variable refers to the Transparency criterion, with mean 16.5 and standard deviation 8.7

(4) The AcInf dependent variable refers to the Access to Information criterion, with mean 5.8 and standard deviation 8.1

(5) The AcPol dependent variable refers to the Access to the political system criterion, with mean 6.8 and standard deviation 8.3

(6) The EdAll dependent variable refers to the Guaranteed education for all criterion, with mean 12.1 and standard deviation 9.6

(7) The RspEn dependent variable refers to the Respect for the environment criterion, with mean 5.7 and standard deviation 7.6

(8) The DivMd dependent variable refers to the Diverse media criterion, with mean 3.3 and standard deviation 5.4

(9) The FrGov dependent variable refers to the Justice and fairness in government criterion, with mean 11.2 and standard deviation 9.3

(10) The AcCit dependent variable refers to the Active citizenship criterion, with mean 5.5 and standard deviation 7.8

(11) The SimEl dependent variable refers to the Simplified electoral system criterion, with mean 6.1 and standard deviation 8.7

Statement of Originality

This chapter expands on earlier work published in the edited book *The Australian Citizens' Parliament and the Future of Deliberative Democracy.* That chapter was co-authored with Professor John Gastil, one of the editors. Professor Gastil offered me invaluable help in understanding the dataset and the design of the original experiment in 2009. The analysis reported in this chapter, however, is entirely my own and I received no external help in producing these results.

Statement from Professor Gastil:

> *"To whom it may concern, I has been a pleasure working with Luc Tucker, but rest assured, I am along for the ride. The piece we wrote on the Australian Citizens' Parliament (ACP) was his idea and method. I injected the "cultural cognition" angle, but that's really a matter of slotting a variable into an analysis I had never conceived. You're welcome to look through my record and you'll find nothing there with the approach Luc takes to the work. In fact, I've cited him many times (including at public talks) as what an academic dreams of when creating a big public dataset–that people will come along and find new ways of working with the data, in the spirit of collaboration but with an approach all their own. I've had a few people come along over the years promising to do so, but this is the first such effort to be published where the previously-unknown collaborator has taken a truly original approach to the data. I wish Luc every success in his work and am proud to have produced data that have helped advance his studies and scholarship."*

- John Gastil, Head and Professor, Communication Arts and Sciences and Political Science, Pennsylvania State University.

Chapter 2

Parliamentary Questions and the Probability of Reelection in the UK House of Commons

2.1 Introduction

In the UK House of Commons, the daily agenda typically consists of one or more parliamentary debates. These debates are usually focused on a particular government department, with the ministers responsible for that department facing questions posed by other members. As well as simply finding out the answers to their questions, such debates offer members the chance to hold the government to account for their policies, as well as raising awareness of particular issues. Alongside these objectives, this chapter focuses on the payoffs that members themselves can accrue by having their questions answered.

The present chapter exploits the institutional structure of the UK parliamentary system to create a quasi-experiment. This approach means that this chapter can hope to infer a causal link between the act of asking parliamentary questions and the probability of reelection

2.1.1 UK Parliamentary Questions Procedure

Since 1989, the order in which parliamentary questions are addressed has been determined by a random ballot (Sandford (2012)). From the pool of submitted questions, those to be answered are drawn in order, up to some pre-determined cut-off. This limit is chosen as the maximum number of questions that could conceivably be reached during the time allocated to the debate, the current maximum being 25 questions. All these questions are placed on the 'Order Paper' for the debate, whereas questions not drawn among this group are discarded.

The debate proceeds through these listed questions in numerical order, with each asking member allowed one 'follow-up' question. The 'Speaker' presiding over the debate can also exercise some influence over how many questions are reached on the Order Paper. In doing so, the Speaker faces a trade-off between ensuring that each question is treated adequately, and ensuring that as many questions as possible are given oral answers.

Given that the number of questions listed on the Order Paper is chosen as the maximum that could potentially be answered during the time allocated to the debate, this debating process usually results in a set of questions which are not reached, and therefore not given an oral answer in the House of Commons. Such questions listed on the Order Paper but not reached during the debate itself are given written answers.

There are various examples of international parliamentary systems that use random ballots to select which questions are asked in parliamentary bodies in the event that debates are over-subscribed (Canada and Scotland, for example, use similar ballots). A key component of the UK system, and one that is crucial to this analysis, is the recording of those questions not selected for direct oral answer. This facilitates the construction of a comparable control group, against which the answered questions can be compared.

2.1.2 Oral Versus Written Questions

It can safely be assumed that Members of Parliament who listed questions for oral answer have an interest in their questions being reached in the debate, given that the option of submitting questions specifically for written answer is also available to them, with such questions always guaranteed to be answered.

This can be stated more formally using an expected utility setting, where a member who would like to ask a question in Parliament has the choice of submitting that question for oral or written answer. Both are assumed to have the same cost (e.g. time and preparation), denoted c. All questions submitted for written answer are guaranteed a written response, which can be assumed to offer the member a guaranteed utility payoff U_w. This guaranteed payoff is assumed to make the member exactly indifferent between submitting a written question and not doing so ($U_w = c$).

By submitting a question for oral answer, the member faces a probability p that their question will be selected to go on the Order Paper. This implies a probability $1 - p$ that the question is not selected for the Order Paper, in which case their question receives no answer at all. This is assumed to offer the member a payoff zero. If selected for the Order Paper, the question then has a chance q of being reached in the debate. If the question is reached, it receives an oral answer, giving a payoff U_o, whereas if it is not reached, it receives a written answer.

By choosing to submit an oral question in the first place, the member has revealed that their expected payoff from doing so is greater than the cost of doing so, and also exceeds that of submitting a written question. This means the following two conditions must hold:

$$p\left(qU_o + (1-q)\,U_w\right) \geq c$$

$$p\left(qU_o + (1-q)\,U_w\right) - c \geq U_w - c$$

Given that $U_w = c$, these participation and incentive constraints are effectively equivalent, and can be rewritten as:

$$pU_w + pq\left(U_o - U_w\right) \geq U_w$$

Given that $pU_w \leq U_w$, the only way that this expression can be satisfied is if you have $U_o \geq U_w$. Furthermore, if either $p < 1$ or $q < 1$, it must be the case that $U_o > U_w$. So, if there is either any chance of not being selected for the Order Paper, or of the question not being reached in the debate, if a member has submitted an oral question, it must be the case that the payoff to that member from having their question answered on the floor of the House exceeds the payoff from having that same question receive a written answer.

This chapter takes the preferences described above as given, assuming throughout that members submitting questions for oral answer would prefer their questions to be reached in the debate, as opposed to receiving a written answer.

2.1.3 Parliamentary Questions Literature

The political science literature includes many studies regarding questions submitted in Parliament. Martin and Rozenberg (2012) list many international examples where data on parliamentary questions has been used make inferences about the role of legislators and the impact that these questions may have. Saalfeld (2011) uses questions submitted in the UK House of Commons during the 2005 Parliament to examine whether MPs with “visible-minority” backgrounds are more likely to ask questions relating to these minorities, finding in fact that the ethnicity of constituents plays a more prominent role. John and Bevan (2011) consider trends in the types of questions asked during Prime Minister’s Questions between 1997 and 2008. Using Irish data, Martin (2011) also looks in detail at the content of questions asked in Parliament, to assess to what extent questions are focused towards local as opposed to national issues.

Many studies have been conducted on the wider behavior of political representatives, such as their parliamentary voting behavior. Cowley and Stuart (2008) and Cowley and Stuart (2009) are examples which study such voting behavior in the UK case.

There is a wide literature in the area of voter responses to political behavior, even restricting attention to the UK case. In one example, Sanders and Norris (2005) consider the effect of political advertising on voter behavior. In another related paper, Sanders et al. (2007) consider the effectiveness of internet-based surveys in predicting voter behavior.

There has been very little work in the political science literature, however, on the way in which the political success of legislators is shaped by their behavior. An important reason for this, is that such studies face the problem of distinguishing whether any correlation is caused by the characteristics of politicians themselves, as opposed to their observed behavior. An exception is Loewen et al. (2013), which uses a natural experiment in the Canadian House of Commons to test whether political representatives who are chosen at random to propose legislation are more likely to achieve reelection. That study is therefore complimentary to the one presented here, in that both exploit randomizations within parliamentary institutions to reveal ways in which constituents reward certain behavior by their legislators.

To my knowledge, however, there is no study which uses the randomization process with which parliamentary questions are chosen to make inferences about their effects. This may be a result of the fact that in most international settings, the questions randomized out according to such processes are discarded, making it impossible to use those cases as a comparison group. Furthermore, even in the UK case there is no dataset available which records the answered and unanswered questions in one place. This chapter combines a number of different sources to conduct the analysis.

The use of this randomization forms the basis of all the findings reported in this chapter. Under an effective randomization, reported results cannot be attributed to self-selection into treatment or control groups, which in turn means that all unobserved characteristics of the members that make up these two groups should be equivalently distributed across the two groups.

2.1.4 Data Collection

The UK Parliament website lists the Order Papers for each day in the House of Commons since 27 October 1997.[1] This translates to three full parliamentary terms, leading up to elections in 2001, 2005, and 2010, with the exception of the first few months following the 1997 election.[2] These Order Papers contain all the questions listed for all the debates taking place on that day, in the order that they will be addressed, as well as a numeric code, which is assigned to each question.

Alongside these Order Papers, the UK Parliament also publishes online transcripts for all the debates which take place in the House of Commons on their 'Hansard' pages. These transcripts contain the questions from the Order Paper that were actually asked when the debates took place. By combining the two, it is possible to create a dataset which lists all the questions from the Order Papers, as well as whether each of these questions was reached within the time allocated to the debate, in which case it would have received an oral answer.

[1] The Order Papers for two days, 15 July 1999 and 18 January 2002, are missing, but of a total of 1657 days over the time period in question, these represent just 0.12% of the dataset.

[2] The 1997 General Election took place on 1 May. Following the election, oral debates restarted at the end of May, and continued through June and July. Parliament was then in recess until 27 October, from which point Order Papers are available online.

In 2010, the UK Parliament Library published a research paper containing a list of all Members of Parliament, between 1979 and 2010. This paper is used to gather background data on MPs, including gender, age, party affiliation and ministerial positions. The dependent variable of interest (reelection) is collected from datasets compiled by the UK Electoral Commission. In both cases, the data is matched with the data on individual questions, based on constituency and member names.

2.1.5 Research Questions

This chapter will consider whether the act of asking parliamentary questions affects the reelection prospects of Members of Parliament (MPs) in the UK House of Commons. If having a parliamentary question successfully chosen to be answered orally in the House of Commons is valued by constituents, this will be reflected in their voting behavior, and therefore the probability that the member is successful in achieving reelection.

A second related research question involves the degree to which the selection of parliamentary questions can truly be considered as random. In particular it is necessary to ascertain whether the randomization in ordering questions is effective in offering all members who submit questions an equal chance of having their question selected to receive an oral answer. Questions submitted by members who are then absent from the debates are not answered, and questions that are submitted by more than one member for the same debate are also treated altogether. Furthermore, the number of questions answered during the debate will depend on the speed at which the debate progresses. As all these factors may be non-random, it may be possible for members to circumvent the randomized nature of the debates to influence the probability that their question receives an oral answer.

Any non-randomness in the allocation to treatment and control groups (i.e. whether the question was reached during the debate) will have to be explicitly accounted for in the estimation methods that follow.

2.1.6 Chapter Outline

The next section of this chapter will set the scene by using a simple ordinary least squares estimation to determine whether members who have more questions selected for oral answer are more likely to be reelected. Section 2.3 will then highlight some ways in which the randomized procedure by which questions are selected to receive an oral answer could be undermined. Next, Section 2.4 will propose an alternative estimation method, based on a matched-sampling approach, which aims to address the problems posed by this endogeneity in the selection of questions to be given oral answers. Section 2.5 will provide the results from some robustness checks and the final section will outline the conclusions of the study.

2.2 Regression Analysis Based on Individual Members

2.2.1 A Basic Ordinary Least Squares (OLS) Specification

The first method used will be to take each Member of Parliament over a parliamentary term as a single observation, and count how many times that member was successful and unsuccessful in the ballot. The hypothesis behind such a model is that if the selection of questions to be answered is indeed a random one, then the inclusion of the number of unanswered questions in the regression would control for the fact that members who ask more questions are likely to have particular characteristics, which may also influence their probability of reelection.

In particular, once the total number of questions asked is controlled for (answered plus unanswered), it is assumed that the number of questions answered is effectively allocated at random, and is therefore exogenous in the regression equation.

- $i = 1, \ldots, I$ is the set of Members of Parliament (MPs)
- $t = 1, \ldots, T$ is the set of elections for which data is available (2001, 2005, 2010)
- y_{it} is a binary variable indicating whether member i was reelected in election t (dependent variable)[3]
- a_{it} is the number of questions asked by member i in the lead-up to election t that received oral answers

[3]Throughout this study, MPs who did not seek reelection are treated in exactly the same way as those who were unsuccessful. There is likely to be some degree of endogeneity involved in members' decisions as to whether to stand for reelection, with those members who suspect they are unlikely to win being disproportionately more likely not to stand.

- s_{it} is the number of questions submitted by member i in the lead-up to election t that did not receive an oral answer (not reached during the debate)

- X_{it} is a set of other characteristics of member i in the lead-up to election t (party, age, gender)

- E_{it} is a set of dummy variables indicating which election followed the tabling of the question

- C_{it} is a set of dummy variables capturing constituency fixed effects

Using these definitions, the basic model can be written as:

$$y_{it} = \gamma_0 + \gamma_1 a_{it} + \gamma_2 s_{it} + \gamma_3 X_{it} + \gamma_4 E_{it} + \gamma_5 C_{it} + \epsilon_{it}$$

In this model, if two members i and j share the same characteristics ($s_{it} = s_{jt} = s$, $X_{it} = X_{jt} = x$, $E_{it} = E_{jt} = e$, $C_{it} = C_{jt} = c$), including the number of unanswered questions, but member j has one more answered question than member i, you have:

$$E\left[y_{it} \mid a, s, x, e, c\right] = \gamma_0 + \gamma_1 a + \gamma_2 s + \gamma_3 x + \gamma_4 e + \gamma_5 c + E\left[\epsilon_{it} \mid a, s, x, e, c\right]$$
$$E\left[y_{jt} \mid a+1, s, x, e, c\right] = \gamma_0 + \gamma_1 \left(a+1\right) + \gamma_2 s + \gamma_3 x + \gamma_4 e + \gamma_5 c + E\left[\epsilon_{jt} \mid a+1, s, x, e, c\right]$$

The change in the expected probability of reelection from having an extra answered question is then given by:

$$E\left[y_{jt} \mid a+1, s, x, e, c\right] - E\left[y_{it} \mid a, s, x, e, c\right] = \gamma_1 + E\left[\epsilon_{jt} \mid a+1, s, x, e, c\right] - E\left[\epsilon_{it} \mid a, s, x, e, c\right]$$

The assumption that questions are chosen at random to be answered from those on the Order Paper implies that for any given combination of member characteristics and fixed effects, you have $E\left[\epsilon_{it} \mid a, s, x, e, c\right] \equiv E\left[\epsilon_{it} \mid s, x, e, c\right]$, which in turn implies that the effect of an increase in the number of questions asked by a member can be identified simply by calculating the resulting change in the probability of reelection, conditional on the number of unanswered questions:

$$\gamma_1 = E\left[y_{jt} - y_{it} \mid s, x, e, c\right] = \triangle E\left[y \mid s, x, e, c\right]$$

Table 2.1 shows the results of regressions using such specifications, with standard errors clustered at the member level. The first column in Table 2.1 includes no controls, and simply calculates the effect of members having an extra question answered and unanswered on the probability of reelection. In this simplest specification, with no controls, results suggest that members who have one more question answered over the course of a parliamentary term have a lower probability of being reelected.

The second column shows the results of a similar specification, but in this case including dummy variables to control for the parliamentary term for which the total number of questions has been calculated. After including these dummy variables, a member who asks a higher number of parliamentary questions is found to have a higher probability of reelection. Specifically, having an extra parliamentary question answered is found to increase the probability of reelection by 0.18 percentage points. This is found to be very close to significant at the 10% level, with a t-statistic of 1.64.

Columns 3 and 4 introduce some extra control variables, first for the party affiliation of members, and then for various other characteristics relating to seniority and gender. The introduction of these extra control variables slightly reduces the size of the coefficient of interest, as well as the t-statistic.

Discussion of the other coefficients is kept to a minimum, as none benefit from the same type of randomization as the one for answered questions, making it hard to infer causality. In short, members representing the Conservative Party are found to be more likely to be reelected. This reflects a generally increasing share of Conservative MPs in the House of Commons over the time-period covered here, following Labour's landslide victory in the 1997 election. Changing parties within an electoral term is found to have a strongly negative effect on reelection prospects, as many voters presumably retain a strong loyalty to the member's former party (for which they were probably previously elected). Older MPs and those with ministerial status are more likely to be reelected, but a longer period of service reduces a member's chances of reelection.

Column 5 includes the same set of control variables, and adds a set of dummy variables

Table 2.1: Linear probability models estimating the effect of having an extra question answered in the House of Commons on a member's probability of reelection. The dependent variable is a binary variable indicating whether the member was reelected in the following election (1 = 'yes', 0 = 'no'), with mean = 0.752 and standard deviation = 0.432).

	(1) Reelected	(2) Reelected	(3) Reelected	(4) Reelected	(5) Reelected
Total answered questions	-0.00209*	0.00176	0.00149	0.00140	0.00310*
	(-2.24)	(1.64)	(1.42)	(1.45)	(2.11)
Total unanswered questions	0.00485***	0.00115	0.000871	0.0000872	-0.00133
	(6.04)	(1.24)	(0.94)	(0.10)	(-0.89)
Conservative Party			0.0991***	0.0715**	0.276**
			(4.44)	(2.99)	(3.09)
Liberal Democrat Party			0.0398	-0.0259	0.111
			(1.12)	(-0.71)	(0.74)
Other party, excluding Labour			-0.108*	-0.0882	-0.0622
			(-2.02)	(-1.68)	(-0.39)
Changed party			-0.334***	-0.284**	-0.196
			(-3.73)	(-3.04)	(-1.27)
Ministerial position				0.0752**	0.153***
				(3.11)	(3.56)
Age				0.0338**	0.0777***
				(2.88)	(4.04)
Age squared				-0.000387***	-0.000842***
				(-3.39)	(-4.70)
Years service				-0.0231***	-0.0523***
				(-4.65)	(-6.27)
Years service squared				0.000482***	0.000820***
				(3.34)	(4.08)
Female				-0.0796**	-0.0183
				(-3.16)	(-0.25)
Election dummies	No	Yes	Yes	Yes	Yes
Constituency dummies	No	No	No	No	Yes
Observations	1659	1659	1659	1659	1659
Adjusted R^2	0.023	0.053	0.083	0.176	0.297

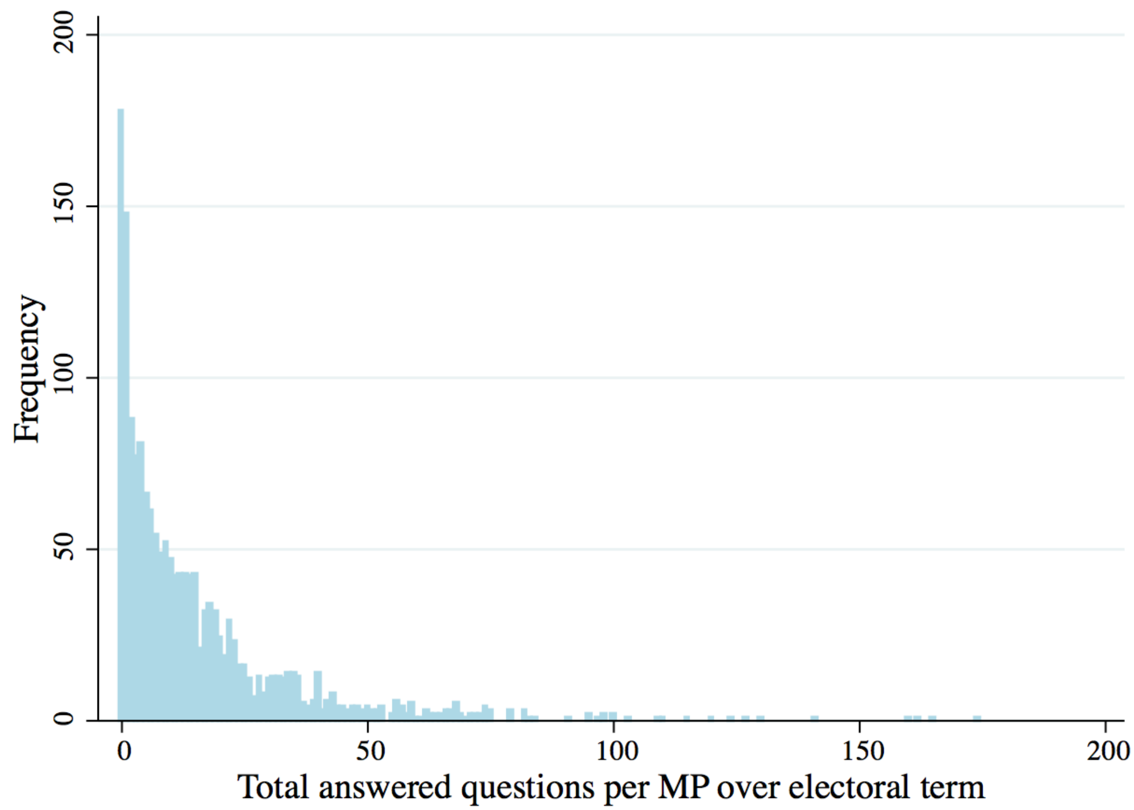
t statistics in parentheses

Standard errors clustered at member level

Dataset includes all questions over three parliamentary terms, except the first few months of 1997 (see footnote)

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure 2.1: Total answered questions for each member in each electoral term (mean = 15.38, standard deviation = 19.98)



to control for the constituencies that the members represent. The inclusion of these extra controls increases the size of the coefficient associated with having an extra question answered to 0.31 percentage points. This represents a large increase in the estimated coefficient, and is also associated with a t-statistic that represents statistical significance at the 5% level.

2.2.2 Interpretation of Initial Results

The identification strategy pursued in this section reports a coefficient which calculates the effect of a member asking one extra question during a parliamentary term. Among all the members in the sample, Figure 2.1 shows that there are many members who ask no questions at all over the parliamentary term. By contrast, however, the maximum number of questions successfully asked by a single MP in a parliamentary term is 174, with a standard deviation of 19.98 questions.

The implied linear relationship reported in Table 2.1 therefore suggests that a one standard deviation increase in the number of questions successfully asked by an MP will result in a 6.2 percentage point increase in the probability of reelection. This effect shows that not only is the effect statistically significant, but that it is also of sufficient magnitude to have a non-negligible impact on the probability of reelection.

2.2.3 A Critique of the Basic Methodology

The method employed up to this point faces the potential criticism, however, that in certain debates questions may be more likely to be answered than in others, in a way that violates the assumptions of the model.

For example, if one particular government department evokes a lot of interest among members, it may be that debates related to this department generate longer questions and answers, with more follow-up questions from other members, and therefore proceed at a slower pace. In such debates a lower proportion of listed questions will be answered than would be expected in other debates. If a member has a particular interest in this department, they may submit a disproportionate number of questions for answer in these debates. Such members would therefore be likely to see a high number of unanswered questions, and a low number of answered questions. This would violate the assumptions of the model above.

To address this problem, a matching-style method will be used, which in effect directly compares the reelection outcomes of members whose questions received an oral answer, with the outcomes of members who submitted a question to be answered in the same debate, but whose question was not reached in the allotted time.

2.3 Randomization In Practice

Throughout this chapter, much of the analysis will exploit the fact that the ordering of questions, which in turn determines which ones are asked, is determined at random. However, it is necessary to test the degree to which this randomization process is effective.

2.3.1 Sources of Endogeneity in the Allocation to Treatment

Even with the knowledge that the ordering of questions is determined by a random draw, it is still potentially possible for members with a particular interest in asking a question to influence their chances of doing so.

The first way that this could be achieved is for more than one member to agree to ask the same question. In such cases parliamentary procedure determines that the member with the first occurrence of a question on the Order Paper asks the question, but that other members who submitted the same question also have a chance to ask a follow-up question in the debate itself.

Clearly, not all cases where duplicate questions appear on the Order Paper are a result of this type of collusion between members. If a subject is particularly topical at the time when questions are submitted, it is also plausible to assume that members independently chose to submit the same question to the minister responsible. Furthermore, some members have been known to complain that their questions have been 'grouped' with others, when they considered the questions to be sufficiently distinct to warrant separate treatment. Whatever the reasons behind such duplications, it is possible that their occurrence is non-random, and therefore a system which treats such questions differently could have a non-random allocation to treatment or control.

If a member is unable to attend a debate for which they have a question listed on the Order Paper, then that question is not asked during the debate. Such non-attendance could be correlated with the characteristics of members, as well as with their position on the Order Paper (later questions are less likely to be reached), which means such absences could be non-random and also create an endogenous element to the selection of questions to receive oral responses.

Another factor which could undermine the randomness of questions answered is the speed at which the debate is conducted. Given that each debate continues for a fixed amount of time, the rate at which questions are answered will in turn affect how many questions are reached among those listed on the Order Paper. Any member taking part in the debate, and particularly the minister facing the questions, may be able to exert some control over which questions are reached. For example, a minister may provide long-winded answers to earlier questions, in cases where they see many potentially hostile questions from opposition party members further down the Order Paper.

The 'Speaker' in charge of the debate may also play a role in the speed at which debates are conducted. The Speaker is an elected member of Parliament, who presides over debates in the House. The Speaker has the authority to decide which (if any) members are able to ask supplementary questions related to those on the Order Paper, aside from the questioner themself, and is charged with ensuring that the rules of the House are upheld. On taking the role, the Speaker is required to sever all previous links to political parties, to ensure their impartiality in the position.

As an unbiased member, the Speaker faces a trade-off between on one hand ensuring that questions are answered fully, and on the other, ensuring that as many questions are answered as possible. Through this influence, the Speaker could also potentially undermine the randomness with which questions are selected for oral answer in the House of Commons.

2.3.2 Descriptive Statistics Comparing Answered and Unanswered Questions

2.3.2.1 Difference-in-Means Tests Based on the Complete Dataset

Table 2.2 compares the background characteristics of MPs among those questions that received oral answers in the House of Commons, with those among the questions that were not reached during debates, and therefore received written answers. The final column shows the results of t-tests looking for statistically significant differences between the mean values of these characteristics between the two groups. In this setting, all answered questions, duplicate or not, are treated in the same way, and questions not asked because the member was not in attendance are treated exactly the same as all other unanswered questions.

To be clear, given that the order of questions is determined completely at random, if the occurrence of duplicated questions, the non-attendance of members, and the speed of debates were not influenced by any of the members present, it should be expected that there would be no significant differences between the averages of any of the observable characteristics across the two groups.

Results in Table 2.2 show, however, that there are statistically significant differences between the two groups in terms of observable characteristics. Among those questions which are reached during the debate and therefore receive oral answers, there is a higher

Table 2.2: Descriptive statistics and difference-in-means tests for the treatment and control groups across the entire dataset. Taking each question asked as a separate data point, the treatment group includes all questions asked in Parliament ($N = 25521$), whereas the control group includes all questions submitted, but not reached during the debate ($N = 28934$).

	Answered questions	Unanswered questions	Difference
Conservative Party	0.354	0.330	0.0243**
			(3.144)
Liberal Democrat Party	0.114	0.103	0.0113**
			(3.034)
Other party, excluding Labour	0.0322	0.0336	-0.00138
			(-0.522)
Changed party	0.0139	0.0108	0.00309
			(1.549)
Ministerial position	0.458	0.422	0.0364***
			(4.849)
Age	53.34	52.88	0.461***
			(3.333)
Age squared	2924.0	2874.2	49.87***
			(3.335)
Years service	11.46	10.86	0.601***
			(4.107)
Years service squared	201.9	184.9	17.00**
			(3.079)
Female	0.185	0.183	0.00189
			(0.355)
Observations	54455		

Standard errors clustered at member level

t-statistics reported in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

proportion of opposition party members. Furthermore, among the questions that are reached, there is a higher proportion of questions posed by members who have held a ministerial position (defined as a position on the government or opposition front-benches in the House of Commons). Finally, questions in the treatment group are asked on average by older members, and members who have held office for a longer period of time.

Taken as a whole, Table 2.2 shows statistically significant differences on many of the observable characteristics between the treatment and control groups, despite the fact that questions are drawn and ordered completely at random.

2.3.2.2 The Cause of Observed Endogeneity

Given that it is not the main focus of this study, detailed discussions of the reasons behind these observed differences between the treatment and control groups are left to Chapter 3. In short, results point to the speed at which the debates progress being endogenously determined as the main cause of the non-random allocation to treatment. The matched sampling approach that will be introduced in Section 2.4 is found to be effective in eradicating these differences, while simultaneously ensuring that each debate contributes an equal number of questions to the treatment and control groups in the regressions that follow.

Specifically, results point to the Speaker being more likely to conduct debates in such a way as to admit questions from members representing opposition parties, and who have higher levels of authority. This is the first time that such a natural experiment has been used to report such a finding, and warrants further attention elsewhere. The focus of the rest of this chapter, however, will be to devise an estimation approach which addresses this non-random element in the determination of which parliamentary questions receive an oral answer.

2.4 Ordinary Least Squares Based on Matched Sampling

To control for the observed non-randomness in determining which parliamentary questions receive oral answers in the UK House of Commons, a method will be used which drops those observations most likely to be allocated to the treatment or control group in a non-random way. Observations are also dropped in such a way that each debate is equally represented in the treatment and control groups. As such, this method also simultaneously controls for the influence that specific debates might have on outcomes, by ensuring that any such effects influence the treatment and control groups in equal measure. This method is akin to a matching estimator (Rosenbaum and Rubin (1983); LaLonde (1986); Heckman et al. (1998); Angrist and Lavy (2001); Diamond and Sekhon (2012); Smith and Todd (2005)), where questions in the treatment group are matched with a corresponding question in the control group, based on the debate for which they were submitted. As with a simple matching estimator, each observation that received the treatment (having a question answered in this case) is effectively compared to a single counterpart that did not receive the treatment (an unanswered question), which is deemed comparable on the basis of some observed characteristic, which in this case is the debate for which the questions were submitted. Ho et al. (2007) propose a similar approach as a means for reducing a model's dependence on the parametric assumptions that are made after the data has been "preprocessed" in this way, following related work by Rubin (1973) and Rosenbaum and Rubin (1984).

2.4.1 Matched Sampling Methodology

Questions where the member was not present at the debate (1932 questions, or 3.5% of the dataset) and duplicated questions (1451 questions, or 2.7% of the dataset) are not subject to the same randomization process as other questions, so both types of question are removed from the dataset for the results reported in this section.

After removing such cases, the matched sampling method involves dropping observations whenever there are an unequal number of questions in the treatment and control group for a given debate. To simultaneously solve the problem of non-random allocation to the treatment and control groups, through endogeneity in the speed at which the debate is conducted, the questions chosen to be dropped are those closest to the cutoff point (at which the debate ended), as these are seen as the most likely to be subject to non-random allocation to treatment or control.

This can be shown more formally as follows:

- i represents the set of members
- d represents the set of debates
- a_{idt} is a binary variable indicating whether the question asked in debate d by member i received an oral answer

Let n_{id} represent the number at which the question of member i is drawn to be asked in the debate. Furthermore, let N_d represent the total number of questions listed on the Order Paper for the debate. This is a subset of the total number of questions submitted for the debate (regardless of whether they made it onto the Order Paper), itself denoted S_d. Finally, let $\overline{n}_d$ represent the number assigned to the last question reached in debate d, i.e. the 'cutoff' point.

Now you have:

$$a_{idt} = \begin{cases} 1 & \text{if } n_{id} \leq \overline{n}_d \\ 0 & \text{otherwise} \end{cases}$$

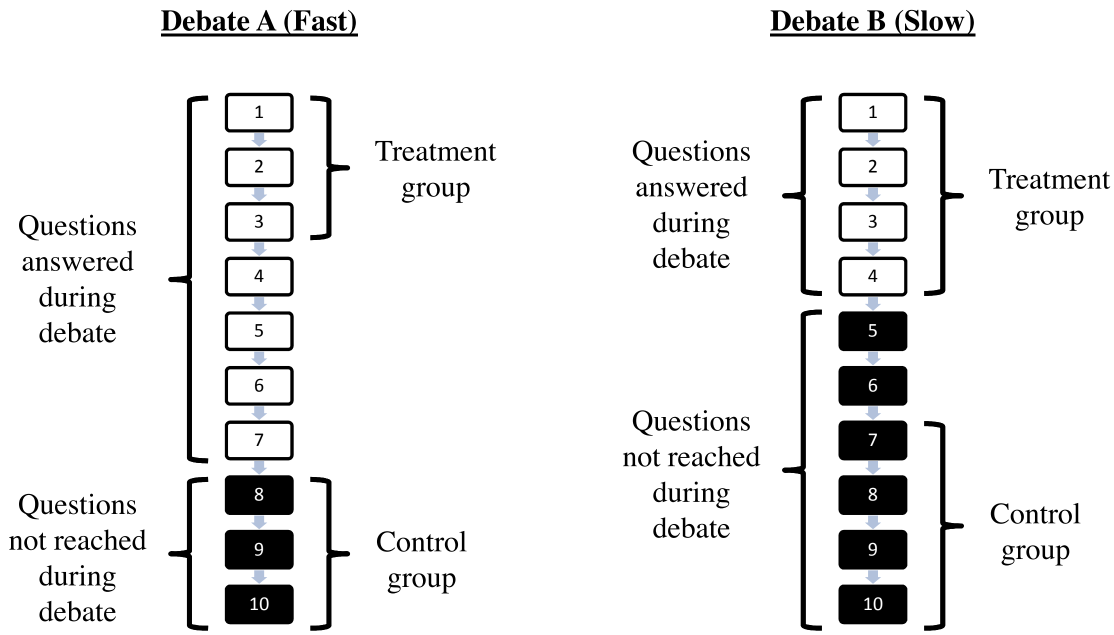
Now, if $\overline{n}_d > {}^{N_d}\!/_2$, this means that more than half of the questions listed for answer in debate d received an oral answer. In such cases, the last questions from the treatment group will be dropped from the analysis. More specifically, any question such that $N_d - \overline{n}_d < n_{id} \leq \overline{n}_d$ will be dropped from the sample if $\overline{n}_d > {}^{N_d}\!/_2$.

Conversely, if less than half of the questions listed on the Order Paper for debate d received an oral answer ($\overline{n}_d < {}^{N_d}\!/_2$), then the first questions from the control group will be dropped from the analysis. In particular, any question such that $\overline{n}_d < n_{id} \leq N_d - \overline{n}_d$ will be dropped from the sample if $\overline{n}_d < {}^{N_d}\!/_2$.

To illustrate, Figure 2.2 shows the questions listed on the Order Paper for two debates, one which proceeds fast (Debate A), and another which proceeds slowly (Debate B).

Debate A in Figure 2.2 shows the case where more than half of the questions listed on the Order Paper are reached during the debate (7 of the 10 questions listed, illustrated as clear boxes). If the number of questions answered in such debates is non-random (as was shown to be the case in Section 2.4), the questions which are likely to have particular (non-random) characteristics, are those that were reached only because the debate proceeded

Figure 2.2: Two example debates, one where more than half the questions are answered ($\overline{n}_A > {}^{N_A}\!/2$), and another where less than half are answered ($\overline{n}_B < {}^{N_B}\!/2$)



quickly (questions 4 to 7 in this case). The questions at the start of the debate would have been reached anyway, and those at the end were not reached, even though the debate proceeded quickly. In debates where more than half of the questions are reached, the treatment group is taken as the first questions in the debate, up to the number of questions which matches the size of the control group (questions 1 to 3 in Debate A).

In Debate B in Figure 2.2, less than half of the questions are reached during the debate itself, so in this case the control group is larger. In such cases, any non-randomness in the speed at which the debate is conducted is likely to have resulted in fewer questions being answered than would otherwise have been the case. This means that the questions most likely to be non-random are those appearing immediately after the cutoff at which the debate ended. Again, to ensure equal numbers in the treatment and control groups, some observations must be dropped, and in this case, it is questions immediately after the cutoff which are removed.

Table 2.3: Descriptive statistics and difference-in-means tests for the treatment and control groups with both groups equivalently distributed across debates. Taking each question asked as a separate data point, the treatment group includes all questions asked in Parliament ($N = 17193$), whereas the control group includes questions not reached during the debate ($N = 17193$).

	Answered questions	Unanswered questions	Difference
Conservative Party	0.335	0.341	-0.00535
			(-0.986)
Liberal Democrat Party	0.111	0.106	0.00465
			(1.437)
Other party, excluding Labour	0.0332	0.0328	0.000349
			(0.178)
Changed party	0.0132	0.0117	0.00145
			(1.230)
Ministerial position	0.438	0.436	0.00279
			(0.493)
Age	52.98	53.05	-0.0718
			(-0.727)
Age squared	2885.5	2892.9	-7.410
			(-0.696)
Years service	11.03	11.03	0.00151
			(0.0158)
Years service squared	190.5	189.4	1.109
			(0.305)
Female	0.187	0.186	0.000582
			(0.131)
Observations	34386		

Standard errors clustered at member level
t-statistics reported in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

2.4.2 Descriptive Statistics Under Matched Sampling

Before reporting the results of the ordinary least squares estimator based on the matched sample, it is necessary to consider whether the process of dropping observations has any effect on the comparability of the treatment and control groups, by looking at the descriptive statistics of the two groups in terms of observable characteristics of members.

Previously in Section 2.4, when looking at the dataset as a whole, it was shown that there were statistically significant differences between the treatment and control groups. In particular, the questions that were reached during the debates were on average asked more often by opposition members, with a higher average age, ministerial status, and length of parliamentary service.

Table 2.3 shows that the process of dropping observations has resulted in the treatment and control groups becoming far more comparable, with no statistically significant differences reported between any of the observable characteristics of the members asking questions in either group.

This supports the claim that it is those questions closest to the cutoff which are the cause of the statistically significant differences between the two groups that were found previously when considering the entire sample. The matched sampling approach used here is effective in eradicating those differences because it disproportionately removes the questions closest to the cutoff, which are the source of the bias.

2.4.3 Estimation Based on the Matched Sample

The first key difference between the following estimation strategy and the OLS framework reported in Section 2.2 is that in this case each observation corresponds to a question, as opposed to a member. The second is that in this specification, comparisons are made between members who successfully and unsuccessfully answered questions, so the control group is now exclusively made up of members whose questions were listed on the Order Paper, but were not asked. Members who had no questions listed on the Order Paper during a parliamentary term do not contribute to the findings. By contrast, in the first specification, non-ministerial members who asked no questions during the parliament were counted as zeros.

Once the dataset has been reduced in this way, the effect of a question being asked as opposed to receiving a written answer is estimated using ordinary least squares (OLS) on the remaining observations. Formally:

- $t = 1, \ldots, T$ represents the set of elections.
- y_{idt} is a binary variable reflecting the reelection outcome of a member i in election t, who asked a question in debate d (dependent variable).
- q_{idt} is a binary variable indicating whether the question of member i in debate d received an oral answer (1), or was not reached and thus received a written answer (0).
- X_{idt} is a set of control variables associated with member i at the time of debate d.
- D_{idt} is a set of dummy variables capturing debate fixed effects.
- C_{idt} is a set of dummy variables capturing constituency fixed effects.

Using these definitions, this model can be written as:

$$y_{idt} = \beta_0 + \beta_1 q_{idt} + \beta_2 X_{idt} + \beta_3 D_{idt} + \beta_4 C_{idt} + \epsilon_{idt}$$

In this specification, given that a member i had a question selected to be placed on the Order Paper in debate d, the probability that the question was answered is 0.5, independent of any characteristics of the member asking the question, the constituency they represent, or the debate for which the question was submitted. This means that any effect from q_{idt} on the probability of reelection can be attributed to q_{idt} itself, as opposed to any other characteristics (observed or otherwise) associated with the observation. Some controls are still included in the estimated regressions, however, given that they improve the precision of the findings. Standard errors will once again be clustered at the member level in all the results that follow.

Reducing the dataset as outlined above, but without any further controls, Table 2.4 shows that a question being reached in the debate increases the probability that the member who asked the question was reelected in the following election by 0.42 percentage points over another member, whose question was on the Order Paper but was not answered (Column 1). This effect is found to be far from significant at the 5% level, however, with a t-statistic of 0.98.

The subsequent two columns (Column 2 and Column 3) each include further control variables, first for party affiliation, and next for various other member characteristics. The adjusted-R^2 value jumps from zero to 0.18 after including these variables. The coefficient of interest remains close to 0.004, implying that among the answered questions the members who posed the questions have a probability of being reelected that is 0.40 percentage points higher than it would have been had their question appeared on the Order Paper, but too far down the list to receive an oral answer.

After including a set of dummy variables for each of the constituencies in the sample (Column 4), the adjusted-R^2 jumps to 0.59. It is unsurprising that the inclusion of variables which control for the constituency represented by each member explains a large part of the variation in reelection probabilities, given the strength of geographical voting norms which exist among the UK electorate. The coefficient of interest remains fairly constant

Table 2.4: Linear probability models with reelection at the forthcoming election as the dependent variable (mean = 0.823, standard deviation = 0.381), based on the sub-sample of the dataset which ensures that the distribution of debates is equivalent across the treatment and control groups. The treatment group includes questions reached in the debate, and control group includes all questions further down the Order Paper and therefore not asked ($N = 17193$ in both cases).

	(1) Reelected	(2) Reelected	(3) Reelected	(4) Reelected	(5) Reelected
Question asked	0.00419	0.00447	0.00396	0.00428	0.00421
	(0.98)	(1.06)	(0.99)	(1.49)	(1.48)
Conservative Party		0.0932***	0.0396	0.316***	0.370***
		(3.52)	(1.20)	(3.86)	(4.27)
Liberal Democrat Party		0.0690	-0.0171	0.434**	0.483***
		(1.85)	(-0.37)	(2.96)	(3.34)
Other party, excluding Labour		-0.313***	-0.247*	-0.110	-0.0376
		(-3.46)	(-2.55)	(-0.57)	(-0.20)
Changed party			-0.240	-0.145	-0.151
			(-1.60)	(-0.65)	(-0.74)
Ministerial position			0.0680*	-0.0229	0.0317
			(2.03)	(-0.41)	(0.57)
Age			0.0481**	0.145***	0.134***
			(3.11)	(5.45)	(5.44)
Age squared			-0.000534***	-0.00152***	-0.00140***
			(-3.44)	(-6.02)	(-6.02)
Years service			-0.0163**	-0.0407***	-0.0348***
			(-2.78)	(-4.11)	(-3.54)
Years service squared			0.000305	0.000684**	0.000539*
			(1.80)	(2.67)	(2.10)
Female			-0.0830**	-0.0686	-0.0295
			(-2.95)	(-0.79)	(-0.36)
Constituency dummies	No	No	No	Yes	Yes
Debate dummies	No	No	No	No	Yes
Observations	34386	34386	34386	34386	34386
Adjusted R^2	0.000	0.040	0.176	0.586	0.593

t statistics in parentheses

Standard errors clustered at member level

Dataset includes all questions over three parliamentary terms, except the first few months of 1997 (see footnote)

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

at just over 0.004, but is not found to be statistically significant at the 5% level, with a t-statistic of 1.49.

The inclusion of a set of dummy variables controlling for debate-specific effects has very little effect on the adjusted-R^2 (Column 5). This in unsurprising, given that the matched sampling approach used here to remove those questions most likely to have been allocated to treatment or control in a non-random way, also implies that each debate is equally represented in the treatment and control groups. The effects of individual debates is therefore already being controlled for through the model specification, which means that the inclusion of debate-fixed effects adds little to the explanatory power of the model.

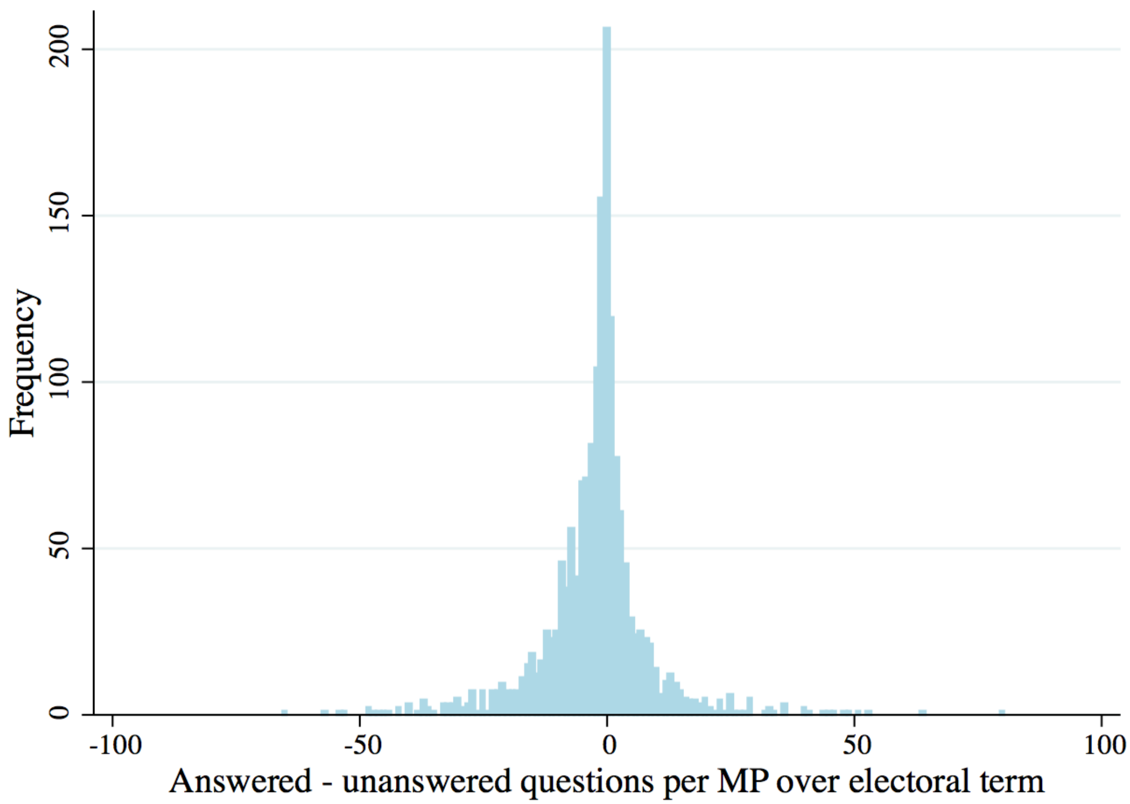
2.4.4 Interpretation of Findings

Results from this adapted matched sampling model show that after controlling for member characteristics, as well as fixed effects associated with constituencies and debates, the effect of having a question answered, as opposed to being placed on the Order Paper, but not being reached and therefore remaining unanswered, is to increase the member's probability of reelection by 0.42 percentage points.

The control group here consists of questions listed on the Order Paper which did not receive an oral answer. When interpreting these reported findings, it is therefore the difference between the number of answered and unanswered questions which is of interest. In this sense, the most successful member in the dataset over the course of an electoral term had 174 questions answered, compared with 94 unanswered, implying a difference of +80 questions. Conversely, the most unsuccessful member only had 35 questions answered, compared with 100 questions unanswered, implying a difference of -65 questions. Figure 2.3 plots all such differences across all members and all electoral terms.

The standard deviation associated with the number of answered minus unanswered questions across all the members is 11.05 questions. A coefficient of 0.004 therefore implies that a member who has a number of answered minus unanswered questions one standard deviation higher than another member should expect to have their chances of reelection increased by 4.6 percentage points.

Figure 2.3: Answered questions minus unanswered questions for each member in each electoral term (mean = -2.06, standard deviation = 11.05)



2.5 Extensions and Robustness Checks

2.5.1 The Complete Dataset

Given that the method employed in Section 2.4 involves dropping some observations, it is informative to consider whether the results would have been affected if the dataset had not been reduced in this way.

Results in Table 2.4 showed a positive relationship between having questions answered as opposed to unanswered, although the effect was not found to be statistically significant at the 5% level. It is possible to run a similar analysis, but without dropping observations in the same way. These specifications rely on a set of dummy variables capturing debate fixed effects to control for the effects of different debates. However, the benefits from the larger dataset come at the price of including those observations deemed most likely to be allocated to the treatment or control group in a non-random way.

For the sake of brevity, the results are left to the Appendix, where Table 2.5 reports the results of similar regressions as those in Table 2.4, but based on the entire dataset. In short, these specifications show that the results reported in Table 2.4 are not reliant on the use of the matched sampling approach, and are equally evident when considering the dataset as a whole. Once all control variables are included in the specification, questions that receive an oral answer are found to be asked by members who are 0.40 percentage points more likely to be reelected, as opposed to those questions that appeared on the Order Paper but were not asked. This effect is roughly equivalent to the one found when the dataset was reduced under the previous matched sampling approach. Furthermore, owing to the fact that this finding is based on a larger sample size, this result is found to be statistically significant at the 10% level, with a t-statistic of 1.69.

2.5.2 Intention-to-Treat Specifications

An alternative method for which results are robust to any potential non-randomness in the selection of questions to receive an oral answer is to use an 'intention-to-treat' (ITT) specification. Again, this specification is based on the full dataset where each observation corresponds to a parliamentary question.

If a question appears towards the top of the Order Paper (a 'low-numbered' question), it is taken as part of the treatment group in this model, whereas any question further down the Order Paper is placed in the control group (a 'high-numbered' question). Across all the debates recorded in the dataset, 47% of the questions listed on the Order Paper receive oral answers. In this ITT analysis, the first 47% of questions in each debate are therefore counted as 'low-numbered', with all the remaining questions marked as 'high-numbered'. Given that the position of a question on the Order Paper cannot be influenced by any of the participants in the debate, this allocation is truly exogenous.

Results listed in Table 2.6 in the Appendix show a consistently positive effect from having a 'low-numbered' question on the Order Paper, as opposed to a 'high-numbered' question. The effect is found to be smaller than the estimated effect from Tables 2.4 and 2.5 of actually having a question answered, however. With all controls included, questions that are assigned a low number on the Order Paper are found to be asked by members who are 0.16 percentage points more likely to be reelected. While these results are far from statistically significant, they continue to suggest a positive relationship between members' randomly allocated positions on the Order Paper and their subsequent probability of reelection.

2.5.3 The Ordinary Least Squares Specification Based on the Reduced Sample

A final robustness check involves running the basic ordinary least squares specification proposed in Section 2.2 but using the reduced dataset from the matched sampling approach. The results are again left to the Appendix (Table 2.7), and show a slightly larger positive effect effect than was found originally when counting all questions from the entire sample. Specifically, once all controls are included, the act of asking an extra parliamentary question is found to increase a member's probability of reelection by 0.32 percentage points, whereas the effect was estimated as 0.31 percentage points in Section 2.2. Furthermore, this effect is again found to be statistically significant at the 5% level, with an associated t-statistic of 2.11.

2.6 Conclusion

This chapter aimed to test whether the asking of oral parliamentary questions in the House of Commons increases a member's probability of being reelected to Parliament in the following election.

The way in which constituents choose their political representatives is of great interest to social scientists. Economists should be particularly interested in the degree to which legislators have an incentive to partake in day-to-day parliamentary business. The relationship between a political representative and their constituents can be thought of in a principal-agent setting, where constituents observe some proportion (or signal) of the work of their representative over a parliamentary term before choosing whether to reelect them for the following term. A crucial part of this relationship is whether constituents can be shown to reward parliamentary activity in their voting behavior.

While many previous studies have considered the effect of legislator behavior on election outcomes, none have considered the impact of asking parliamentary questions on reelection chances. A key reason for this is that members who ask many parliamentary questions are likely to have particular characteristics, which may be unobservable. A study which does not address this issue would not be able to separate the effect of members' parliamentary activity from the effect of these unobservable characteristics on reelection outcomes. By exploiting a natural experiment within the institutional structure of the House of Commons, this chapter is able to rule out the impact of unobservable characteristics by assuming them to be equally distributed among the treatment and control groups, and therefore makes a valuable contribution to the understanding of the relationship in question.

The identification strategy used to answer this question relied on the randomized way in which parliamentary questions are ordered, which in turn affects their probability of receiving an oral answer. In particular, members wishing to ask a parliamentary question are invited to submit their questions ahead of the debate. Once submitted, a random ballot is used to determine which of these questions appear on the Order Paper, and the order in which they will be answered. This ballot has a direct effect on which questions receive an oral answer, because time constraints usually dictate that only the first questions listed on the Order Paper will be reached during the time allotted to the debate.

A basic ordinary least squares specification counting the number of answered and unanswered questions for each MP over each parliamentary term revealed that after controlling for the number of unanswered questions, members with a higher number of answered questions were more likely to be reelected in the following election.

While the ordering of questions is determined at random, this finding relies on the idea that each time a member's question appears on the Order Paper, there is an equal (or at least exogenous) chance that their question receives an oral answer. One way in which this assumption could potentially be violated would be if any of the debate participants, including the Speaker, exerted some control over the speed at which debates progressed, and therefore the number of questions that received an oral answer in debates.

In fact it has been shown that there exist some statistically significant differences between the observable characteristics of the members whose questions receive an oral answer, and those members whose questions are selected to be placed on the Order Paper for debates, but are not reached and therefore receive written answers. Further analysis in Chapter 3 has been conducted into the reasons behind these differences. Results suggest that the influence of the Speaker plays a key role.

To account for this non-random element in determining whether questions are answered, a matched sampling approach was adopted, which dropped those observations most likely to have been allocated to the treatment or control group in a non-random way. The method also ensures that debates are equally distributed across the treatment and control groups, which implies that once a member's question is listed on the Order Paper, it has a probability of being answered of 0.5. By comparing means across the same set of observable characteristics between the treatment and control groups (answered and unanswered questions), it was shown that this matched sampling approach was successful in eliminating the statistically significant differences between the two parts of the sample. Using this reduced sample, it was possible to estimate the effect on reelection probability of having a question answered in Parliament, as opposed to submitting a question which was not reached during the debate (and instead received a written answer).

Results again showed a positive relationship between having questions answered in

Parliament and the probability of reelection, of the same order of magnitude as was found under the basic ordinary least squares estimation. To ensure that this positive result was not caused by reducing the sample in this way, a similar analysis was conducted using the entire dataset, and found the results unchanged. While the size of the effect was reduced under an intention-to-treat (ITT) specification, the results remained positive under all combinations of control variables.

This chapter says nothing about the paths of such influence and this could be a topic for future research. While debates are televised, it seems implausible to imagine that a substantial proportion of the electorate would watch these debates directly. It seems more likely that the media has an important role to play, for example through local newspapers reporting the actions of legislators at the national level. Personal networks within the constituency could also play an important role. As an example, a constituent may raise a very specific concern to their local MP, who might then choose to pursue the issue at the national level by posing the question directly to the minister responsible. In such cases, while the issue itself may be narrow and therefore not deemed newsworthy, other constituents who hear of this episode via word-of-mouth may be heartened by the legislator's willingness to pursue the issue. Such sentiments could then be reflected in election outcomes.

To summarize, the motivation of this chapter was to exploit a natural experiment brought about by the institutional structure in the House of Commons, to determine whether members who ask parliamentary questions are more likely to be reelected by their constituents. Initial results found this to be the case, and significantly so, but faced the potential criticism that the randomization process could potentially have been undermined. A matched sampling approach was used to reduce the dataset to those observations least likely to have been affected by any non-random allocation to the treatment or control group. This was shown to result in a dataset where the treatment and control groups were comparable in terms of all observable characteristics, as should be expected under a randomization. Results based on this reduced sample were found to support the original finding.

References

Angrist, J. and V. Lavy (2001). Does teacher training affect pupil learning? Evidence from matched comparisons in Jerusalem public schools. *Journal of Labor Economics*, 343–369.

Blackburn, R. and A. Kennon (2003). *Parliament: Functions, Practice and Procedures*. Sweet and Maxwell.

Clarke, H. D., D. Sanders, M. C. Stewart, and P. F. Whiteley (2009). *Performance Politics and the British Voter*. Cambridge: Cambridge University Press.

Cowley, P. and M. Stuart (2008). *A Rebellious Decade: Backbench Rebellions under Tony Blair, 1997-2007*. Palgrave.

Cowley, P. and M. Stuart (2009). There was a doctor, a journalist and two Welshmen: The voting behaviour of independent MPs in the United Kingdom House of Commons, 1997-2007. *Parliamentary Affairs*, 19–31.

Diamond, A. and J. S. Sekhon (2012). Genetic matching for estimating causal effects: A general multivariate matching method for achieving balance in observational studies. *Review of Economics and Statistics*.

Franklin, M. and P. Norton (1993). *Parliamentary Questions*. Study of Parliament Group. Oxford: Clarendon Press.

Gay, O., R. Cracknell, J. Hardacre, and J. Fessey (2010). Members 1979 - 2010.

Heckman, J. J., H. Ichimura, and P. Todd (1998). Matching as an econometric evaluation estimator. *The Review of Economic Studies*, 261–294.

Ho, D. E., K. Imai, G. King, and E. A. Stuart (2007). Matching as nonparametric preprocessing for reducing model dependence in parametric causal inference. *Political Analysis*, 199–236.

House of Commons Information Office (2010). Parliamentary questions. Factsheet P1 Procedure Series.

House of Commons Information Office (2013). Parliamentary questions: Brief guide.

John, P. and S. Bevan (2011). Project report on Prime Minister's Parliamentary Questions. UK Policy Agendas Project.

LaLonde, R. J. (1986). Evaluating the econometric evaluations of training programs with experimental data. *The American Economic Review*, 604–620.

Loewen, Peter, J., R. Koop, E. Settle, Jaime, and H. Fowler, James (2013). A natural experiment in proposal power and electoral success. *American Journal of Political Science*.

Marleau, R. and C. Montpetit (2009). House of Commons procedure and practice. 2nd Edition.

Martin, S. (2011). Using parliamentary questions to measure constituency focus: An application to the Irish case. *Political Studies*, 472–488.

Martin, S. and O. Rozenberg (2012). *The Roles and Function of Parliamentary Questions*. Library of Legislative Studies.

May, T. E. (2011). *A Treatise upon the Law, Privileges, Proceedings and Usage of Parliament* (24 ed.). London: LexisNexis.

Rosenbaum, P. R. and D. B. Rubin (1983). The central role of the propensity score in observational studies for causal effects. *Biometrika*, 41–55.

Rosenbaum, P. R. and D. B. Rubin (1984). Reducing bias in observational studies using subclassification on the propensity score. *Journal of the American Statistical Association*, 516–524.

Rubin, D. B. (1973). The use of matched sampling and regression adjustment to remove bias in observational studies. *Biometrics*, 185–203.

Saalfeld, T. (2011). Parliamentary questions as instruments of substantive representation: Visible minorities in the UK House of Commons, 2005-10. *The Journal of Legislative Studies*, 271–289.

Sanders, D., H. D. Clarke, M. C. Stewart, and P. Whiteley (2007). Does mode matter for modeling political choice? Evidence from the 2005 British Election Study. *Political Analysis*, 257–285.

Sanders, D. and P. Norris (2005). The impact of political advertising in the 2001 U.K. General Election. *Political Research Quarterly*, 525–536.

Sandford, M. (2012). House of Commons background paper: Parliamentary questions: Recent issues.

Smith, J. A. and P. E. Todd (2005). Does matching overcome LaLonde's critique of non-experimental estimators? *Experimental and non-experimental evaluation of economic policy and models*, 305–353.

The Scottish Parliament (2012). Guidance on parliamentary questions. 1st Edition.

Walgrave, S. and R. Vliegenthart (2009). Agenda-setting in parliamentary questioning: The government-opposition dynamic in a comparative perspective. *APSA 2009 Toronto Meeting Paper*.

Appendix

Table 2.5: Regressions controlling for debate fixed effects, counting each question (answered or unanswered) as an observation, based on the whole sample. The dependent variable is a binary variable indicating whether the member asking the question was reelected in the following election, with mean = 0.820 and standard deviation = 0.384.

	(1) Reelected	(2) Reelected	(3) Reelected	(4) Reelected	(5) Reelected
Question asked	-0.0194**	0.00615	0.00512	0.00486	0.00396
	(-3.01)	(1.72)	(1.46)	(1.46)	(1.69)
Conservative Party			0.119***	0.0496	0.342***
			(4.26)	(1.46)	(3.92)
Liberal Democrat Party			0.0949*	-0.0104	0.411**
			(2.53)	(-0.23)	(2.76)
Other party, excluding Labour			-0.303***	-0.244**	-0.0593
			(-3.62)	(-2.70)	(-0.33)
Changed party				-0.236	-0.162
				(-1.87)	(-0.81)
Ministerial position				0.0907**	0.0294
				(2.80)	(0.53)
Age				0.0422**	0.137***
				(2.92)	(5.58)
Age squared				-0.000471**	-0.00143***
				(-3.26)	(-6.17)
Years service				-0.0104	-0.0365***
				(-1.79)	(-3.68)
Years service squared				0.000150	0.000599*
				(0.88)	(2.34)
Female				-0.0695**	-0.0139
				(-2.59)	(-0.17)
Debate dummies	No	Yes	Yes	Yes	Yes
Constituency dummies	No	No	No	No	Yes
Observations	54455	54455	54455	54455	54455
Adjusted R^2	0.001	0.054	0.101	0.209	0.598

t statistics in parentheses

Standard errors clustered at member level

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2.6: Linear probability models estimating the effect of having a question high up the Order Paper (likely to be answered), as opposed to a question lower down the Order Paper (unlikely to be answered) on the probability of reelection. This represents an 'intention-to-treat' (ITT) model, where reelection is captured by a binary variable, with mean = 0.820, standard deviation = 0.384.

	(1) Reelected	(2) Reelected	(3) Reelected	(4) Reelected	(5) Reelected
Low-numbered question	0.00559	0.00526	0.00203	0.00186	0.00163
	(1.55)	(1.49)	(0.64)	(0.82)	(0.74)
Conservative Party		0.0962***	0.0417	0.292***	0.342***
		(3.52)	(1.28)	(3.59)	(3.92)
Liberal Democrat Party		0.0690	-0.0187	0.367*	0.411**
		(1.74)	(-0.39)	(2.44)	(2.76)
Other party, excluding Labour		-0.323***	-0.253**	-0.124	-0.0593
		(-3.68)	(-2.66)	(-0.66)	(-0.33)
Changed party			-0.255	-0.154	-0.162
			(-1.80)	(-0.72)	(-0.81)
Ministerial position			0.0672*	-0.0151	0.0295
			(2.03)	(-0.27)	(0.53)
Age			0.0477**	0.147***	0.137***
			(3.05)	(5.59)	(5.58)
Age squared			-0.000530***	-0.00154***	-0.00143***
			(-3.38)	(-6.16)	(-6.17)
Years service			-0.0162**	-0.0418***	-0.0365***
			(-2.74)	(-4.27)	(-3.68)
Years service squared			0.000298	0.000729**	0.000600*
			(1.73)	(2.91)	(2.34)
Female			-0.0851**	-0.0487	-0.0138
			(-2.98)	(-0.58)	(-0.17)
Constituency dummies	No	No	No	Yes	Yes
Debate dummies	No	No	No	No	Yes
Observations	54455	54455	54455	54455	54455
Adjusted R^2	0.000	0.042	0.177	0.593	0.598

t statistics in parentheses

Standard errors clustered at member level

Dataset includes all questions over three parliamentary terms, except the first few months of 1997 (see footnote)

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2.7: Linear probability models estimating the effect of having an extra question answered in the House of Commons on a member's probability of reelection (mean = 0.752, standard deviation = 0.432). Estimates here are based on the dataset which counts only those questions which were used under the matched sampling approach.

	(1) Reelected	(2) Reelected	(3) Reelected	(4) Reelected	(5) Reelected
Total answered questions	-0.00213*	0.00182	0.00155	0.00143	0.00319*
	(-2.29)	(1.70)	(1.46)	(1.47)	(2.16)
Total unanswered questions	0.00482***	0.00102	0.000752	0.0000495	-0.00136
	(6.01)	(1.10)	(0.82)	(0.05)	(-0.90)
Conservative Party			0.102***	0.0697**	0.281**
			(4.56)	(2.90)	(3.15)
Liberal Democrat Party			0.0367	-0.0277	0.116
			(1.03)	(-0.76)	(0.77)
Other party, excluding Labour			-0.108*	-0.0882	-0.0610
			(-2.01)	(-1.68)	(-0.38)
Changed party			-0.317***	-0.271**	-0.197
			(-3.40)	(-2.77)	(-1.27)
Ministerial position				0.0791**	0.154***
				(3.25)	(3.50)
Age				0.0340**	0.0775***
				(2.90)	(3.97)
Age squared				-0.000389***	-0.000839***
				(-3.41)	(-4.61)
Years service				-0.0243***	-0.0533***
				(-4.89)	(-6.35)
Years service squared				0.000532***	0.000858***
				(3.68)	(4.24)
Female				-0.0831**	-0.0168
				(-3.26)	(-0.23)
Election dummies	No	Yes	Yes	Yes	Yes
Constituency dummies	No	No	No	No	Yes
Observations	1632	1632	1632	1632	1632
Adjusted R^2	0.023	0.054	0.083	0.174	0.289

t statistics in parentheses

Standard errors clustered at member level

Dataset includes all questions over three parliamentary terms, except the first few months of 1997 (see footnote)

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Chapter 3

Is the Speaker Biased in the UK House of Commons?

3.1 Introduction

"The Speaker must be above party political controversy and must be seen to be completely impartial in all public matters. All sides in the House rely on the Speaker's disinterest, and understand that he or she must stand aside from controversy."

- House of Commons Information Office

In the UK House of Commons, Members of Parliament (MPs) submit questions which they hope to put to the minister responsible during oral debates. From this list of submitted questions, a number are chosen to be placed on the 'Order Paper', which is a document listing all the questions tabled for debate on a given day. The choice as to which questions are placed on the Order Paper and the order in which they appear is determined by a random ballot. The remaining questions are discarded. The number of questions admitted to the Order Paper is set as the maximum number of questions that could be expected to be reached in the time allocated to the debate. In practice this means that in most debates not all listed questions are reached. All questions not reached receive written responses. The ordering of questions as determined by the ballot is therefore important in that questions 'higher up' the Order Paper (i.e. those with lower question numbers) are more likely to receive an oral response in the House of Commons.

A House of Commons Factsheet says *"This ballot, or shuffle, is a lottery and blind to considerations of party, seniority, method of tabling, time of submission or the results of previous shuffles."* The shuffle is therefore in place to ensure that all members have a fair chance of having their questions receive an oral answer. The practicalities associated with conducting debates, however, mean that it could be the case that participants in the debates are still able to influence the probability that certain questions receive oral answers. This chapter will assess the extent to which the ballot procedure is effective in ensuring that members submitting questions have a fair chance of having their questions answered. This will be achieved by comparing the observable characteristics of the members whose questions are answered with those of the members whose questions are not reached and therefore receive written answers.

Given that this ordering is determined completely at random, it should be expected that the answered and unanswered questions in each debate have similar characteristics on average. As such this chapter exploits a 'natural experiment' in the UK parliamentary process. In particular, because the system is designed to ensure that questions are chosen to be answered by a random process, any observed differences between the characteristics of members whose questions are and are not answered orally in the House must mean that the randomization is being undermined in some way.

3.1.1 Previous Related Studies

In the UK political environment, there have long been concerns raised by members of the main political parties regarding fair treatment in Parliament. These are important questions in maintaining the credibility of a legislature, as any evidence of bias in a parliamentary chamber could be potentially damaging to its reputation. The Speaker is required to renounce any previous affiliation to a political party on taking the position in order to conduct the role with impartiality. This position in particular has led to a number of complaints from members believing they are not being treated fairly.

In 2000, the Speaker was accused of bias against the Conservative Party by intervening to prevent a question being asked regarding the Labour Party leadership. More recently (since 2011) the Conservative MP Rob Wilson has cited data on the number of interventions against members of each of the main UK political parties to accuse the Speaker of bias against the Conservative Party. Neither of these claims, however, made use of the kind of 'natural experiment' that this chapter is able to exploit. By simply comparing the number of interventions against a political party, for example, it is not possible to rule out the possibility that the members of the political party for which the most interventions were observed had some particular characteristics which caused this high number of interventions and were not shared by the other parties. Following such accusations, this chapter will pay particular attention to the role of the Speakers which have served in the UK House of Commons over the time period under consideration.

In testing impartiality, this chapter is also related to an extensive literature analyzing the impartiality of sports referees. Parsons et al. (2011) and Price and Wolfers (2010) examine the behavior of referees towards different racial groups, finding in both cases that referees penalize players from other racial groups more heavily than those of their own racial group. Garicano et al. (2005), Sutter and Kocher (2004) and Dohmen (2008) all find that soccer referees show a bias towards the team playing in their home stadium. Rickman and Witt (2008) also showed that such effects could be mitigated using financial incentives. The Speaker's role as an arbitrator can be likened to that of an impartial referee facing social pressure in the House of Commons chamber and as such this chapter offers a contribution to this literature by testing for impartiality in a political as opposed to sporting context.

3.1.2 Data

This chapter uses data on debates in the UK Parliament across three parliamentary terms since 27 October 1997. The dataset includes all the parliamentary questions listed for oral answer for each day in which Parliament was sitting (those listed on the Order Paper) up to the 2010 General Election.[1] Among these questions, it is possible to refer to debate transcripts to determine which questions received oral answers in the debates themselves. All questions not reached during the debates instead received written answers. Since this study focuses only on questions answered orally and to avoid confusion, such questions are referred to as simply being 'unanswered'.

Data on the background characteristics of MPs, including party affiliation, age and

[1]Except two days, for which the Order Papers are missing.

gender, are all sourced from a research paper published by the House of Commons Library in 2010 which listed the characteristics of all MPs from 1979 to 2010.

3.2 Estimation

Under an effective randomization, among the questions which are reached during debates and therefore receive oral answers, members should have the same characteristics on average as those not reached, as there would be no way for any participant to influence whether their question was answered or unanswered. To determine whether this is in fact the case, simple 'difference-in-means' tests will be reported, which take the mean values of a number of observed characteristics across the two groups of questions (those that do and do not receive oral answers). The tests then take the difference between these two mean values across each of the observed characteristics and then calculate t-statistics for each of these observed differences to determine whether each difference can be viewed as 'statistically significant'.

The t-statistics reported in the following tables are calculated using standard errors which account for the fact that single members often ask multiple questions in the dataset. In particular, standard errors are clustered at the member-level.

3.2.1 Comparing Answered and Unanswered Questions

Table 3.1 shows the results of difference-in-means tests comparing the characteristics of the members whose questions received oral answers with those members whose questions were not reached and therefore received written answers. The first column reports the average values of each of the observed characteristics among the questions which received oral answers (the 'treatment group'). The second column reports the corresponding averages among the questions which did not receive oral responses, as they appeared too far down the Order Paper to be reached during the time allotted to their debate (the 'control group'). The third column reports the difference between these two values, as well as the t-statistics associated with each of these differences. A positive reported difference implies that the average was higher among the answered as opposed to unanswered questions. The t-statistics can then be compared to the associated critical values of the t-distribution to

Table 3.1: Difference-in-means tests between the average characteristics of members whose questions received oral answers in the House of Commons ($N_a = 25521$) and those that were not reached during the debates ($N_u = 28934$) and therefore received written answers. The first column takes all the questions which received oral answers (the 'treatment group') and reports the mean characteristics among the members who asked each of those questions. The second column gives the corresponding mean characteristics among the questions which did not receive oral answers ('the control group'). The final column reports the difference between these means.

	Answered questions	Unanswered questions	Difference
Conservative Party	0.354	0.330	0.0243**
			(3.144)
Lib. Dem. Party	0.114	0.103	0.0113**
			(3.034)
Changed party	0.0139	0.0108	0.00309
			(1.549)
Ministerial position	0.458	0.422	0.0364***
			(4.849)
Age	53.34	52.88	0.461***
			(3.333)
Age squared	2924.0	2874.2	49.87***
			(3.335)
Years service	11.46	10.86	0.601***
			(4.107)
Years svc. sqrd.	201.9	184.9	17.00**
			(3.079)
Female	0.185	0.183	0.00189
			(0.355)
Observations	54455		

Standard errors clustered at member level

t-statistics reported in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

infer whether the results can be interpreted as sufficiently different from zero to be treated as statistically significant. Such results are marked with stars.

Comparing the first two columns in Table 3.1, as well as the third column reporting the differences, it is clear that there exist statistically significant differences between almost all of the characteristics of the members whose questions received oral answers and those whose questions did not get answered. Among the questions which are answered, a higher proportion are found to be asked by opposition party members than among those questions which do not receive an oral answer. This suggests that in cases where they have a question selected for the Order Paper, members of both of the main opposition parties are more likely to have their question answered than would be expected under an effective randomization.

Among the answered questions, 45.8% are found to have been asked by members who achieved a ministerial position at some point before 2010. By contrast, among the unanswered questions, 42.2% were found to have been asked by members who achieved ministerial positions. This difference of 3.6 percentage points is found to be statistically significant at the 0.1% level. Similarly, among the answered questions, the average age of the members posing the questions is found to be 0.46 years higher than among the unanswered questions and likewise among the answered questions, the members asking the questions were found to have served 0.60 more years in Parliament than those members whose questions went unanswered. Both of these results are highly significant, and suggest that older and more senior members who have questions selected for the Order Paper are more likely to have their questions answered than would be expected if the randomization was being effectively applied.

Female members were also found to have their questions answered more often than would be expected under a true randomization, although the difference is small and not found to be statistically significant.

Overall, these results suggest that the randomization is being undermined in some way and the following sections will aim to determine exactly why this is the case.

3.2.2 Within-Debate Intention-to-Treat Comparisons

Table 3.1 takes no explicit account of duplicated questions and questions asked by members who were not present at the debate. In fact, parliamentary procedure determines that these questions will be treated differently from other questions, in a way which could undermine the randomness of the ballot system. Specifically, duplicated questions are grouped to be answered together and questions where the member is not present are left unanswered, regardless of their position on the Order Paper.

To check that it is not the treatment of such questions which is causing the statistically significant differences reported in Table 3.1, similar difference-in-means tests are conducted on the same dataset, but using an intention-to-treat analysis. In particular, this process simply counts the number of parliamentary questions asked in each debate and assumes that the first questions listed on the Order Paper up to that number were answered in

Table 3.2: Difference-in-means tests between the questions which likely received oral answers in the House of Commons ($N_a = 25521$) and those that were likely not reached during the debates ($N_u = 28934$), based on their position on the Order Paper. If a questions were answered in a debate, the first a questions are placed in the treatment group, regardless of whether they received an answer. The first column reports the mean characteristics among the members whose questions were higher up the Order Paper and therefore likely received an oral answer (the 'treatment group'). The second column gives the corresponding mean characteristics among the questions which were lower down the Order Paper and therefore likely did not receive oral answers (the 'control group'). The final column reports the difference between these means.

	Likely answered questions	Likely unanswered questions	Difference
Conservative Party	0.349	0.334	0.0148*
			(2.124)
Lib. Dem. Party	0.115	0.102	0.0135***
			(3.372)
Changed party	0.0138	0.0109	0.00287
			(1.441)
Ministerial position	0.458	0.423	0.0349***
			(5.010)
Age	53.33	52.89	0.436***
			(3.318)
Age squared	2922.4	2875.6	46.84***
			(3.337)
Years service	11.44	10.87	0.575***
			(4.097)
Years svc. sqrd.	200.4	186.2	14.20**
			(2.693)
Female	0.186	0.183	0.00300
			(0.618)
Observations	54455		

Standard errors clustered at member level
t-statistics reported in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

the debate. Under this method, if a questions are answered orally in a debate, the first a questions on the Order Paper for that debate are placed into the 'treatment group' (first column). The remaining u questions in the debate are then placed into the 'control group' (second column). In this case, questions are allocated to the treatment and control groups based on their position on the Order Paper and the number of questions reached during each debate. Any differences which persist between the two groups cannot be attributed to the treatment of duplicate questions, or questions not answered because the member was not present, because in this test such questions are treated in exactly the same way as all others.

Conservative Party members are again found more frequently in the 'treatment group' of low-numbered questions than would be expected under a true randomization, although the difference is slightly smaller than the one reported between the corresponding treatment and control groups in Table 3.1. A higher proportion of Liberal Democrat MPs are also found among the low-numbered questions as opposed to the high-numbered (and therefore likely unanswered) ones. This effect is even larger than the one reported previously and is statistically significant at the 0.1% level.

Members who held ministerial positions, older members, and those with more years of service were again found to be more prevalent among the 'treatment group' of low-numbered questions, with reported differences being of a similar magnitude to those reported in Table 3.1 (where answered and unanswered questions were compared directly) and again are found to be statistically significant.

Overall, results in Table 3.2 show that the statistically significant differences persist even in such an intention-to-treat setting. This means it is possible to rule out the notion that it is the non-random treatment of duplicated questions or those asked by absent members that creates the statistically significant differences between the observed characteristics of the two groups.

3.2.3 Across-Debate Intention-to-Treat Comparisons

The remaining possible explanation for these observed differences between the two groups is an endogeneity associated with the speed at which debates progress. Firstly, if members know before submitting a question which debates will progress quickly, they will be able to influence their probability of asking a question, in the event that their question is selected for the Order Paper, by submitting questions to debates in which they expect a high proportion of questions on the Order Paper to be answered. Secondly, once all questions are submitted and the ballot has taken place, members taking part in the debate, including the Speaker, may be able to influence how many questions are answered by controlling the speed at which the debate progresses.

If either of these were the cause of the differences between the two groups, it would be expected that the differences would disappear under an intention-to-treat specification which assumed that the same proportion of questions were reached across all the debates in the dataset.

Across all debates in the dataset, 46.9% of questions receive an oral answer. In the

Table 3.3: Difference-in-means tests between the questions which likely received oral answers in the House of Commons ($N_a = 24128$) and those that were likely not reached during the debates ($N_u = 30327$), assuming that a constant proportion of questions listed on the Order Paper received an answer across all debates. Any question numbered q in a debate will be placed in the treatment group if $q < 0.469 \times (a+u)$, regardless of whether it received an answer. The first column reports the mean characteristics among the members whose questions were higher up the Order Paper and therefore likely received an oral answer (the 'treatment group'). The second column gives the corresponding mean characteristics among the questions which were lower down the Order Paper and therefore likely did not receive oral answers (the 'control group'). The final column reports the difference between these means.

	Likely answered questions	Likely unanswered questions	Difference
Conservative Party	0.338	0.344	-0.00613
			(-1.415)
Lib. Dem. Party	0.111	0.106	0.00507
			(1.660)
Changed party	0.0129	0.0118	0.00112
			(1.190)
Ministerial position	0.440	0.438	0.00228
			(0.510)
Age	52.95	53.21	-0.264**
			(-3.180)
Age squared	2881.5	2910.3	-28.78**
			(-3.163)
Years service	11.07	11.20	-0.130
			(-1.559)
Years svc. sqrd.	190.5	194.8	-4.312
			(-1.286)
Female	0.184	0.185	-0.000933
			(-0.262)
Observations	54455		

Standard errors clustered at member level
t-statistics reported in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

analysis that follows, it is assumed that this proportion of questions is answered in each debate. On an Order Paper with questions numbered $1, \ldots, a+u$, if a question has a number q such that $q < 0.469 \times (a+u)$ it is therefore placed in the 'treatment group' (i.e. it is likely to have received an answer), otherwise it is placed in the 'control group'.

By comparing the means of the observable characteristics across the two groups defined in this way, most of the previously-reported differences between the treatment and control groups in Tables 3.1 and 3.2 disappear. Neither Conservative nor Liberal Democrat members are found to be more likely to have their questions in the proportion of the Order Paper where they are likely to be answered orally. Similarly, neither members holding ministerial positions, nor those with many years of parliamentary service are found to have their questions appearing more often towards the top of the Order Paper.

The only case in which statistically significant differences are found is between the average age of the two groups. In this case, however, it is found that the average age among the questions in the 'control group' (likely unanswered) are higher than among the 'treatment group'. This is the opposite to the results in Tables 3.1 and 3.2.

These results therefore generally confirm that the randomization itself was effective in offering equal opportunities for all members to pose oral questions in the House of Commons and cannot be held responsible for the observed differences in Tables 3.1 and 3.2. It has been shown that when the speed at which debates are conducted (and therefore the proportion of listed questions admitted) is held constant, characteristics are very similar across the 'treatment' and 'control' groups. The findings therefore point to a manipulation of this speed at which debates are conducted as the reason behind the observed differences between the members whose questions are answered and unanswered.

3.3 The Role of the Speaker

The analysis presented in the previous section points to an endogeneity associated with the speed at which debates progress as the reason behind the differences in observed characteristics between the members whose questions are answered and those whose questions remain unanswered. To find out to what extent these differences can be attributed to the role of the Speaker, further difference-in-means tests will be conducted which compare only those questions which are marginally admitted or left out of the debate.

The reason for making this comparison is that while members submitting questions may have some knowledge of the debates in which a high proportion of the questions on the Order Paper will be reached, it can safely be assumed that they will not be able to predict exactly which question listed on the Order Paper for a debate will be the last question admitted. This implies that members will not be able to influence whether they fall into the treatment or control group in this comparison. On an Order Paper with a answered and u unanswered questions numbered $1, \ldots, a+u$, they will therefore have as much probability of their question being the question numbered a (the last one answered), as the question numbered $a+1$ (the first one unanswered). This makes allocation to treatment or control group ex ante exogenous in this case. If differences between the

Table 3.4: Difference-in-means tests between the characteristics of the members whose questions were the last ones to receive oral answers in House of Commons debates ($N_a = 2588$) and the corresponding characteristics of members whose questions were the first ones that were not reached during the debates and therefore received written answers ($N_u = 2588$). The first column reports the mean characteristics among the members whose questions were the last to receive an oral answer in a particular debate (the 'treatment group'). The second column gives the corresponding mean characteristics among the questions which were the first ones listed on the Order Paper not to receive oral answers (the 'control group'). The final column reports the difference between these means.

	Just-answered questions	Just-unanswered questions	Difference
Conservative Party	0.355	0.342	0.0128
			(0.894)
Lib. Dem. Party	0.105	0.122	-0.0178
			(-1.897)
Changed party	0.0143	0.0116	0.00270
			(0.695)
Ministerial position	0.461	0.452	0.00850
			(0.574)
Age	53.39	52.81	0.583*
			(2.188)
Age squared	2929.3	2867.6	61.77*
			(2.146)
Years service	11.46	11.11	0.349
			(1.385)
Years svc. sqrd.	202.9	187.7	15.22
			(1.722)
Female	0.206	0.184	0.0220*
			(1.992)
Observations	5176		

Standard errors clustered at member level

t-statistics reported in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

treatment and control groups persist under this comparison, it will therefore be possible to conclude that it is an ex post manipulation of the speed of the debate after seeing the Order Paper (i.e. during the debate itself) which creates an environment where certain questions are more likely to be admitted than others.

Table 3.4 reports the results of difference-in-means tests where the 'treatment group' consists of the last questions answered across all debates and the 'control group' consists of the first questions unanswered across all debates.

Results in Table 3.4 are based on a much smaller sample size, given that attention is focused solely on questions marginally admitted and excluded from oral debates. As a result, few of the results show statistical significance when compared with earlier findings. It is informative, however, to compare the magnitudes of the reported differences.

Once again, Conservative Party members are found to be more likely to have their questions just admitted to debates than have their questions just excluded. Among the 'just-answered' questions, 35.5% were asked by Conservative Party MPs, whereas among the 'just-unanswered' questions, 34.2% were asked by Conservative Party MPs. Given the small sample size, this 1.3% difference is not found to be statistically significant. In terms of magnitudes, however, it is not far short of the 2.4% and 1.5% differences reported in Tables 3.1 and 3.2 respectively. This offers some tentative evidence that endogeneity associated with the speed at which debates progress works in favor of Conservative Party MPs in offering them a higher probability of having their questions answered than would be expected under a true randomization.

Interestingly, Table 3.4 reports that the average age of members among the 'just-answered' questions is 0.58 years older than among the 'just unanswered' questions. This compares with a difference of 0.46 years in Table 3.1. This large effect is found to be statistically significant at the 5% level, even accounting for the much smaller sample size using this method. The endogeneity in the speed at which debates progress is therefore found to favor older members in allowing them a greater chance of having their questions answered once their questions are selected for the Order Paper. Among the 'just-answered' questions, members also have an average number of years' service that is 0.35 years higher than among the 'just-unanswered' questions. This compares with a difference of 0.6 years in Table 3.1, but is not found to be statistically significant in this case, again owing to the small sample size.

Finally, Table 3.4 shows that female members are more likely to have their questions 'just-answered' than 'just-unanswered'. 20.6% of the 'just-answered' questions were asked by female members, whereas only 18.4% of the 'just-unanswered' questions were asked by female members. The 2.2 percentage point difference between these two populations is found to be statistically significant at the 5% level. This suggests an endogeneity in the debating process which gives preferential treatment to female members when admitting marginal questions into debates.

Given that the members posing questions are limited in their involvement in debates, the two most likely causes of these differences are the ministers facing the questions in debates, and the Speaker. The ministers may have an incentive to give longer answers to early questions, for example, in the event that they see potentially hostile questions from opposition party members further down the Order Paper. The Speaker's role is to correct such behavior and they may also have a further influence on which questions are answered by deciding how many follow-up questions to allow for each question listed on the Order Paper.

If ministers played the dominant role in influencing which questions receive an oral answer, it would surely be expected that a lower proportion of questions by opposition party members would receive oral answers. The fact that a higher proportion of questions asked by opposition party members are admitted into debates than are excluded suggests that the Speaker plays a more prominent role in this process than the ministers. While this could be interpreted as a bias against the Labour Party (which governed throughout the period covered by this dataset) it could also be interpreted as the Speaker counteracting the type of tactics which would otherwise be employed by ministers to manipulate the pattern of debates so as to face a lower proportion of hostile questions than would otherwise be the case.

3.4 Breakdowns Over Time

The above analysis shows that there exist differences between the observable characteristics of members who have their questions answered in the House of Commons and those whose questions instead receive written answers. The fact that many of these differences are also seen when comparing only those questions that are marginally included or excluded from debates, coupled with the nature of the differences (accepting more questions from opposition party members), suggests that the Speaker's role in determining the speed at which debates progress is at least partly responsible for these differences.

Over the time period in question, three members served as Speakers in the House of Commons. From 27 April 1992 to 23 October 2000 a former Labour Party member (Baroness Boothroyd) served as Speaker. Another former Labour Party member held the post from 23 October 2000 to 22 June 2009 (Baron Martin of Springburn). Finally, a former Conservative Party member (Rt. Hon. John Bercow MP) served as Speaker from 22 June 2009 to the time of writing this chapter. Given that the Labour Party was in government across the entire period covered by this dataset, this means that in debates taking place before 22 June 2009 the Speaker was a former member of the governing party. For all debates taking place after this date the Speaker was a former member of the opposition party.

This section considers each of the time periods in turn to determine whether the differences between the observed characteristics of the members who posed answered and unanswered questions are particularly prevalent in any of those time periods. Table 3.5 reports only the differences between the average values of the characteristics of members asking answered and unanswered questions. This is purely for ease of comparison and the averages across the two groups in each case are available on request. The final column in Table 3.5 repeats the results of difference-in-means tests using the entire dataset (previously reported in Table 3.1), again for ease of comparison.

Results in Table 3.5 show that during the period served by the first of the three Speakers (1997-2000), more questions posed by Liberal Democrat MPs were admitted during debates than would be expected if questions were chosen purely at random. This finding is close in magnitude to the one found in the dataset as a whole but is not found to be statistically significant at the 5% level, given that it is based on a smaller sample size. By contrast in the dataset as a whole it was found that more questions from Conservative Party MPs were answered during debates than should be expected, but this is not found to be the case during this period.

In fact across all the observed characteristics during this first period, there are no statistically significant differences between the members whose questions were answered and those that remained unanswered. The absence of statistically significant differences between the treatment and control groups cannot be simply attributed to the smaller sample size that results from considering a smaller time period. In fact the observed differences are in all cases smaller in magnitude than those reported in the final column for the dataset as a whole.

Table 3.5: Difference-in-means tests across the three time periods covered by the Speakers who served in the House of Commons between 1997 and 2010. Only the differences themselves, with associated t-statistics, are reported in this table, alongside the corresponding differences for the dataset as a whole (previously reported in Table 3.1) for ease of comparison. The first column reports the differences between the mean characteristics for the period during which the Speaker position was held by Betty Boothroyd, the second column reports the differences for the period during which Michael Martin was the Speaker and the third column reports the differences for the period during which John Bercow was the Speaker.

	Betty Boothroyd	Michael Martin	John Bercow	Overall
Conservative Party	-0.00463	0.0209**	0.0442	0.0243**
	(-0.567)	(2.942)	(1.893)	(3.144)
Lib. Dem. Party	0.00906	0.00723	-0.00471	0.0113**
	(1.764)	(1.957)	(-0.319)	(3.034)
Changed party	0.000958	0.00183	0.00814	0.00309
	(0.725)	(1.156)	(1.266)	(1.549)
Ministerial position	0.0125	0.0285***	-0.00575	0.0364***
	(1.426)	(4.010)	(-0.246)	(4.849)
Age	-0.0505	0.277*	0.640	0.461***
	(-0.289)	(2.172)	(1.485)	(3.333)
Age squared	-4.542	29.40*	69.54	49.87***
	(-0.235)	(2.164)	(1.560)	(3.335)
Years service	-0.00748	0.429**	0.642	0.601***
	(-0.0430)	(3.287)	(1.589)	(4.107)
Years svc. sqrd.	4.447	11.51*	24.92	17.00**
	(0.691)	(2.334)	(1.811)	(3.079)
Female	-0.0000964	0.00486	-0.0114	0.00189
	(-0.0162)	(0.896)	(-0.734)	(0.355)
Observations	16204	35227	3024	54455

Betty Boothroyd served as Speaker between 27 April 1992 and 23 October 2000

Michael Martin served as Speaker between 23 October 2000 and 22 June 2009

John Bercow served as Speaker from 22 June 2009 to the time of writing this paper

Standard errors clustered at member level

t-statistics reported in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The second column reports the results of the second time period under consideration (2000-2009). The differences found between the two groups of questions across the dataset as a whole are largely replicated when attention is focused on this time period alone. Conservative Party members are found to have a higher probability of having their questions answered than would be the case if the questions to be answered were truly chosen at random. This difference is found to be statistically significant at the 1% level, with a t-statistic of 2.94. This result comes in spite of the fact that the Speaker during this period was a former member of the Labour Party.

Similarly, members who at some point held a ministerial position are found to be more likely to have their questions answered, as are older members and those who had served for longer as an MP at the time the question was asked. Each of these effects is also found to be statistically significant at the 5% level, despite the smaller sample size.

The third column reports results from the third and final time period under consideration (2009-2010). Once again, Conservative MPs are found to have a higher probability of having their questions answered during parliamentary debates than would be expected under a truly random selection of questions. The size of the difference is found to be more than twice as large as the effect reported in Column 2 for the time period between 2000 and 2009. The t-statistic associated with this effect reflects the fact that this finding is based on a much smaller time period and therefore sample size than the result in Column 2 and is therefore not found to be statistically significant at the 5% level.

This result is potentially important, however, given that despite being a former member of the Conservative Party himself, this Speaker has been accused of bias against the Conservative Party in enforcing parliamentary procedure. This result suggests that in fact, during this Speaker's term, a higher number of questions from Conservative Party members were admitted in debates than would have been the case had the randomization process been followed exactly.

No other statistically significant differences were found between the observed characteristics of members whose questions were answered and unanswered during this third time period. This is largely driven by the small sample size available during this period, however, and masks some large observed coefficients when compared with the other time periods. In this third column, for example, questions asked by members who changed party allegiance during the parliamentary term in which the question was asked were found to have their questions answered more often than should be expected under a randomization. Specifically, 2.7% of the answered questions in this period were asked by members who changed party, whereas only 1.9% of the unanswered questions were asked by members who changed party, implying a difference of 0.81 percentage points. While this in itself is not found to be statistically significant, with a t-statistic of 1.27, the effect is large compared with the other time periods, where the comparable differences were estimated as 0.096 and 0.183 percentage points respectively. This could be interpreted as a preference by this Speaker for admitting questions posed by more radical members, or those at the fringes of their parties, who in turn would be the most likely to have switched party affiliation.

Older members, as well as those who have served for longer as MPs, are found to have their questions answered more often than would be expected under an effective randomization in this third time period. The differences between the two groups are again found to be larger than those reported in either of the previous time periods, but just fall short of implying statistical significance at the 5% level, again as a result of the small sample size.

By contrast, members of the Liberal Democrat Party were not found to have their questions answered more often than would be expected under an effective randomization (as was the case across the other two time periods). Similarly, members who achieved ministerial positions were not given any preferential treatment during this time period.

3.5 Conclusions

This chapter has shown that there exist statistically significant differences between the characteristics of members whose questions receive oral answers in UK parliamentary debates and those whose questions are not reached. By comparing the characteristics of members whose questions are marginally included or excluded from debates, it has also been possible to show that the Speaker plays an important role in determining which questions receive oral answers from all those that are listed on the Order Paper.

The Speaker's role in Parliament is demanding in that it requires the holder of the post to ensure that parliamentary rules are respected throughout debates, in an environment where both sides of the House may well look to gain advantages by stretching the interpretation of those rules. In all debates of this kind, including the ones studied here, it is likely that government ministers facing questions could aim to avoid difficult questions further down the Order Paper and it is the Speaker's job to prevent this from happening. In doing so, it may be that the Speaker over-compensates for the government's perceived unwillingness to answer inconvenient questions, which would present itself in the data as a greater proportion of opposition party questions among the answered questions than those which remain unanswered. This was precisely what was found when looking over the course of the time-period studied here.

As well as the finding that opposition party members were more likely to have their questions answered orally, it was consistently shown to be the case that older members and those who had served for a longer period in the House of Commons were more likely to have their questions answered, even when restricting attention to marginal questions which were either 'just-answered' or 'just-unanswered'. One possible explanation would be that such MPs hold a higher level of authority and are therefore able to exert more pressure on the Speaker to ensure that their questions are admitted to debates. This undermines the randomization process which determines the ordering of parliamentary questions and is designed precisely to ensure that all MPs have an equal chance of having their question answered, regardless of their level of seniority or any other characteristics.

The breakdowns over time show that the large majority of the differences in the treatment of questions from members with particular characteristics took place under the tenure of the two most recent Speakers to serve in the House of Commons. Michael Martin was the Speaker for a large part of the period covered by the dataset (2000-2009), and during his tenure, statistically significant differences can be observed between the characteristics of members whose questions were answered and unanswered. During the later period covered by John Bercow, some of the observed differences are in fact of a larger magnitude, but are not found to be statistically significant, given that they are based on a smaller sample size.

An important caveat to these findings is that the Speakers themselves do not chair all the debates during their tenure. In some cases where the Speaker is unable to attend or where parliamentary protocol determines, the Deputy Speaker will chair debates. The Deputy Speaker is typically taken from the opposite party to the one previously served by the Speaker themself. From the data available, it is not possible to determine whether the Speaker themself or one of their deputies chaired debates.

The findings reported in this chapter have implications for other studies which attempt to exploit natural experiments in parliamentary procedure. Loewen et al. (2013) and Chapter 2 in this thesis use such parliamentary procedures to infer whether the behavior of legislators in Parliament influences their chances of reelection. The results reported here show that in systems where a priority list is randomly-ordered, adequate care must be taken to ensure that the allocation to the treatment and control groups is truly exogenous. Chapter 2 does this by dropping questions which are deemed most likely to have been either admitted or omitted from the treatment group in a non-random way.

References

Blackburn, R. and A. Kennon (2003). *Parliament: Functions, Practice and Procedures*. Sweet and Maxwell.

Dohmen, T. J. (2008). The influence of social forces: Evidence from the behavior of football referees. *Economic Inquiry*, 411–424.

Franklin, M. and P. Norton (1993). *Parliamentary Questions*. Study of Parliament Group. Oxford: Clarendon Press.

Garicano, L., I. Palacios-Huerta, and C. Prendergast (2005). Favoritism under social pressure. *Review of Economics and Statistics*, 208–216.

Gay, O., R. Cracknell, J. Hardacre, and J. Fessey (2010). Members 1979 - 2010.

House of Commons Information Office (2010a). Parliamentary questions. Factsheet P1 Procedure Series.

House of Commons Information Office (2010b). The Speaker.

House of Commons Information Office (2013). Parliamentary questions: Brief guide.

John, P. and S. Bevan (2011). Project report on Prime Minister's Parliamentary Questions. UK Policy Agendas Project.

Loewen, Peter, J., R. Koop, E. Settle, Jaime, and H. Fowler, James (2013). A natural experiment in proposal power and electoral success. *American Journal of Political Science*.

Marleau, R. and C. Montpetit (2009). House of Commons procedure and practice. 2nd Edition.

Martin, S. and O. Rozenberg (2012). *The Roles and Function of Parliamentary Questions*. Library of Legislative Studies.

May, T. E. (2011). *A Treatise upon the Law, Privileges, Proceedings and Usage of Parliament* (24 ed.). London: LexisNexis.

Parsons, C. A., J. Sulaeman, M. C. Yates, and D. S. Hamermesh (2011). Strike three: Discrimination, incentives, and evaluation. *American Economic Review*, 1410–1435.

Price, J. and J. Wolfers (2010). Racial discrimination among NBA referees. *The Quarterly Journal of Economics*.

Rickman, N. and R. Witt (2008, May). Favouritism and financial incentives: A natural experiment. *Economica*, 296–309.

Sandford, M. (2012). House of Commons background paper: Parliamentary questions: Recent issues.

Sutter, M. and M. G. Kocher (2004). Favoritism of agents: The case of referees' home bias. *Journal of Economic Psychology*, 461–469.

The Scottish Parliament (2012). Guidance on parliamentary questions. 1st Edition.

Tucker, L. (2013). Parliamentary questions and the probability of re-election in the UK House of Commons. *The Warwick Economics Research Paper Series*.

Chapter 4

The House of Representatives to the Senate: US Congress Elections and their Effect on the Economic Outcomes of Constituents

4.1 Introduction

An important question which has received relatively little attention in the social sciences literature is whether legislators' political backgrounds influence their choices and outcomes during their time in office. This study attempts to investigate this issue by focusing attention on a particular group of politicians whose political backgrounds can be observed and compared. The group in question are members of the US Congress who sought election to the Senate, having previously served in the House of Representatives.

In the US political system, Congress is formed of two chambers, each filled by elected members from a given region of the USA. Each member of the lower house (the House of Representatives) is elected by, and therefore representative of, a congressional district within a US state. The total number of representatives is currently fixed at 435, with some states being made up of only one congressional district, and others being comprised of more than fifty. Conversely, the upper house (the Senate) contains two members elected from each state, so the total number of senators is fixed at 100.

Many members of the House of Representatives subsequently seek election to the Senate (Ostermeier (2011)). This transition from the lower to the upper house is often viewed as a 'promotion' and will frequently be referred to as such in this chapter. By focusing attention on these cases, this chapter will compare the incomes of the constituents in the congressional districts represented by these legislators under different electoral outcomes. For example, it will be possible to determine whether representatives who seek election to the upper house tend to have served states with higher incomes than other constituents whose representatives do not seek election to the upper house. Previous studies have also specifically considered the effects of transition between the House and the Senate. Grofman et al. (1995) in particular compares the voting behavior of senators who previously served in the House of Representatives with various control groups. This chapter is the first, however, to compare the economic outcomes of the constituents who reside in districts previously served by legislators who move between legislative chambers in this way.

The question as to what type of candidate is put forward for election is particularly important if it is believed that the characteristics and experiences of legislators will affect their decision-making when in office and there is a large literature which aims to determine whether this is the case. It has been argued, for example, that electing females to office improves gender equality. This 'politics of presence' literature (Phillips (1995)) argues that female legislators pay greater regard to the rights of females in the constituencies they serve. There could also exist another path of influence by affecting the aspirations of female citizens by demonstrating the possibility of achieving high office. As well as personal characteristics, it is possible that political backgrounds may influence legislator decision-making and this chapter will contribute to this body of research in particular.

This chapter is also relevant to the literature studying the link between election results and the economic outcomes of constituents. Dolan et al. (2008) find for example that the outcomes of recent UK elections had no discernible effect on the well-being of British citizens. Conversely, many authors (Ray (1980); Stein and Bickers (1994); Clark and Milcent (2011)) have looked for evidence of a link between 'pork barrel' effects and electoral success.

There is also a substantial literature which considers whether different income levels are adequately represented in the US political system. Bartels (2008) finds that senators take account of public opinion among high-income constituents more so than the opinion among low-income constituents. Rigby and Wright (2013) find that low-income citizens have little influence on the campaign appeals of US political parties, given that they offer lower potential resources to the parties. There also exists a more general debate regarding the relative levels of political influence of different US citizens. Given that each state has two representatives in the Senate, for example, Levinson (2006) argues that the democratic system offers a disproportionate influence to citizens of smaller states.

This chapter compares the incomes of constituents from districts where the representative subsequently sought election to the upper house. Section 4.2 reviews the data which was used to conduct the analysis and Section 4.3 sets out the method by which the analysis will be carried out. Section 4.4 reports the results of regressions comparing constituents whose representatives sought election to the upper house (successfully or otherwise) with those from other districts in the same state in the same year. Section 4.5 then restricts attention to those citizens from districts where the representatives sought election to the upper house and compares average incomes between those citizens whose representatives were successful in being elected to the upper house and those whose representatives were unsuccessful in doing so. Section 4.6 offers some conclusions from the findings.

4.2 Data

The study uses a combination of data from the Behavioral Risk Factor Surveillance System (BRFSS) dataset and data collected from historical accounts of US Senate elections. The BRFSS dataset is a telephone survey conducted across the USA each year since 1984. A number of checks have been carried out on the validity of these survey responses (Nelson et al. (2003)), finding generally positive results. The data has been used as the basis for a wide number of health studies, including recent studies of well-being (Oswald and Wu (2010)).

Since 1988, this study also collected data on respondents' counties of residence, which can then be linked to congressional districts. To complete the process of matching counties to congressional districts, the 'Missouri Census Data Center' was used, which allows the analysis to take into account changes to the boundaries of congressional districts over the time period considered in the study. While these do not in all cases provide a one-to-one match for districts, it is possible to match such counties with all districts which have any overlap, to generate a set of possible districts for each respondent. In most cases, the number of possible districts is small, with 51% of the full dataset under consideration able to be matched to a single district, and a further 20% able to be matched to one of two districts. There are a small number of cases, however, where respondents reside in a county that can be associated with many districts, the highest number being eighteen. As part of these surveys, respondents were asked to give details regarding their gender, age, height, weight, education levels, employment status, marital status and racial background, as well as a series of health questions. While respondents in the BRFSS survey were not asked to report their individual incomes, they were asked to place their household income into one of the following categories:

- Category 1: \$0 to \$10000
- Category 2: \$10000 to \$15000
- Category 3: \$15000 to \$20000
- Category 4: \$20000 to \$25000
- Category 5: \$25000 to \$35000
- Category 6: \$35000 to \$50000
- Category 7: \$50000 to \$75000
- Category 8: Over \$75000 (only available from 1994 onwards)

The highest of these categories was not offered as a possible response in the years up to 1993, so all regressions will be based on data from 1994 onwards, with previous years discarded. To create a dependent variable from these responses, each respondent's income level is taken to be the mid-range of these response groups, with incomes of \$100000 allocated arbitrarily to the top income group. The dependent variable is then taken to be the natural logarithm of these income levels.

Coupled with this dataset, records of all Senate elections covering this period are freely available on-line, so for the purposes of this study, it was also possible to go through such records to form a new dataset recording not only those lower-house representatives who were successfully elected to the upper house, but also those representatives who launched unsuccessful bids to be elected to the upper house. The availability of data on these unsuccessful candidates will be crucial in the identification strategy of the models that follow. Tables 4.3 and 4.4 in the Appendix list all of the members of Congress who sought election to the upper house between the years 1994 and 2012, having previously served in the lower house. Table 4.3 lists those members who were successfully elected to the upper house during that time period, whereas Table 4.4 lists those who were unsuccessful in their election bids. In total, 56 lower house representatives were elected to the upper house between 1994 and 2012. Conversely, 57 legislators who served in the lower house made unsuccessful election bids to the upper house during the period between 1994 and 2012.

4.3 Method

All of the following results will be based on ordinary least squares (OLS) regressions. Let $i = 1, \ldots, I$ represent the set of respondents to the BRFSS survey over the years $t = 1994, \ldots 2012$. Let y_i^t represent the income of player i, who was surveyed as part of the BRFSS in year t. For each player, this refers to the mid-point of the income range to which they assigned themself in their BRFSS survey responses. The dependent variable in each of the regressions that follows will be the natural logarithm of this income.

Lower house representatives are elected to serve a district, whereas upper house legislators are elected to serve a state, which is divided into a certain number of districts. In terms of notation, let s_i^t represent the state in which player i resides in year t and let d_i^t represent the district in which player i resides in year t.

The only thing that will change in the regressions that follow is the definition of the explanatory variable of interest. Each of the regressions will compare the average incomes of BRFSS survey respondents whose representatives sought promotion to the upper house with some control group of BRFSS survey respondents who are deemed comparable. Letting Z_i^t represent a set of control variables relating to the individual characteristics of player i as recorded in year t, it is possible to estimate the following regression by OLS:

$$\ln\left(y_i^t\right) = \alpha_0 + \alpha_1 x_i^t + \alpha_2 Z_i^t + \epsilon_i$$

4.4 Comparing Districts Where Representatives Sought Promotion

The first part of the study aims to determine whether legislators in the lower house who seek election to the upper house tend to represent districts with higher average incomes. In this comparison, no distinction is made between the legislators who successfully achieve election to the upper house and those who are unsuccessful in doing so. The control group used here will be those citizens who resided in the same state as a representative who sought promotion in the same year, but who lived in a district other than the one served by the representative.

Extending the notation from Section 4.3, let S^t denote the set of all 50 states in year t and $s^{\star t} \in S^t$ denote the set of states within that set where a representative sought election to the upper house in the year t. Similarly, D^t denotes the set of all congressional districts in year t and $d^{\star t} \in D^t$ denotes the set of districts within that set where the representative sought election to the upper house in the year t.

With these definitions in hand, let $El\left(s_i^t, d_i^t\right) \rightarrow \left(\{0,1\}, \{0,1\}\right)$ be a function which maps the state and district of player i in year t on to two separate binary variables, which each indicate whether a representative of that state or that district sought promotion to the upper house in the year t. Specifically, you have:

$$El\left(s_i^t, d_i^t\right) = \begin{cases} (1,1) & \text{if } s_i^t \in s^{\star t} \text{ and } d_i^t \in d^{\star t} \\ (1,0) & \text{if } s_i^t \in s^{\star t} \text{ and } d_i^t \notin d^{\star t} \\ (0,0) & \text{if } s_i^t \notin s^{\star t} \text{ and } d_i^t \notin d^{\star t} \end{cases}$$

Or more succinctly, $El\left(s_i^t, d_i^t\right) = \left(\mathbf{1}_{s^{\star t}}\left(s_i^t\right), \mathbf{1}_{d^{\star t}}\left(d_i^t\right)\right)$. In this case the explanatory variable of interest can be written as:

$$x_i^t = \begin{cases} 1 & \text{if } El\left(s_i^t, d_i^t\right) = (1,1) \\ 0 & \text{if } El\left(s_i^t, d_i^t\right) = (1,0) \end{cases}$$

This regression takes no account of respondents residing in states where no lower house representative sought election to the upper house. Alternatively, the regression above can be rewritten as:

$$\ln\left(y_i^t\right) = \alpha_0 + \alpha_1 El\left(d_i^t\right) + \alpha_2 Z_i^t + \epsilon_i, \quad \text{for all } i \text{ such that } El\left(s_i^t\right) = 1$$

In all the results that follow, each coefficient is interpreted as representing the percentage increase in average incomes that is associated with a one unit increase in the explanatory variable. The consistent use of this approximation makes comparison of coefficients straightforward. It is also accurate, given that all the coefficients of interest reported in this chapter are below 0.05.

Table 4.1 shows that constituents from districts where the representative sought election to the upper house in the same year are found to have incomes that are approximately 4.75 percent higher than respondents from the same states, but whose representatives did not seek election to the upper house, without including any further controls. This effect is found to be statistically significant at the 5% level. When including dummy variables to control for the fixed effects associated with the year and state in which the respondent was surveyed, constituents in districts where the representatives sought election to the upper house are found to have incomes that are on average around 4.2 percent higher than constituents from other districts in the state. This effect is again found to be statistically significant at the 5% level.

By including variables to control for respondent characteristics in Columns 3 and 4 of Table 4.1, the effect is reduced slightly, so that constituents from congressional districts where the lower house representative sought election to the upper house are found to have 2.8 percent higher incomes than constituents residing in the same state but different congressional districts. This effect remains statistically different from zero at the 5% level. With regard to the control variables, older respondents are found to have higher incomes, with the effect decreasing as their age increases. Female and hispanic respondents are found

Table 4.1: Table of regressions comparing constituent incomes in districts where the representative sought election to the upper house compared with other residents of the same state in the same year, but who came from different districts. The dependent variable is the natural logarithm of the mid-point of the income bracket of each respondent (mean = 10.52 and standard deviation = 0.82 for the entire sample, where $N = 404,394$).

	(1) Income (log)	(2) Income (log)	(3) Income (log)	(4) Income (log)
Rep. sought promotion to Senate	0.0475*	0.0421*	0.0407*	0.0277*
	(2.05)	(2.13)	(2.21)	(2.24)
Respondent age			0.0455***	0.0189***
			(21.18)	(12.51)
Respondent age sqd			-0.000500***	-0.000233***
			(-20.94)	(-14.44)
Female respondent			-0.184***	-0.115***
			(-38.96)	(-32.39)
Hispanic respondent			-0.428***	-0.197***
			(-17.65)	(-12.41)
Year dummies	No	Yes	Yes	Yes
State dummies	No	Yes	Yes	Yes
Education dummies	No	No	No	Yes
Marital status dummies	No	No	No	Yes
Race dummies	No	No	No	Yes
Observations	404394	404394	401168	390641
Adjusted R^2	0.001	0.038	0.123	0.404

t statistics in parentheses

Standard errors clustered at state-year level

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

to have lower incomes on average than male and non-hispanic respondents respectively.

Overall, these results imply that in the year where a representative in the lower house of the US Congress seeks election to the upper house, constituents in the district which those legislators represent have higher incomes on average than constituents from other congressional districts within the same state.

4.4.1 Lagged Regressions

These reported differences could come about as a result of representatives seeking promotion having represented a particular type of district. Conversely, the differences could be related to the representative themself. For example, representatives seeking promotion may be able to artificially increase incomes in their districts to help their promotion bid, or simply wait for an opportune moment when incomes are high to seek promotion.

To offer some insights into which of these effects is taking place, it will be informative to consider whether the districts from which legislators sought promotion to the upper house had higher incomes than other districts within the same state over a prolonged period, or whether the difference is only observed temporarily. Further regressions are therefore estimated, which follow exactly the same procedure, but which match constituents to legislators with varying time lags.

For example, it will be interesting to calculate whether incomes were higher on average among the districts where the representative sought election to the upper house one year before they did so. This will involve finding all the respondents to the BRFSS survey who resided in a state whereby in the following year $(t+1)$ a representative from that state sought election to the upper house having previously served in the lower house. Within this group, it will be possible to compare the average incomes of the respondents who resided in the district previously served by the lower house representative with those from other districts in the same state. In this case the explanatory variable of interest can be written as:

$$x_i^{t+1} = \begin{cases} 1 & \text{if } El\left(s_i^{t+1}, d_i^{t+1}\right) = (1,1) \\ 0 & \text{if } El\left(s_i^{t+1}, d_i^{t+1}\right) = (1,0) \end{cases}$$

To generalize the above example, introduce the variable lag, which reports the number of years between the year in which the survey was conducted and the year in which the representative sought promotion to the upper house. In the case above, you have $lag = -1$ because a comparison is being made between respondents one year before the elections took place.

By introducing this lag variable, the following regression can be estimated:

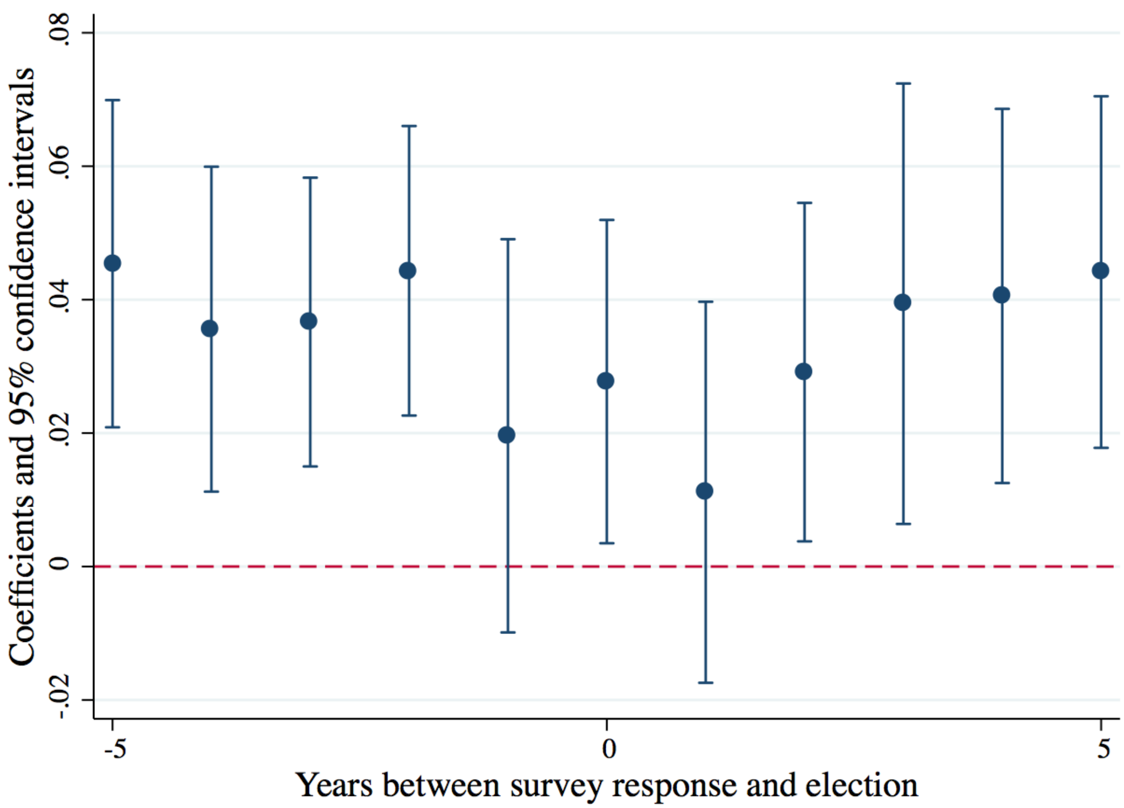
$$\ln\left(y_i^t\right) = \alpha_0 + \alpha_1 El\left(d_i^{t-lag}\right) + \alpha_2 Z_i^t + \epsilon_i^t, \quad \text{for all } i \text{ such that } El\left(s_i^{t-lag}\right) = 1$$

To compare outcomes over time, the above regression will be estimated using a number of different lags, from $lag = -5$ to $lag = +5$. The results are reported in detail in Table 4.5 in the Appendix. These results are based on regressions which include all the control variables used in Table 4.1. The result for $lag = 0$ in Table 4.5 therefore corresponds to the results reported in Column 4 of Table 4.1. To give an idea of how the coefficient changes over different lag values, Figure 4.1 plots the coefficient of interest along with 95% confidence intervals for lag values ranging from -5 to +5.

Figure 4.1 shows that incomes among the districts where representatives sought promotion to the upper house were consistently higher than incomes in other districts in the same states. This is found to be the case both before and after the elections to the upper house took place. Survey respondents in districts where the representative sought election to the upper house five years after the constituent was surveyed (the leftmost data point) are found to have incomes approximately 4.5 percent higher than respondents from other districts in the same state. Similarly, respondents living in districts where the constituent was surveyed five years after the representative sought election to the upper house are found to have incomes that are around 4.4 percent higher than respondents from other districts in the same state (the rightmost data point).

Although all coefficients are positive, the differences between the average incomes of the survey respondents from districts where the representative sought election to the upper house and other respondents from other districts in the same state are reduced when comparing respondents who answered the BRFSS survey around the same time as their representatives sought election to the upper house (i.e. when lag takes the values -1, 0, or

Figure 4.1: Each of the points in the graph shows the result of a regression estimating the difference in incomes between residents of districts where the lower house representative sought election to the upper house and residents of different congressional districts in the same state. The 95% confidence intervals around these estimated coefficients are also shown. The horizontal axis plots the *lag* variable. Each point therefore shows the difference between the average incomes of the two groups at some interval before or after the representative sought election to the upper house. For example, the point at $lag = -5$ shows the coefficient described above 5 years before the representative sought promotion. The graph below therefore illustrates how the coefficient of interest evolves over time either side of the period where the representative sought election to the upper house (i.e. when $lag = 0$).



1). For *lag* values of -1 and 1, the difference between the average incomes of respondents from districts where the representative sought election to the upper house and respondents from other districts in the same state is not found to be significantly different from zero at the 5% level.

Figure 4.1 is interpreted as showing that lower house representatives who seek election to the upper house in the US Congress come from districts where incomes are permanently higher on average than respondents from other districts in the same states. This figure serves to counter the hypothesis that incomes are higher among the constituents in districts where representatives seek election to the upper house because representatives choose an opportune moment to seek promotion to the upper house. If this hypothesis were true, it would be expected that incomes among survey respondents from the districts where the representative sought promotion would increase relative to the incomes of respondents from other districts in the years leading up to the point where the representative sought promotion. The pattern seen in Figure 4.1 shows no evidence of this, however, with the differences particularly large and significant in the years before the representative sought election to the upper house. If anything, the difference is reduced when comparing survey respondents in the years immediately before and after the representative sought election to the upper house.

This pattern, alongside the fact that in the years after the representative sought election to the upper house the difference between the incomes of the two groups is restored to its original level of around 4 percent, suggests that the difference between the incomes is a more permanent one. In particular, this implies that representatives who seek election to the upper house (without necessarily being successful) tend to have previously represented congressional districts where incomes are permanently higher than those of other districts in the same state.

4.5 Comparing Successful and Failed Election Bids

Whereas Section 4.4 considered the relationship between constituent incomes and representatives who seek election to the upper house, this section attempts to capture the relationship between constituent incomes and representatives who win those elections.

This section again uses ordinary least squares estimation, whereby incomes are contrasted between those survey respondents whose representatives were successful in achieving promotion to the upper house and a control group whose representatives sought election to the upper house but were unsuccessful in doing so. This method effectively restricts attention to the treatment group from Section 4.4, i.e. survey respondents from districts where the lower-house representative sought election to the upper house.

Let $Win\left(d_i^t\right) = 1$ if respondent i resides in a district where the lower house representative was successfully elected to the upper house in year t. Conversely, $Win\left(d_i^t\right) = 0$ if respondent i resides in a district where the lower house representative sought election to the upper house in year t, but was unsuccessful in doing so. In this case the explanatory variable of interest can be written as:

$$x_i = \begin{cases} 1 & \text{if } Win\left(d_i^t\right) = 1 \\ 0 & \text{if } Win\left(d_i^t\right) = 0 \end{cases}$$

Such an approach restricts attention to those observations relating to respondents residing in congressional districts where a lower-house representative sought election to the upper house. Survey responses from all other respondents are therefore discarded. This substantially reduces the size of the dataset, from around 400,000 to 140,000 respondents (the exact figure depends on which control variables are included in regressions). The regression equation can be written as:

$$\ln\left(y_i^t\right) = \alpha_0 + \alpha_1 Win\left(d_i^t\right) + \alpha_2 Z_i^t + \epsilon_i^t$$

The first result reported in Table 4.2 compares the log-incomes of citizens from congressional districts where the representative was successfully elected to the upper house in the year in which they responded to the BRFSS survey and other constituents who reside in congressional districts where the representative unsuccessfully sought election to the upper house in the year in which they were surveyed, with no additional controls. This result suggests that respondents whose representatives are successfully promoted to the upper house in the same year have incomes that are 1.45 percent higher than respondents whose representatives are unsuccessful in seeking election to the upper house. This effect

Table 4.2: Table of regressions comparing average incomes in districts where the representative was successfully elected to the upper house in the same year against average incomes in districts where the representative unsuccessfully sought election to the upper house in the same year. The dependent variable is the natural logarithm of the mid-point of the income bracket of each respondent (mean = 10.55 and standard deviation = 0.81 for the entire sample, where $N = 144,817$).

	(1)	(2)	(3)	(4)
	Income (log)	Income (log)	Income (log)	Income (log)
Rep. elected to Senate	0.0145	0.0262	0.0257	0.0261*
	(0.35)	(1.68)	(1.76)	(2.24)
Respondent age			0.0493***	0.0207***
			(34.18)	(17.03)
Respondent age sqd			-0.000537***	-0.000253***
			(-37.35)	(-21.19)
Female respondent			-0.163***	-0.108***
			(-28.36)	(-23.50)
Hispanic respondent			-0.426***	-0.186***
			(-16.49)	(-11.64)
Year dummies	No	Yes	Yes	Yes
State dummies	No	Yes	Yes	Yes
Education dummies	No	No	No	Yes
Marital status dummies	No	No	No	Yes
Race dummies	No	No	No	Yes
Observations	144817	144817	143675	139041
Adjusted R^2	0.000	0.051	0.141	0.406

t statistics in parentheses

Standard errors clustered at state-year level

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

is not found to be statistically significant, however, with a t-statistic of just 0.35. Column 2 introduces dummy variables to control for fixed effects associated with the year and state in which the respondent was surveyed. The coefficient of interest increases so that citizens of districts where the representatives are successfully promoted to the upper house have incomes that are 2.6 percent higher than citizens of districts where the representative was unsuccessful in their bid to be elected to the upper house. The t-statistic remains below the threshold required to indicate statistical significance at the 5% level.

Columns 3 and 4 in Table 4.2 report the results of further regressions, each including controls for the characteristics of the survey respondents (the constituents). The coefficient of interest remains largely unchanged by the inclusion of these additional controls, indicating that citizens of congressional districts where the representative was successfully promoted to the upper house in the same year have incomes that are 2.6 percent higher than those from districts where the representative unsuccessfully sought election to the upper house in the same year. The added precision that is achieved by including all these additional controls in Column 4 means that in this case the difference is found to be statistically significant at the 5% level.

With regard to the control variables, older respondents are again found to have higher incomes, with the effect decreasing as their age increases. Female and hispanic respondents are again found to have lower incomes on average than male and non-hispanic respondents respectively.

These results suggest that legislators who are successful in their attempts to gain promotion from the lower to the upper house served congressional districts with higher incomes on average than the districts served by similar legislators who unsuccessfully tried to gain promotion to the upper house. Based on these results alone, however, it is not possible to offer any insight as to whether that effect comes about from a preference among voters for representatives from already-wealthy districts (for example if the wealth of the district was positively correlated with campaign spending), or whether this could be interpreted as voters rewarding representatives who served congressional districts where average incomes were seen to have increased under their tenure. The following analysis will follow a similar pattern to that in Section 4.4 by comparing survey respondents at different times before and after their representatives sought election to the upper house.

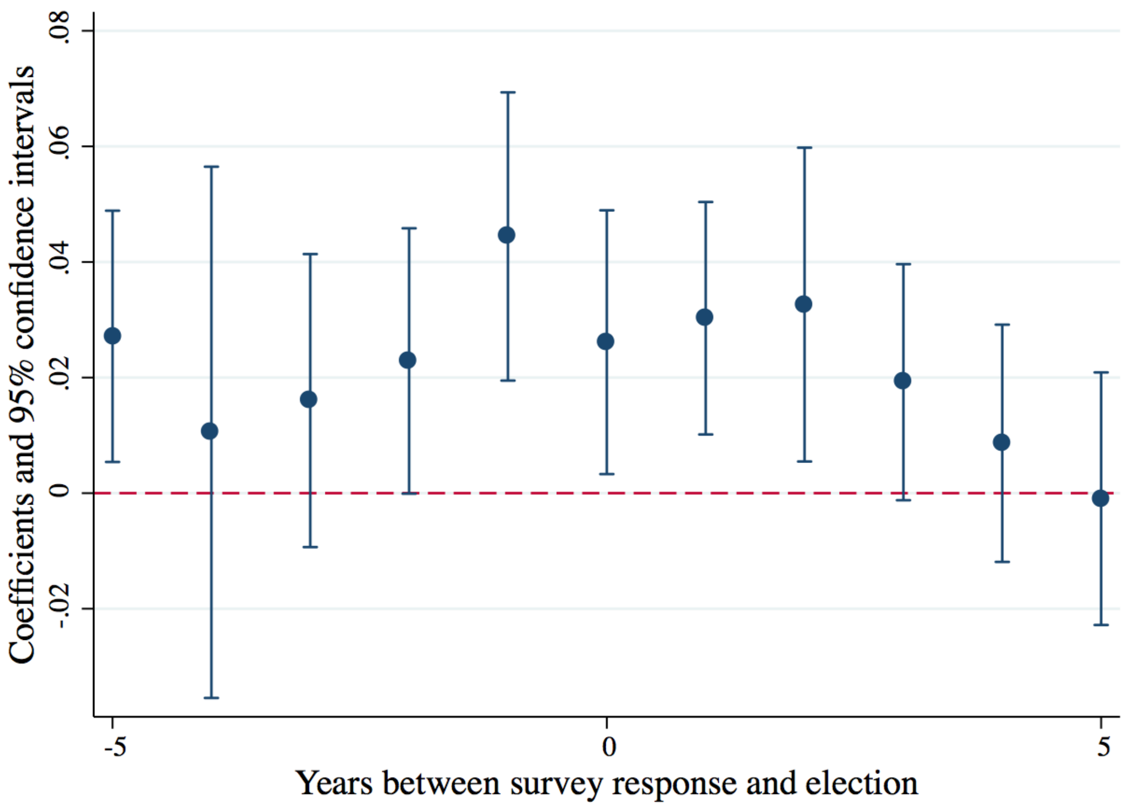
4.5.1 Lagged Regressions

To shed some light on the reason behind the differences in incomes between the districts where representatives are successful in being promoted to the upper house and those that seek promotion unsuccessfully, it is useful to consider to what extent these differences persist over time. To achieve this, similar regressions will be run as were reported in Section 4.4, where survey responses are taken at different time periods before and after a representative sought election to the upper house. All the possible control variables for respondent characteristics are included in these regressions (as in Column 4 of Table 4.2) and the estimation will again be carried out under time-lags ranging from -5 to +5. The detailed results of such regressions (including coefficients associated with the control variables) are left to the Appendix (Table 4.6). Instead, Figure 4.2 shows a graph of the coefficients of interest over time, as well as the 95% confidence intervals associated with these estimated coefficients.

The leftmost datapoint in Figure 4.2 reports the difference between the log-incomes of BRFSS respondents from districts where five years later the representative was successfully elected to the upper house and the log-incomes of respondents whose representatives unsuccessfully attempted to gain election to the upper house five years after they answered the BRFSS survey. This datapoint shows that incomes were found to be higher among those respondents whose representatives were successful in gaining election to the upper house five years later and significantly so.

Moving to the right in Figure 4.2, comparing respondents four years before their representatives sought election to the upper house, the difference is substantially reduced and is no longer statistically significant. From this point, the coefficient then increases slightly when comparing respondents three years and then two years before their representatives sought election to the upper house. In neither case is the difference found to be significantly different from zero at the 5% level. Moving rightwards again, however, when comparing respondents one year before their representatives sought election to the upper house, the coefficient increases quite dramatically and is now found to be statistically significant at the 5% level. This implies that when comparing survey respondents whose representatives

Figure 4.2: Each of the points in the graph shows the result of a regression estimating the difference in incomes between residents of districts where the lower house political representative was successfully elected to the upper house and those where the lower-house representative unsuccessfully sought promotion to the upper house. The 95% confidence intervals around these estimated coefficients are also shown. The horizontal axis plots the lag variable. Each point therefore shows the difference between the average incomes of the two groups at some interval before or after the representative sought election to the upper house. For example, the point at $lag = -5$ shows the coefficient described above 5 years before the representative sought promotion. The graph below therefore illustrates how the coefficient of interest evolves over time either side of the period where the representative sought election to the upper house (i.e. when $lag = 0$).



sought election to the upper house in the following year, those respondents whose representatives were successful are found to have incomes that are around 4.4 percent higher on average than respondents whose representatives unsuccessfully attempted to gain election to the upper house the year after the citizens responded to the BRFSS survey.

Continuing to the right in Figure 4.2, coefficients are found to remain positive and statistically significant when comparing respondents whose representatives sought election to the upper house in the same year (as shown previously in Table 4.2). Furthermore, for citizens who were surveyed one year and two years after their representative sought election to the upper house, incomes were again found to be higher among those respondents whose representatives were successful in being elected to the upper house, with the difference being statistically significant at the 5% level. Next however, comparing citizens who responded to the BRFSS survey three years after their representative sought election to the upper house ($lag = +3$), the coefficient of interest is lower and no longer statistically significant at the 5% level. This implies that while incomes are still found to be higher among the survey respondents from the districts where the representatives were successful in being elected to the upper house when comparisons are made three years after the representative sought election to the upper house, the difference is reduced. This is again found to be the case when comparing respondents four years after their representative sought election to the upper house. Finally, when comparing respondents five years after their representatives sought election to the upper house (the rightmost data point) the difference is found to be very slightly negative, which implies that when comparing the incomes of citizens five years after their representatives sought election to the upper house, the citizens from districts where the representative was successful in the election and therefore promoted to the upper house no longer have higher incomes on average than those citizens from districts where the representative unsuccessfully sought election to the upper house.

Overall, the pattern of effects observed in Figure 4.2 suggests that in districts where the representative would later go on to be elected to the upper house, average incomes increase as you compare respondents closer and closer to the year in which their representatives seek election to the upper house, relative to incomes in districts where the representatives go on to unsuccessfully seek election to the upper house. This effect means that at the time where the representatives seek election to the upper house, incomes are higher in the districts served by representatives that go on to win their elections and move to the upper house. Incomes then remain higher among the districts where the representatives were successfully elected to the upper house in the years immediately following the election. From the third year after the elections, however, the difference between the average incomes among the districts where the representatives were successfully elected to the upper house and the districts where the representatives were unsuccessful in their attempts to be elected to the upper house begins to diminish. Eventually, when you compare respondents five years after their representatives sought election to the upper house, the effect completely disappears, with average incomes almost identical between those districts where the representatives were successfully elected to the upper house and those where their election bids were unsuccessful.

This pattern suggests that voters in Senate elections reward representatives who served congressional districts where incomes were seen to increase relative to other districts in the years immediately preceding elections. In particular, the fact that incomes are not found to be significantly different between the two groups in the years before elections, coupled with the fact that the statistically significant differences between the two groups disappear when comparing survey respondents in the years following elections, points to a temporary increase in incomes in the years surrounding the one in which the election takes place among the districts where representatives are successfully elected to the upper house. This pattern goes against the suggestion that representatives that go on to achieve election to the upper house come from congressional districts with permanently higher incomes than those representatives who unsuccessfully seek election to the upper house.

4.6 Conclusion

Many members of the House of Representatives subsequently seek election to the Senate. In the 112th Congress which ran from 2011 to 2013, for example, 49 of the 100 Senators had previously served in the House of Representatives (Ostermeier (2011)). This chapter restricted attention to cases where members of the lower house sought election to the upper house in the US Congress and compared the incomes of constituents in the districts previously served by those representatives under various election outcomes.

Results in Section 4.4 show that representatives who seek election to the upper house having served as lower house representatives tend to have previously served lower house districts with permanently higher incomes on average. Within this group of representatives who seek election to the upper house having served in the lower house, it is found in Section 4.5 that those representatives who are successful in their election bids tend to come from districts where incomes are higher on average than in those districts served by representatives who unsuccessfully sought election to the upper house. Interestingly, in this case, the difference in incomes between the two groups is not so clearly observed in the years before and after the representatives sought election to the upper house. This points to a temporary increase in income, which suggests that those representatives who are successful in achieving election to the upper house seek election at a time when the incomes in the districts in which they previously served are high. In particular, after they have been successful in their election bid and therefore promoted to the Senate, incomes in the congressional districts which they previously served are found to fall back to the same level as the congressional districts in which representatives unsuccessfully sought election to the upper house.

Overall, this shows that while voters in Senate elections tend to be offered candidates who have previously served in districts with permanently higher incomes, voters are found to reward legislators who previously served districts in the lower house where average incomes increased in the years leading up to their bid to be elected to the upper house. These two key findings point to an interesting diversion between political parties and voters in choosing their preferred candidates. In particular, while parties tend to put forward candidates from high-income districts for election to the upper house, voters tend to actually elect the candidates where incomes have risen temporarily under that representative's tenure. Further work that considers cases beyond only those representatives who subsequently seek election to the Senate would be needed to properly assess the external validity of this finding.

While it is possible to draw interesting conclusions about how voters reward legislator performance, it is not possible to determine from this analysis whether voters are necessarily justified in doing so. For example, the fact that incomes increase before representatives seek election to the upper house could be reflecting the fact that incomes were inflated artificially in the lead-up to a promotion bid. For example, large public spending programs which have to be paid for after the legislator has been promoted to the upper house could be politically popular, resulting in legislators winning elections. Citizens in districts where legislators introduce such programs would see higher incomes, which could be rewarded in elections to the upper house, before average incomes fall back after the elections. Conversely, the observed rise in incomes before elections could be attributed to some form of 'political skill', which may in turn also be correlated with the economic outcomes (including incomes) of the citizens of the congressional district. The subsequent absence of this legislator at the local level after they are successfully elected to the upper house could also explain the fall in incomes after they achieve election to the upper house.

Distinguishing more precisely between these potential paths of influence could be a useful area for future research.

References

Bartels, L. (2008). *Unequal Democracy: The Political Economy of the New Gilded Age.* Princeton University Press.

Brunner, E., S. L. Ross, and E. Washington (2013). Does less income mean less representation? *American Economic Journal: Economic Policy*, 53–76.

Centers for Disease Control and Prevention (1994). Behavioral Risk Factor Surveillance System Survey Data.

Clark, A. E. and C. Milcent (2011). Public employment and political pressure: The case of French hospitals. *Journal of Health Economics*, 1103–1112.

Dolan, P., R. Metcalfe, and N. Powdthavee (2008). Electing happiness: Does happiness effect voting and do elections affect happiness? *Discussion Papers 08/30, Department of Economics, University of York.*.

Eggers, A. C. and J. Hainmuller (2009). MPs for sale? Returns to office in postwar British politics. *The American Political Science Review*, 513–533.

Glassman, M. E. and A. H. Wilhelm (2013). Congressional careers: Service tenure and patterns of member service, 1789-2013.

Grimmer, J., E. Hersh, B. Feinstein, and D. Carpenter (2011). Are close elections random? *Stanford University Working Papers*.

Grofman, B., R. Griffin, and G. Berry (1995). House members who become senators: Learning from a 'Natural experiment' in representation. *Legislative Studies Quarterly*, 513–529.

Hainmueller, J. and H. L. Kern (2008). Incumbency as a source of spillover effects in mixed electoral systems: Evidence from a regression-discontinuity design. *Electoral Studies*, 213–227.

Jenkins, J. A., M. H. Crespin, and J. L. Carson (2005). Parties as procedural coalitions in Congress: An examination of differing career tracks. *Legislative Studies Quarterly*, 365–389.

Lee, D. S. (2008). Randomized experiments from non-random selection in U.S. House elections. *The regression discontinuity design: Theory and applications*, 675–697.

Lee, D. S., E. Moretti, and M. J. Butler (2004). Do voters affect or elect policies? Evidence from the U.S. House. *The Quarterly Journal of Economics*, 807–859.

Levinson, S. (2006). *Our Undemocratic Constitution: Where the Constitution Goes Wrong (And How We the People Can Correct It)*. Oxford University Press.

Lovenduski, J. and P. Norris (2003). Westminster women: The politics of presence. *Political Studies*, 84–102.

Nelson, D. E., E. Powell-Griner, M. Town, and M. G. Kovar (2003). A comparison of national estimates from the National Health Interview Survey and the Behavioral Risk Factor Surveillance System. *American Journal of Public Health*, 1335–41.

Ostermeier, E. (2011). Which state's US senators are drawn from the House at the highest rate?

Oswald, A. J. and S. Wu (2010). Objective confirmation of subjective measures of human well-being: Evidence from the U.S.A. *Science*, 576–579.

Phillips, A. (1995). *The Politics of Presence*. Oxford: Clarendon Press.

Ray, B. A. (1980). Congressional losers in the U.S. federal spending process. *Legislative Studies Quarterly*, 359–372.

Rigby, E. and G. C. Wright (2013). Political parties and representation of the poor in the American states. *American Journal of Political Science 57*(3), 552–565.

Stein, R. M. and K. N. Bickers (1994). Congressional elections and the pork barrel. *Journal of Politics*, 377.

Wangnerud, L. and A. Sundell (2012). Do politics matter? Women in Swedish local elected assemblies 1970-2010 and gender equality in outcomes. *European Political Science Review*, 97–120.

Appendix

Table 4.3: Former members of the lower house who were successfully elected to the upper house, 1994-2012.

Year	Name	State	Party	Birth Yr.	House District	House Yrs.	Elec. Margin
1994	Jon Kyl	AZ	R	1942	4	1987-1995	14.2
1994	Olympia Snowe	ME	R	1947	2	1979-1995	23.8
1994	Rod Grams	MN	R	1948	6	1993-1995	5
1994	Mike DeWine	OH	R	1947	7	1983-1991	14.2
1994	James Inhofe	OK	R	1934	1	1987-1995	15.2
1994	Rick Santorum	PA	R	1958	18	1991-1995	2.5
1994	Craig Thomas	WY	R	1933	0	1989-1995	19.6
1996	Ron Wyden	OR	D	1949	3	1981-1996	1.6
1996	Tim Hutchinson	AR	R	1949	3	1993-1997	5.4
1996	Wayne Allard	CO	R	1943	4	1991-1997	5.7
1996	Dick Durbin	IL	D	1944	20	1983-1997	15.4
1996	Pat Roberts	KS	R	1936	1	1981-1997	27.6
1996	Sam Brownback	KS	R	1956	2	1995-1997	10.6
1996	Robert Torricelli	NJ	D	1951	9	1983-1997	10.2
1996	Jack Reed	RI	D	1949	2	1991-1997	28.3
1996	Tim Johnson	SD	D	1946	0	1987-1997	2.6
1998	Blanche Lincoln	AR	D	1960	1	1993-1997	12.9
1998	Mike Crapo	ID	R	1951	2	1993-1999	41.1
1998	Jim Bunning	KY	R	1931	4	1987-1999	0.5
1998	Chuck Schumer	NY	D	1950	9	1993-1999	10.5
2000	Thomas R. Carper	DE	D	1947	0	1983-1993	11.8
2000	Bill Nelson	FL	D	1942	11	1983-1991	4.8
2000	Debbie Stabenow	MI	D	1950	8	1997-2001	1.5
2000	John Ensign	NV	R	1958	1	1995-1999	15.4
2000	George Allen	VA	R	1952	7	1991-1993	4.6
2000	Maria Cantwell	WA	D	1958	1	1993-1995	0.1
2002	Saxby Chambliss	GA	R	1943	8	1995-2003	0
2002	Jim Talent	MO	R	1956	2	1993-2001	1.1
2002	John E. Sununu	NH	R	1964	1	1997-2003	4.4
2002	Lindsey Graham	SC	R	1955	3	1995-2003	10.2

2004	Johnny Isakson	GA	R	1944	6	1999-2005	17.9
2004	David Vitter	LA	R	1961	1	1999-2005	21.7
2004	Richard Burr	NC	R	1955	5	1995-2005	4.6
2004	Tom Coburn	OK	R	1948	2	1995-2001	11.6
2004	Jim DeMint	SC	R	1951	4	1999-2005	9.6
2004	John Thune	SD	R	1961	0	1997-2003	1.1
2006	Ben Cardin	MD	D	1943	3	1987-2007	10
2006	Bob Menendez	NJ	D	1954	13	1993-2006	9.1
2006	Sherrod Brown	OH	D	1952	13	1993-2007	12.4
2006	Bernie Sanders	VT	Other	1941	0	1991-2007	33.1
2008	Mark Udall	CO	D	1950	2	1999-2009	10.3
2008	Tom Udall	NM	D	1948	3	1999-2009	22.6
2010	John Boozman	AR	R	1950	3	2001-2011	21.1
2010	Mark Kirk	IL	R	1959	10	2001-2010	1.9
2010	Dan Coats	IN	R	1943	4	1981-1989	18.3
2010	Jerry Moran	KS	R	1954	1	1997-2011	44.1
2010	Roy Blunt	MO	R	1950	7	1997-2011	13.7
2010	Kirsten Gillibrand	NY	D	1966	20	2007-2009	26.2
2010	Rob Portman	OH	R	1955	2	1993-2005	18.3
2010	Pat Toomey	PA	R	1961	15	1999-2005	2
2012	Jeff Flake	AZ	R	1962	6	2003-2013	3.1
2012	Chris Murphy	CT	D	1973	5	2007-2013	11.7
2012	Mazie Hirono	HI	D	1947	2	2007-2013	25.2
2012	Joe Donnelly	IN	D	1955	2	2007-2013	5.8
2012	Martin Heinrich	NM	D	1971	1	2009-2013	5.7
2012	Tammy Baldwin	WI	D	1962	2	1999-2013	5.5

Table 4.4: Former members of the lower house who unsuccessfully sought election to the upper house, 1994-2012.

Year	Name	State	Party	Birth Yr.	House District	House Yrs.	Elec. Margin
1994	Sam Coppersmith	AZ	D	1955	1	1993-1995	-14.2
1994	Michael Huffington	CA	R	1947	22	1993-1995	-1.9
1994	Jim Jontz	IN	D	1951	5	1987-1993	-36.9
1994	Thomas Andrews	ME	D	1953	1	1991-1995	-23.8
1994	Bill Brock	MD	R	1930	3	1963-1971	-18.2
1994	Bob Carr	MI	D	1943	8	1993-1995	-9.2
1994	Alan Wheat	MO	D	1951	5	1983-1995	-24.1
1994	Dave McCurdy	OK	D	1950	4	1981-1995	-15.2
1994	Jim Cooper	TN	D	1954	4	1983-1995	-21.8
1996	Jim Ross Lightfoot	IA	R	1938	3	1993-1997	-5.1
1996	Joseph E. Brennan	ME	D	1934	1	1987-1991	-5.4
1996	Dick Swett	NH	D	1957	2	1991-1995	-3.1
1996	Dick Zimmer	NJ	R	1944	12	1991-1997	-10.2
1998	Gary Franks	CT	R	1953	5	1991-1997	-32.7
1998	Scotty Baesler	KY	D	1941	6	1993-1999	-0.5
1998	John Ensign	NV	R	1958	1	1995-1999	-0.1
1998	Bob Inglis	SC	R	1959	4	1993-1999	-7
1998	Linda Smith	WA	R	1950	3	1995-1999	-16.8
1998	Mark Neumann	WI	R	1954	1	1995-1999	-2.2
2000	Tom Campbell	CA	R	1952	15	1995-2001	-19.2
2000	Bill McCollum	FL	R	1944	8	1983-2001	-4.8
2000	Bob Franks	NJ	R	1951	7	1993-2001	-3
2000	William T. Redmond	NM	R	1954	3	1997-1999	-23.4
2000	Rick Lazio	NY	R	1958	2	1993-2001	-12.3
2000	Ron Klink	PA	D	1951	4	1993-2001	-6.9
2000	Robert Weygand	RI	D	1948	2	1997-2001	-15.7
2002	Greg Ganske	IA	R	1949	4	1995-2003	-10.4
2002	John Thune	SD	R	1961	0	1997-2003	-0.1
2002	Bob Clement	TN	D	1943	5	1988-2003	-10.1
2004	Denise Majette	GA	D	1955	4	2003-2005	-17.9
2004	Chris John	LA	D	1960	7	1997-2005	-21.7

2004	Eric Fingerhut	OH	D	1959	19	1993-1995	-27.8
2004	Brad Carson	OK	D	1967	2	2001-2005	-11.6
2004	Joe Hoeffel	PA	D	1950	13	1999-2005	-10.6
2004	George Nethercutt	WA	R	1944	5	1995-2005	-12.3
2006	Katherine Harris	FL	R	1957	13	2003-2007	-22.2
2006	Mark Kennedy	MN	R	1957	6	2003-2007	-20.2
2006	Harold Ford, Jr.	TN	D	1970	9	1997-2007	-2.7
2008	Bob Schaffer	CO	R	1962	4	1997-2003	-10.3
2008	Larry LaRocco	ID	D	1946	1	1991-1995	-23.6
2008	Jim Slattery	KS	D	1948	2	1983-1995	-23.6
2008	Tom Allen	ME	D	1945	1	1997-2009	-22.7
2008	Dick Zimmer	NJ	R	1944	12	1991-1997	-14
2008	Steve Pearce	NM	R	1947	2	2003-2009	-22.6
2010	Kendrick Meek	FL	D	1966	17	2003-2011	-28.8
2010	Brad Ellsworth	IN	D	1958	8	2007-2011	-18.3
2010	Charles Melancon	LA	D	1947	3	2005-2011	-18.9
2010	Paul Hodes	NH	D	1951	2	2007-2011	-23.5
2010	Joseph DioGuardi	NY	R	1940	20	1985-1989	-26.2
2010	Joe Sestak	PA	D	1951	7	2007-2011	-2
2012	Connie Mack I	FL	R	1967	14	2005-2013	-13
2012	Pete Hoekstra	MI	R	1953	2	1993-2011	-20.8
2012	Todd Akin	MO	R	1947	2	2001-2013	-15.8
2012	Denny Rehberg	MT	R	1955	0	2001-2013	-3.7
2012	Shelley Berkley	NV	D	1951	1	1999-2013	-1.2
2012	Heather Wilson	NM	R	1960	1	1998-2009	-5.7
2012	Rick Berg	ND	R	1959	0	2011-2013	-0.9

Table 4.5: Full table reporting regressions comparing constituent incomes in districts where the representative sought election to the upper house compared with other residents of the same state, but who came from different districts, where the *lag* variable (indicating the number of years between the survey responses and the representative seeking election to the upper house) takes all possible integer values from -5 to $+5$. The dependent variable in all cases is the natural logarithm of the mid-point of the income bracket of each respondent.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	-5 yrs	-4 yrs	-3 yrs	-2 yrs	-1 yr	Same yr	+1 yr	+2 yrs	+3 yrs	+4 yrs	+5 yrs
Rep. sought promotion to Senate	0.0454	0.0356	0.0366	0.0443	0.0196	0.0277	0.0111	0.0291	0.0394	0.0406	0.0441
	(3.63)	(2.86)	(3.32)	(4.00)	(1.30)	(2.24)	(0.76)	(2.25)	(2.34)	(2.83)	(3.28)
Respondent age	0.0239	0.0232	0.0207	0.0198	0.0207	0.0189	0.0217	0.0219	0.0228	0.0204	0.0220
	(9.82)	(17.02)	(11.44)	(9.85)	(16.96)	(12.51)	(16.69)	(21.75)	(20.91)	(12.18)	(17.93)
Respondent age sqd	-0.000289	-0.000283	-0.000255	-0.000245	-0.000251	-0.000233	-0.000260	-0.000261	-0.000271	-0.000246	-0.000265
	(-12.20)	(-21.01)	(-14.57)	(-11.39)	(-20.16)	(-14.44)	(-21.15)	(-25.27)	(-25.06)	(-14.09)	(-22.09)
Female respondent	-0.111	-0.119	-0.114	-0.119	-0.107	-0.115	-0.112	-0.111	-0.106	-0.116	-0.108
	(-25.70)	(-30.90)	(-30.67)	(-36.48)	(-30.39)	(-32.39)	(-31.53)	(-27.72)	(-29.29)	(-34.85)	(-28.90)
Hispanic respondent	-0.196	-0.194	-0.191	-0.185	-0.198	-0.197	-0.191	-0.202	-0.216	-0.211	-0.229
	(-10.12)	(-11.79)	(-11.03)	(-12.63)	(-13.16)	(-12.41)	(-10.98)	(-11.00)	(-11.19)	(-12.07)	(-10.79)
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Education dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Marital status dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Race dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	215089	233024	274913	318019	340046	390641	381908	384655	357911	394428	374046
Adjusted R^2	0.395	0.403	0.398	0.402	0.397	0.404	0.401	0.408	0.403	0.400	0.398

t statistics in parentheses

Standard errors clustered at representative level

Table 4.6: Full table reporting regressions comparing average incomes in districts where the representative was successfully elected to the upper house against average incomes in districts where the representative unsuccessfully sought election to the upper house, where the *lag* variable (indicating the number of years between the survey responses and the representative seeking election to the upper house) takes all possible integer values from -5 to $+5$. The dependent variable in all cases is the natural logarithm of the mid-point of the income bracket of each respondent.

	(1) -5 yrs	(2) -4 yrs	(3) -3 yrs	(4) -2 yrs	(5) -1 yr	(6) Same yr	(7) +1 yr	(8) +2 yrs	(9) +3 yrs	(10) +4 yrs	(11) +5 yrs
Rep. elected to Senate	0.0271	0.0105	0.0160	0.0229	0.0444	0.0261	0.0303	0.0326	0.0192	0.00863	-0.000961
	(2.45)	(0.45)	(1.24)	(1.95)	(3.49)	(2.24)	(2.95)	(2.36)	(1.84)	(0.82)	(-0.09)
Respondent age	0.0258	0.0243	0.0210	0.0223	0.0217	0.0207	0.0237	0.0239	0.0245	0.0236	0.0256
	(13.64)	(15.05)	(11.18)	(13.35)	(14.30)	(17.03)	(16.52)	(16.79)	(15.04)	(15.86)	(18.85)
Respondent age sqd	-0.000307	-0.000291	-0.000259	-0.000271	-0.000260	-0.000253	-0.000281	-0.000283	-0.000286	-0.000276	-0.000302
	(-16.25)	(-18.24)	(-14.13)	(-15.95)	(-16.52)	(-21.19)	(-20.33)	(-20.08)	(-17.02)	(-17.92)	(-22.90)
Female respondent	-0.112	-0.120	-0.114	-0.119	-0.106	-0.108	-0.118	-0.113	-0.103	-0.116	-0.106
	(-19.03)	(-20.46)	(-19.14)	(-20.53)	(-23.32)	(-23.50)	(-24.33)	(-23.46)	(-20.51)	(-24.70)	(-20.98)
Hispanic respondent	-0.191	-0.192	-0.178	-0.177	-0.189	-0.186	-0.182	-0.168	-0.184	-0.184	-0.206
	(-8.86)	(-12.92)	(-11.01)	(-11.43)	(-14.14)	(-11.64)	(-9.87)	(-8.52)	(-10.64)	(-11.07)	(-8.52)
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Education dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Marital status dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Race dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	77648	87716	95549	110520	126732	139041	127846	126569	124574	131455	120792
Adjusted R^2	0.404	0.406	0.405	0.403	0.394	0.406	0.409	0.411	0.400	0.399	0.398

t statistics in parentheses

Standard errors clustered at representative level

Chapter 5

Thesis Conclusions

The four chapters that comprise this thesis are linked in that they aim to shed light on the incentives and effects of political legislators.

There is a growing literature on 'Deliberative Democracy', whereby the process of discussion and deliberation is central to the democratic process. One might argue that legislators will only have an incentive to participate effectively in such systems if they can achieve personal payoffs by doing so. These chapters therefore supplement this literature by measuring whether legislators can expect to achieve payoffs by participating in the political process, either by achieving their preferred political outcomes, or by being more likely to be reelected to their constituency seat.

The first two chapters consider the incentives that political legislators face to be active participants in the democratic process. In particular, the first considers the incentives in terms of the political payoffs that legislators might hope to achieve by winning support from others for their preferred political outcomes. The second considers the payoffs that can be achieved by being reelected to office.

Taking each of these in turn, the first chapter looks in particular at the effects that legislators can have on one-another. Such a path of influence is key if legislators are to feel that there is a possibility of gaining the support of others for political outcomes that they consider to be desirable. This chapter builds on the extensive literature on 'Deliberative Democracy', using techniques formulated to investigate the presence of peer effects in social networks. The economics of such social networks is another area of the literature which has attracted huge interest in recent years and this chapter builds on this literature in a similar way.

The second chapter considers the effect that legislators can have on the probability that they are reelected to office by being active in parliament, in particular by asking questions in debates. This can be observed by constituents who might in turn reward legislators because they see them as more visible, or because they are seen to be promoting causes that are particularly significant for those constituents. A vast literature in the area of political science considers how voters respond to various political characteristics and behaviors. Much of this literature, however, is hampered by its inability to distinguish between the effects of the innate characteristics of the legislators being studied and the effects of the specific actions they take, such as asking parliamentary questions. The inclusion of control variables in model specifications can only ever control for those characteristics that can be observed and as such these studies will always be subject to criticism on the grounds of omitted variable bias. The second chapter presented in this thesis uses the randomization associated with a ballot system used in parliament to address these problems in an original way.

Once it has been established that participants in the democratic system can influence their outcomes both by gaining the support of others for their own preferred political outcomes and by improving their own reelection probability, it is important to consider whether the democratic system itself is conducive to promoting such desirable behaviour.

There is a large literature which considers whether impartial participants can be expected to act without bias when balancing the opposing interests of other players in the game. This literature includes extensive analysis of referees in sports environments. The third chapter of this thesis is the first to apply this type of analysis to a political setting and does so to shed light on a question which has attracted much media attention in recent years; namely whether the Speaker in the House of Commons, who is mandated to be politically unbiased, can be relied upon to give all Members of Parliament an equal opportunity to ask questions in debates conducted in the House of Commons.

The final part of this thesis builds on the previous chapters, by considering the relationship between legislators' actions and the outcomes of the constituents they represent. While this chapter is not able to identify a causal relationship, due to the possibility of reverse causality, it asks the question of whether certain types of legislator can be expected to deliver particular outcomes for the constituents they represent.

The 'bigger picture' around all four of these chapters is that it is important to understand the motivations of legislators to have any chance of designing these incentives to ensure that they will effectively carry out their duties as elected representatives. These incentives may be purely centred around achieving re-election, or by achieving their wider political objectives through effective lobbying in Parliament.

These questions are of huge importance in the effective functioning of democracy and there is a vast literature, a small proportion of which has been cited in this thesis, which aims to provide answers to these questions. Beyond this literature, there is a media operation which interprets political data to hold legislators to account for their actions, particularly when their incentives appear to diverge from those that are expected of them. The multitude of political scandals exposed by the media are testament to that fact.

Much of this media commentary, however, and even a large part of the academic literature, draws conclusions from simply observing anomalies in the data either over time or across legislators, thus implicitly making assumptions about constant behaviour or political backgrounds over time and across geographical areas. In essence, many of these studies fail to effectively control for the individual characteristics of MPs. An example of such inference would be cases where politicians and commentators infer that the Speaker of the House of Commons is biased, based on the fact that he or she intervenes in debates more often against one particular party. Such an inference ignores the fact that members of that party may have certain characteristics that differ from the members of other parties and lead them to act in a way which merits greater intervention by the Speaker, even if that Speaker was completely unbiased. These studies can draw attention to a correlation between political outcomes, but it would be dangerous to infer causality under such strong assumptions.

All of the chapters in this thesis address these concerns by exploiting some form of randomization, or in the case of the final chapter, by looking at how coefficients evolve over short time-windows either side of a change in the political environment.

So to summarize, this thesis considers whether political legislators have an incentive to participate effectively in the political systems in which they operate and whether they can hope to influence outcomes, including legislators who are mandated to be politically unbiased. On finding that they can do so, the thesis goes on to consider whether these actions are correlated with the outcomes of the constituents they represent, finding tentative evidence that this is indeed the case. Looking forward, the preparation of this thesis has involved the collection and compilation of new datasets which could be extremely informative on further aspects of the incentives faced by legislators. Further work is planned using these datasets, for example in posing the question of whether legislators who ask parliamentary questions are more likely to be elected to senior positions within their political parties.