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Three Empirical Essays Inspired by Major Political Economy Events

by

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Abstract

This thesis presents three empirical studies, each motivated by separate major political economy events that took place in the recent past, namely the 2014 Scottish independence referendum, 2016 EU referendum and the 2016 US presidential election.

This initial empirical chapter considers the causes of fiscal decentralization with a focus on the role of ethnic diversity. Using two new measures of decentralization that capture decision making autonomy and accounting for the depth of divisions between ethno-linguistic groups using the structure of language trees, I find that ethnic diversity has a positive effect on decentralization. It is the amount of fractionalization towards the leaves of the trees, where groups are more numerous and less distinct, that drives decentralization.

The subsequent chapter explores the causal effect of exposure to the government's mailshot on the 2016 EU referendum. I find that individuals who were exposed to the leaflet were less likely to vote to leave the EU in the referendum. I show that the effect is driven by certain groups who only read the leaflet as a source of information and Conservative supporters who were exposed to other sources of information. The evidence is consistent with the idea that voters vary in their susceptibility to persuasion bias, which allows for heterogeneous effects across demographic groups.

The final empirical chapter investigates the role of partisan alignment in the allocation of federal transfers using data from the US states. This chapter finds that the president shows a bias towards his co-partisans with federal transfers. The size of this manipulation is larger when accounting for electoral incentives. The results are in accordance with the theory that the president aims to increase his party's re-election probability by vertical performance spill-overs from lower tiers of governance.

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Chapter 1 Introduction

1.1 Motivation and aims

This thesis is motivated by three seismic political economy events that occurred in recent history. Namely, the 2014 Scottish independence referendum, 2016 EU referendum and the 2016 US presidential election. All of which were of huge international interest and importance. The ramifications of each event, whether successful or not, stretch far beyond the domestic markets they immediately impact and spill onto the international landscape.

The first of these events, the Scottish independence referendum, took place on the 18th September 2014. This referendum gave Scottish voters the opportunity to express whether they would like to remain a part of the United Kingdom with further devolved fiscal powers or leave the union altogether. The referendum was the panacea of the Scottish independence movement -cultured and endorsed by the Scottish National Party, the governing party in Scotland- as it gave the people a binding say on independence. Naturally, the fact that a referendum was even taking place spilled over to other independence movements and furthered support for their own separatist aspirations. The organisers of the 1st October 2017 illegal referendum on Catalan independence likely took confidence from the past Scottish attempt. The economic implications of independence were a key factor in the outcome. Not only would an independent Scotland struggle to join the EU, for example Spain said they veto any attempt in order to prevent further secessionist momentum in the Basque and Catalan regions, but independence would also require an entirely new currency

as Scotland would be unable to continue to use the British pound sterling. Using this situation as a point of motivation, it is interesting to explore how countries have arrived at their current level of devolved fiscal powers. In the final referendum count the 'No' side prevailed with 55.3% of the vote to 44.7% and Scotland remained part of the United Kingdom. Support for a second such vote on Scottish independence has been building since Scotland, on the whole, voted to remain in the UK's EU referendum.

With this as a clear point of motivation, the first aim of this thesis is to explore how countries have arrived at their current level of decentralized fiscal powers. There is a small portion of literature that assess this this question (Oates, 1972, Panizza, 1999, Letelier, 2005, Treisman, 2006, Bodman and Hodge, 2010, Sambanis and Milanovic, 2014). However, the previous work is plagued by insufficient measures of the degree of decentralization and lacks a clear focus on any one particular driver of decentralization. In this chapter, I introduce a new set of measures by Hooghe et al. (2016) which are a marked improvement on the current ones. Moreover, I employ an instrumental variable estimator to ensure a proper identification of the effect of interest, which is the amount of ethnic diversity – an area where there are currently mixed results in the current literature.

The next event of study, and chronologically, is the aforementioned 2016 EU referendum. An in-out referendum on EU membership was a promised by David Cameron, then leader of the Conservative party, in the Conservative manifesto for the 2015 general election. This saw the Conservative party elected to government by a narrow majority of parliamentary seats. However, the origins of the EU referendum go back to John Major's premiership, which created a rift within the Conservative party on the issue of EU membership. The unchecked rise of UKIP after the 2010 and 2015 general elections, a party whose seemingly sole objective was to leave the EU, was a further motivating factor. So, to appease party in-fighting and combat the growth of UKIP, Cameron called a referendum for the 23rd June 2016. He began to negotiate with the EU over special status for the UK within the EU, however, the

consignments were considered to be trivial. The campaign itself was characterised by a misuse of information and statistics, particularly the leave campaign for the argument that the UK sends £350 million to the EU per week, whereas the actual figure is much smaller. The remain side was accused of scaremongering, dubbed "Project Fear" by prominent leave campaigners, in response to the remain campaign's apparent focus on economic pain after leaving the EU. The leave side won the referendum with a vote share of 51.9% to 48.1% for remain. The driving force behind this is argued by some to be because of the 'left-behind' voters. Individuals whose personal situation since the global financial crisis has not improved and have seen their real wages stagnate used their vote to rail against the current order. It also is argued that another element behind the outcome was the populist overtones in campaigning. Anti-immigration, anti-intellectual, nationalistic and isolationist rhetoric appealed to a large proportion of the electorate who had been "left behind" after the financial crisis. Understanding the determinants of the leave vote is clearly a critical area of research.

The second aim of this thesis is to consider what factors acted as aggravating or mitigating factors to an individual's propensity to vote to the leave the EU. The rapidly expanding current literature on Brexit has focused on mere correlations between individual or regional-level characteristics and the leave vote (Langella and Manning, 2016, Goodwin and Heath, 2016*a*,*b*, Antonucci et al., 2017, Los et al., 2017, Colantone and Stanig, 2017, Becker et al., 2017). This chapter contributes the first evidence from a quasi-natural experiment on why a person may, or may not, be more likely to vote leave. More specifically, I explore the impact of the UK government's pro-remain leaflet on voting behaviour for the populist outcome Brexit. Populism had seemingly won out in the UK, and Italy on a constitutional referendum, although it fared worse in the French, German, Dutch and Austrian elections. The United States, however, is a very different case.

The 2016 presidential election in the US was contested between Democratic nominee, Hillary Clinton, and Donald Trump, the Republican candidate. The election on

the 8th November 2016 saw Donald Trump elected president winning the electoral college by 304 to 227 votes. Despite losing the popular vote by 2.1 percentage points, 46.1% to 48.2%, Trump took office in January 2017. The outcome was heralded by many as one of the greatest upsets in political history. The campaign pitted one of the most qualified people to ever run for president, a person seen as a typical politician that would signal business as usual if elected, in the mould of Clinton. Against a reality television star and real estate tycoon who offered something the polar opposite. Trump's campaign was modern day populism typified, which often found him marred in controversy. Everything you need to know about the style can be boiled down to his catchy campaign slogan 'Make America Great Again'. Since being elected, Trump has changed the status-quo. The economic implications are vast given the size of the US economy. Withdrawing from free trade deals like NAFTA (North American Free Trade Agreement) signalled a more protectionist US trade policy, whilst simultaneously taking on foreign low wage manufacturing sectors, such as China and Mexico, that Trump believed to be taking US jobs. The US stock markets grew their market capitalisation on the speculation of corporate tax cuts, relaxing fossil fuel regulations and huge domestic infrastructure spending, the latter of which is unlikely to happen. These are just two examples of the economic consequences of the election, but both have global repercussions and spill-overs.

Even the Trump case in isolation makes it crucial to understand how the president can consolidate his, and his party's, power. The literature has established that central leaders redistribute funds disproportionately to their co-partisans across the world (see, e.g., Veiga and Pinho (2007) for Portugal, Fouirnaies and Mutlu-Eren (2015) for the UK or Rumi (2014) for Argentina), and that electoral cycles exist at the both the federal and state-level in the US (Tufte, 1978, Alesina et al., 1997, Levitt, 1997). However, they have yet to be combined to explore the electoral incentives that underpin why, in this context, a president may favour his co-partisans at the lower tiers of government. This is the third and final aim of this thesis.

1.1.1 Overview of chapter 2

This chapter analyses the causes of fiscal decentralization. The first objective is to identify the specific role ethnic diversity plays and contribute to the relatively new strand of diversity and economic development literature. The second is to contribute a new set of decentralization measures that better capture the decision making autonomy each sub-national government has.

The analysis uses evidence from a cross-section of 78 countries, which features small island nations like Haiti and large wealthy countries such as the US. A new dataset of decentralization measures are used to better capture the true degree of fiscal decentralization. In addition, this chapter employs data on long-run ethnic diversity that accounts for the depth of cleavages between ethno-linguistic groups. A methodical contribution is also made by using an instrumental variable approach. This is adopted to account for endogeneity concerns, in particular reverse causality. The methodology involves instrumenting diversity with the origin of anatomically modern human life in a given country – a plausibly exogenous driver of ethnic diversity.

The findings show that increasing levels of diversity leads to a higher degree of of decentralization. More specifically, I find that it is the finer, more superficial cleavages between the ethno-linguistic groups that is driving the degree of decentralization. Using events of civil unrest, this chapter also argues that more sub-national autonomy is provided for reasons of political inclusivity –to integrate the diverse population into a locality, which is in accordance with Alesina and Ferrara (2005)– and economic efficiency. The policy implications of this chapter are simple yet important. As the world continues on its path becoming a more open and globalised place, authority will continue to become more decentralized in order to avoid tensions developing. The extent to which decentralization can be seen as positive outcome

relies on its ability to increase ethnic integration and create economic growth.

1.1.2 Overview of chapter 3

Chapter 3 investigates the UK government's pro-remain leaflet as a potential determinant of vote preference in the 2016 EU referendum. This chapter is the first evidence from a quasi-natural experiment in a novel setting of the EU referendum. It directly contributes to the literature on what, or who, drove the Brexit vote.

Using data from the British Election Study (BES), this chapter implements a matching and difference-in-difference methodology to identify the causal effect of exposure to the government's EU leaflet. I use a sample of 6,123 individuals who were surveyed in the weeks leading up to the referendum and are located across the UK. After careful considerations regarding the identification strategy, the argument is further supported using two sub-samples of individuals: those who were exposed to low levels of other referendum information and those who used multiple sources of information. Furthermore, this chapter allows for a heterogeneous treatment effect across various demographic groups, which is in accordance with the idea that groups of individuals who are more liable to persuasion bias should be affected to a greater extent. This chapter also examines the mechanism of impact by exploiting additional survey questions. More specifically, it tests whether the leaflet affected turnout or acted as a persuasive tool for turning leave votes into remain votes.

The findings of this chapter show that exposure to the leaflet did in fact reduce an individuals probability of voting leave by 3 percentage points. By splitting the sample between individuals with levels of high and low levels of exposure to other referendum information, I show that demographics who are more liable to persuasion bias, with low levels of other information exposure, are driving this effect. For instance, females and the risk averse are even more likely to vote remain after expo-

sure. In terms of partisanship, this research finds that individuals with high levels of other information exposure, who are Conservative partisans, are again even more likely to vote remain. The chapter presents the persuasive mechanism by examining how an individual's opinion of a particular contentious issue if a leave vote won changes after exposure. In terms of policy implications, this chapter offers insights into a more effective political campaign based on undiluted, clear information and a more targeted campaign based on persuasion.

1.1.3 Overview of chapter 4

Chapter 4 focuses on the role of partisan alignment, between the US president and state governors, in the allocation of federal grants to US states over the period 1950 – 2008. This is an exploration of how an elected president may seek to consolidate power and maintain office at the presidential level for his party.

A fixed effects methodology is applied here and the effect is identified using the quasi-randomness of alignment in the US. This chapter constructs an argument to show that the alignment bias towards co-partisans is driven by electoral incentives. Here, I use the exogenous variation of the national electoral calendar to identify the alignment effect in election years. Additional evidence to support the electorally driven argument is shown by exploiting the existence of swing states based on the margin of victory in past presidential elections. I also show which political party is driving the effect and provide an explanation as to why this may be. This chapter also investigates the role of the governors personal characteristics that the president may duly consider when allocating funds. In particular, gender, age, term limits and prior political careers.

The findings show that the growth rate of grants increases when the president and governor are of the same party, that is, when they are politically aligned. Moreover,

I find that in election and pre-election years the amount of grants increases significantly more. This electorally driven argument is supported by exploring the role of swing states. These are states that are critical to winning the electoral college on the way to the presidency, I find that these are disproportionately targeted when a co-partisan of the president is governor. This chapter also finds that governors with prior political experience in congress receive more transfers on a bi-partisan basis, which reflects their superior lobbying skills. The results are in accordance with the idea that the president wishes to benefit from positive vertical spill-over effects of governance. The policy implications here are based on a more merit orientated system of federal money allocation in order to avoid partisan bias.

1.2 Organisation of this thesis

The following three chapters employ modern econometric techniques to identify the effect of interest. The type of data varies from chapter-to-chapter; starting with cross-country in chapter 2, then using individual-level data in chapter 3 and culminating with the use of regional-level data in chapter 4. The remainder of this thesis is organised as follows.

Chapter 2 begins by introducing the research in terms of the previous literature and highlighting the various pieces of literature that have explored determinants of decentralization. The proceeding section then discusses the relevant data. Here, I outline the construction and benefits of the new set of decentralization measures. In the next subsection I set out the OLS and instrumental variable empirical strategies. Following this I present the results from a replication of a seminal paper, the baseline model and the main results, which includes a discussion of instrument strength and relevance. I also present some supporting anecdotal evidence here. Then I conduct a number of robustness checks, which are standard in the literature. The final section

in this chapter concludes.

I begin chapter 3 by giving some information on the EU referendum and the government's leaflet. The idea here is to not provide a deep survey of the current literature on Brexit, but to give a background on how the leaflet acted as a possible determinant of vote preference. The next section characterises the individual-level data and sets out the main explanatory variable. Some further motivation is provided by the descriptive statistics and a brief look at the polling data around the time the leaflet was sent out to households. Following this, I set out the empirical strategy to identify the leaflet's effect on voting behaviour. The following section presents the results. First, to illustrate the quality of the data, I show that the data is an accurate predictor of who voted for Brexit.[1] Second, I perform the matching exercise for the full sample of individuals. I then split the sample between individuals who were exposed to other sources of referendum information to a low and high degree of exposure. I focus on the low exposed group first and re-estimate the matching approach by demographic group. In doing so, this allows for a heterogeneous treatment effect. I conduct a number of placebo tests here and support my findings with an alternative difference-in-difference methodology. Then the matching exercise by demographic is repeated for the sub-sample of individuals who were exposed to multiple sources of information. The last part of the results section performs a number of robustness checks. The next section examines the persuasive mechanism behind the leaflet and supports the findings with a set of placebo tests. The final section in chapter 3 concludes and offers some avenues of future research.

In chapter 4 I start by giving an overview of the related literature on partisan alignment, electoral cycles and then set out this chapter's contribution. I then discuss the theoretical reasons why a politician would favour a co-partisan. This entails going through the arguments of 'credit claiming', the underlying electoral incentives and

[1] That is, the results reflect the associations of who voted for Brexit that are common in the media and other scholarly work.

the personal traits of the governor – as I expect that governors with certain traits are likely to attract more favouritism. The next section in this chapter then sets out the institutional framework in the US. In particular, I make clear how the president and governors interact with one-another. Some quotes that further motivate the research are presented that showcase a direct relationship between the two tiers of political office. The following section sets out the dataset used and characterises the variables of interest. I present the spatial distribution of the alignment variable and show how alignment can take place in a quasi-random manner. The next section sets out the main results. I first focus on partisan alignment exclusively. Secondly, I pursue the electoral motives by exploring alignment's interaction the electoral calendar and then with swing states. Thirdly, I examine the role of experience in determining the amount of transfers to the governors. In the next section I split the alignment into two: Democratic and Republican alignment. This is to assess which party is driving the effect. This is followed by a section of falsification tests to strengthen my overall suppositions. The final element to this chapter is to conclude, provide some limitations and ideas for further research.

This thesis concludes in chapter 5. Here, I summarise the implications of this thesis for social scientists, economists and the wider society. I begin by presenting each chapter's main findings and provide an economic interpretation of the main effect of interest. Then I discuss the policy implications, limitations and possible future research. For the second chapter, I highlight the potential roadmap of decentralization in an increasingly globalised world. With respect to the third chapter, I propose more effective strategies for future political campaigns based on a targeted scheme of persuasion rather than a canvassed approach. And in the fourth chapter this thesis suggests some policy reforms in order to retreat from ineffectual partisan politics.

Chapter 2 Explaining fiscal decentralization and the role of ethnic diversity

The first empirical chapter is motivated by the first of three major political economy events, namely the 2014 Scottish independence referendum. The Scottish referendum took place to determine whether Scotland would break away from the United Kingdom and operate as an independent nation. The Scottish electorate voted to remain a member of the UK and further fiscal powers were devolved from Westminster to the Scottish parliament. Using this referendum as the backdrop for this empirical chapter, I ask what are the specific drivers of decentralization across countries and focus particularly on the role of ethnic diversity.

2.1 Introduction

Decentralization has been linked to various economic outcomes. For instance, a higher degree of fiscal decentralization is associated with less corruption (Fisman and Gatti, 2002), a smaller informal sector (Teobaldelli, 2010) and a more efficient provision of public goods (Faguet, 2004, Escaleras and Register, 2010). Conversely, more decentralization has been shown to reduce the effectiveness of aid (Lessmann and Markwardt, 2016) and to lower the rate of inflation (Neyapti, 2004, Baskaran, 2012). Across countries, we observe very different levels of decentralization and there is portion of literature that seeks to address how this variation has come about, which this paper contributes to. Moreover, given the variety of outcomes decentralization is associated with, it is critical to understand how countries have

become relatively more decentralized than others.

The overarching objective of this paper is to explore the determinants of fiscal decentralization with a specific focus on the role of ethno-linguistic fractionalization. The relationship between the two is not new to the literature, although the empirical evidence is mixed (see e.g., Treisman (2006) for a negative effect or Panizza (1999) and Dreher et al. (2018) for a positive effect). Not only has fractionalization been shown to have an affect on decentralization, but it has also been shown to affect a wide variety of economic and political outcomes, which is addressed in a wide body of literature. First abridged by Easterly and Levine (1997), who show the negative consequences of fractionalization on development, which has since been confirmed by Alesina et al. (2003), Desmet et al. (2012), Ashraf and Galor (2013*b*) and Papyrakis and Mo (2014). Additionally, many papers demonstrate how internal fractionalization increases the risk of the onset of violent civil conflict (Fearon and Laitin, 2003, Montalvo and Reynal-Querol, 2005, Desmet et al., 2012).

Whilst addressing the main goal of this paper, I make 2 contributions. The first of which is in relation to the measurement of decentralization. On the whole, the literature has used sub-national revenue or expenditure shares as proxies for the degree of fiscal decentralization but has persitently noted a desire for a better measure (Fisman and Gatti, 2002, Rodden, 2004, Enikolopov and Zhuraskaya, 2007, Teobaldelli, 2010). The revenue and expenditure share measures are of course subject to severe criticism for many reasons, the two main reasons being that they severely overestimate the extent of fiscal decentralization and capture little decision making autonomy (Stegarescu, 2005). The measures introduced in this paper, from the Regional Authority Index (RAI) produced by Hooghe et al. (2016), are a distinct improvement. These measures capture the degree of actual decision-making authority, on fiscal issues, that sub-national governments constitutionally hold.[1]

[1] These measures are also an improvement on the measures of policy decentralization as they measure actual decision making-authority on fiscal issues. It is noteworthy to mention however that fiscal and political decentralization are highly correlated (see, e.g., Treisman (1997) or Rodden (2004)

The second contribution lies in the treatment of fractionalization as an endogenous variable, which is currently not the case in the decentralization literature. The previous work treats fractionalization as exogenous, however, this assumption is being challenged in recent work that examines the effect of fractionalization on growth (Ahlerup and Olsson, 2012, Casey and Owen, 2014). As most migration occurs into those countries with higher levels of economic and institutional development, both related to decentralization, it implies that OLS coefficients will be downwardly biased (Freeman, 2006). Reverse causality is likely another source of endogeneity as diverse groups would sort themselves into more decentralized nations. In light of these concerns, I build primarily on the previous work by Ahlerup and Olsson (2012) on the historical determinants of fractionalization, which identifies a suitable instrument for examining the role ethno-linguistic heterogeneity.[2] Specifically, I use the origin of anatomically modern human life in each country as an instrument for ethno-linguistic diversity. This follows from the premise that a longer settlement duration breeds more ethnic heterogeneity.

This paper finds that there is a positive relationship between the level of ethno-linguistic fractionalization and the degree of decentralization for reasons of economic efficiency and political inclusivity. More specifically, it is the amount of fractionalization at the lower levels of aggregation where there are more groups, which are less distinctly defined, that drive the decentralizing of fiscal authority. Whereas, the perennial groups, where cleavages are at their deepest, bear no impact for on the degree of decentralization. Importantly, when treating fractionalization as an endogenous variable in the appropriate manner, the relationship persists.

The rest of this paper is organised as follows. The proceeding section, 2.2, presents and discusses the theoretical determinants of decentralization that have been used

for a discussion on the different concepts of decentralization).

[2] There are other seminal works on the origins of ethno-linguistic diversity (see, e.g., Michalopoulos (2012) and Ashraf and Galor (2013*a*)).

in the previous literature. Section 2.3 describes the data, presents the empirical strategy and some anecdotal evidence. Section 2.4 presents the main results of the empirical analysis. The following section, 2.5, presents a number of robustness checks on the results and section 2.6 concludes.

2.2 Theoretical determinants

2.2.1 Ethno-linguistic heterogeneity

The primary focus of this paper is the role of ethno-linguistic diversity, which has a strong theoretical grounding with respect to decentralization – first introduced into the decentralization theorem by Oates (1972). There are at least two such reasons exist that warrant ethno-linguistic diversity's inclusion into a model that explains the degree of decentralization. The first focuses on economic efficiency. As different ethnic groups show heterogeneity in their preferences for public good provision it is expected that more fractionalized countries will therefore tend to be relatively more decentralized to provide different public goods regionally (Oates, 1972, Treisman, 2006). This relies on the idea that local governments are better placed to get constituents to reveal such preference heterogeneity (Tanzi, 2000). The example used in the literature to illustrate this channel is the decentralization of education policy. If the setting of this policy is decentralized to the sub-national level, then different ethnic groups can set the syllabi and financing in-line with their preferences.

The second reason is for political inclusivity. Decentralization can be used as a policy to integrate minorities into society. In particular if the central government decides to decentralize authority over issues of contention it will help to restrain communal violence, ethnic tensions or even civil war organized along ethnic lines (Treisman,

2006, Sambanis and Milanovic, 2014). By doing so, decentralizing authority will involve minorities into the policy making process to create and set decentralized policies. This falls in accordance with Alesina and Ferrara (2005), who argue that the implications of an ever increasingly fractionalized country fall partly on the local authorities to implement policies that increase racial integration. Prior empirical work has, however, only taken a two-dimensional approach to diversity (Oates, 1972, Panizza, 1999, Garrett and Rodden, 2003, Treisman, 2006, Dreher et al., 2018). This paper allows for a historical dimension in the analysis.

2.2.2 Other determinants of decentralization

The first key determinant is the level of income. Much of the empirical literature has found a positive relationship between income and decentralization (Oates, 1972, Panizza, 1999, Garrett and Rodden, 2003, Letelier, 2005, Bodman and Hodge, 2010, Sambanis and Milanovic, 2014). This follows that decentralization is perceived to be a superior good and demand therefore increases with the level of income (Tanzi, 2000). As individuals become richer, more educated and have more free time, they may also have more motivation to participate in making local policy decisions, so more autonomy is demanded.

Larger countries should also be relatively more decentralized (Garrett and Rodden, 2003, Bodman and Hodge, 2010, Dreher et al., 2018). As the country size increases, the cost of governing from the center increases. The dis-economy of scale of governance increases with country size when governing from the center only. In a large decentralized nation, for instance, there will exist decreasing per capita cost of public goods and economies of scale in taxation beyond a certain level. With a larger country surface area, the difficulty of satisfying a diverse population is particularly costly and can result in secession or civil conflict (Oates, 1972, Alesina and Spolaore, 1997). Implicit in this, is the idea that there are clear efficiency gains to make from decentralizing authority in larger nations.

A less straight forward determinant is the amount of natural resources. Sambanis and Milanovic (2014) show that, on a regional-level, areas with a greater share of natural resources in regional GDP should demand more autonomy to control the natural resources in their jurisdiction. However, on a country-level, it is possible that countries with a greater endowment of natural resources will tend to be more centralized as central policy makers will seek to gain from the externalities of consumption and production, whereas the costs are born locally (Dreher et al., 2018).

The quality of democracy has been shown to go hand-in-hand with decentralization (Alesina and Spolaore, 1997, Garrett and Rodden, 2003, Treisman, 2006). Central governments may wish to create strong sub-national governments to involve the citizens in policy making rather than exploiting their agenda-setting power. Stronger lower tier governments also act as a check on the central government from being abusive. Furthermore, the fall of autocratic regimes and creation of better political institutions was quickly followed a dispersion of power and drive toward the provision of local rights. Examples here include the fall of Franco in Spain and the Communist party in the Soviet Union.

The system of governance may also affect the degree of decentralization. Presidential regimes are often associated with smaller governments relative to parliamentary ones (Persson and Tabellini, 2003). Thus, parliamentary regimes may be more decentralized in order to effectively allocate the budget across the different layers of government.

There are a number of other drivers of decentralization. First, as discussed by Gorodnichenko and Roland (2015), historical disease prevalence should have a centralizing effect on the government due to the negative externalities from disease transmission. Thus, a country with a more repeated history of disease should be more centralized.

Globalization has potential impacts on the fiscal system. On one hand, Alesina and Spolaore (1997) and Bolton and Roland (1997) argue that decentralization is likely

to occur on the way to secession because of globalization. Whereas, Garrett and Rodden (2003) show empirically that globalization has a centralizing effect. One reason why one may expect to find a centralizing effect in more globalized countries is because these countries may need to concentrate export and import taxes at the central government level. Taxing global trade at the local level is not feasible (Letelier, 2005, Bodman and Hodge, 2010).

Population heterogeneity in terms of the demographic structure may also drive decentralization. The share of the elderly in a country goes beyond what ethnic diversity captures. That is, it captures public good and service preference heterogeneity between age groups. Consistent with this logic, the population living in more variable land areas may also have different preferences.

More tiers of governance may seem obviously correlated with more decentralization. However, with more tiers of governance real decision making authority may actually be diluted. This is especially interesting to examine with the decentralization measures used in this paper.

Lastly, inequality may also have an effect. As Sambanis and Milanovic (2014) argues high inequality will hinder collective action in pursuit of regional autonomy. It would ultimately undermine social cohesion and therefore have a centralizing effect. Moreover, fiscal redistribution in a highly unequal country may be more difficult in highly decentralized systems.

2.3 Data and methods

2.3.1 Data and variables

In order to characterise the baseline model, I begin by following the previous literature, in particular the paper by Panizza (1999), to which this one is most closely

related. The author addresses the determinants of decentralization and finds no significant effect for fractionalization. Despite treating fractionalization as an exogenous variable, the specification used is a natural starting point for my specifications.

I use two different measures of decentralization as dependent variables. These are taken from the RAI derived by Hooghe et al. (2016). This dataset contains information on 80 countries over the period 1950 – 2010.[3] The sample contains a set of countries that have a varied set of characteristics. There is a mix of developed and developing nations with different systems of governance, democratic rights and levels of income. The full list of countries contained in the sample is found in Table 2.1 and the spatial distribution in Figures 2.1 and 2.2. This paper focuses on two particular aspects of decentralization, that is, the degree of fiscal and borrowing autonomy. The former, denoted $FISC$, represents the extent to which a sub-national government can independently tax its population and is scored between 0 and 4. 0 means that the central government sets the base rate of all regional taxes, and 4 means that the regional government sets the base rate of at least one major tax. The latter, denoted $BORRO$, represents the extent to which a sub-national government can borrow autonomously and is scored between 0 and 3. 0 indicates that borrowing is prohibited and 3 means that a sub-national government may borrow without any centrally imposed restrictions. Both variables are naturally ordered so that lower values represent a lower degree of decentralization.[4][5] These measures are a significant improvement on the commonly used expenditure or revenue shares as they capture actual decision making autonomy, are not affected by business cycle fluctuations as changes in the RAI measures require constitutional change and therefore they should be accurate measures of the extent of decentralization.

The measure of ethnic heterogeneity, $Fractionalization$, is the probability that two

[3] Two countries are dropped due to the availability of independent variable data availability.

[4] Both measures, can take values higher than their maximum due to population weighting. See Hooghe et al. (2016) for a complete discussion on the construction of these measures.

[5] For a full breakdown of each value of the decentralization measures see Table 2.2.

randomly selected individuals will be from a different ethno-linguistic group. The favoured measures are the Desmet et al. (2012) measures, which account for how perennial or superficial the cleavages across groups are by using country specific language trees. The fractionalization measure at the highest level of aggregation (towards the tree leaves), where cleavages are at their deepest and least numerous is denoted $ELF1$. The lowest level of aggregation (towards the tree roots), where cleavages are at their most superficial and most numerous is captured by $ELF15$. The chosen measures in this paper offer a significant improvement on approaches in the previous literature that use the older measures, as I allow for a three-dimensional approach to diversity. The fractionalization measures are discussed and motivated in greater detail in the following subsection.

A number of studies have begun to question the exogeneity assumption in fractionalization. For example, as most migration occurs into countries with higher levels of economic and institutional development, this imples that OLS coefficients will underestimate the impact of fractionalization (Freeman, 2006). Darity et al. (2006) argue that ethnic identities are not definite categories and the choice to identify as a group is in response to costs and benefits. As argued by Alesina and Spolaore (1997) national boundaries may also be endogenous. This would create reverse causality between decentralization and fractionalization. Moreover, it is plausible that more diverse groups will sort themselves into more decentralized countries as there is likely a better chance of integrating into society. There are potential unobserved factors that are correlated with the dependent and the main independent variable, for instance the preferences of the centralized policy maker with respect to their desired degree of decentralization. Thus, the contribution here lies in the treatment of ethno-linguistic fractionalization as endogenous with respect to the degree of decentralization. The instrumental variable method in this paper should alleviate these concerns and represents an improvement over the current approaches in the established literature. This approach will also provide more consistent estimates in the presence of measurement error in the independent variable (Angrist and Pischke,

2014).

Given the endogeneity concerns for fractionalization, identifying a valid instrument is crucial to correctly identifying the effect. That is, meeting the exclusion restriction (the instrument must be uncorrelated with decentralization), the independence assumption (unrelated to unobservables in the second-stage) and is sufficiently correlated with fractionalization. Fortunately, there exists a recent literature on the historical determinants of fractionalization which provides such an instrument. Thus, to instrument for fractionalization, I follow Ahlerup and Olsson (2012) who show that the origin of anatomically modern human life, *Origtime*, in a given country is positively correlated with more ethno-linguistic heterogeneity. Life started in the Rift valley in Ethopia 160,000 years ago and spread throughout Africa, and this where one finds the most diversity. Conversely, nations in Latin America were colonised by humans much later and have relatively little diversity. This follows from the evidence that a country being colonised by modern humans for longer creates more ethno-linguistic diversity. Moreover, in this IV framework, I assume that the settlement date is not correlated with countries' current unobserved characteristics, as long as I condition on current income the instrument should be valid. Using the variation in modern human settlement dates as an instrument for fractionalization is not new to the literature as it has been successfully employed in the same manner by Ahlerup (2010) and Casey and Owen (2014) to explain variation in income.[6]

The remaining control variables are as follows. The first three covariates form the specification from Panizza (1999). To capture to the level of income I use the natural log of real GDP per capita in 2000 US dollars. To capture country size I use the natural log of country surface area (in square kilometres), and to capture political institutions I use the level of democracy – measured by the average of the civil liberties and political rights indices from Freedom House. The following main

[6] Other relevant instruments are tested, such as genetic diversity Ashraf and Galor (2013*b*), migratory distance Ashraf and Galor (2013*a*) and absolute latitude. $ORIGTIME$, however, statistically performs the best.

control variables are from other related previous literatures, these are: proportion of the population aged 65 or over, total natural resource rents as a share of GDP and the historical prevalence of seven disease causing pathogens from Murray and Schaller (2010).

A set of previously discussed extended controls are also used for robustness purposes. The KOF index of trade globalization is used to proxy for globalization (Gygli et al., 2018). The share of arable land from Ashraf and Galor (2013*b*), the Gini index of market income inequality (Solt, 2016), a dummy inductor for a parliamentary government (Cruz et al., 2016) and the number of tiers of elected sub-national government (World Bank, 1999). Further controls include historical dummies that are typical to this type of literature, namely colonial history and legal heritage dummies, which will help alleviate the omitted variable bias. Summary statistics, cross correlation tables and complete variable descriptions with sources are detailed in Table 2.4, 2.5, 2.6 and listed in the Appendix.

2.3.2 Fractionalization measures

Ethnic fractionalization was first introduced into economic models by Easterly and Levine (1997) who considered the role it has for growth in Africa. As mentioned above, the chosen fractionalization measure to be used in this chapter comes from Desmet et al. (2012). There are, however, a variety of other measures that capture ethnic and ethno-linguistic differences in the population. Measures constructed prior to Desmet et al. (2012) are the Fearon (2003) index of ethnic fractionalization and the Alesina et al. (2003) measure of ethno-linguistic fractionalization. The Fearon (2003) measure is constructed using data from *Library of Congress Country Study*, *Encyclopedia Britannica* and the *CIA World Factbook*, it does not take into account linguistic distance, and the Alesina et al. (2003) measure is constructed from data

contained in the *Encyclopedia Britannica* and the *CIA World Factbook*.[7] Whilst both these measures imply divisions across groups, they have come under criticism as they convey no information about how deep-set these divisions are (Posner, 2004) and are very likely out of date (Laitin and Posner, 2001).

The newer Desmet et al. (2012) measure captures how deep the cleavages between groups are by using data grouped in hierarchical linguistic trees in the *Ethnologue 15th edition* database. Thus the key advantage of this data is that it allows this paper to bring a historical dimension to the analysis. A hypothetical linguistic tree is shown in Figure 2.6 to illustrate how the size of groups matter, and an example linguistic tree is shown in Figure 2.7 to depict the types of group at each level of aggregation. At the top of the hierarchy, i.e. the roots of the tree, the cleavages are a result of prehistoric group formation (Ashraf and Galor, 2013*a*). As you move further down the tree, towards the leaves and away from the roots, the groups become less distinct and more numerous. The cleavages here are created as a legacy of colonialism or the dynamics of cultural drift from uninterrupted human settlement in a given location (Ahlerup and Olsson, 2012). The following formula is applied to the different branches of the trees to create the Desmet et al. (2012) measures:

$$ELF(j) = 1 - \sum_{i(j)=1}^{N(j)} [s_{i(j)}]^2 \tag{2.1}$$

for $i(j) = 1, ..., N(j)$ groups of size s_{ij}, where $j = 1, ..., J$ denotes the level of aggregation. This ranges from 1 to 15, 1 being the most aggregated and 15 the most disaggregated levels of groups. These disaggregated measures of fractionalization are the ones this paper will use in the empirical analysis.

[7] The algebraic formula to derive each of these indices are found in Table 2.3.

2.3.3 Anecdotal evidence

For the cross-country analysis in the following sections it requires that nations do not vary much over time in their values of decentralization. Inevitably this is not always the case. Therefore, I can assess a particular example where there has been a change in population diversity followed by a change in the degree of decentralization with a descriptive lens to first motivate the relationship.

To illustrate this, consider the case of the EU and Italy. The EU project has undergone repeated enlargements since its inception, ranging from the countries under the Iron Curtain in the east to those on the Iberian peninsula in the west. The accession of the Mediterranean and northern European countries throughout the latter part of the 20th century and the principle of free movement allowed the unrestricted flows of various ethno-linguistic groups to different nations within the EU. For example, Italy, whose geographical placement is unique in Europe because of its proximity to Africa and short distance to the Anatolian peninsula, has seen drastic changes in the recent past. Italy, who joined the EU in 1958, has experienced a persistent increase in foreign nationals settling within its borders, with a large proportion coming from eastern Europe, according to the World Bank's migration data. This experience exemplifies an increase in the internal ethno-linguistic diversity of Italy. Over the same period, there has been a steady increase in the amount of fiscal authority decentralized to Italian sub-national governments. There has been constitutional reforms to municipal and regional governments, larger cities now elect a mayor and there are presidents of each region (Grisorio and Prota, 2015). Figure 2.8 shows the years where the EU was expanded and plots the level of fiscal and borrowing authority over the period 1980–2010. Turning to the fractionalization data at the lower levels of aggregation, where intra-european migration would impact, Italy has a value of 0.5922. Implying that there is 59.33% chance that two randomly selected individuals will be from the different ethno-linguistic groups. For comparison, the UK's $ELF15$ value is 0.1395, or a 13.95% chance. Whilst migration is unlikely to

be the sole cause of such changes, there is some anecdotal evidence that supports a correlation between diversity and decentralization.

2.3.4 Empirical strategy

The strategy is to first present results from a basic cross-country OLS estimation to show the association between fractionalization and decentralization. I then perform Two Stage Least Squares (2SLS) estimation to apply a causal interpretation to the relationship. The second- and first-stage regression equations can be formalised, respectively, as follows:

$$y_i = \alpha_0 + \beta_1 \widehat{Fractionalization}_i + \beta_2 X_i + \epsilon_i \tag{2.2}$$

$$Fractionalization_i = \alpha_1 + \phi_1 Origtime_i + \phi_2 X_i + \nu_i \tag{2.3}$$

where y_i is one of the measures of decentralization, either fiscal or borrowing autonomy, in country i. $Fractionalization_i$ is one of the various ethno-linguistic fractionalization measures that takes a value between 0 and 1, where 1 represents certainty that two randomly selected individuals will be from different ethno-linguistic groups. X_i is a set of previously discussed control variables and ϵ_i and ν_i are the error terms. As mentioned above, the instrument $Origtime_i$, is the number of years anatomically modern humans have been settled in a given country and is scaled to 100,000 years. The decentralization measures are available in country-year format, however, there is little variation over time to exploit (see Figure 2.3 and 2.4). So for these and all other variables that vary over time, they are collapsed into their average value for the 2005 – 2010 period.[8] Consistent with the main hypothesis and reasoning previously discussed, I expect β_1 and ϕ_1 to be positive and statistically significant.

[8] As a robustness check, the regressions are repeated using 1999 – 2004 data and the results are unchanged.

A positive and significant ϕ_1 coefficient indicates that a longer duration of human settlement creates more fractionalization. A positive and significant β_1 will then mean that more fractionalized countries causes a higher degree of decentralization.

2.4 Results

2.4.1 Panizza (1999) replication

As a brief exercise before beginning the main analysis, I replicate the analysis produced by Panizza (1999). This is useful in this context as it allows the results from the new data to be compared to the seminal paper in the literature.

The results are presented in Table 2.7. Using $FISC$ and $BORRO$ as a dependent variable in odd and even columns, respectively, the results align with those in Panizza (1999). Country size and income are both robustly positively associated with more decentralization. Fractionalization, as a proxy for heterogeneity in public good preferences, is again positively associated with more decentralization. The coefficient for democracy is generally positive but not significantly different from 0, which is again parallel to the findings of Panizza (1999). Overall, one can be confident that the new decentralization and fractionalization measures produce a comparable relationship to that shown in previous seminal work.

2.4.2 Baseline model

Given the number of ethno-lignuistic indices available, I begin by cycling the indices for one another in separate regressions that includes the baseline set of controls. This "cycling" technique is especially useful in this case as it allows the data to guide the research to the most appropriate measure of diversity and is used by Desmet et al.

(2012) and Michalopoulos (2012).[9] The results of this exercise are presented in Table 2.8.

Before discussing the fractionalization variables of interest, I make a comparison of the control variables here to those in the previous in order to motivate the validity of the new data as viable measures of decentralization. Income and country size are robustly positive and significant across all specifications, which chimes with a large portion of the empirical literature (Panizza, 1999, Garrett and Rodden, 2003). The share of the elderly and democracy are positively related to decentralization albeit insignificantly so in most columns.[10] Natural resource rents appear to have a persistent centralizing effect as in Dreher et al. (2018). The effect of historic pathogen prevalence is also very small and not significantly different from 0. All things considered, one can be confident that these are valid measures of fiscal decentralization.

Turning to the fractionalization measures, the results are notably interesting. At the higher levels of aggregation, closer to $ELF1$, the coefficients are statistically insignificant. When the level of aggregation is decreased, moving towards $ELF15$, capturing a larger number of ethno-linguistic groups, the coefficients become significant at the 1% level. Qualitatively, $ELF15$ can be interpreted as a country moving from a population of complete homogeneity to complete heterogeneity would be 1.5 points more decentralized. Evaluated at the mean $FISC$ score, the average country with complete population heterogeneity would have sub-national governments that are able to set the base and rate of minor taxes, holding all else constant. This indicates that the deep ancestral cleavages between groups bear no impact on the level of decentralization. It is, however, the more recent superficial cleavages that effect the degree of decentralization. These are the cleavages created by the legacy of colonialism and interrupted settlement. The reason why this time dimension ef-

[9] Desmet et al. (2012) use the cycling technique to show that ELF15 is associated with lower growth and public good provision, whereas ELF1 is related to the onset of civil war and redistribution.

[10] The results here remain the same when using the dichotomous measure of democracy from Boix et al. (2013).

fect of fractionalization on decentralization exists may be because it is the sheer number of ethno-linguistic groups that drive decentralization rather than the deep differences between a small number of groups.

Whilst this exercise has produce some intriguing results, they are taken with caution because of the endogeneity problem and further controls to introduce.[11] To proceed to the next stage of the analysis, this paper focuses on the $ELF15$ measure of fractionalization.[12]

2.4.3 Additional controls

Following from analysis of the previous subsection, I now proceed to explore how the coefficient for $ELF15$ is affected in different OLS model specifications. In Table 2.9 columns (1) - (5) the dependent variable is the degree of fiscal autonomy and in (6) - (10) it is the degree of borrowing autonomy. Simple bivariate specifications in column (1) and (6), controlling only for income, fractionalization is positive and significant. Columns (3) and (8) introduce the extended set of controls which have a generally insignificant effect on decentralization. The variable acting as a proxy for globalization, $Trade$, is negative and only significant in the borrowing autonomy regressions. This indicates that more globalized countries, holding all else constant, are more centralized, which supports the empirical findings in Garrett and Rodden (2003).[13] In column (4) and (9), I include controls for colonial history and in column (5) and (10) I introduce legal origin controls. A history of disease, captured by pathogen prevalence, is negative and significant for borrowing autonomy only,

[11] Another concern may be that as the aggregation of the Desmet et al. (2012) measures fall, the mean increases. To remedy this, Table 2.8 is re-estimated with standardised measures of fractionalization and the results remain qualitatively the same.

[12] The spatial distribution of $ELF15$ is presented in Figure 2.5.

[13] The results remain unaffected when including each variable progressively or removing Tiers of Government, see Table 2.10.

in line with the predictions of Gorodnichenko and Roland (2015). Here, it is unlikely the decentralized governments could respond in unison to disease transmission and therefore required a centralization of spending (borrowing) powers to alleviate and eradicate disease. The fractionalization coefficient remains highly significant throughout the columns. The average effect across all columns for $FISC$ is 1.5 and for $BORRO$ it is 1. This difference in magnitude reflects the reluctance of central governments to decentralize spending powers relative to revenue raising powers. From these results it appears that a fractionalization does indeed effect the degree of decentralization, to attach a causal interpretation to this effect I now proceed to the 2SLS estimates.

2.4.4 Addressing endogeneity concerns: 2SLS

Prior to discussing the main results from the instrumental variable regressions, I examine some tests of instrument validity and strength to support the estimation strategy. Specifically, in the bottom rows of Table 2.11 I report tests of instrument strength and weak identification that support the effort to correctly identify the effect of fractionalization. The first thing to note is the positive and statistical significance of the instrument, *Origtime*, in the first-stage. As expected the longer anatomically modern humans have been settled in a country, the more ethnolinguistically diverse that country is. In column (2), the size of the coefficient implies that 10,000 years earlier settlement is associated with a 5.46 percentage point increase in the probability that two randomly selected individuals in a population will come from the different ethno-linguistic groups. The full first stage results are reported in Table 2.12 and the correlation is shown graphically in Table 2.9. The F-statistic is the first-stage F-statistic which measures the strength of the instrument. Values below the rule of thumb figure of 10, given by Stock and Yogo (2005), indicate a weak instrument. In all cases but two, I can reject this null hypothesis, thus the

instrument is sufficiently strong.[14] In the presence of a weak instrument, the results can be biased (Stock and Wright, 2000). To test whether the instrument still has a significant effect in the presence of weak instrument, I report the Anderson-Rubin Wald Chi^2 test. The p-values indicate in all but two specifications, that should the instrument be deemed weak, fractionalization does have a significant effect. In sum, these diagnostic tests suggest that *Origtime* is a sufficiently good instrument for identifying the effects of fractionalization.

The instrumental variable specifications directly mirror those in Table 2.9 for reasons of openness. Across all columns but one, instrumented $ELF15$ exerts a positive and highly significant impact on the degree of decentralization. For example, the results in column (2) imply that a country moving from complete population homogeneity to complete heterogeneity would be 2 points more decentralized in terms of taxing power. In column (7) the results predict that a country moving from complete homogeneity to heterogeneity would be 2.4 points more decentralized in terms of borrowing autonomy. The magnitude of the coefficients are larger than those produced by OLS estimation, meaning that the OLS estimates were downwardly biased due to the endogeneity issue. To determine whether the results are affected by omitted variables, I include the colonial and legal origin controls. The magnitude of the fractionalization coefficient estimate falls (compared to the baseline specification in (2) and (7)) but remains statistically significant at the 5% level with the exception of coefficient in column (4).

On the whole, the results of Table 2.11 imply that fractionalization has a positive causal effect on the degree of decentralization, which may act through the economic efficiency and political inclusivity channels. This is consistent with much of the theoretical predictions in the literature, despite the shortcoming of empirical evidence.

[14] The instrument would be an even stronger predictor of diversity if the sample were to include African countries to create more variation to exploit, however they are unfortunately not contained in the RAI. Nonetheless, there is still ample variation in the sample, with old nations, such as Indonesia and Malaysia that were first populated 75,000 years ago, and younger ones like the UK and Iceland, colonised 8,000 and 1,200 years ago, respectively.

2.5 Robustness checks

This section conducts a number of checks on the sensitivity of the results. The first thing to consider is whether the same relationship, based on the depth and number of cleavages, exists between borrowing decentralization and fractionalization. To do so, I repeat the specifications in Table 2.8 but change the dependent variable to *BORRO*. The results, presented in Table 2.13, remain qualitatively the same.

In a more technical aspect, I assess the robustness of the results when removing certain groups of countries. The results are depicted in Table 2.14. I first focus on the stability of the dependent variable, given that I am conducting cross-country analysis, this requires there to be little variation over time. I remove countries that vary 2 or 1 points in their degree of decentralization over time in columns (1) - (4). In the proceeding columns, I remove the top and bottom 10% countries by surface area. The coefficient for fractionalization across all columns remains positive and significant. Second, I exclude countries in the tails of the distribution of their surface area. Columns (5) and (6) remove the 7 largest countries and columns (7) and (8) remove the 7 smallest. The coefficient of fractionalization results remain similarly robust in all 2SLS regressions.

To probe the findings further, I conduct a number of checks that are common to the literature. The results are shown in Table 2.15. First, I run a "horserace" regression between fractionalization and polarization.[15] At the lower levels of aggregation the two measures are correlated, however the variance inflation factor indicates no serious problems of multicollinearity. The regressions in column (1) and (2) confirm

[15] Similar to fractionalization in construction, polarization takes the value 1 when there are two groups of equal size and is also available at differing levels of aggregation.

my result, as only $ELF15$ is statistically significant at the 5% level.

As a second check, I examine whether the results differ between the New World and Old World. When splitting the sample between the old and new world - defined as the Americas and Oceania - I find that the results are less robust for the New World. Consistent with the findings in Desmet et al. (2012) this is perhaps a result of a weak link between linguistic cleavages and historic divisions in the New World. To account for the small sample of countries when splitting the sample, I introduce a dummy variable for the New World and an interaction between that and fractionalization, the results remain qualitatively the same.

For a third exercise, I introduce a quadratic term to test for any non-linearity in fractionalization. The premise for this comes from Ashraf and Galor (2013*b*) who argue that very low or very high levels of diversity is detrimental to growth. However, in this instance, there is no evidence of a inverse-U shaped relationship.

In a fourth robustness check, I include a set of country-specific geographic controls. These are the log of the mean elevation, the log distance between the nearest river or coast, the share of tropical land, the log of the standard deviation of mean elevation (as a measure of land variability) and the log of absolute latitude. The fractionalization estimate remains statistically significant. As a fifth, I replace the measure of democracy with the Boix et al. (2013) dichotomous measure of democracy, this appears insignificantly as the previous measure did.

As one final check, I can use an alternative main explanatory variable.[16] Using a different measure of ethno-linguistic diversity than a fractionalization measure is useful to show that the diversity effect is not driven by something spurious contained in the construction of the indices. By doing so, it emphasises the importance of accounting

[16] When using the older Fearon (2003) or Alesina et al. (2003) measures, the results are far weaker, which is expected as they do not account for the depth of divisions and the Fearon (2003) measures does not account for linguistic distance.

for linguistic distance in the fractionalization measures and the implications of language diversity for the degree of decentralization. Moreover, it again helps validate the new RAI data as useful measures of decentralization. I follow Michalopoulos (2012) and use the number of languages with at least 1,000 speakers in a country. I log-normalise the variable, replace the fractionalization measure with it and re-estimate the baseline specifications. The OLS and 2SLS results for this exercise are presented in Table 2.16. The coefficient estimate is statistically significant across all columns.

2.6 Conclusion

This paper has provided evidence that ethno-linguistic fractionalization is an important factor in the decentralization of fiscal and borrowing decision making. Moreover, I have also shown that the new RAI measures of autonomy are adequate and valid measures of decentralization by comparing the control variables with the sign and significance found in the previous literature using older measures of decentralization. These results add to a long literature on decentralization and diversity (Oates, 1972, Panizza, 1999, Garrett and Rodden, 2003, Rodden, 2004, Sambanis and Milanovic, 2014, Dreher et al., 2018). The results also have implications for the literature strand that examines the role of the depth of cleavages across groups and their impact on economic outcomes (Laitin and Posner, 2001, Alesina et al., 2003, Desmet et al., 2009, 2012). Whilst instrumenting for fractionalization is not a new (Ahlerup, 2010, Casey and Owen, 2014), it is however new to the decentralization literature which typically treats fractionalization as an exogenous variable.

In summary, the results show that fractonalization where the divisions are finer, more numerous and less distinct drive the degree of decentralization. No effect is found for the deep ancestral cleavages that were formed thousands of years ago. The results

remain robust to a variety of controls and across two measures of decentralization. To address endogeneity concerns, an instrumental variable methodology is used, which involves instrumenting fractionalization with the settlement dates of anatomically modern human life, and the results similarly persist.

There is obviously more work to be done on this matter. Whilst it now seems clear that fractionalization is positively related to decentralization, a further exploration into the channels of impact would be especially useful to the literature. More generally, a reasonable interpretation of the results would indicate as the world continues to become a more globalized and open place, countries will become more decentralized in response to the permutation and spread of ethno-linguistic groups.

Chapter Appendices

2.A Chapter 2 variable definitions

FISC. The extent to which a regional government can independently tax its population. *Source:* Hooghe et al. (2016)

BORRO. The extent to which a regional government can borrow autonomously. *Source:* Hooghe et al. (2016)

EF/ELF/ELF(j). Ethno-linguistic fractionalization. The probability that two randomly selected individuals belong to different ethnolinguistic groups. *Source:* Fearon (2003); Alesina et al. (2003); Desmet et al. (2012)

GDP per capita. The natural log of real GDP per capita, 2005 US $'s. *Source:* World Bank's WDI

Country size. The natural log of surface area in km^2. *Source:* World Bank's WDI

Proportion of 65+. The proportion of persons in a given population over the age of 65. *Source:* World Bank's WDI

Democracy. Average of indexes for civil liberties and political rights, where each index is measured on a one-to-seven scale with one representing the highest degree of freedom and seven the lowest. *Source:* Freedom House

Natural resource rents. Total natural resources rents are the sum of oil rents, natural gas rents, coal rents (hard and soft), mineral rents, and forest rents. *Source:* World Bank

Pathogen prevalence. Historical prevalence of 7 infectious disease causing pathogens. *Source:* Murray and Schaller (2010)

Arable. The share of arable land. *Source:* Ashraf and Galor (2013*b*)

Trade. Trade globalization in the KOF index. *Source:* Gygli et al. (2018)

Tiers of government. The number of elected sub-national tiers of government. *Source:* World Bank (1999)

Parliament. A dummy variable equal to one if the country operates under a Parliamentary system of governance. *Source:* Cruz et al. (2016) and author's elaboration

Gini. Gini coefficient of income inequality before taxes and transfer (Gini market). *Source:* Solt (2016)

Colonial history$_k$. A dummy variable equal to 1 if the country has colonial history of country k. Where k is one of; Britain, France, Russian, Spanish or Portuguese, other or none. *Source:* CEPII

Legal origin$_m$. A dummy variable equal to 1 if the country has a legal origin of country m. Where m is one of; British, France, Socialist, German or Scandanavian. *Source:* La Porta et al. (1998)

Origtime. Duration of anatomically modern human settlement. Scaled to 100,000 years. *Source:* Ahlerup and Olsson (2012)

POL15. The probability that there will be two ethno-linguistic groups of equal size at the 15th level of aggregation. *Source:* Desmet et al. (2012)

New World. A dummy variable defining countries in the "New World", namely countries in the Americas and Oceania. *Source:* Author's own elaboration

Ln(mean elevation). The natural log of mean elevation. *Source:* Gallup et al. (2010)

Ln(distance coast river). The natural log the distance to the nearest coast or river. *Source:* Gallup et al. (2010)

Tropical. The share of tropical land. *Source:* Gallup et al. (2010)

Ln(std. dev. mean elevation). The natural log of the standard deviation of the mean elevation. *Source:* Gallup et al. (2010)

Ln(absolute latitude). The natural log of absolute latitude. *Source:* Gallup et al. (2010)

Democracy01. A dummy variable equal to 1 if the country is a democracy, which is defined as satisfying political contestation and participation criteria. *Source:* Boix

et al. (2013)

Ln(Number of languages). The number of languages, with over 1,000 speakers, in a given country, as defined in the 15th Ethnologue. *Source:* Michalopoulos (2012)

2.B Chapter 2 Figures

Figure 2.1: Spatial distribution of FISC

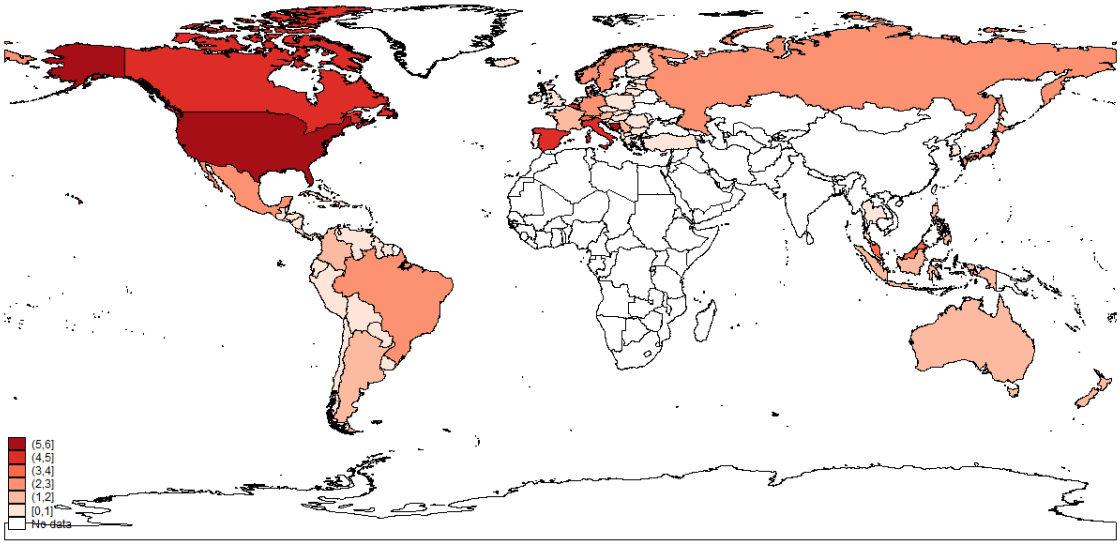
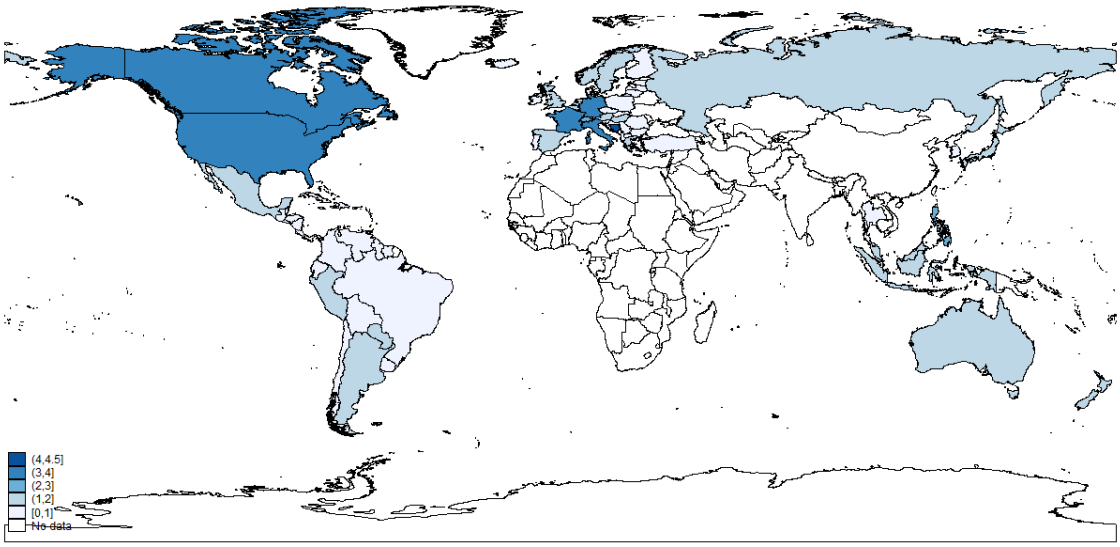


Figure 2.2: Spatial distribution of BORRO



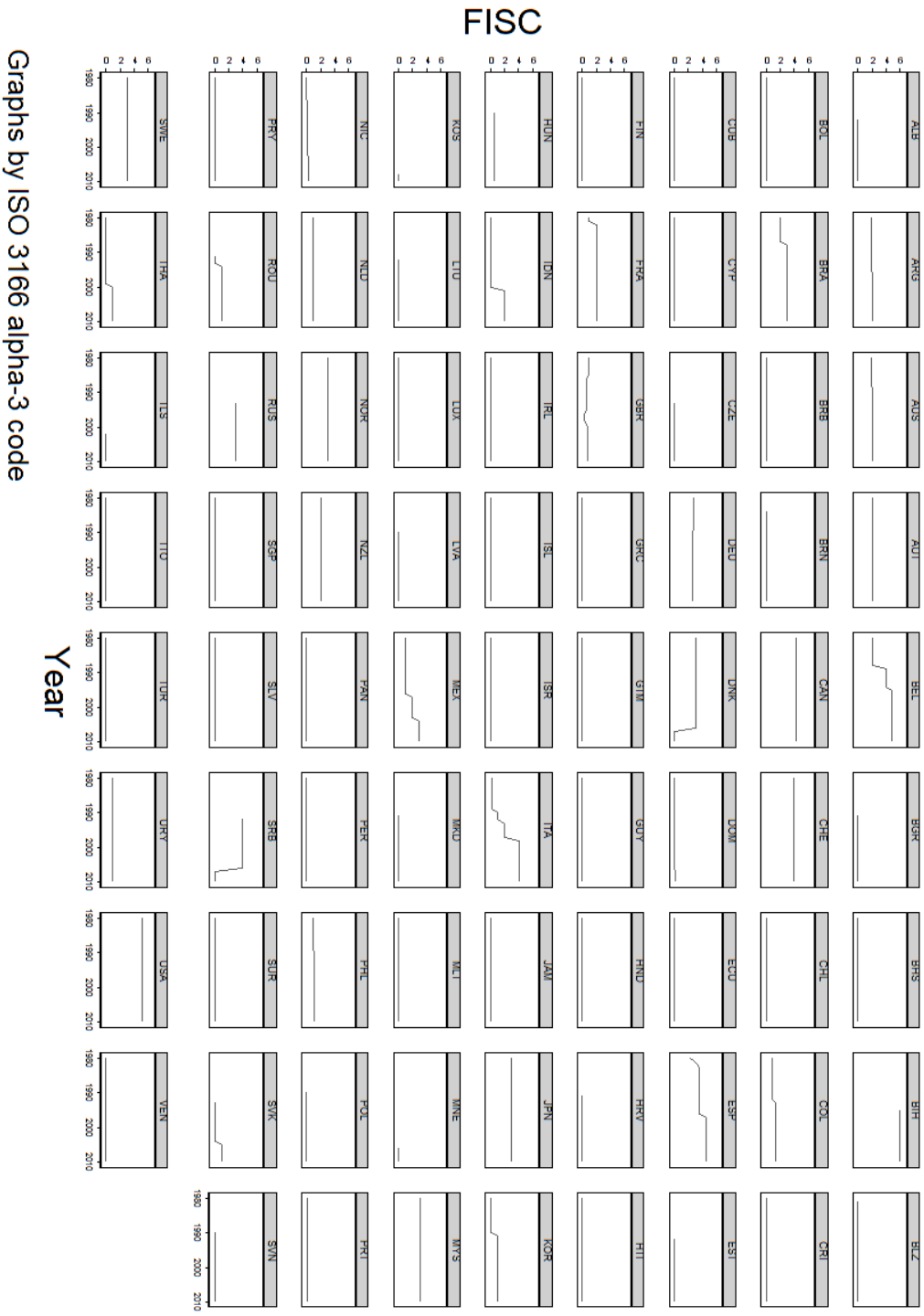


Figure 2.3: Fiscal autonomy over time

Figure 2.4: Borrowing autonomy over time

Figure 2.5: Spatial distribution of $ELF15$

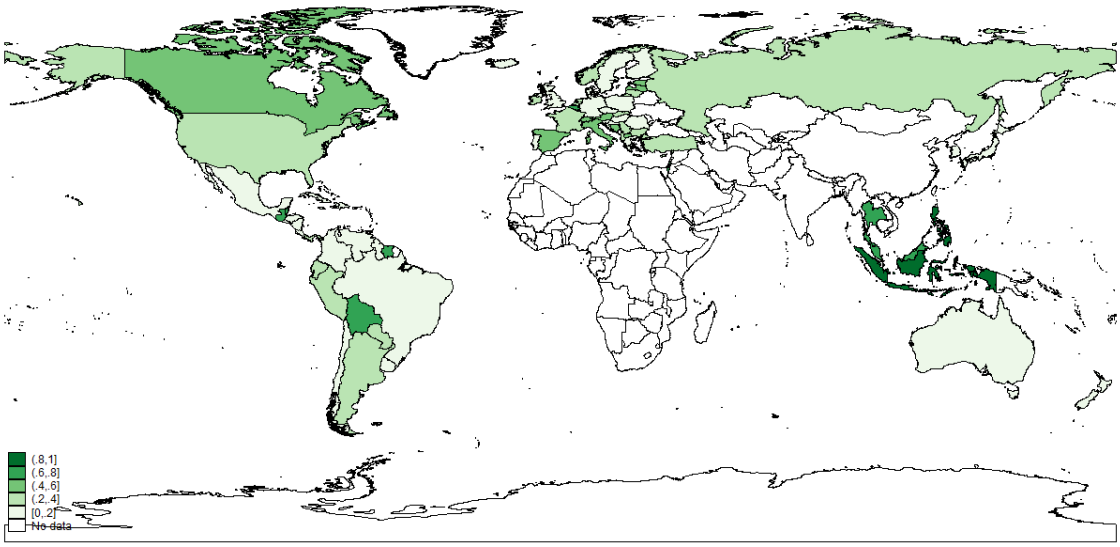


Figure 2.6: Hypothetical linguistic tree

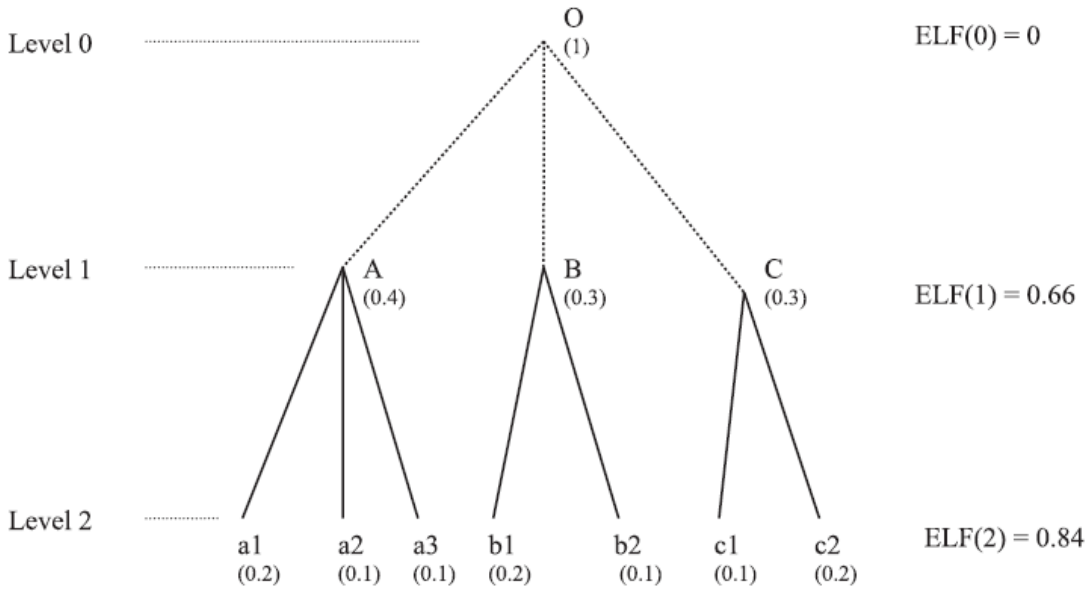


Figure 2.7: Pakistan linguistic tree

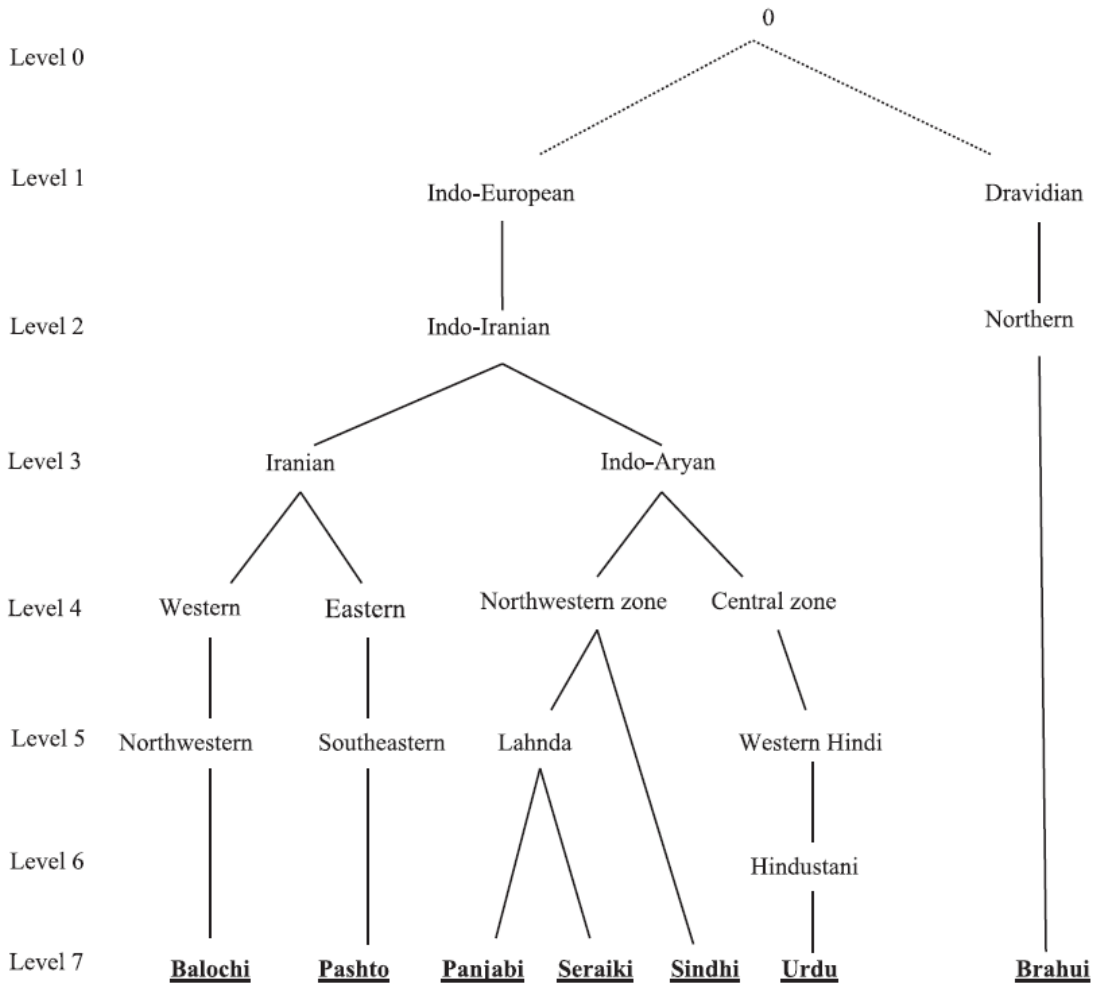
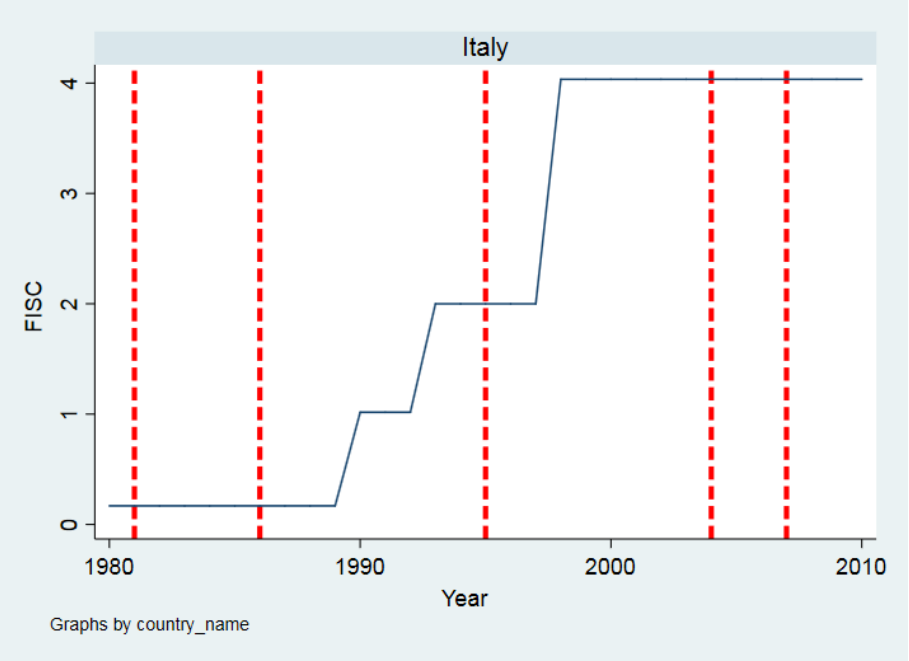
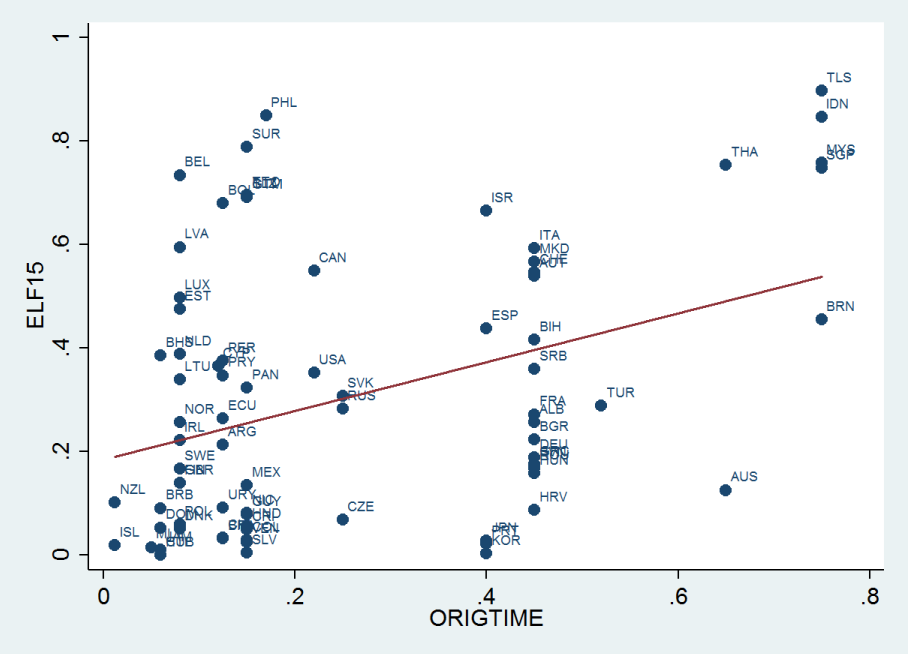


Figure 2.8: Time series plots of FISC and EU expansion dates



Notes: The graph shows the evolution of FISC over time in Italy. Red perforated lines indicate years where the EU has been expanded.

Figure 2.9: Correlation between ORIGTIME and ELF15



2.C Chapter 2 Tables

Table 2.1: Country sample

Albania	**Finland**	Nicaragua
Argentina	**France**	**Norway**
Australia	**Germany**	Panama
Austria	**Greece**	Paraguay
Bahamas	Guatemala	Peru
Barbados	Guyana	Philippines
Belgium	Haiti	**Poland**
Belize	Honduras	**Portugal**
Bolivia	**Hungary**	Romania
Bosnia and Herzegovina	**Iceland**	Russian Federation
Brazil	Indonesia	Serbia
Brunei	**Ireland**	Singapore
Bulgaria	**Israel**	**Slovak Republic**
Canada	**Italy**	**Slovenia**
Chile	Jamaica	**South Korea**
Colombia	**Japan**	**Spain**
Costa Rica	**Latvia**	Suriname
Croatia	Lithuania	**Sweden**
Cuba	**Luxembourg**	**Switzerland**
Cyprus	Macedonia	Thailand
Czech Republic	Malaysia	Trinidad and Tobago
Denmark	Malta	**Turkey**
Dominican Republic	**Mexico**	**United Kingdom**
Ecuador	Montenegro	**United States**
El Salvador	**Netherlands**	Uruguay
Estonia	**New Zealand**	Venezuela

Notes: Countries in **bold** are OECD member states.

Table 2.2: Decentralization measures

Variable name	Score	Description
FISC		The extent to which a regional government can independently tax its population:
	0	Central government sets base and rate of all regional taxes.
	1	Regional government sets the rate of minor taxes
	2	Regional government sets base and rate of minor taxes
	3	Regional government sets the rate of at least one major tax: personal income, corporate, value added, or sales tax
	4	Regional government sets base and rate of at least one major tax.
BORRO		The extent to which a regional government can borrow:
	0	The regional government does not borrow (e.g. centrally imposed rules prohibit borrowing)
	1	The regional government may borrow under prior authorization (ex ante) by the central government and with one or more of the following centrally imposed restrictions: a) Golden rule (e.g. no borrowing to cover current account deficits) b) No foreign borrowing or borrowing from the central bank c) No borrowing above a ceiling d) Borrowing is limited to specific purposes
	2	The regional government may borrow without prior authorization (ex post) and under one or more of a), b), c), d)
	3	The regional government may borrow without centrally imposed restrictions.

Table 2.3: Fractionalization measures

Variable name and source	Description of construction
Fearon (2003)	
EF	The size and number of ethnicity groups are identified using a variety of sources: *CIA World Factbook, Encyclopaedia Britannica* and the *Library of Congress Country Study*, to which the following formula is applied. Denoting population shares of the ethnic groups in a country as $p_1, p_2, p_3, ..., p_m$ then fractionalization is $EF = 1 - \sum_{i=1}^{n} p_i^2$.
Alesina et al. (2003)	
ELF	Average of five different indices of ethno-linguistic fractionalization. The 5 component indices are: an index of ethno-linguistic fractionalization in 1960; probability of two randomly selected individuals speaking different languages; the probability of two randomly selected individuals do not speak the same language; percent of of the population not speaking the official language; and the percent of population not speaking the most widely used language.
Desmet et al. (2012)	
ELF(j)	Derived using data on the size of ethno-linguistic groups, which is contained in the *Ethnologue* (15th edition) database. This source allows the utilisation of 'hierarchical language tress', which means that there are data on the differing levels of divisions between distinct groups. To the data on the size of these ethno-linguistic groups, the following formula is applied. $ELF(j) = 1 - \sum_{i(j)=1}^{N(j)} [s_{i(j)}]^2$. The measures is most disaggregated at level 15 ($ELF15$) and most aggregated at 1 ($ELF1$). A more illuminating explanation of the language trees and disaggregation is found in Desmet et al. (2012).

Notes: Fractionalization, itself, is the probability that two individuals chosen at random in a given country are from the same ethno-linguistic (ethnic) group. All measures take a value between 0 and 1, where 1 is certainty.

Table 2.4: Summary statistics of main variables

	No. of obs.	Mean	Std. Dev.	Min.	Max.
FISC	78	1.035	1.515	0	5.924
BORRO	78	0.988	1.131	0	4.282
EF	67	0.342	0.210	0.004	0.766
ELF	77	0.349	0.219	0.002	0.740
ELF1	78	0.139	0.174	0	0.647
ELF4	78	0.249	0.226	0	0.797
ELF7	78	0.295	0.252	0	0.846
ELF10	78	0.307	0.259	0.0002	0.897
ELF13	78	0.307	0.260	0.0002	0.897
ELF15	78	0.307	0.260	0.0002	0.897
GDP per capita	78	9.358	1.193	6.395	11.50
Country size	78	7.080	2.132	1.163	12.01
Proportion of 65+	78	11.23	5.096	3.275	21.32
Democracy	78	2.057	1.322	1	6.750
Natural resource rents	78	3.704	5.732	0	28.01
Pathogen prevalence	78	-0.139	0.558	-1.180	1.060
Trade	78	57.09	18.92	19.85	97.47
Arable land	76	17.43	13.32	0.0700	53.76
Tiers of government	78	1.551	0.658	1	3
Parliament	78	0.577	0.497	0	1
Gini	75	47.58	4.827	33.73	57.70
Origtime	78	0.259	0.209	0.012	0.750
Socio-cultural conflict	78	3.182	1.773	0	8.267

Table 2.5: Cross-correlation table of dependent and control variables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
(1) FISC	1.000												
(2) BORRO	0.833	1.000											
(3) GDP per capita	0.325	0.285	1.000										
(4) Country size	0.456	0.546	-0.067	1.000									
(5) Proportion of 65+	0.333	0.326	0.676	-0.015	1.000								
(6) Democracy	-0.134	-0.143	-0.590	0.056	-0.561	1.000							
(7) Natural resource rents	-0.066	-0.047	-0.189	0.244	-0.436	0.433	1.000						
(8) Pathogen prevalence	-0.155	-0.193	-0.587	0.124	-0.591	0.486	0.288	1.000					
(9) Trade	-0.245	-0.309	0.142	-0.625	0.187	-0.191	-0.229	-0.311	1.000				
(10) Arable land	-0.015	0.055	0.075	-0.181	0.477	-0.171	-0.445	-0.283	0.222	1.000			
(11) Tiers of government	0.243	0.110	0.194	-0.075	0.100	-0.124	-0.053	-0.082	0.169	-0.108	1.000		
(12) Parliament	0.173	0.150	0.576	-0.269	0.553	-0.401	-0.314	-0.368	0.403	0.213	0.286	1.000	
(13) Gini	-0.010	-0.018	-0.133	0.135	-0.051	0.007	0.068	0.170	-0.083	0.078	-0.037	-0.086	1.000

Table 2.6: Cross-correlation table of fractionalization measures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(1) EF	1.000							
(2) ELF	0.890	1.000						
(3) ELF1	0.406	0.425	1.000					
(4) ELF4	0.550	0.582	0.717	1.000				
(5) ELF7	0.440	0.505	0.620	0.900	1.000			
(6) ELF10	0.452	0.511	0.619	0.873	0.977	1.000		
(7) ELF13	0.453	0.511	0.614	0.873	0.977	1.000	1.000	
(8) ELF15	0.453	0.511	0.614	0.873	0.977	1.000	1.000	1.000

Table 2.7: Replication of Panizza (1999)

	FISC (1)	BORRO (2)	FISC (3)	BORRO (4)	FISC (5)	BORRO (6)
Country size	0.356***	0.311***	0.340***	0.301***	0.355***	0.312***
	(0.057)	(0.039)	(0.055)	(0.039)	(0.059)	(0.040)
GDP per capita	0.485***	0.327***	0.511***	0.319***	0.515***	0.322***
	(0.118)	(0.082)	(0.119)	(0.087)	(0.112)	(0.077)
ELF15	1.389***	0.934***			1.365***	0.938***
	(0.494)	(0.308)			(0.504)	(0.310)
Democracy			0.087	0.020	0.047	-0.008
			(0.103)	(0.069)	(0.092)	(0.059)
Constant	-6.450***	-4.560***	-6.333***	-4.169***	-6.815***	-4.501***
	(1.295)	(0.875)	(1.266)	(0.916)	(1.227)	(0.838)
R-squared	0.391	0.448	0.339	0.402	0.392	0.448
Observations	78	78	78	78	78	78

Notes: Robust standard errors are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 2.8: The effect of fractionalization on decentralization: baseline "cycling" results

	(1)	(2)	(3)	(4)	(5)	(6)
ELF1	-0.290					
	(0.677)					
ELF4		1.016				
		(0.625)				
ELF7			1.342**			
			(0.546)			
ELF10				1.506***		
				(0.507)		
ELF13					1.532***	
					(0.515)	
ELF15						1.532***
						(0.515)
GDP per capita	0.441**	0.434**	0.413**	0.416**	0.415**	0.415**
	(0.175)	(0.179)	(0.179)	(0.177)	(0.177)	(0.177)
Country size	0.351***	0.370***	0.373***	0.374***	0.374***	0.374***
	(0.064)	(0.063)	(0.063)	(0.064)	(0.064)	(0.064)
Proportion of 65+	0.047	0.063	0.062	0.062	0.062	0.062
	(0.049)	(0.051)	(0.050)	(0.049)	(0.049)	(0.049)
Democracy	0.200	0.200	0.182	0.178	0.177	0.177
	(0.143)	(0.135)	(0.125)	(0.124)	(0.124)	(0.124)
Natural resource rents	-0.035	-0.043**	-0.041**	-0.039*	-0.039*	-0.039*
	(0.021)	(0.021)	(0.020)	(0.020)	(0.020)	(0.020)
Pathogen prevalence	0.114	0.162	0.103	0.082	0.083	0.083
	(0.343)	(0.333)	(0.322)	(0.310)	(0.310)	(0.310)
Constant	-6.330***	-6.832***	-6.774***	-6.862***	-6.867***	-6.867***
	(1.609)	(1.602)	(1.537)	(1.530)	(1.532)	(1.532)
R-squared	0.377	0.396	0.423	0.439	0.442	0.442
Observations	78	78	78	78	78	78

Notes: The dependent variable in all columns is *FISC*. Robust standard errors are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 2.9: The effect of fractionalization on decentralization: extended controls

	FISC (1)	FISC (2)	FISC (3)	FISC (4)	FISC (5)	BORRO (6)	BORRO (7)	BORRO (8)	BORRO (9)	BORRO (10)
ELF15	1.125*	1.532***	1.638***	1.709***	1.444***	0.704*	1.077***	1.262***	1.328***	0.934***
	(0.583)	(0.515)	(0.545)	(0.515)	(0.497)	(0.419)	(0.312)	(0.354)	(0.292)	(0.306)
GDP per capita	0.437***	0.415**	0.430*	0.191	0.219	0.284***	0.203	0.208	0.009	0.072
	(0.133)	(0.177)	(0.252)	(0.185)	(0.196)	(0.105)	(0.122)	(0.168)	(0.119)	(0.115)
Country size		0.374***	0.271***	0.390***	0.365***		0.328***	0.256***	0.342***	0.320***
		(0.064)	(0.087)	(0.064)	(0.065)		(0.043)	(0.059)	(0.036)	(0.037)
Proportion of 65+		0.062	0.086	0.116*	0.101*		0.044	0.043	0.100**	0.075**
		(0.049)	(0.058)	(0.061)	(0.057)		(0.035)	(0.039)	(0.040)	(0.032)
Democracy		0.177	0.315**	0.201	0.218*		0.098	0.174	0.137	0.151*
		(0.124)	(0.156)	(0.158)	(0.110)		(0.095)	(0.121)	(0.120)	(0.086)
Natural resource rents		-0.039*	-0.038	-0.027	-0.031		-0.025*	-0.022	-0.010	-0.016
		(0.020)	(0.023)	(0.022)	(0.022)		(0.015)	(0.017)	(0.015)	(0.016)
Pathogen prevalence		0.083	-0.102	-0.220	-0.166		-0.151	-0.291	-0.443**	-0.509**
		(0.310)	(0.350)	(0.326)	(0.381)		(0.189)	(0.227)	(0.176)	(0.210)
Trade			-0.017					-0.016**		
			(0.010)					(0.007)		
Arable land			0.000					0.009		
			(0.012)					(0.009)		
Tiers of government			0.474*					0.123		
			(0.240)					(0.167)		
Parliament			-0.211					0.093		
			(0.441)					(0.301)		
Gini			-0.001					-0.009		
			(0.023)					(0.018)		
Constant	-3.396***	-6.867***	-6.447**	-5.452***	-5.749***	-1.889*	-4.193***	-2.958	-3.222***	-4.168***
	(1.264)	(1.532)	(2.626)	(1.535)	(1.865)	(0.953)	(1.035)	(1.863)	(0.959)	(1.191)
Colony FEs?				Yes					Yes	
Legal origins FEs?					Yes					Yes
R-squared	0.143	0.442	0.498	0.513	0.473	0.107	0.501	0.538	0.643	0.583
Observations	78	78	73	78	78	78	78	73	78	78

Notes: Robust standard errors are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 2.10: Extended controls: robustness

	FISC						BORRO					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
ELF15	1.781***	1.538***	1.303***	1.481***	1.768***	1.638***	1.319***	1.120***	1.028***	1.021***	1.296***	1.262***
	(0.524)	(0.524)	(0.471)	(0.525)	(0.588)	(0.545)	(0.331)	(0.314)	(0.319)	(0.303)	(0.360)	(0.354)
GDP per capita	0.380**	0.411*	0.366**	0.419**	0.405	0.430*	0.170	0.247*	0.193	0.200	0.202	0.208
	(0.177)	(0.207)	(0.182)	(0.179)	(0.257)	(0.252)	(0.120)	(0.143)	(0.124)	(0.123)	(0.167)	(0.168)
Country size	0.312***	0.361***	0.379***	0.386***	0.287***	0.271***	0.267***	0.327***	0.329***	0.338***	0.260***	0.256***
	(0.077)	(0.069)	(0.067)	(0.073)	(0.088)	(0.087)	(0.052)	(0.044)	(0.044)	(0.048)	(0.058)	(0.059)
Proportion of 65+	0.069	0.070	0.063	0.070	0.085	0.086	0.051	0.033	0.045	0.051	0.043	0.043
	(0.049)	(0.061)	(0.051)	(0.051)	(0.058)	(0.058)	(0.034)	(0.041)	(0.035)	(0.036)	(0.039)	(0.039)
Democracy	0.157	0.190	0.187	0.252	0.283*	0.315**	0.078	0.095	0.100	0.157	0.166	0.174
	(0.129)	(0.130)	(0.115)	(0.167)	(0.158)	(0.156)	(0.101)	(0.098)	(0.096)	(0.122)	(0.112)	(0.121)
Natural resource rents	-0.038*	-0.043**	-0.038*	-0.046*	-0.046*	-0.038	-0.024	-0.023	-0.025*	-0.032*	-0.024	-0.022
	(0.021)	(0.020)	(0.020)	(0.024)	(0.024)	(0.023)	(0.014)	(0.015)	(0.015)	(0.016)	(0.017)	(0.017)
Pathogen prevalence	-0.013	0.058	0.063	0.069	-0.090	-0.102	-0.244	-0.144	-0.155	-0.153	-0.288	-0.291
	(0.318)	(0.329)	(0.308)	(0.313)	(0.345)	(0.350)	(0.197)	(0.200)	(0.192)	(0.193)	(0.219)	(0.227)
Trade	-0.012				-0.016	-0.017	-0.012*				-0.016**	-0.016**
	(0.009)				(0.011)	(0.010)	(0.006)				(0.007)	(0.007)
Arable		-0.006			-0.004	0.000		0.006			0.008	0.009
		(0.012)			(0.013)	(0.012)		(0.010)			(0.010)	(0.009)
Tiers of government			0.401*			0.474*			0.086			0.123
			(0.227)			(0.240)			(0.152)			(0.167)
Gini				-0.008	-0.003	-0.001				-0.011	-0.010	-0.009
				(0.023)	(0.023)	(0.023)				(0.019)	(0.018)	(0.018)
Parliament					-0.002	-0.211					0.148	0.093
					(0.436)	(0.441)					(0.300)	(0.301)
Constant	-5.551***	-6.720***	-7.035***	-6.801***	-5.579**	-6.447**	-2.916**	-4.576***	-4.229***	-3.888**	-2.732	-2.958
	(1.853)	(1.801)	(1.514)	(1.878)	(2.602)	(2.626)	(1.194)	(1.231)	(1.044)	(1.509)	(1.880)	(1.863)
R-squared	0.452	0.445	0.469	0.444	0.463	0.498	0.518	0.503	0.503	0.501	0.534	0.538
Observations	78	76	78	75	73	73	78	76	78	75	73	73

Notes: Robust standard errors are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 2.11: The effect of fractionalization on decentralization: 2SLS results

	FISC (1)	FISC (2)	FISC (3)	FISC (4)	FISC (5)	BORRO (6)	BORRO (7)	BORRO (8)	BORRO (9)	BORRO (10)
Second-stage results										
ELF15	2.956*	2.004**	2.348*	1.534	1.787**	2.703*	2.357***	2.943**	1.763**	2.085**
	(1.721)	(0.998)	(1.352)	(0.959)	(0.879)	(1.504)	(0.828)	(1.335)	(0.713)	(0.813)
GDP per capita	0.475***	0.407**	0.441*	0.194	0.221	0.326***	0.183	0.233	0.001	0.077
	(0.143)	(0.171)	(0.233)	(0.165)	(0.189)	(0.120)	(0.131)	(0.172)	(0.116)	(0.138)
Country size		0.380***	0.256***	0.389***	0.368***		0.344***	0.220***	0.344***	0.330***
		(0.065)	(0.082)	(0.059)	(0.062)		(0.051)	(0.071)	(0.035)	(0.043)
Proportion of 65+		0.065	0.091*	0.113**	0.102*		0.054	0.056	0.108***	0.077**
		(0.047)	(0.052)	(0.054)	(0.052)		(0.034)	(0.036)	(0.036)	(0.031)
Democracy		0.170	0.290*	0.202	0.210**		0.077	0.115	0.135	0.127*
		(0.113)	(0.149)	(0.147)	(0.100)		(0.086)	(0.130)	(0.108)	(0.075)
Natural resource rents		-0.040*	-0.038*	-0.027	-0.032		-0.028	-0.022	-0.010	-0.018
		(0.021)	(0.023)	(0.020)	(0.021)		(0.017)	(0.024)	(0.014)	(0.016)
Pathogen prevalence		0.075	-0.131	-0.208	-0.157		-0.173	-0.359*	-0.472***	-0.478**
		(0.289)	(0.302)	(0.292)	(0.358)		(0.185)	(0.214)	(0.172)	(0.211)
Trade			-0.021*					-0.025**		
			(0.011)					(0.011)		
Arable land			0.002					0.013		
			(0.012)					(0.010)		
Tiers of government			0.447**					0.059		
			(0.224)					(0.164)		
Parliament			-0.305					-0.130		
			(0.419)					(0.349)		
Gini			-0.002					-0.011		
			(0.021)					(0.020)		
Constant	-4.316***	-7.002***	-6.361***	-5.371***	-4.973***	-2.893**	-4.559***	-2.756	-3.424***	-3.107**
	(1.513)	(1.555)	(2.342)	(1.458)	(1.702)	(1.304)	(1.237)	(2.068)	(0.911)	(1.316)
First-stage results										
Origtime	0.469***	0.546***	0.515**	0.607***	0.577***	0.469***	0.546***	0.515**	0.607***	0.577***
	(0.128)	(0.142)	(0.224)	(0.166)	(0.146)	(0.128)	(0.142)	(0.224)	(0.166)	(0.146)
Colony FEs?	No	No	No	Yes	No	No	No	No	Yes	No
Legal origin FEs?	No	No	No	No	Yes	No	No	No	No	Yes
AR Wald Chi^2	0.068	0.073	0.146	0.156	0.059	0.035	0.001	0.014	0.012	0.002
F-stat	13.35	14.71	5.297	13.37	15.54	13.35	14.71	5.297	13.37	15.54
Observations	78	78	73	78	78	78	78	73	78	78

Notes: Origtime is used as an instrument for $ELF15$. First stage results are reported in full in Table 2.12. The F-stat is F statistic for the explanatory power excluded instruments in first stage regressions. Robust standard errors are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 2.12: First stage regressions

	(1)	(2)	(3)	(4)	(5)
Origtime	0.469***	0.546***	0.515**	0.607***	0.577***
	(0.128)	(0.142)	(0.224)	(0.166)	(0.146)
GDP per capita	-0.020	-0.000	-0.003	0.009	-0.025
	(0.025)	(0.034)	(0.040)	(0.042)	(0.045)
Country size		-0.012	0.002	-0.013	-0.011
		(0.013)	(0.021)	(0.015)	(0.014)
Proportion of 65+		-0.015*	-0.014	-0.024**	-0.006
		(0.008)	(0.009)	(0.010)	(0.011)
Democracy		-0.017	0.008	-0.032	-0.007
		(0.026)	(0.037)	(0.024)	(0.029)
Natural resource rents		0.002	0.004	0.001	0.003
		(0.007)	(0.009)	(0.007)	(0.007)
Pathogen prevalence		-0.053	-0.045	-0.019	-0.113
		(0.067)	(0.082)	(0.081)	(0.079)
Trade			0.004		
			(0.002)		
Arable land			-0.003		
			(0.002)		
Tiers of government			0.043		
			(0.046)		
Parliament			0.047		
			(0.080)		
Gini			0.009		
			(0.006)		
Constant	0.368	0.441	-0.346	0.522	0.429
	(0.249)	(0.335)	(0.528)	(0.385)	(0.420)
Legal origin FEs?	No	No	No	No	Yes
Colony FEs?	No	No	No	Yes	No
R-squared	0.151	0.208	0.322	0.273	0.242
Observations	78	78	73	78	78

Notes: The table corresponds to the second-stage results in Table 2.11. The dependent variable in columns is $ELF15$. Robust standard errors are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 2.13: The effect of fractionalization on decentralization: baseline "cycling" results: alternate dependent variable

	(1)	(2)	(3)	(4)	(5)	(6)
ELF1	0.104					
	(0.504)					
ELF4		0.709*				
		(0.381)				
ELF7			1.105***			
			(0.340)			
ELF10				1.078***		
				(0.316)		
ELF13					1.076***	
					(0.312)	
ELF15						1.077***
						(0.312)
GDP per capita	0.221*	0.217*	0.199	0.204	0.203	0.203
	(0.123)	(0.123)	(0.124)	(0.122)	(0.122)	(0.122)
Country size	0.316***	0.325***	0.329***	0.328***	0.328***	0.328***
	(0.043)	(0.043)	(0.043)	(0.043)	(0.043)	(0.043)
Proportion of 65+	0.038	0.045	0.046	0.044	0.044	0.044
	(0.037)	(0.037)	(0.035)	(0.035)	(0.035)	(0.035)
Democracy	0.116	0.114	0.098	0.098	0.098	0.098
	(0.112)	(0.104)	(0.095)	(0.095)	(0.095)	(0.095)
Natural resource rents	-0.023	-0.028*	-0.027*	-0.025*	-0.025*	-0.025*
	(0.016)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)
Pathogen prevalence	-0.133	-0.095	-0.137	-0.151	-0.151	-0.151
	(0.219)	(0.210)	(0.190)	(0.189)	(0.189)	(0.189)
Constant	-3.922***	-4.166***	-4.169***	-4.195***	-4.193***	-4.193***
	(1.130)	(1.063)	(1.034)	(1.035)	(1.035)	(1.035)
R-squared	0.443	0.460	0.499	0.500	0.501	0.501
Observations	78	78	78	78	78	78

Notes: The dependent variable in all columns is *BORRO*. Robust standard errors are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 2.14: Robustness checks: 2SLS results

	FISC (1)	BORRO (2)	FISC (3)	BORRO (4)	FISC (5)	BORRO (6)	FISC (7)	BORRO (8)
ELF15	2.408**	2.564**	3.021**	2.502**	2.448**	2.368***	2.389**	2.587***
	(1.183)	(1.075)	(1.367)	(1.162)	(1.002)	(0.870)	(1.170)	(0.938)
AR Wald Chi^2	0.049	0.005	0.032	0.015	0.021	0.001	0.061	0.002
F-stat	12.17	12.17	10.52	10.52	16.28	16.28	13.61	13.61
Observations	71	71	64	64	71	71	71	71

Notes: Origtime is used as an instrument for ELF15. In all columns GDP per capita, Country size, Proportion of 65+, Democracy, Natural resource rents, Pathogen prevalence and a constant are omitted. Column (1) and (2) removes countries that vary 2 points or more in the dependent variable and column (3) and (4) removes countries that vary by 1 point or more in the dependent variable. Columns (5) and (6) remove the 7 largest countries by surface area. Columns (7) and (8) remove the 7 smallest countries by surface area. The F-stat is F statistic for the explanatory power excluded instruments in first stage regressions. Robust standard errors are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 2.15: Further robustness checks

	Horserace		New World		Old World		Quadratic		Geographic controls		Binary democracy	
	FISC (1)	BORRO (2)	FISC (3)	BORRO (4)	FISC (5)	BORRO (6)	FISC (7)	BORRO (8)	FISC (9)	BORRO (10)	FISC (11)	BORRO (12)
ELF15	1.924**	1.256**	0.175	0.500	2.957***	1.475**	1.279	2.161	1.690***	1.337***	1.564***	1.097***
	(0.515)	(0.313)	(0.440)	(0.431)	(1.029)	(0.594)	(2.350)	(1.590)	(0.542)	(0.281)	(1.055)	(0.905)
POL15	-0.442	-0.202										
	(0.980)	(0.686)										
ELF15 × ELF15							0.332	-1.424				
							(2.961)	(1.943)				
R-squared	0.443	0.501	0.784	0.811	0.394	0.361	0.442	0.505	0.485	0.593	0.421	0.391
Observations	78	78	31	31	47	47	78	78	73	73	78	78

Notes: Results are from OLS regressions. In all columns GDP per capita, Country size, Proportion of 65+, Democracy (not included in columns (11) and (12)), Natural resource rents, Pathogen prevalence and a constant are omitted. Column (1) and (2) are a "horserace" between ELF15 and POL15. Column (3) - (6) split the sample by the new and old world, the new world countries are defined as those in the Americas and Oceania. Column (7) and (8) introduce a quadratic term for fractionalization. Column (9) and (10) include a set of geographic controls, which are the log the mean elevation, the log distance the nearest river or coast, the share of tropical land, the log of the standard deviation of mean elevation and the log of absolute latitude. Column (11) and (12) replace the measure of democracy with the Boix et al. (2013) dichotomous measure of democracy. Robust standard errors are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 2.16: Robustness checks: alternate independent variable of interest

	OLS		2SLS	
	FISC	BORRO	FISC	BORRO
	(1)	(2)	(3)	(4)
Ln(Number of languages)	0.361**	0.383***	0.546*	0.652***
	(0.143)	(0.091)	(0.300)	(0.216)
R-squared	0.403	0.496		
AR Wald Chi^2			0.105	0.002
F-stat			11.92	11.92
Observations	65	65	65	65

Notes: Origtime is used as an instrument for *Ln(Number of languages)* in columns (3) and (4). In all columns GDP per capita, Country size, Proportion of 65+, Democracy, Natural resource rents, Pathogen prevalence and a constant are omitted. The F-stat is F statistic for the explanatory power excluded instruments in first stage regressions. Robust standard errors are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Chapter 3 A mailshot in the dark? The impact of the UK government's leaflet on the 2016 EU referendum

The second empirical chapter continues the theme in this thesis of work influenced by major political economy events. The previous chapter was only motivated by the 2014 Scottish independence referendum, however, in this chapter, I *directly* consider the case of the 2016 EU referendum. Here, I use individual-level data to ask what factors contributed to a populist outcome in the form of Brexit. In this context, I explicitly focus on the role of the government's pro-remain leaflet that was sent to UK households in the run-up to the referendum.

3.1 Introduction

There is an array of evidence that exposure to mass media can influence voting decisions and therefore political outcomes. This is true across various outlets, for instance, newspapers (Gerber et al., 2009, Gentzkow et al., 2011), biased news programs and television (DellaVigna and Kaplan, 2007, Enikolopov et al., 2011, Durante et al., 2017, Martin and Yurukoglu, 2017) and radio (DellaVigna et al., 2014, Yanagizawa-Drott, 2014, Adena et al., 2015). In addition, there is evidence that the effects of exposure can vary across demographics (DellaVigna et al., 2014, Yanagizawa-Drott, 2014, Kearney and Levine, 2015), which is attributed to certain groups being more susceptible to persuasion bias (Gerber et al., 2011, Barone et al.,

2015, Galasso and Nannicini, 2016).

In this chapter I show that government mass media regarding the 2016 EU referendum influenced voting behaviour. I exploit an individual's exposure to the government's mailshot that was sent to UK households before the referendum and I show that exposure, on average, led to a drop in the probability of voting leave by 3 percentage points. Britain voting to leave the European Union was a seismic event in European politics. A clear majority of economists and the UK government had warned that leaving the EU would depress the economy and create a lengthy period of uncertainty, and the government did not standby and let the referendum pass without conveying their stance to voters. The mailshot, in the form of a leaflet, contained information on the benefits of EU membership and reasons why the government supported the "Remain" side. They were, of course, sent to households before the referendum day and, either intentionally or by chance, not every person was exposed to the mailshot. Hence, this offers a unique opportunity to isolate the impact of the leaflet on voting behaviour.

I employ both a matching and difference-in-difference approach, which allows this chapter to address endogeneity concerns and selection issues, to estimate the causal effect of exposure to the leaflet on voting preference. In order to strengthen the inferences of the results, I conduct numerous placebo and robustness checks throughout the analysis. For the first part of the main analysis, I use all available individuals and find that exposure to the leaflet led to individuals being about 3 percentage points more likely to vote to remain in the EU. This effect is economically and statistically significant, and is robust to various specifications and placebo tests. In the second part, to explore who is driving this effect, I identify a group of respondents that had a low degree of exposure to other sources of referendum information and were exposed to the leaflet. Here, I allow for a heterogeneous treatment effect across different fixed demographic groups. I show that there is a much larger drop in the probability of voting leave for females, low income, the risk averse and those who expressed a preference to vote leave at a prior to treatment date after exposure to

the leaflet. I argue that persuasion bias is plausible explanation for this evidence, which allows certain groups to be more affected than others. In fact, the evidence of heterogenous effects echoes, and is complementary to, the work of DellaVigna et al. (2014), Barone et al. (2015) and Galasso and Nannicini (2016). Using the high exposure sample of individuals who were exposed to multiple sources of information and the leaflet, I show that Conservative partisans were also significantly negatively impacted, by about 6.2 percentage points. This occurs due to the visible splits in the Conservative party, thus party supporters took the leaflet as a signal from the Conservative government to back a remain vote.

To further support the findings, this chapter examines the persuasive mechanisms at work by exploring how exposure to the leaflet affected a individual's opinions about information contained in the leaflet. I find that individuals who were exposed internalized the leaflets information on a possible leave scenario. For example, the leaflet made the case that unemployment would be higher if the UK were to leave the UK, and the results suggest that exposed individuals believe that this would indeed be the case. Voters internalized the leaflet's persuasive information of likely vote leave scenarios and became less likely to for Brexit because of this. I confirm these channels by conducting a number of falsification tests of scenarios and information not contained in the leaflet.

This research makes significant contributions to the literature in three distinct ways. First, it shows that clear, descriptive and realistic information from the government can have a significant impact on vote preference, and explore the possible explanations for why this effect exists. In this manner, the findings sit somewhere between the previous contributions by Barone et al. (2015) on the negative impact of digital TV on Berlusconi's vote share, and by Gerber et al. (2011) on the positive impact of campaign mail on the Democratic candidate for the 2006 attorney general election in Kansas – of which this chapter is most closely related. Uniquely, in this research I diverge from the current mass media literature on the television and radio mediums by showing that a government backed mailshot can also be a persuasive strategy for

changing political preference in the context of a referendum. Moreover, my identification strategy allows me to identify the causal impact given that I can isolate the specific effect of the leaflet unlike in some of the previous media literature.

Second, the findings contribute to the work on inherent cognitive biases (Choi et al., 2014, Dohmen et al., 2010, DellaVigna and Gentzkow, 2010, Barone et al., 2015, Galasso and Nannicini, 2016). In particular, how certain demographics are heterogeneously affected by exposure to the information due to the existence of persuasion bias. The findings here shed new light on the existence of persuasion bias by isolating the effect of government information on certain groups, and directly supported by the findings of DellaVigna and Kaplan (2007), Barone et al. (2015) and Galasso and Nannicini (2016).

Third and finally, this research contributes to the quickly expanding literature on the determinants of Brexit and the support for populist policies (see, e.g., Goodwin and Heath 2016*b*, Antonucci et al. 2017, Los et al. 2017, Colantone and Stanig 2017, Becker et al. 2017). All current work has focused on the characteristics of an individual, or the population itself, in determining their vote preference, however, to the best of my knowledge, this is the first exploration into the impact of direct government intervention in a quasi-natural experimental setup for the 2016 EU referendum.

The remainder of this chapter is organized as follows. Section 3.2 provides some background on the referendum and leaflet itself. Section 3.3 describes the data and identification strategy used in the empirical analysis. Section 3.4 presents the main findings and various robustness tests. Section 3.5 presents results regarding the mechanisms and section 3.6 concludes.

3.2 Background

On June 23rd 2016, UK citizens were offered the opportunity to vote in a referendum as to whether the UK should "Remain a member of the European Union" or "Leave the European Union". The "Leave" side prevailed by 51.9% to 48.1%. Figure 3.1 and 3.2 present maps of vote shares by local authority district. Since this time, there has been a vast amount of debate in the media and amongst academics as to what drove the referendum result. Much of the empirical academic work has focussed on drivers mostly at the regional-level and only a small portion at the individual-level (see, e.g., Langella and Manning 2016 for an overview of vote leave determinants). For instance, Goodwin and Heath (2016*b*) show that support for leave closely mapped that of past support for UKIP, and Antonucci et al. (2017) highlight the role of educational attainment. Los et al. (2017) provide evidence that areas with a higher density of leave votes were areas that were relatively larger beneficiaries of EU funds. Similarly, Colantone and Stanig (2017) show that areas that were more exposed to globalisation had a greater tendency to vote to leave. Becker et al. (2017) focus on the vote break down at the counting areas (by local authority) and find evidence that key fundamentals of the population were at the heart of the leave vote, rather than exposure to immigration or their education profiles. Liberini et al. (2017) suggest that it was in fact dissatisfaction with ones own financial situation rather than general unhappiness that contributed to Brexit. Additionally, they show that it was only the young that were substantially pro-remain. On the whole, the academic work confirms much of the narratives in the media about "who voted for Brexit".

Despite the growing scholarly focus on individual and regional determinants, to the best of my knowledge, no other research has been conducted into the impact of the government's EU leaflet. It is of particular interest not only because of the impact it may or may not have had on the final result, but the leaflet came at significant cost to the UK taxpayer, £9.3 million. The leaflet, entitled "Why the Government

believes that voting to remain in the European Union is the best decision for the UK", was sent to all UK households on Monday 11th to Wednesday 13th April 2016. The 16-page document contained information on the benefits on EU membership and likely scenarios should Britian leave the EU with a particular focus on jobs, the economy and security. Examples of the front and back page of the leaflet are shown in Figure 3.3 and 3.4. Even at a glance, under no uncertain circumstances, the leaflet ubiquitously makes the case for remaining a member of the EU. The back page in particular makes clear the aforementioned three areas of focus, which the government believed would be better off if the UK remained a member of the EU. The leaflet was intended to make clear the government's position and therefore persuade voters to back a remain vote by providing a fair assessment of EU membership and likely scenarios should the UK vote to leave.[1] This, however, is not the first leaflet of its kind. In the 1975 EU referendum, Harold Wilson's Labour government sent a pamphlet to all UK households that made the case for remaining a member of the European Community, see Figure 3.5.[2] The pamphlet-backed "remain" side won with 67.2% of the vote, this apparent success may have acted as a catalyst in prompting the government into action to repeat the past.

Figure 3.6 and 3.7 present the spatial distribution of the share of respondents that received and read (exposed to) the leaflet by local authority district. Darker shades represent local authorities that contain a greater proportion of exposed people. The rationale for showing the geographic distribution of exposure is not to only set the scene but to show that no systemic pattern in exposure exists. Exposure levels appear randomly distributed in respect to a local authority's actual vote count, except for Scotland where the exposure share is lower. As the leaflet was sent to *all* households, there is no concern about the possible targeting of areas expected to

[1] In a referendum characterised by contentious and a misuse of statistics, the leaflet was assessed to contain largely factual and realistic content (Giles, 2016).

[2] A full transcript of the leaflet can be found here: `http://www.harvard-digital.co.uk/euro/pamphlet.htm`.

be marginal or strong leave areas, which would cause a selection bias.[3] The leaflet reached a wide and diverse audience across the UK,[4] the mean value of exposure by local authority is 52%, the largest and smallest proportions are 74% and 24%, respectively.

The exploration of this particular type of media is a distinct divergence from the current mass media literature which analyses the effects ideologically biased media consumption. However, in accordance with evidence in the related literature, I would expect certain demographics that are more likely to be affected by persuasion bias to change their voting behaviour to a greater extent, as in, for example, Kearney and Levine (2015), Barone et al. (2015) and Durante et al. (2017). This is expected as different groups will suffer to various degrees of inherent cognitive biases regarding decision making quality (Dohmen et al., 2010, Choi et al., 2014). There is also evidence that supporters the idea that persuasion bias is larger for voters than for, say, consumers (DellaVigna and Gentzkow, 2010).

[3] In this instance, there was no targeted campaign because the referendum was decided by absolute majority based on a national, rather than regional, vote. This is unlike a general election in the UK, which is conducted under a majoritarian system, where marginal areas are targeted with political party leaflets to win parliamentary seats.

[4] The leaflet was sent to Northern Ireland households throughout the week commencing 9 May to avoid disruption ahead of their local elections. All individuals in Northern Ireland are therefore removed from the sample.

3.3 Data and empirical strategy

3.3.1 Data

Individual-level data

The empirical analysis in this chapter draws on data from the British Election Study (BES) internet panel survey. It is a nationally representative survey of individuals that contains extensive information on personal political preferences, identity and demographics. I focus on one particular wave, wave 8 – á la Colantone and Stanig (2017). This wave was conducted between 6th May and 22nd June 2016, prior to the 23rd June referendum and contains a total of 33,502 individuals.[5]

The main advantage of these data, particularly for this chapter, is that wave 8 contains a variety of questions regarding the EU referendum. More specifically, there are questions regarding voting intention, exposure to referendum information and sets of questions created to elucidate opinions about the campaign and the EU on the whole. Moreover, the respondents can be linked to their parliamentary constituency and EU referendum counting area, which allows one to control for regional and individual factors simultaneously.

In this wave, respondents are asked about their voting intention in the EU referendum. Respondents are asked "If you do vote in the referendum on Britain's membership of the European Union, how do you think you will vote?" To create the primary dependent variable of voting intention, I code a dummy variable 1 for respondents who replied "Leave the EU", and 0 for those who reply "Remain in the EU". Those who responded "Don't know" are removed from the sample.

[5] Due to missing responses the number of analysed respondents is smaller than the full sample. Formatively, results do not change when models are re-estimated with survey weighting.

Another uniqueness of this wave is that respondents are asked about their exposure to the government's EU leaflet, which made the case for the government backed remain campaign. The relevant question reads as follows: "Have you received and read the UK Government's leaflet "Why the Government believes that voting to remain in the European Union is the best decision for the UK"?" Those replying "Yes, I received and read it" are coded 1, whereas those who respond "Yes, I received it but I haven't read it" and "No, I haven't received it" are coded as 0. This forms the primary explanatory variable in this chapter.

I follow much of the previous literature on voting and partisanship in selecting the set of individual-level control variables (Clarke et al., 2017, Colantone and Stanig, 2017, Aidt and Rauh, 2017, Liberini et al., 2017) and use the individual information of some aggregate data used by Langella and Manning (2016) and Becker et al. (2017). I also include some other potentially relevant variables such as a dummy for whether the respondent has friends from the EU and whether they speak another language other than English. To control for partisanship, I include a person's vote from the 2015 general election, which follows from the intuition that people who voted for Brexit typically voted for the right-leaning parties in the 2015 general election.[6] In order to capture this effect, I rank parties based on their Euroscepticism. More specifically, they are ranked by the percent of negative mentions of the European Union in that party's 2015 general election manifesto, taken from the Manifesto Project Database by Volkens et al. (2017).[7] The variable is centered by normalization (mean of 0 and standard deviation of 1). The non-normalized ranks are reported in Table 3.2. I also include measures of the Big Five personality traits for each individual, this helps to further unpick the black-box of factors that influenced a individual's referendum vote. Given the importance for these traits in

[6] This is arguably better than using a persons party affiliation due to the small number of UKIP supporters.

[7] Here, alternative rankings were tested, such as: negative mentions of multiculturalism and internationalism. The interpretation of a positive and significant effect is unchanged regardless of the information used to rank the parties.

determining partisan support (Bakker et al., 2015, Aidt and Rauh, 2017), it is entirely plausible that persons of particular fixed personality traits favoured one side more than the other in the referendum.[8]

Descriptive statistics

As a basic statistical point of motivation for this chapter, I split the summary statistics by exposure status in Table 3.3. It shows that "exposed" individuals are less likely to express a preference for a leave vote compared to non-treated people. Moreover, on average, people exposed to the leaflet are more likely to be male, older and to hold a university degree than those non-exposed individuals; they are also less likely to have children but more likely to be a home owner relative to individuals who were not exposed to the leaflet; there is no difference across exposure status for the income groups. These are in line with expectations as those with a degree are expected to be more interested in information regarding the referendum, whereas those with children and who are employed are typically busier. The leave-remain vote split is only 0.3 percentage points away from the actual vote shares, which highlights the representativeness of the sample is.

The effect of the leaflet is plausibly reflected in the polls when examining the trends around the time of exposure. As shown in Figure 3.8, there is a clear upward swing, of those people who were polled, reporting that they would vote to remain in the EU after the leaflets were sent out.[9] Obviously, here it is not clear if this up-tick for remain is due to the leaflet or a result of other referendum campaign rhetoric.

[8] For a discussion of the extent to which the Big Five personality traits can be considered 'fixed', and therefore exogenous, see Brown and Taylor (2014). If we are to assume they are indeed fixed throughout an individual's adult life, this coincides with their ability to participate in the referendum, as voters must have been over the age of 18.

[9] As similar trend is found in the FT's poll of polls, found here: `https://ig.ft.com/sites/brexit-polling/`.

For instance, on the 18th April, then Chancellor of the Exchequer, George Osborne, defended claims that Brexit would cost households an average of £4,300 per year. And on the 22nd April, then President of the United States, Barack Obama, said that the UK would be at the back of the queue to establish a trade deal with the US. Robust econometric methods are required to disentangle this relationship and to allow a causal reading.

3.3.2 Empirical strategy

Exposure to the government's EU leaflet should reduce an individuals probability of voting leave by making the case for remaining in the European Union because individuals will be persuaded by the information provided, therefore internalizing the government's remain position. However, I do not expect this effect to be unambiguous. As the related literature has shown (DellaVigna and Kaplan, 2007, Barone et al., 2015, Durante et al., 2017), certain demographics are more susceptible to persuasion bias. This is entirely plausible in this scenario given that the Brexit literature has established that there are 'core' leave and remain supporters. If this is indeed the case, one would expect a heterogeneous effect across the different population groups.

I empirically investigate whether voting behaviour for the 2016 EU referendum changed post-exposure. I formalize this relationship with the following reduced form regression model:

$$Vote\ Leave_i = \alpha\ Exposed_i + \psi X_i + \epsilon_i \tag{3.1}$$

where $Vote\ Leave_i$ denotes the outcome variable, taking the value 1 if individual i will vote to leave the EU, and 0 if they will vote remain. $Exposed_i$ is the indicator variable for whether an individual has been exposed to the government's leaflet. X_i

is vector of control variables which contains a rich set of controls for gender, age, marital status, ethnicity, employment status, whether the individual has children, home ownership, highest academic qualification, whether individual has friends from the EU or speaks a language other than English, their partisan status, a set of income dummies and indicators of the Big Five personality traits. ϵ_i is the error term.

To begin with, equation 3.1 is estimated by probit models. In doing so I cannot infer causality given that I cannot rule out selection bias. That is, for instance, those individuals who were more likely to vote to remain may be more likely to select themselves into a pro-remain treatment. Given that exposure to the leaflet is quite plausibly not truly exogenous, I make use of the quasi-natural experimental setup and apply more appropriate techniques. Specifically, I primarily employ a propensity score matching approach as the workhorse for estimation and conduct a battery of robustness checks to ensure causal inference. Matching gains it legitimacy as a technique for causal inference from the idea that there is no selection into treatment, conditional on covariates, then difference in means across exposed and control groups have a causal interepretation (Imbens and Wooldridge, 2009). This is similar to the approach of Broockman and Green (2014) who use a difference-in-means approach to assess the impact of exposure to online ads on political candidates. I also track a reduced number of individuals to a previous wave and use a difference-in-difference approach to further strengthen my suppositions.

3.4 Results

This section presents the main results of my empirical analysis. I estimate the effect of the leaflet on voting intention for the 2016 EU referendum. I estimate this effect via multiple identification strategies, across several samples and conduct some placebo and robustness tests to support the findings.

3.4.1 Who voted for Brexit?

First, I establish the characteristics, at the individual-level, that drove a person into voting in favour of Brexit.[10] This is a useful exercise for getting to grips with the data in terms of quality and representativeness. Table 3.4 shows the results for probit regressions testing the various determinants. Column (1) reports a relatively parsimonious specification, which includes only basic demographics, education and partisanship. Column (2) and (3) then introduce household income and the Big Five personality traits, respectively. Columns (4), (5) and (6) progressively include an array of fixed effects to soak up various degrees of unobserved heterogeneity. In column (4) I include local authority fixed effects to control for local authority characteristics, this was also the aggregation at which the referendum votes were counted at. 11 local authorities of the 379 are dropped as they contain no respondents in this sample.[11] Column (5) includes county fixed effects to soak up more regional trends, for instance, Scotland's overwhelming preference for remain, 39 counties are included. Column (6) introduced proximity to referendum date fixed effects, this a dummy for the day the respondent completed the survey. The inclusion of these dummies is in order to capture one-off day-to-day events that would influence ones vote preference. For example, the murder of pro-remain MP, Jo Cox, on the June 16th caused referendum campaigning to be suspended.[12][13] Casting an eye across these 6 columns it appears that regardless of the fixed effects included the point estimates remain qualitatively the same. The elderly are more likely to vote leave, although this is at a decreasing rate. The average turning point, across columns (1)-(9) is estimated to be about 59 years old. This is in line with the findings of

[10] In the spirit of the previous research, I also include local authority district controls as a replacement for local area fixed effects and the main result remains unaffected, see Table 3.5.

[11] The referendum count was made for 382 local authority districts, I exclude Northern Ireland, Gibraltar and the Isles of Scilly.

[12] Details on the murder in context of the referendum are provided here: `https:/www.theguardian.com/politics/2017/may/21/jo-cox-uk-general-election-campaign-pause`.

[13] Results remain the same when removing individuals who responded after 16th June.

Liberini et al. (2017), who argue that it was only the very young that were substantially pro-remain. More educated people are associated with a lower likelihood of voting leave, relative to those with no formal academic qualifications, highlighting the educational gap in voting preference. Those with a degree are, on average, about 28 percentage points less likely to vote leave than those with no formal qualifications.[14] Home owners are also less likely to report that they would vote leave. For those individuals with a household income of over £70,000 per annum, they are about 12 percentage points more likely to vote to remain than those with a yearly income of 0-£20,000, holding everything else constant. Conversely, those who voted for relatively more eurosceptic parties in the previous general election are associated with a higher probability of voting leave. Briefly, in terms of the Big Five personality traits, the direction of the effects are in accordance with the prior expectations. Those individuals who are more conscientious or extroverted are, on average, more likely to favour a leave vote. Whereas some weak evidence is found for those who are more agreeable and more open are less likely to vote to leave. No significant effect is found for neuroticism.

So far, the results presented here confirm the media tropes about "who voted for Brexit" and are consistent with what has been shown the existing literature. The point estimates remain consistent across various models with assorted fixed effects. However, I do not assign a causal inference to the findings at this stage. The role of the government's pro-remain leaflet is explored in the proceeding sections.

[14] This is very close to the estimate by Goodwin and Heath (2016*a*), who estimate an effect of 30 percentage points.

3.4.2 The impact of the government's EU leaflet on voting behaviour

This research question poses a difficult puzzle in identifying a causal effect of leaflet exposure. As the leaflet was sent to all UK households there are no areas intentionally untreated nor a particular discontinuity to exploit. The variation in treatment assignment comes from the selection into treatment i.e. a persons decision to read the leaflet if they received it. Thus, in order to apply a causal reading to the impact of exposure to the leaflet on voting behaviour, I implement a matching strategy. I, in theory, should only need to control for the probability of treatment itself. In the obvious absence of this, I control for the observed characteristics that select an individual into treatment (Angrist and Pischke, 2008). Matching gains its legitimacy a technique for causal inference from the idea that there is no selection into treatment, conditional on covariates, then difference in means across exposed and control groups have a causal interpretation (Imbens and Wooldridge, 2009). This strategy does not, however, deal with the possible measurement error that is a result of self-reporting exposure status. This issue is not exclusive to this paper. It is likely a potential issue in many studies that use self-reported data. In this context, it is possible that individuals will report that they read the leaflet when they in fact did not. This would bias the estimate of the effect of exposure.

In order to match homogeneous individuals with one-another, I use the comprehensive set of observed characteristics in Table 3.3 and I crucially include two extra matching covariates: an individual's pre-existing Brexit preference, which is captured by their voting intention from a previous wave before treatment and an indicator variable of referendum interest.[15] Naturally, those who report themselves

[15] I match, with replacement (each control observation can serve as the counter-factual for more than one treated observation), on the following set of variables: male, age, age^2 / 100, married, widowed, non-white, employed, kids01, ln(household size), home owner, degree, a-level, gcse, friends from the EU, speaks another language, partisan, 3 categories of household income, agreeableness, conscientiousness, extraversion, neuroticism, openness, a dummy for pre-existing Brexit preference

as interested in the referendum are more likely to select themselves into treatment. The pre-existing Brexit preference is included as a restriction to ensure that individuals are matched to like-minded voters. This explicitly accounts for the fact that voters who are more likely to vote remain are more likely to select into a remain treatment. Both of these variables are powerful predictors of treatment status. I employ 5 nearest-neighbour matching where the nearest neighbours are identified from their propensity scores, conditional on the full set of control covariates. The propensity scores are obtained from a probit regression of the form:

$$Pr(Exposed_i = 1 \mid Z_i) \;=\; \Phi\left(\psi X_i + \delta\, VoteLeave_{it-1} + \zeta\, ReferendumInterest_i + \epsilon_i\right) \tag{3.2}$$

where $Exposed_i$ is the indicator of exposure status to the government's EU leaflet for individual i; Z_i is a function of all covariates that effect treatment status; Φ is the cumulative distribution function of a standard normally distributed random variable; X_i is a vector of control variables from Table 3.4 column (3); $VoteLeave_{it-1}$ is whether the individual would vote leave from a wave prior to treatment; $ReferendumInterest_i$ is a dummy for a respondents interest in the referendum; and ϵ_i is the error term. The corresponding propensity score estimations are shown in Table 3.15.

In Table 3.6 I examine the effect of leaflet exposure on voting for Brexit. In columns (1) and (2), the coefficient is negative and statistically significant: being exposed to the leaflet is associated with a 4.8 percentage point decrease in probability of voting for Brexit.

As mentioned, without being careful about endogeneity concerns, this effect cannot

and a dummy for referendum interest.

be labelled as causal. In columns (3)-(6), I implement the propensity score matching strategy. The coefficients report the average treatment effect on the treated (ATT) for leaflet exposure. In column (3) individuals are matched on their individual characteristics. In columns (4), (5) and (6) I begin to impose restrictions that the matched control observations must come from the same day of survey response, county as the treated individual, and both simultaneously, respectively. The point estimates are consistently estimated, remain negative and statistically significant. The coefficients are now smaller in absolute magnitude, compared to the OLS estimates, which indicates that the matching approach has been successful in reducing the endogeneity bias. Exposure to the leaflet reduced the probability of voting for Brexit by about 3 percentage points. Formal t-tests between treated and control groups fail to reject the balancing hypothesis for all variables that enter the propensity score equation, which confirms that the matching procedure has been successful in matching together homogeneous individuals and reducing the covariate bias (see Table 3.7 and Figure 3.11).

Thus far, the results establish that individuals became around 3 percentage points less likely to vote for Brexit after being exposed to the leaflet, which is in accordance with the other campaign mailing literature by Gerber et al. (2011). I can now proceed to focus on particular groups of the population which may be driving this result.

Low degree of exposure to other information sources

To ensure a clean identification of the leaflet's effect, I restrict the sample to individuals who had a low degree of exposure to other sources of referendum information. To be specific, individuals are dropped from the sample who report themselves to have watched any of the 5 major televised EU debates; have heard about the EU from television, newspaper, radio, the internet or talking to other people; or have been contacted by referendum campaigners. This leaves a sample of 513 individuals.

The summary statistics and spatial distribution for this sample are shown in Table 3.8 and Figure 3.9 and 3.10, respectively.[16]

Table 3.9 shows the ATT of exposure to the government's EU leaflet on the likelihood of voting leave. For the full low degree of exposure sample, column (1), the coefficient suggest that exposure is negatively related to voting leave, however, the effect is statistically insignificant. In order to test how the leaflet impacted demographics that are more or less susceptible to persuasion bias, I split the sample across various groups to allow for a heterogeneous treatment effect.

In column (2), I find that females are 8.8 percentage points less likely to vote leave after exposure, whereas no significant effect is found for males. This is consistent with the findings of Galasso and Nannicini (2016), that females respond better than men to a campaign with positive rather than negative message. Low income and the risk averse are also significantly less likely to vote leave after exposure relative to the control group, the effects are 11.4 and 10.2 percentage points, respectively, and they are precisely estimated. As voting in favour of leaving the EU was widely perceived as a risky outcome, it is not suprising that risk averse individuals were less likely to vote in favour of Brexit once they received information from the government advising them not to. The estimates are precisely estimated and far larger than the effect for the whole sample in Table 3.6. The findings here match up nicely with the priors that particular groups will be more affected by exposure due to a greater susceptibility to persuasion bias, which is also consistent with DellaVigna and Kaplan (2007) and Barone et al. (2015).

In columns (8) and (9) I explore how different partisan groups were affected. At this stage, I find no difference in the way Conservative or Labour partisans reacted, in terms of voting preference, to exposure.

[16] When comparing the summary statistics of the full and low exposure sample, the statistics show that low exposure sample are made up of individuals of similar characteristics to the full sample and are located across the UK.

As a final, but crucial, investigation, I test the effectiveness of the leaflet of changing voting intention. Here, I should observe that voters who reported in a prior treatment wave that they would vote leave, should have a larger drop in the propensity to vote leave after exposure. In column (11) I test this hypothesis by using only individuals who said they would vote leave in wave 6, and find that after exposure, they are 9.3 percentage points less likely to vote to leave the EU than the control group. This is an important distinction given the primary aim of the leaflet was to garner support for the remain side.

In all columns covariate balancedness is achieved for all variables in the propensity score equation, thus homogeneous individuals have been matched together and covariate bias reduced – these diagnostic results are available on request.

Overall, the presented matching results confirm that there is a larger drop in the probability for voting for Brexit amongst certain groups of individuals that had a low degree of exposure to other sources of referendum information and to the leaflet. This is, again, consistent with the hypothesis put forward by Gerber et al. (2011), that the effects of the mailshot should be larger in a low information environment. I also confirm that the leaflet had an especially salient effect on demographics that are more susceptible to a persuasion bias, which is in accordance with the existing literature.

Placebo analysis

As a robustness check and to complement the matching strategy, I conduct a number of placebo tests. If the documented effect on voting behaviour is due to the leaflet, I should observe no effect of exposure to the leaflet on earlier political outcomes for the governing Conservative Party or other incumbent governments. In Table 3.10, columns (1), (2) and (4) re-estimate the matching procedure using whether a respondent voted for the Conservative party in 2010, 2005 or the 2016 May local

elections as the dependent variable, respectively. In (3), I check whether there is an effect for voting for Labour in 2005, then the incumbent party. All respondents were not exposed to the leaflet at these dates. I find no such evidence that there were any prior treatment effects driving the results, even in the local elections which allow for examination of voting behaviour just a month before the referendum. Furthermore, under the identifying assumption that exposure exclusively affected referendum voting behaviour, I should also find no effect that exposed individuals were more likely to take favourable views of David Cameron (leader of the Conservative Party and Prime Minister at the time). I find no evidence to support this hypothesis, which suggests that the results are in fact due to the leaflet conveying its pro-remain information acutely.

Difference-in-difference

To further support the matching strategy in applying a causal inference to the results, I now employ a difference-in-difference (DiD) approach. I identify a number of individuals in the low degree of exposure sample who also completed wave 6 (pre-treatment) of the BES as well wave 8 (post-treatment), this however reduces the total number of individuals ($N = 430$). I now estimate the following regression model:

$$y_{it} = \alpha_i + \beta_1 Post_t + \beta_2 Exposed_i + \beta_3 (Post_t \cdot Exposed_i) + \psi X_{it} + \epsilon_{it} \qquad (3.3)$$

where $t = 0, 1$. y_{it} denotes the outcome variable, whether individual i will vote to leave at time t. The model includes individual fixed effects (α_i), a vector of controls (X_{it}) and an error term (ϵ_{it}). $Post_t$ takes the value 1 for the post-treatment period and $Exposed_i$ takes the value 1 for all individuals who were exposed, 0 otherwise. The coefficient of interest is β_3 which is the DiD estimate of the effect of

the leaflet on voting behaviour for the exposed. The models are estimated using a fixed effects estimator to account for unobserved individual heterogeneity. I focus on separating individuals by their inherent characteristics (risk aversion and gender) rather than attempt to split the sample by characteristics that plausibly vary over time (partisanship and income). Given the imbalance of certain variables between the treated and untreated groups, I also balance the sample using each an individual's propensity score from a probit regression and re-estimate equation 3.3 in a linear probability model. Again, I allow for heterogeneous effects across the fixed demographics groups.

Table 3.11 reports estimates from the DiD analysis. The point estimates from the fixed effects specifications are similar to those from the matching approach. They show that females and the risk averse are 11.1 and 9.6 percentage points less likely to vote to leave than the relevant control group. In the balanced sample the DiD term again shows results qualitatively the same to those produced in the matching approach.

On the whole, the difference-in-difference results confirm what has been shown previously. That is, a negative and significant impact on the exposed group –which is a significantly larger drop if an individual belongs to a demographic that is affected more by persuasion bias– on their probability of voting leave.

High degree of exposure to other information sources

I can now take the analysis a step further and investigate whether individuals who were exposed to other sources of information during the referendum campaign were impacted by the leaflet. Given that there was an array of media sources discussing the referendum during the campaign this is an interesting exercise. I use the set of 5,619 individuals who were excluded from the previous sections. These individuals were treated with multiple sources of information as well as the leaflet. Explicitly, I

keep individuals who have watched any of the televised debates, heard about the EU from various sources or had been contacted by referendum campaigners, as well as being exposed to the leaflet. I repeat the same propensity score matching as before for this heavily treated sample.

The corresponding results are reported in Table 3.12. In column (1), insignificantly, the coefficient is now absolutely smaller in size than the low exposure sample. In this sample, however, the results suggest some effect for males. In the groups where a large or significant effect is found in the low exposure sample, the coefficients are now severely demeaned or insignificant at the conventional levels, with the exception of Conservative partisans. Why might this be? For at least two decades there has been a rift between members of the Conservative party on the party's stance on the EU and this split naturally translated into the referendum campaign.[17] In this high exposure sample, individuals will have seen various Conservative party members on either side of the referendum debate. For instance, even in the 5 major debates, there was a Conservative MP on both the remain and leave side in every debate. Thus, exposed Conservative individuals will have interpreted the leaflet as a signal from the Conservative Party, who were in government at the time and sanctioned the leaflet, to vote to remain. They were, on average, 6.2 percentage points less likely to vote to leave after exposure.

One area that warrants further investigation is the heterogeneous effects within partisan groups. The most interesting group to explore more deeply is the Conservatives. One would expect a larger treatment effect on those supporters who are from the Cameron wing of the party. Conversely, there may be a negative, or no, response to the leaflet from those Tory supporters from the right wing of the party.

[17] In contrast, all other major political parties ran a united campaign about how their supporters should vote. The Labour Party (only 10 members backed leave), the Liberal Democrats, the Scottish National Party and the Green party all supported remain. UKIP and the Democratic Unionist Party supported leave.

On the whole, this suggests that the impact of the leaflet was far less pronounced in respondents who were exposed to other sources of information regarding the referendum. The null-result here goes some way to explain the outcome of the referendum as many individuals were not uniquely exposed to the leaflet as a source of referendum information.

3.4.3 Robustness checks

I now perform a series of checks to further assess the robustness of the main findings. First, in the full sample, I consider alternate matching strategies, including Epanechnikov kernel matching and a fewer number of nearest neighbour matches. Table 3.16 presents the results. The results obtained from these alternate strategies support the results from the previous section: exposure leads to a significant reduction in the probability of voting to leave the EU.

Second, in the full sample, I alter the set of matching covariates. I systemically exclude certain characteristics, such as income and personality traits, and allow various fixed effects to enter the propensity score equation. The results remain the same – the exposed group, on average, became significantly more likely to vote to remain than the control group. The results are shown in Table 3.17.

Similarly, third, I repeat the systematic exclusion and inclusion of matching variables for the low exposure sample, whilst allowing for heterogeneous treatment effects. I also introduce government office region fixed effects in all estimations.[18] The results are presented in Table 3.18. The results here support that of the previous section: for individuals who were only exposed to the leaflet, certain demographics who are more susceptible to persuasion bias, were even more likely to vote to remain than the control group.

[18] There is no distinction between party supporters due to the small sample size, therefore being unable to locate sufficient matches.

Fourth, I include calipers of varying sizes into the matching strategy in an attempt to improve the quality of the matching. The corresponding results are shown in Table 3.19. Here, the results remain qualitatively the same and a slightly larger coefficient is produced when the caliper is 0.005 and 0.001.

And fifthly, I repeat the analysis whilst removing individuals from Wales and Scotland, where exposure is lower on average. I expect an unambiguous treatment effect regardless of the region an individual is located in. I drop individuals from Scotland, then Wales and then both simultaneously. The results, presented in Table 3.20, remain the qualitatively the same.

3.5 Mechanisms

The results presented thus far establish that exposure to the government's EU leaflet decreased an individual's propensity to vote leave in the 2016 EU referendum. The leaflet presented information on key topics that were of concern to voters and this section assess how informed voters became on these issues after exposure. I also conduct some falsification tests of these channels and consider some alternative mechanisms.

3.5.1 The effect of the government's EU leaflet on perceptions of leave vote outcomes

I now explore the mechanisms through which the leaflet should have influenced an exposed individual's referendum vote. To do so, I return to the sample of 6,123 individuals used in Table 3.4. I use individuals where the answers to the relevant question are non-missing. By being exposed to the leaflet it should have highlighted particular areas of contention which would be typically worse off if a leave vote

were to win. For instance, one area highlighted by the leaflet was job security, the word "job" was used 8 times on 5 separate pages.[19] The leaflet made the case that jobs would be protected conditional on the remain side winning. By making minor change the econometric strategy, I can establish these channels of impact. To test the diffusion of the leaflets information on job security, I regress a person's opinion on unemployment if the UK were to leave the EU on their exposure status. The relevant question reads: "Do you think the following [unemployment] will be higher, lower, or about the same if the UK leaves the European Union?" From this I create a variable coded 1 for respondents who believe that unemployment will be "higher" or "much higher" if the UK were to leave and 0 otherwise.[20] This is repeated for other key areas covered in the booklet: the general economic situation, security, whether they believe the referendum to be final, their personal financial situation, UK workers working conditions and world influence.

The results are reported in Table 3.13. The specifications used in all columns are identical to that of Table 3.4 column (8) (i.e., controlling for individual characteristics, local authority, and date fixed effects). Column (1) shows that those who were exposed to the leaflet were, on average, 4.6 percentage points less likely to believe that the UK economy would better off if the UK were to leave, relative to the unexposed control group and is significant at 1% level. Analogous results are obtained that suggest exposure results in individuals being more likely to believe that there would be higher unemployment, column (2); the risk of terror would be higher, column (3); the referendum is the final say on the EU, column (4); and UK workers would not be better off, column (6). No effect is found for personal financial situation and the UK's world influence. For the former, this is plausibly due to the macro focus of the leaflet, with only minimal mentions of the cost of living impacts of leaving the EU. And for the latter, it may be due to lack of clarity in the leaflet

[19] The words "economy" and "economic" were used a total of 12 times, and "security" was used 5 times.

[20] The responses: "about the same/unchanged", "lower" and "much lower", are coded as 0.

about the UK's role in the EU's actions on the world stage. There is only a very brief mention about the EU's role in the Iranian nuclear deal and tackling climate change.

These results suggest that the leaflet was effective at conveying its persuasive message about contentious issues on the referendum trail. Voters idealized the government's position on certain scenarios and therefore became less likely to vote to leave. In essence, individuals believed the information the leaflet provided. This is perhaps testament to the fair and realistic assessment of a possible leave outcome.

3.5.2 Placebo tests and alternate mechanisms

The results of the previous subsection suggest that by reading government's media on particular areas of contention influenced a voter's perceptions of a likely scenario should the leave side win the referendum. To further support these results as channels of impact, I now consider some placebo tests and alternate mechanisms. First, as a placebo, I explore whether reader's views of specific outcomes that were not addressed in the leaflet were affected. Second, whether readers became more or less receptive to prompts from the government after exposure. And third, after being exposed, readers may take favourable views of prominent leave campaigners as an act of rebellion against the government.

To investigate these hypotheses, I exploit additional questions in the BES. I estimate the specifications as in Table 3.13 but similarly change the outcome measure. The results are presented in Table 3.14. In columns (1)-(4) I show the falsification tests of the channels of impact. I regress whether a person believes that: there will be more international trade if the UK leaves the EU; the EU would be better off if Turkey were to join; the EU has undermined UK parliamentary sovereignty; and immigration is increasing, on exposure status. All of which were extreme areas of discussion in the televised debates and in the wider media during the campaign trail,

however, –critical to the placebo test assumption– no information on these topics was provided in the leaflet.[21] As expected, the coefficients are not significantly different from 0. Now I attempt to rule out alternate mechanisms. In column (5) I test whether exposed individuals became more trusting in general and no effect from exposure is found. In columns (6), (7) and (8) I also find no evidence that individuals take more favourable views of prominent leave campaigners, Boris Johnson, Michael Gove or Nigel Farage, after exposure.

These findings suggest that the government's message was acutely conveyed to voters. The transmission mechanism was through the information provided rather than altering voters sense of trust or pushing them toward the leave campaign as an act of dissent. Importantly, I also show that exposure did not affect voter's opinions of scenarios not mentioned in the leaflet.

3.6 Conclusions

While much research has been undertaken on mass media's impact on political outcomes, the impact on a single political event is largely unexplored. Moreover, much of the work on Brexit has been focussed on individual or regional characteristics that drove the leave vote, this work is a distinct aberration from this strand as I offer causal estimates from a quasi-natural experiment around the time of the 2016 EU referendum.

This chapter has addressed the role of the government's mailshot to UK households on the 2016 EU referendum. I find that those individuals who were exposed to the leaflet displayed a higher probability of voting to remain in the EU than the

[21] In the survey, respondents were asked what is the most important issue in the referendum. About 20% of individuals said the economy (in the leaflet) and another 20% said UK sovereignty (not in the leaflet).

untreated control group. The effect is economically significant, about 3 percentage points, and statistically robust across different specifications. In terms of the magnitude, the effect is comparable to that found in Gerber et al. (2011), who find the effect of mailing to be between 1.5 and 3.5 percentage points. When exploring the groups that are driving this result, I find that the individuals who were exposed to other referendum informtion to a low degree and exposed to the leaflet displayed a much lower propensity to vote leave. It is those demographics who are more susceptible to persuasion bias that were affected to a larger extent, which is consistent with the literature by DellaVigna and Kaplan (2007), DellaVigna et al. (2014), Barone et al. (2015) and Galasso and Nannicini (2016). Specifically, I find that the effect is larger in absolute terms for females, low income and the risk averse, I also show that the leaflet was indeed effective at changing individuals voting intention using data from prior to exposure. I present evidence that shows in the midst of Conservative party in-fighting over the party's position, exposed Conservative supporters were more likely to vote to remain as they took the leaflet as a signal from the Conservative government.

In terms of the mechanisms at work, I show that the leaflet was effective at conveying the likely scenarios of a leave vote and highlighting the benefits of EU membership, which persuaded voters into voting to remain in the EU. This is because of the realistic, concise nature of the information provided. Here, I also reject alternate mechanisms and can hence be confident that the channel of impact was through leaflet exposure.

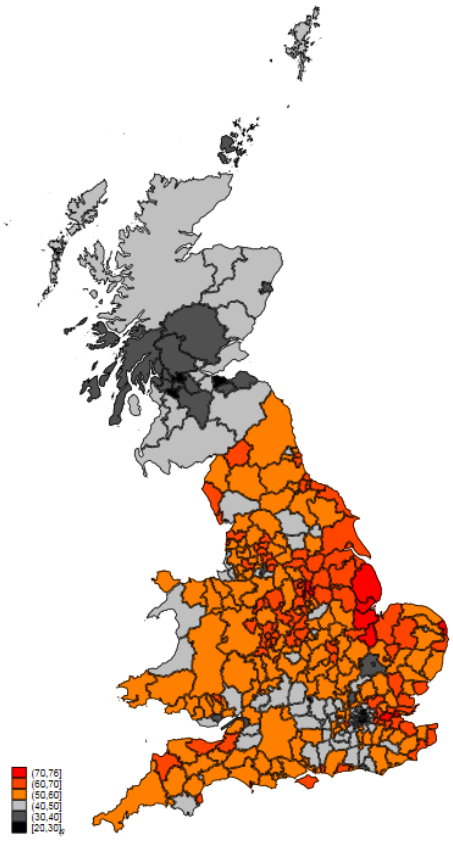
My findings offer the first systematic evidence that exposure to the government's leaflet influenced voting behaviour in the 2016 EU referendum. Whilst this research is specific to the referendum, my analysis provides more general insights into the economic allocation of scarce public resources and strategies for political campaigning based on persuasion bias. A more targeted campaign, for example, based on reaching specifically those demographics who were more likely to believe the information and change their voting behaviour accordingly may have been more fruitful.

As this chapter considers only a single source of referendum information, there are several possible areas for future research. The sheer number of sources of information on the referendum may prove to be an intersting area to explore. The televised debates, the murder of Jo Cox MP or the role of a declining industrial sector all warrant further exploration. Allcott and Gentzkow (2017) consider 'fake news' in the context of the 2016 US election. Here, there are obvious parallels to the EU referendum where fake news was ingrained in the campaign trail.

Chapter Appendices

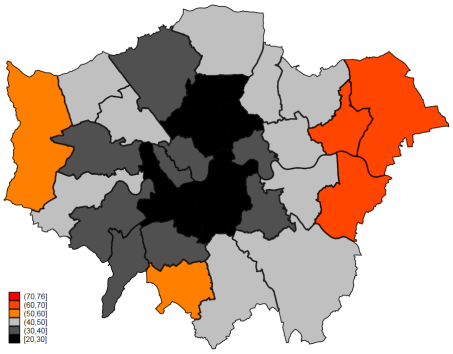
3.A Chapter 3 Figures

Figure 3.1: Spatial distribution of the Leave share (in %) across local authority districts in the 2016 EU referendum. England, Wales and Scotland.



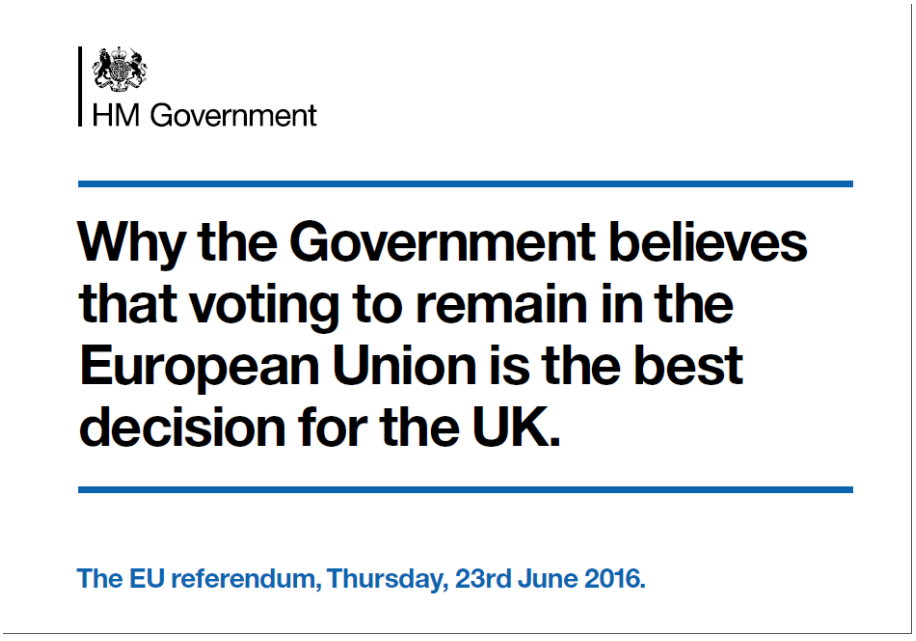
Notes: The map shows the vote leave share by each local authority district in England, Scotland and Wales. Redder areas are more leave, and blacker areas are more remain. Data are obtained from the Electoral Commission.

Figure 3.2: Spatial distribution of the Leave share (in %) across local authority districts in the 2016 EU referendum. London only.



Notes: The map shows the vote leave share by each local authority district in London only. Redder areas are more leave, and blacker areas are more remain. Data are obtained from the Electoral Commission.

Figure 3.3: Example front cover of the government's 2016 EU leaflet.


HM Government

Why the Government believes that voting to remain in the European Union is the best decision for the UK.

The EU referendum, Thursday, 23rd June 2016.

Figure 3.4: Example back page of the government's 2016 EU leaflet

HM Government

The Government believes that voting to remain in the European Union is the best decision for the UK.

☑ Protecting jobs ☑ A stronger economy ☑ Providing security

The EU referendum is a once in a generation decision. The Government believes it is in you and your family's best interests that the UK remains in the European Union.

Vote on Thursday, 23rd June 2016.

If you would like further information, please visit the Government's EU referendum website at EUReferendum.gov.uk

Figure 3.5: Example front and back page of the government's 1975 EU pamphlet

Figure 3.6: Spatial distribution of the share of exposed to total respondents per local authority district (as a %). England, Scotland and Wales.

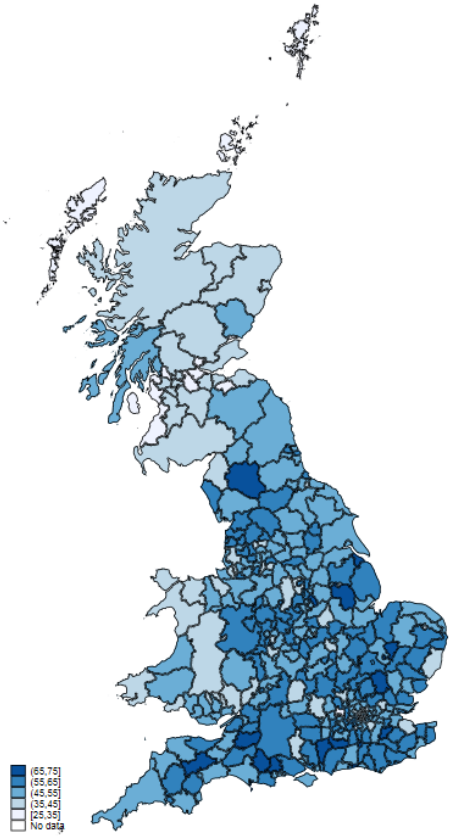


Figure 3.7: Spatial distribution of the share of exposed to total respondents per local authority district (as a %). London only.

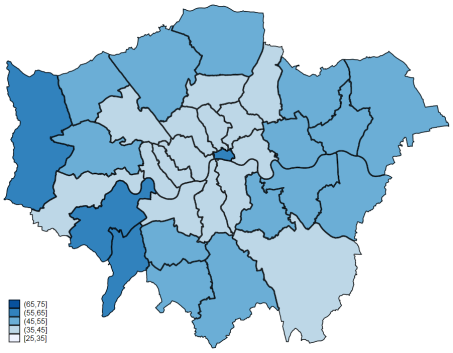


Figure 3.8: Average of polls through April 2016

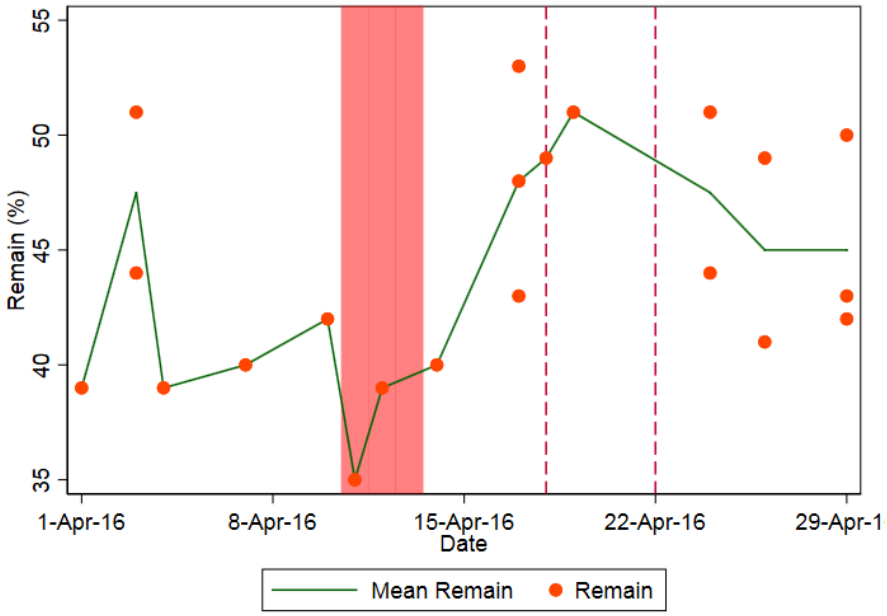


Figure 3.9: Spatial distribution of the low exposure sample, the number of respondents by local authority district. England, Scotland and Wales.

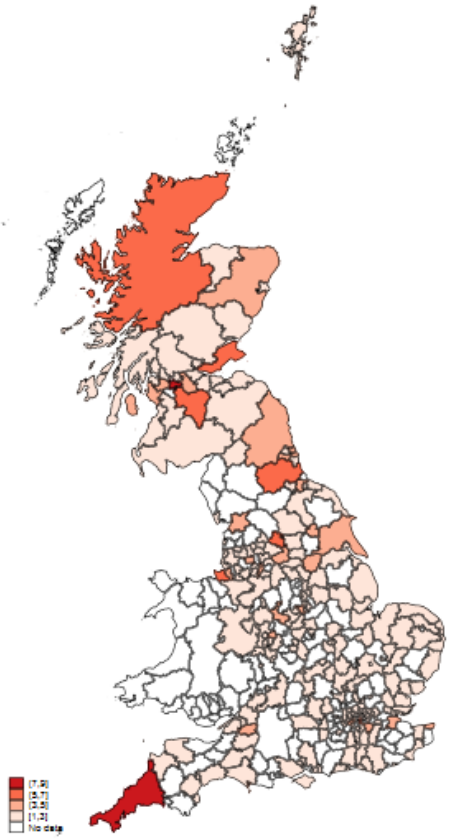


Figure 3.10: Spatial distribution of the low exposure sample, the number of respondents by local authority district. London only.

Figure 3.11: Bias before and after propensity score matching – Table 3.6 column (3).

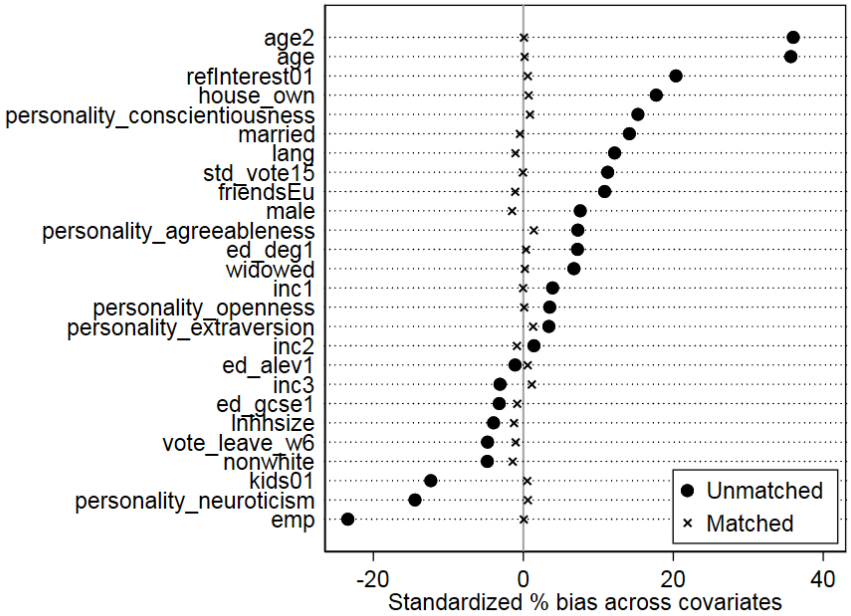
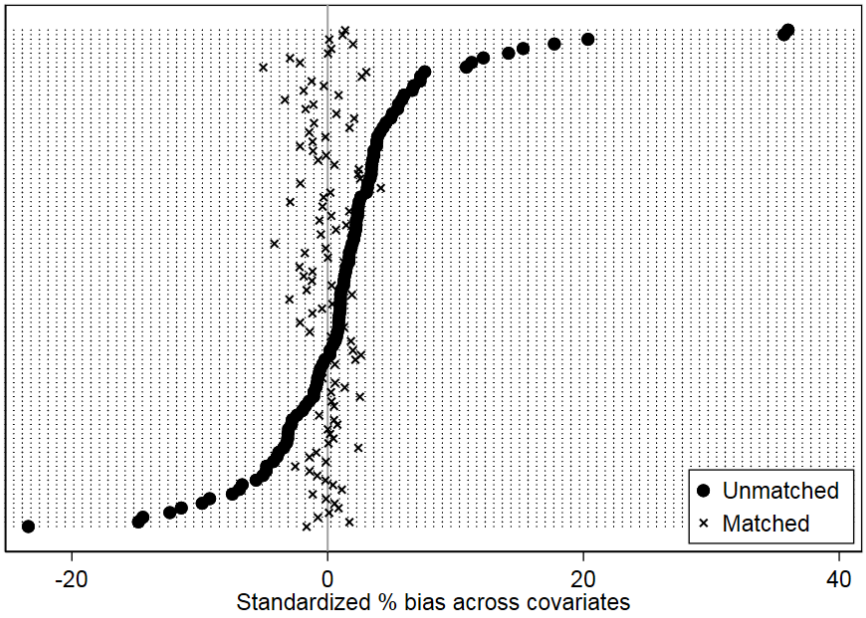


Figure 3.12: Bias before and after propensity score matching, with county and referendum proximity fixed effects – Table 3.6 column (6).



3.B Chapter 3 Tables

Table 3.1: 2016 Timeline

8th May 2015	BES wave 6 begins
26th May 2015	BES wave 6 ends
11-13th April	Government's EU leaflet is sent out to all UK households
14th April	BES wave 7 begins
4th May	BES wave 7 ends
5th May	Local elections
6th May	BES wave 8 begins
4th June	BES wave 9 begins
9th June	Postal vote registration ends
22nd June	BES wave 8 ends
23rd June	Referendum day
4th July	BES wave 9 ends

Table 3.2: Partisan ranking

Party name	% of negative EU mentions	Coding
United Kingdom Independence Party (UKIP)	16.53	8
Conservative Party	4.594	7
Liberal Democrats	0.782	6
Other	–	5
Green Party of England and Wales	0.403	4
Plaid Cymru (The Party of Wales)	0.387	3
Scottish Nationalist Party	0.112	2
Labour Party	0.099	1

Notes: The table shows the percent of the corresponding party's 2015 general election manifesto dedicated to negative mentions of the European Union, taken from the variable *per110* as coded in the Manifesto Project Database by Volkens et al. (2017). Other, refers to all other parties that were voted for in the 2015 general election by respondents.

Table 3.3: Summary statistics for selected variables

	Full sample		Exposed = 1		Exposed = 0	
	mean	sd	mean	sd	mean	sd
Vote leave	0.513	0.500	0.509	0.500	0.516	0.500
Male	0.506	0.500	0.525	0.499	0.485	0.500
Age	50.25	15.37	52.24	15.64	47.96	14.72
Age^2 / 100	27.61	15.27	29.74	15.74	25.16	14.32
Married	0.510	0.500	0.527	0.499	0.490	0.500
Widowed	0.035	0.183	0.043	0.203	0.025	0.157
Non-white	0.092	0.288	0.082	0.274	0.103	0.304
Employed	0.592	0.491	0.545	0.498	0.646	0.478
Kids01	0.253	0.435	0.216	0.412	0.295	0.456
Ln(household size)	0.793	0.496	0.774	0.487	0.814	0.505
Home owner	0.656	0.475	0.685	0.465	0.622	0.485
Degree – education	0.397	0.489	0.404	0.491	0.389	0.488
A-level – education	0.218	0.413	0.222	0.416	0.214	0.410
GCSE – education	0.309	0.462	0.300	0.458	0.319	0.466
None – education	0.077	0.266	0.075	0.263	0.079	0.269
Friends from the EU	0.463	0.499	0.487	0.500	0.434	0.496
Speaks another language	0.200	0.400	0.228	0.420	0.169	0.375
Partisan	-0.048	1.012	-0.009	1.005	-0.093	1.018
Household income (Over £70k)	0.070	0.255	0.072	0.259	0.066	0.249
Household income (£40-70k)	0.219	0.413	0.218	0.413	0.220	0.414
Household income (£20-40k)	0.393	0.488	0.387	0.487	0.400	0.464
Household income (£0-20k)	0.319	0.466	0.323	0.468	0.314	0.464
Agreeableness	6.095	1.801	6.142	1.806	6.040	1.794
Conscientiousness	6.771	1.832	6.899	1.821	6.623	1.834
Extraversion	4.076	2.176	4.091	2.202	4.058	2.146
Neuroticism	3.730	2.196	3.590	2.192	3.891	2.190
Openness	5.449	1.665	5.488	1.658	5.405	1.672
Observations	5,921		3,208		2,713	

Notes: Statistics are weighted by the BES core sample weight. *Exposed* refers to the Government EU leaflet dummy, which is an indicator variable that is equal to one if a person has received and read the governments leaflet backing a remain vote.

Table 3.4: Who voted for Brexit?

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Male	-0.021*	-0.010	-0.009	-0.012	-0.009	-0.013	-0.012
	(0.011)	(0.011)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)
Age	0.015***	0.016***	0.016***	0.015***	0.015***	0.015***	0.015***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Age^2 / 100	-0.012***	-0.012***	-0.013***	-0.012***	-0.012***	-0.011***	-0.012***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Married	-0.010	0.002	-0.000	0.001	-0.001	0.002	-0.001
	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)
Widowed	0.031	0.035	0.034	0.033	0.035	0.030	0.030
	(0.030)	(0.030)	(0.030)	(0.032)	(0.030)	(0.032)	(0.031)
Non-white	0.001	-0.004	-0.003	0.000	0.000	0.002	0.001
	(0.021)	(0.021)	(0.021)	(0.022)	(0.022)	(0.022)	(0.022)
Employed	-0.034**	-0.015	-0.015	-0.011	-0.014	-0.011	-0.014
	(0.014)	(0.014)	(0.014)	(0.015)	(0.014)	(0.015)	(0.014)
Kids01	0.011	0.001	-0.000	-0.000	-0.001	0.002	0.002
	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)
Ln(household size)	0.033**	0.059***	0.059***	0.053***	0.055***	0.051***	0.053***
	(0.017)	(0.017)	(0.017)	(0.018)	(0.017)	(0.018)	(0.017)
Home owner	-0.061***	-0.038***	-0.040***	-0.051***	-0.043***	-0.050***	-0.042***
	(0.014)	(0.014)	(0.014)	(0.015)	(0.014)	(0.015)	(0.014)
Degree – education	-0.291***	-0.261***	-0.257***	-0.248***	-0.253***	-0.248***	-0.252***
	(0.022)	(0.023)	(0.023)	(0.024)	(0.023)	(0.024)	(0.023)
A-level – education	-0.161***	-0.146***	-0.145***	-0.142***	-0.142***	-0.141***	-0.141***
	(0.023)	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)
GCSE – education	-0.037*	-0.029	-0.028	-0.031	-0.028	-0.028	-0.025
	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)
Friends from the EU	-0.055***	-0.046***	-0.045***	-0.040***	-0.040***	-0.038***	-0.039***
	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)
Speaks another language	-0.033**	-0.029**	-0.029**	-0.028*	-0.029**	-0.028*	-0.028*
	(0.014)	(0.014)	(0.014)	(0.015)	(0.014)	(0.015)	(0.014)
Partisan	0.174***	0.176***	0.175***	0.175***	0.174***	0.175***	0.174***
	(0.006)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Household income (Over £70k)		-0.153***	-0.160***	-0.140***	-0.142***	-0.143***	-0.144***
		(0.024)	(0.024)	(0.026)	(0.025)	(0.026)	(0.025)
Household income (£40-70k)		-0.108***	-0.111***	-0.098***	-0.101***	-0.099***	-0.101***
		(0.018)	(0.018)	(0.019)	(0.018)	(0.019)	(0.018)
Household income (£20-40k)		-0.051***	-0.053***	-0.049***	-0.048***	-0.050***	-0.049***
		(0.014)	(0.014)	(0.015)	(0.015)	(0.015)	(0.015)
Agreeableness			-0.003	-0.004	-0.003	-0.004	-0.002
			(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Conscientiousness			0.007**	0.007**	0.007**	0.008**	0.007**
			(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Extraversion			0.005**	0.005*	0.005**	0.005*	0.005**
			(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Neuroticism			0.000	0.000	0.001	0.000	0.000
			(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Openness			-0.005	-0.005	-0.005	-0.005	-0.005
			(0.003)	(0.003)	(0.003)	(0.004)	(0.003)
Constant	0.301***	0.287***	0.259***	0.178**	0.319***	0.152	0.289***
	(0.072)	(0.072)	(0.081)	(0.089)	(0.092)	(0.108)	(0.107)
Local authority FEs?	No	No	No	Yes	No	Yes	No
County FEs?	No	No	No	No	Yes	No	Yes
Ref. proximity FEs?	No	No	No	No	No	Yes	Yes
R-squared	0.249	0.255	0.257	0.309	0.267	0.314	0.273
Observations	6,132	6,132	6,132	6,132	6,132	6,132	6,132

Notes: Coefficients reported show the average marginal effect from probit regressions. Local authority districts are government areas at which the referendum count was reported. County areas are NUTS2 regions. Referendum proximity fixed effects are time fixed effects for the day of survey completion. The omitted category for income is households that earn less than £20k per year. The education variables refer to the respondents highest level of qualification, the omitted category is no formal qualifications. Robust standard errors are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 3.5: Individual-level analysis with local authority district controls

	(1)	(2)	(3)	(4)	(5)	(6)
Govt. EU leaflet (Exposed)	-0.025*	-0.023*	-0.027**	-0.026*	-0.027*	-0.026*
	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)
EU dependence						
Primary Industries	0.006*	0.004			-0.003	-0.001
	(0.003)	(0.003)			(0.005)	(0.005)
Manufacturing	0.005*	0.005*			0.009***	0.008**
	(0.003)	(0.003)			(0.004)	(0.004)
Construction	-0.019*	-0.026***			0.005	-0.004
	(0.010)	(0.010)			(0.012)	(0.012)
Services	-0.005	0.004			-0.025	-0.014
	(0.013)	(0.013)			(0.017)	(0.018)
LAD migrant share						
EU15	-1.396	-0.552	-1.047	-0.694	-1.348	-0.789
	(0.964)	(0.955)	(1.106)	(1.126)	(0.974)	(0.978)
EU12	-0.049	-0.272	0.311	-0.292	0.344	-0.034
	(0.672)	(0.670)	(0.726)	(0.752)	(0.681)	(0.691)
NON-EU	0.103	-0.068	-0.176	-0.279	-0.145	-0.272
	(0.179)	(0.184)	(0.227)	(0.230)	(0.198)	(0.202)
Other LAD controls						
Ln (Local authority cuts per capita)		0.158***		0.135***		0.138***
		(0.031)		(0.040)		(0.034)
Gross domestic household income (2015)	0.000	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Individual controls?	Yes	Yes	Yes	Yes	Yes	Yes
Ref. proximity FEs?	Yes	Yes	Yes	Yes	Yes	Yes
Local authority FEs?	No	No	No	No	No	No
County FEs?	No	No	Yes	Yes	No	No
GOR FEs?	No	No	No	No	Yes	Yes
Observations	5,957	5,957	5,957	5,957	5,957	5,957

Notes: Coefficients reported show the average marginal effect from probit regressions. Local authority districts are government areas at which the referendum count was reported. County areas are NUTS2 regions. Referendum proximity fixed effects are time fixed effects for the day of survey completion. GOR refers to the government office regions. The omitted category for income is households that earn less than £20k per year. The education variables refer to the respondents highest level of qualification, the omitted category is no formal qualifications. Robust standard errors are clustered by local authority and reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 3.6: The impact of the government's EU leaflet on voting behaviour

	OLS		Matching			
	(1)	(2)	(3)	(4)	(5)	(6)
Exposed	-0.038***	-0.048***	-0.030***	-0.024**	-0.029***	-0.039***
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
County FEs?	No	Yes	No	Yes	No	Yes
Ref. proximity FEs?	No	Yes	No	No	Yes	Yes
Treated/control	3,408/2,724	3,408/2,724	3,408/2,724	3,408/2,724	3,408/2,724	3,408/2,724
Observations	6,132	6,132	6,132	6,132	6,132	6,132

Notes: Exposed is the Government EU leaflet dummy, which is an indicator variable that is equal to one if a person has received and read the governments leaflet backing a remain vote. The dependent variable is a dummy for whether an individual will vote to leave the EU. The set of matching variables, with replacement, in all columns includes male, age, age^2 / 100, married, widowed, non-white, employed, kids01, ln(household size), home owner, degree, a-level, gcse, friends from the EU, speaks another language, partisan, 3 categories of household income, agreeableness, conscientiousness, extroversion, neuroticism, openness, pre-treatment voting intention and a dummy for interest in the referendum. Standard errors are Abadie-Imbens robust standard errors (Abadie and Imbens, 2008, 2016). Columns (1) and (2) report estimates from OLS regressions, variables omitted are the full set of controls, as well as country-level and proximity to referendum date fixed effects in column (2). Columns (3)-(6) report the average treatment effect on the treated using propensity score matching, with 5 nearest-neighbours. In column (3) there is no fixed effects. Column (4) and (5) includes county-level and referendum proximity fixed effects, respectively. In column (6) the specification includes both county-level and proximity to referendum fixed effects. The propensity scores are obtained from a probit regression. Robust standard errors are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 3.7: Covariate balancedness

		Before matching		After matching	
	Treatment group mean	Control group mean	p-value difference in means	Control group means	p-value difference in means
Matching Covariates					
Male	0.540	0.503	0.001	0.549	0.427
Age	58.12	53.26	0.000	58.10	0.940
Age^2 / 100	35.71	30.58	0.000	35.70	0.975
Married	0.584	0.518	0.000	0.579	0.674
Widowed	0.049	0.038	0.019	0.047	0.638
Non-white	0.069	0.081	0.035	0.072	0.530
Employed	0.435	0.550	0.000	0.432	0.772
Kids01	0.167	0.218	0.000	0.171	0.644
Ln(household size)	0.723	0.753	0.004	0.725	0.858
Home owner	0.762	0.681	0.000	0.757	0.638
Degree – education	0.459	0.428	0.006	0.468	0.391
A-level – education	0.197	0.199	0.743	0.187	0.236
GCSE – education	0.260	0.272	0.243	0.259	0.934
Friends from the EU	0.524	0.480	0.000	0.530	0.554
Speaks another language	0.251	0.213	0.000	0.259	0.410
Partisan	0.008	-0.100	0.000	0.003	0.809
Household income (Over £70k)	0.066	0.058	0.131	0.066	0.966
Household income (£40-70k)	0.171	0.167	0.577	0.165	0.447
Household income (£20-40k)	0.290	0.301	0.271	0.282	0.418
Agreeableness	6.191	6.060	0.001	6.158	0.383
Conscientiousness	6.994	6.724	0.000	6.983	0.780
Extraversion	4.150	4.033	0.018	4.124	0.577
Neuroticism	3.467	3.816	0.000	3.455	0.798
Openness	5.535	5.488	0.231	5.542	0.851
Referendum interest	0.968	0.922	0.000	0.968	0.990
Vote $leave_{t-1}$	0.434	0.451	0.126	0.439	0.644
Observations	3,408	2,724		2,724	

Table 3.8: Summary statistics for matching variables in the low exposure sample

	Full sample		Exposed = 1		Exposed = 0	
	mean	sd	mean	sd	mean	sd
Vote leave	0.526	0.500	0.509	0.501	0.535	0.499
Male	0.413	0.493	0.434	0.497	0.403	0.491
Age	50.897	14.682	52.728	14.385	49.965	14.765
Age^2 / 100	28.056	14.812	29.860	15.034	27.138	14.635
Married	0.470	0.500	0.480	0.501	0.465	0.499
Widowed	0.043	0.203	0.052	0.223	0.038	0.192
Non-white	0.074	0.262	0.069	0.255	0.076	0.266
Employed	0.579	0.494	0.555	0.498	0.591	0.492
Kids01	0.275	0.447	0.266	0.443	0.279	0.449
Ln(household size)	0.745	0.500	0.742	0.488	0.747	0.508
Home owner	0.614	0.487	0.659	0.475	0.591	0.492
Degree – education	0.292	0.455	0.306	0.462	0.285	0.452
A-level – education	0.230	0.421	0.225	0.419	0.232	0.423
GCSE – education	0.339	0.474	0.329	0.471	0.344	0.476
None – education	0.138	0.346	0.139	0.347	0.138	0.346
Friends from the EU	0.349	0.477	0.422	0.495	0.312	0.464
Speaks another language	0.127	0.333	0.127	0.334	0.126	0.333
Partisan	-0.118	1.033	-0.065	1.034	-0.145	1.032
Household income (Over £70k)	0.053	0.224	0.029	0.168	0.065	0.246
Household income (£40-70k)	0.164	0.370	0.173	0.380	0.159	0.366
Household income (£20-40k)	0.341	0.475	0.329	0.471	0.347	0.477
Household income (£0-20k)	0.442	0.497	0.468	0.500	0.429	0.496
Agreeableness	6.150	1.812	6.064	1.709	6.194	1.863
Conscientiousness	6.741	1.819	6.832	1.795	6.694	1.832
Extraversion	4.117	2.129	4.121	2.189	4.115	2.101
Neuroticism	3.862	2.170	3.746	2.168	3.921	2.172
Openness	5.327	1.678	5.306	1.594	5.338	1.721
Referendum interest	0.823	0.382	0.879	0.328	0.794	0.405
Vote $leave_{t-1}$	0.489	0.500	0.486	0.501	0.491	0.501
Observations	513		173		340	

Table 3.9: Low exposure sample and heterogenous effects of the government's EU leaflet on voting behaviour

		Gender		Income group		Risk aversion		Partisan		
	Full isolated	Female	Male	£0-20k	> £20k	Avoider	Taker	Con	Lab	Wave 6 Leavers
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Exposed	-0.037	-0.088***	0.072	-0.114***	0.052	-0.102***	0.063	0.005	-0.033	-0.093***
	(0.038)	(0.030)	(0.047)	(0.036)	(0.046)	(0.029)	(0.058)	(0.155)	(0.064)	(0.030)
Treated/control	173/340	98/203	75/137	81/146	92/194	94/183	79/157	42/58	60/94	84/167
Observations	513	301	212	227	286	277	236	100	154	251

Notes: Exposed is the Government EU leaflet dummy, which is an indicator variable that is equal to one if a person has received and read the governments leaflet backing a remain vote. The dependent variable is a dummy for whether an individual will vote to leave the EU. The set of matching variables, with replacement, in all columns includes male, age, age^2 / 100, married, widowed, non-white, employed, kids01, ln(household size), home owner, degree, a-level, gcse, friends from the EU, speaks another language, partisan, 3 categories of household income, agreeableness, conscientiousness, extroversion, neuroticism, openness, pre-treatment voting intention (excluded in column (11)) and a dummy for interest in the referendum. Standard errors are Abadie-Imbens robust standard errors (Abadie and Imbens, 2008, 2016). Columns (1)-(10) report the average treatment effect on the treated using propensity score matching, with 5 nearest-neighbours. The propensity scores are obtained from a probit regression. Robust standard errors are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 3.10: Placebo analysis

	Con 2010 (1)	Con 2005 (2)	Lab 2005 (3)	Con local (4)	Like Cameron [0-10] (5)
Exposed	-0.023	0.041	0.045	-0.005	0.278
	(0.039)	(0.039)	(0.048)	(0.052)	(0.297)
Treated/control	168/326	157/291	157/291	75/115	164/316
Observations	494	448	448	190	480

Notes:Exposed is the Government EU leaflet dummy, which is an indicator variable that is equal to one if a person has received and read the governments leaflet backing a remain vote. The set of matching variables, with replacement, in all columns includes male, age, age^2 / 100, married, widowed, non-white, employed, kids01, ln(household size), home owner, degree, a-level, gcse, friends from the EU, speaks another language, partisan, 3 categories of household income, agreeableness, conscientiousness, extroversion, neuroticism, openness, pre-treatment voting intention and a dummy for interest in the referendum. Standard errors are Abadie-Imbens robust standard errors (Abadie and Imbens, 2008, 2016). Columns (1)-(5) report the average treatment effect on the treated using propensity score matching, with 5 nearest-neighbours. The propensity scores are obtained from a probit regression. Robust standard errors are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 3.11: DiD estimates of the effect of the government's EU leaflet on voting behaviour

	Fixed effects			Balanced sample LPM		
	Full isolated (1)	Female (2)	Risk avoider (3)	Full isolated (4)	Female (5)	Risk avoider (6)
Post × Exposed	-0.036	-0.111**	-0.096*	-0.071	-0.132**	-0.148**
	(0.040)	(0.055)	(0.051)	(0.061)	(0.064)	(0.073)
R^2	0.016	0.036	0.054	0.672	0.812	0.821
Observations	860	502	472	668	386	368

Notes: Exposed is the Government EU leaflet dummy, which is an indicator variable that is equal to one if a person has received and read the governments leaflet backing a remain vote. All regressions include the full set of controls from Table 3.4 column (3). As well as these controls, regional dummies are included in columns (4) - (6). The dependent variable is a dummy for whether an individual will vote to leave the EU. Robust standard errors, clustered at the individual-level, are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 3.12: High exposure sample and heterogenous effects of the government's EU leaflet on voting behaviour

		Gender		Income group		Risk aversion		Partisan		
	Full inverse	Female	Male	£0-20k	> £20k	Avoider	Taker	Con	Lab	Wave 6 Leavers
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Exposed	-0.018	0.006	-0.059***	-0.000	-0.027**	-0.024	-0.010	-0.062***	0.003	-0.022**
	(0.011)	(0.017)	(0.015)	(0.019)	(0.013)	(0.015)	(0.015)	(0.023)	(0.021)	(0.011)
Treated/control	3,235/2,384	1,417/1,111	1,818/1,273	1,023/704	2,212/1,644	1,362/1,022	1,873/1,362	951/560	1,051/731	1,348/1,042
Observations	5,619	2,528	3,091	1,763	3,856	2,384	3,235	1,511	1,782	2,390

Notes:Exposed is the Government EU leaflet dummy, which is an indicator variable that is equal to one if a person has received and read the governments leaflet backing a remain vote. The dependent variable is a dummy for whether an individual will vote to leave the EU. The set of matching variables, with replacement, in all columns includes male, age, age^2 / 100, married, widowed, non-white, employed, kids01, ln(household size), home owner, degree, a-level, gcse, friends from the EU, speaks another language, partisan, 3 categories of household income, agreeableness, conscientiousness, extroversion, neuroticism, openness, pre-treatment voting intention (excluded in column (11)) and a dummy for interest in the referendum. Standard errors are Abadie-Imbens robust standard errors (Abadie and Imbens, 2008, 2016). Columns (1)-(10) report the average treatment effect on the treated using propensity score matching, with 5 nearest-neighbours. The propensity scores are obtained from a probit regression. Robust standard errors are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 3.13: Exposure to the government's leaflet and opinions of a leave vote outcome

	Better UK economy (1)	Higher unemployment (2)	Higher risk of terror (3)	Referendum is final (4)	Better personal financial situation (5)	UK workers better off (6)	Higher UK world influence (7)
Exposed	-0.046***	0.069***	0.056***	0.042***	0.013	-0.026**	-0.009
	(0.012)	(0.013)	(0.012)	(0.015)	(0.010)	(0.011)	(0.011)
Observations	5,717	5,709	5,647	5,146	4,439	5,589	5,743

*Notes:*Coefficients reported show the average marginal effect from probit regressions. *Exposed* is the Government EU leaflet dummy, which is an indicator variable that is equal to one if a person has received and read the governments leaflet backing a remain vote. All regressions include the full set of controls from Table 3.4 column (3) as well as local authority and proximity to referendum date fixed effects. The dependent variable in columns (1)-(7) is a dummy for whether the respondent thinks that if the UK were to leave the EU: the UK economy will generally be better off, there will be higher unemployment, the UK will be at a higher risk of terror, whether they believe the referendum is final, the respondents personal financial situation will generally be better off, UK workers will be better off and the UK's voice in the world would be larger. Robust standard errors are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 3.14: Placebo tests and alternate mechanisms of exposure to the government's EU leaflet

	More int. trade (1)	EU better if Turkey joined (2)	EU undermined UK sovereignty (3)	Immigration is increasing (4)	Generally trusting (5)	Like Boris Johnson [0-10] (6)	Like Michael Gove [0-10] (7)	Like Nigel Farage [0-10] (8)
Exposed	-0.003	0.019*	-0.016	-0.009	-0.004	-0.086	0.093	-0.035
	(0.012)	(0.011)	(0.012)	(0.013)	(0.028)	(0.084)	(0.082)	(0.085)
Observations	5,695	2,423	5,871	5,846	1,420	6,097	5,495	6,101

Notes: Coefficients reported show the average marginal effect from probit regressions, except for column (6), (7) and (8), which are obtained from OLS regressions. *Exposed* is the Government EU leaflet dummy, which is an indicator variable that is equal to one if a person has received and read the governments leaflet backing a remain vote. All regressions include the full set of controls from Table 3.4 column (3) as well as local authority and proximity to referendum date fixed effects. The dependent variable in all columns is a dummy for whether the respondent believes the following: in column (1) it is whether a respondent believes that there will be more international trade if the UK votes to leave. In column (2) it is whether the respondent believes the EU would be better off if Turkey were to join and (3) whether the EU has undermined UK sovereignty. In column (4) whether they believe that the level of immigration is increasing and in column (5) the dependent variable is whether the respondent is generally trusting. And in columns (6), (7) and (8) respondents rank on a scale of 0-10, where 10 is the most favourable view, how much they like Boris Johnson, Michael Gove and Nigel Farage, respectively. Robust standard errors are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 3.15: Propensity score estimation

	(1)	(2)	(3)	(4)
Male	0.078**	0.091***	0.076**	0.089**
	(0.035)	(0.035)	(0.035)	(0.036)
Age	0.011	0.011	0.010	0.010
	(0.008)	(0.008)	(0.008)	(0.008)
Age^2 / 100	0.002	0.002	0.003	0.003
	(0.008)	(0.008)	(0.008)	(0.008)
Married	0.028	0.036	0.021	0.030
	(0.043)	(0.044)	(0.043)	(0.044)
Widowed	0.055	0.068	0.043	0.056
	(0.088)	(0.089)	(0.089)	(0.089)
Non-white	-0.072	-0.059	-0.064	-0.052
	(0.065)	(0.066)	(0.066)	(0.067)
Employed	-0.151***	-0.157***	-0.158***	-0.163***
	(0.042)	(0.042)	(0.042)	(0.042)
Kids01	-0.062	-0.074	-0.053	-0.066
	(0.052)	(0.052)	(0.052)	(0.053)
Ln(household size)	0.086*	0.069	0.083*	0.066
	(0.050)	(0.050)	(0.050)	(0.051)
Home owner	0.080*	0.085**	0.092**	0.098**
	(0.042)	(0.043)	(0.043)	(0.043)
Degree – education	0.288***	0.306***	0.279***	0.298***
	(0.067)	(0.068)	(0.068)	(0.068)
A-level – education	0.236***	0.258***	0.237***	0.260***
	(0.069)	(0.070)	(0.070)	(0.071)
GCSE – education	0.171***	0.172***	0.172***	0.175***
	(0.065)	(0.065)	(0.065)	(0.066)
Friends from the EU	0.086**	0.089**	0.089**	0.091**
	(0.035)	(0.036)	(0.036)	(0.036)
Speaks another language	0.078*	0.077*	0.081*	0.079*
	(0.043)	(0.043)	(0.043)	(0.044)
Partisan	0.069***	0.032*	0.075***	0.037*
	(0.018)	(0.019)	(0.018)	(0.019)
Household income (Over £70k)	-0.031	-0.011	-0.035	-0.017
	(0.074)	(0.076)	(0.075)	(0.077)
Household income (£40-70k)	-0.032	-0.017	-0.032	-0.017
	(0.054)	(0.055)	(0.055)	(0.056)
Household income (£20-40k)	-0.072*	-0.063	-0.079*	-0.070
	(0.043)	(0.044)	(0.043)	(0.044)
Agreeableness	0.004	0.003	0.006	0.005
	(0.010)	(0.010)	(0.010)	(0.010)
Conscientiousness	0.031***	0.028***	0.029***	0.027***
	(0.010)	(0.010)	(0.010)	(0.010)
Extraversion	0.002	0.002	0.002	0.002
	(0.008)	(0.008)	(0.008)	(0.008)
Neuroticism	-0.011	-0.011	-0.011	-0.011
	(0.008)	(0.008)	(0.008)	(0.009)
Openness	0.009	0.010	0.008	0.009
	(0.010)	(0.010)	(0.010)	(0.010)
Referendum interest	0.503***	0.490***	0.501***	0.487***
	(0.076)	(0.076)	(0.076)	(0.077)
Vote $Leave_{t-1}$	-0.144***	-0.157***	-0.155***	-0.170***
	(0.039)	(0.039)	(0.039)	(0.040)
County FEs?	No	Yes	No	Yes
Ref. proximity FEs?	No	No	Yes	Yes
Observations	6,132	6,132	6,132	6,132

Notes: Coefficients reported show the average marginal effect from a probit regression. *Exposed* is the Government EU leaflet dummy, which is an indicator variable that is equal to one if a person has received and read the governments leaflet backing a remain vote. Columns (1) - (4) correspond to Table 3.6 columns (3) - (6).

Table 3.16: Alternate matching strategies

	Epanechnikov kernel		Propensity score	
	50 reps	150 reps	4 neighbours	3 neighbours
	(1)	(2)	(3)	(4)
Exposed	-0.029**	-0.029**	-0.031***	-0.025**
	(0.014)	(0.013)	(0.011)	(0.012)
Treated/control	3,408/2,724	3,408/2,724	3,408/2,724	3,408/2,724
Observations	6,132	6,132	6,132	6,132

Notes: *Exposed* is the Government EU leaflet dummy, which is an indicator variable that is equal to one if a person has received and read the governments leaflet backing a remain vote. The set of matching variables, with replacement, in all columns includes male, age, age^2 / 100, married, widowed, non-white, employed, kids01, ln(household size), home owner, degree, a-level, gcse, friends from the EU, speaks another language, partisan, 3 categories of household income, agreeableness, conscientiousness, extroversion, neuroticism, openness, pre-treatment voting intention and a dummy for interest in the referendum. In column (1) and (2) standard errors are bootstrapped using 50 and 150 replications, respectively. Columns (1) and (2) report the average treatment effect on the treated using Epanechnikov kernal matching. In column (3) and (4) standard errors are Abadie-Imbens robust standard errors (Abadie and Imbens, 2008, 2016). Columns (3) and (4) report the average treatment effect on the treated using propensity score matching, with 4 and 3 nearest neighbours, respectively. The propensity scores are obtained from a probit regression. Robust standard errors are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 3.17: Full sample matching with alternate matching covariates

	(1)	(2)	(3)	(4)
Panel A				
Exposed	-0.023**	-0.027**	-0.021*	-0.031***
	(0.010)	(0.011)	(0.011)	(0.011)
Panel B				
Exposed	-0.023**	-0.034***	-0.027**	-0.034***
	(0.011)	(0.011)	(0.011)	(0.011)
Panel C				
Exposed	-0.022*	-0.028**	-0.021*	-0.039***
	(0.011)	(0.011)	(0.011)	(0.011)
County FEs?	No	Yes	No	Yes
Ref. proximity FEs?	No	No	Yes	Yes
Treated/control	3,408/2,724	3,408/2,724	3,408/2,724	3,408/2,724
Observations	6,132	6,132	6,132	6,132

Notes: *Exposed* is the Government EU leaflet dummy, which is an indicator variable that is equal to one if a person has received and read the governments leaflet backing a remain vote. The dependent variable is a dummy for whether an individual will vote to leave the EU. Standard errors are Abadie-Imbens robust standard errors (Abadie and Imbens, 2008, 2016). Columns (1)-(4) report the average treatment effect on the treated using propensity score matching, with 5 nearest-neighbours. The propensity scores are obtained from a probit regression. The set of matching variables, with replacement, in Panel A is the same as those in Equation 3.2, but exclude the big 5 personality traits. In Panel B, the matching covariates are the same as Panel A, but exclude the big 5 personality traits and whether the respondents has friends from the EU and whether they speak another language. In Panel C, the matching covariates are the same as Panel A, but exclude income dummies. Robust standard errors are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 3.18: Low exposure sample matching with alternate matching covariates

		Gender		Income group		Risk aversion		
	Full isolated	Female	Male	£0-20k	> £20k	Avoider	Taker	Wave 6 Leavers
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A								
Exposed	-0.051	-0.102**	0.032	-0.049	0.024	-0.160***	0.051	-0.093**
	(0.042)	(0.042)	(0.094)	(0.102)	(0.050)	(0.038)	(0.042)	(0.043)
Panel B								
Exposed	-0.068	-0.069*	0.027	-0.081***	0.017	-0.119***	0.084	-0.093**
	(0.042)	(0.036)	(0.051)	(0.023)	(0.050)	(0.036)	(0.084)	(0.043)
Panel C								
Exposed	-0.020	-0.088**	0.008	-0.084***	0.041	-0.109***	0.043	-0.088**
	(0.041)	(0.043)	(0.105)	(0.029)	(0.043)	(0.037)	(0.067)	(0.039)
Panel D								
Exposed	-0.018	-0.063	-0.013	-0.128*	0.030	-0.087***	0.030	-0.090**
	(0.039)	(0.045)	(0.028)	(0.078)	(0.042)	(0.020)	(0.066)	(0.046)
GOR FEs?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Treated/control	173/340	98/203	75/137	81/146	92/194	94/183	79/157	84/167
Observations	513	301	212	227	286	277	236	251

Notes: Exposed is the Government EU leaflet dummy, which is an indicator variable that is equal to one if a person has received and read the governments leaflet backing a remain vote. The dependent variable is a dummy for whether an individual will vote to leave the EU. Standard errors are Abadie-Imbens robust standard errors (Abadie and Imbens, 2008, 2016). Columns (1)-(8) report the average treatment effect on the treated using propensity score matching, with 5 nearest-neighbours. The propensity scores are obtained from a probit regression. The set of matching variables, with replacement, in Panel A is the same as those in Equation 3.2 as well as government office region fixed effects. In Panel B, the matching covariates are the same again, but exclude the big 5 personality traits and includes government office region fixed effects. In Panel C, the matching covariates are the same as Panel A, but exclude the big 5 personality traits and whether the respondents speaks another language or has friends the EU, and includes government office region fixed effects. In Panel D, the matching covariates are the same as Panel A, but now exclude income dummies and include government office region fixed effects. Robust standard errors are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 3.19: Matching with calipers

	(1)	(2)
Panel A		
Exposed	-0.027*	-0.041***
	(0.015)	(0.015)
Treated/control	3,393/2,724	3,401/2,724
Observations	6,117	6,125
Panel B		
Exposed	-0.026*	-0.041***
	(0.015)	(0.015)
Treated/control	3,363/2,724	3,399/2,724
Observations	6,087	6,123
County FEs?	No	Yes
Ref. proximity FEs?	No	Yes

Notes: Exposed is the Government EU leaflet dummy, which is an indicator variable that is equal to one if a person has received and read the governments leaflet backing a remain vote. The dependent variable is a dummy for whether an individual will vote to leave the EU. Columns (1) and (2) report the average treatment effect on the treated using propensity score matching, with 5 nearest-neighbours. The propensity scores are obtained from a probit regression. The set of matching variables, with replacement, in all Panels, is the same as those in Equation 3.2. Panel A finds matches using a caliper of 0.005 and Panel B finds matches using a caliper of 0.001. Robust standard errors are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 3.20: Matching with countries excluded

	(1)	(2)	(3)
Exposed	-0.028**	-0.026**	-0.029**
	(0.012)	(0.011)	(0.012)
Country dropped	Scotland	Wales	Both
Treated/control	2,880/2,069	3,397/2,719	2,869/2,064
Observations	4,949	6,116	4,933

Notes: ***Exposed*** is the Government EU leaflet dummy, which is an indicator variable that is equal to one if a person has received and read the governments leaflet backing a remain vote. The dependent variable is a dummy for whether an individual will vote to leave the EU. Standard errors are Abadie-Imbens robust standard errors (Abadie and Imbens, 2008, 2016). Columns (1)-(3) report the average treatment effect on the treated using propensity score matching, with 5 nearest-neighbours. The propensity scores are obtained from a probit regression. The set of matching variables, with replacement, in Panel A is the same as those in Equation 3.2. Column (1) drops individuals from Scotland, (2) removes individuals from Wales, and (3) removes individuals from both Scotland and Wales. Robust standard errors are reported in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Chapter 4 Partisan alignment, elections and experienced politicians

This final empirical chapter is prompted by the final one of the three major political economy events, specifically the November 2016 US presidential election. Previously, I explored what factors aggravate or mitigate a populist outcome (in the form of Brexit), this chapter however takes its cues from the election of a populist president. This chapter therefore asks what a US president can do to consolidate his, and his party's, political position. This chapter employs federal fiscal transfer data at the state-level and the national and state electoral calendars to assess how a president may favour his co-partisans.

4.1 Introduction

The allocation of funds for political gain has been studied to a wide extent in the existing literature (Cox and McCubbins, 1986, Dixit and Londregan, 1996, Ansolabehere and Snyder, 2006). This strategic allocation causes distributive welfare to be skewed toward co-partisans at lower tiers of government (see, e.g., Larcinese et al. 2006, Baskaran and Hessami 2017). This naturally occurs as politicians seek to favour their own party rather than support competing factions. The degree to which this favouring occurs is deeply contextual, for instance, re-election incentives causes further malapportionment (Veiga and Pinho, 2007, Veiga and Veiga, 2013, Rumi, 2014, Fouirnaies and Mutlu-Eren, 2015).

Favouring co-partisans has had a profound effect upon policy decisions and has seen political parties grow into "brands". Before the establishment of formal left-right parties during the French revolution, crude forms of partisanship date back at least two thousand years. During the late Roman Republic, circa 133 - 27 B.C., two political groups dominated – the *Optimates* and the *Populares*. Both were made up of wealthy individuals that ran politics, although they took polar stances on policy. The latter were the more left-wing grouping, which typically favoured the cause of the plebeians and policies such as inequality reform. Conversely, the former represented the more conservative arm of Roman politics at the time. They wished to extended the power of the Senate – which they controlled, further the interest of aristocrats and opposed generals rising through the course of office, the *cursus honorum*. An example of this type of partisan politics comes from Julius Caesar, a decorated general and *Populare*. Once a *Consul*, the highest elected office, Caesar introduced a bill to the Senate to redistribute land to military veterans -eventual *Populares* themselves- and the urban poor. However, this was essentially filibustered and then abstained by the *Optimates*, which forced the bill to fail (Kuiper, 2010). Naturally, comparisons are often drawn to modern day political theatre, in particular US politics. The *Optimates* are akin to the Republicans and Democrats to the *Populares* in regards to motivation, support and ideological leaning.

With respect to this notion of partisanship, this chapter looks at how a politician, at a higher tier of government, can support another politician at a lower tier, who is affiliated along party lines. Using evidence from the US, this chapter contributes to this literature by providing empirical evidence on the allocation of the federal budget -federal intergovernmental transfers- toward co-partisans of the president around election times. The alignment effect is identified by exploiting the quasi-random nature of alignment in the US. Moreover, when interpreting the alignment effect, it is critical to ensure this acts separate to that of, and not through, the effect of merely favouring loyal supporting areas (Migueis, 2013), which most prior empirical work has failed to do. In the existing literature, alignment and federal

transfers have been considered to an extent, but they have not been analysed in terms of a electoral cycle framework in the case of the US (Larcinese et al.; 2006; Ansolabehere and Snyder; 2006). Additionally, I can further support an electorally motivated channel by making a distinction between swing areas, which should be favoured disproportionately more due to the paramount importance of winning these state's electoral college votes on the way to presidency.

Moreover, a nascent area of the literature has begun to look at the impact of the political figurehead's characteristics on economic outcomes. Some first insights have been provided for education (Besley et al., 2011); career (Dreher et al., 2009) and gender (Brollo and Troiano, 2016). In this chapter, I contribute new understanding on the impact of specific prior political experience to appropriate and lobby for a larger share of federal money. I also allow for the transmission of this effect to act through co-partisan channels.

This chapter also has important contributions regarding ideology on the left-right nexus. As I use the case of the US, I am able to distinguish between left (Democrat) and right (Republican) alignment. By splitting alignment in this way I am able to discern which party is driving the results and allows us to make inferences about ideological aspects.

In short, my results can be summarised as follows. For my core analysis I find that presidents subvert federal revenue towards states that are governed by their co-partisans – this effect is robust to various specifications and controls, in particular in the inclusion of state-specific time trends. Here I test for possible threats to identification, namely a violation of the common trends assumption between treated and untreated states. I find no such evidence of problematic pre- or post-treatment trends when adding lead and lag variables. When I examine if this re-allocation is being driven by the desire of the president to ensure re-election of his party at the national level, I find that this is indeed the case. Aligned governors in election years receive more, there is also evidence that this occurs in pre-election years as well. The

re-election of the president's party narrative is furthered by analysing the allocation to co-partisans in swing states, in which I find a significantly larger effect. I argue that the underlying mechanism to redistribute funds downward to co-partisans is due to performance spill-overs across the different tiers of government, which the president desires to benefit from. I test whether the prior political career of governors induces a larger influx of funds to their states and find that this happens irrespective of alignment. This can be explained by the lobbying efforts of governors with more political nous rather than more experienced governors producing larger spill-over effects for the president's party. By splitting the alignment variable by the two major parties I show that Republican presidents favour their co-partisans governors more than Democrats. However, in election times both parties redistribute to their political allies in similar magnitudes. I conduct some falsification tests to confirm a non-spurious identification of the relationship. As a final element, I assess the sensitivity to selection-on-unobservables, which would have to be 7 times as large as the selection-on-observables to attenuate the relationship to 0 (Altonji et al., 2005).

The rest of the chapter is organised as follows. Section 4.2 discusses the relevant literature and why co-partisans should be favoured. Section 4.3 presents the institutional framework in the US, in particular the power of the president in allocating the federal budget and the interactions with the state governors. Section 4.4 presents the data and methodology. Section 4.5 presents the main results and the proceeding section provides explanations of the driving force behind the results. Section 4.6 conducts some tests on the validity of my results and section 4.7 offers some extensions to the results. Finally, section 4.8 concludes.

4.2 The literature

4.2.1 Related literature

The related literature on the tactical allocation of the federal budget offers an alternative explanation to the role of the government – one that differs from the typical social welfare maximising role. Electorally and non-electorally motivated bias to skew the allocation of intergovernmental grants in favour of co-partisans has been demonstrated with varying degrees of success in a wide variety of institutional settings and cultural contexts. The assorted literature is introduced below.

The manipulation of funds can be traced back to a central leader's desire to support their sub-national co-partisans, this mechanism is discussed in detail in the next section. A core piece of literature in relation to this work is by Larcinese et al. (2006), who examines federal spending in US states from 1982 to 2000. They find that when the governors and president are of the same party –politically affiliated– those aligned states are subject to more federal expenditure. As noted by Migueis (2013), that paper does not control for the vote share, or margin of victory, awarded to the parties, therefore it is not possible to distinguish between the partisan hypothesis –more votes means more transfers– or a credible political alignment affect. It is also worth noting that Larcinese et al. (2006) does not consider intergovernmental transfers, only total federal expenditure. In conjunction with the political alignment impact, Ansolabehere and Snyder (2006) show that more grants are awarded from state governments to counties with strong support. That is, unified democratic states award more to democratic counties. Essentially this is an empirical test of the theoretical loyal voter model of Cox and McCubbins (1986). That if politicians are risk averse, loyal voting areas will be rewarded with more funds than marginal areas and marginals will get more than opposition areas. A positive impact between alignment and transfers is also found in Spain at multiple levels of governance by

Solé-Ollé and Sorribas-Navarro (2008).

Within the sphere of the political economy of intergovernmental transfers looms large the shadow of elections. Veiga and Pinho (2007) assess how transfers are manipulated in municipal and legislative elections years in Portugal from 1979 to 2002 – whilst Portugal was a developing democracy. They find that the Portuguese central government indiscriminately redistributes grants to areas with aligned and non-aligned mayors in election years. However, no evidence is found for an individual alignment effect. The size of the manipulations are also decreasing as democracy becomes more established. Migueis (2013) uses again the case of Portugal to show that alignment does indeed matter, whereas an increase in the vote share for the incumbent does not significantly affect transfers beyond the impact of alignment. Thus confirming that the bias is driven by alignment and not based on the vote share. In an emerging democracy setting, Rumi (2014) assess electoral cycles for federal grants in Argentina. Distinguishing between cash and in-kind transfers, they find that more total transfers are awarded to politically aligned provinces (run by a mayor) in the pre-presidential election year.

In a similarly developing democracy setting, Brollo and Nannicini (2012) use data from Brazilian intergovernmental transfers in a regression discontinuity design. They present evidence that an affiliated mayor of a municipality receives more infrastructure grants in pre-election years. This effect is driven by the fact that opposition party mayors who won election by a narrow margin are severely penalised. Moreover, Brollo and Troiano (2016) use the same dataset to present evidence that a disproportionate amount of transfers are awarded to female mayors irrespective of alignment status. This highlights the gender differences in a politicians lobbying efforts and response to political competition. An extension to the quasi-experimental literature on political alignment, Baskaran and Hessami (2017) assess intergovernmental revenues in German local elections. They show that whilst being affiliated to the state government is important, grant receipts vary with the degree of support at the ballot box. An aligned locality with strong support, receives a larger

share of transfers. Whilst being aligned with low support makes little difference to the amount received. Finally, Fouirnaies and Mutlu-Eren (2015) analyse the transfer framework in England, an established democracy and a parliamentary regime. Breaking away from the federalist literature, they argue that the co-partisan effect should be stronger in a centralised country where the local governments have little autonomous power. They present results that show that the central government allocates up to 17% more funds to aligned local councils, which ramps up the closer the year to the next local election.

Whilst there is a wide exploration of partisan alignment in the political and economics literature, very few studies consider the impact of alignment and transfers around elections. One reason for a lack of literature may be the need for an suitable scenario to analyse, which the US case nicely provides.[1] This break-out strand of literature warrants further analysis given the relatively unexplored nature and to provide robust support for the argument that transfers are manipulated as an electorally charged decision, whilst addressing the oversights of the previous related literature. The importance of building on the current work (Veiga and Pinho, 2007, Rumi, 2014) but in the most developed democracy will offer interesting new insights and policy implications.

4.2.2 Hypotheses development

Given the broad consensus in the literature that political motivations bias the reallocation intergovernmental funds, one must ask why does political alignment, or co-partisanship, have such a prevalent impact in redistributing said funds?

I argue that an incentive exists for the president to allocate more resources to their "own" states. The rationale being that voter's assessment of party performance at

[1] Cross-country studies would not be feasible due to the heterogeneous institutional arrangements of national to sub-national transfers.

the state level spills over and affects the assessment of the party at the federal level (Rodden, 2006). This has been documented at the English central-local level by Fouirnaies and Mutlu-Eren (2015), and in the US state-local level by Ansolabehere and Snyder (2006). It is in the interest of the president that his party performs well at all levels of government as voters will therefore have a positive evaluation of the incumbent president's party.

The implications of being aligned to the president, in terms of fiscal resources, are expected to be positive. So called "credit claiming" is easier for the federal government to do when the state is run by a co-partisan of the president (Levitt and Snyder, 1997; see e.g. Fouirnaies and Mutlu-Eren (2015) for this argument in terms of a Parliamentary regime). That is, good fiscal performance at the state level will be claimed by the incumbent president, which will be rewarded by voters at the next election. Conversely, when the president and governor are not aligned it is less clear whom voters should reward or punish. Therefore, I hypothesize that

Hypothesis 1: Transfers should increase to a state when the president and governor are politically aligned

With a political economy question, there is often electoral incentives. These are clearly prevalent in this chapter. In terms of elections themselves, I consider two levels of elections: federal (presidential) and state (gubernatorial). Firstly, I can consider whether the president uses these grants to keep his party in office at the next federal election. Given that election years are fixed in the US, it is plausible that the president engages in electoral cycle behaviour and allocates more to co-partisan states in pre-election or election years. Secondly, in order to continue to enjoy the benefits of positive spill-over effects, the president needs to ensure co-partisans remain in office at the state level. Therefore one would expect the deliberate re-allocation of resources towards co-partisan governors in pre-election and election years. However, this is essentially a placebo test of the president's power given that governors have no discretion over the allocation of grants and that president's use

the transfers for their own gain. Therefore, one may alternatively expect no effect in gubernatorial years. Thus, I hypothesize that

Hypothesis 2a: When the president and governor are aligned transfers should be higher in the presidential election year (confirming the existence of an electoral cycle in transfers)

Hypothesis 2b: When the president and governor are aligned transfers should not be higher in the gubernatorial election year

Another element to this chapter examines the role of battleground states and alignment, which is complimentary to my election incentives argument. As the process of being elected president is a winner-takes-all game in each state –due to the electoral college– one would expect politically aligned governors located in swing states to receive disproportionately more transfers. This would occur in order to deliver that state's electoral college votes at the next election. Given the media attention placed on governors to deliver their state's vote around election times, this a entirely plausible scenario. The swing states are prime target given that these offer the best chance of increasing the probability of winning the electoral college. Thus, one expects that a larger amount of transfers are given to co-partisan governors located in a swing state.

Hypothesis 3: When the president and governor are aligned electoral cycles in transfers should be stronger in swing states

Finally, unlike in directly related studies, I consider the individual attributes of the incumbent governor. After controlling for age, gender and term limits, one may expect the previous career of a governor to impact the amount of federal funds received. This follows from Dreher et al. (2009) who assess the impact of political leaders previous careers on various economic outcomes. No evidence is found between being a career politician and market-orientated reforms. However, as my case requires direct interaction between two politicians it is plausible that a previous career in politics

would have a significant impact. Two arguments prevail here. First, it may be that governors who have political experience prior to taking office as governor produce larger spill-over effects for the president than the inexperienced. Therefore, aligned governors with political experience may receive more transfer from the president. Alternatively, a relatively more experienced governor may be better placed to lobby the president for more funds using deliberative and lobbying skills learnt in their political career. Hence, one would expect a governor with past political experience to be associated with more intergovernmental revenue regardless of alignment status.

Hypothesis 4: When the president and governor are aligned and the governor has prior political experience, transfers should be larger

Given these four core contributions to the literature, I am able to show that when a governor is a co-partisan of the president they receive relatively more federal resources. I present evidence that the president engages in electoral cycles of transfers in order to keep his party re-elected at the presidential level. However, no such support is found for the president favouring co-partisans at the gubernatorial level when around their elections. In addition, I support the electorally driven argument by examining alignment in swing states. I find that aligned governors, who are located in swing states, get relatively more federal transfers to their aligned counterparts. In terms of the governor's specific role, I show that governors who have been members of Congress before entering office as the state executive are favoured by the president due to their superior lobbying skills. I find no implications for being a co-partisan and having past political experience simultaneously. My final contribution comes from analysing specifically Democratic or Republican alignment. By doing so I provide differences in behaviour across parties. Interestingly, I show that both parties redistribute to their co-partisans of the same magnitude in the presidential election year.

4.3 The US president, the federal budget and state governors

The degree of power enjoyed by the president over the federal budget is an important consideration I need to make before diving into any analysis. A clear understanding will elucidate the mechanism for the federal budget to be manipulated by the president. Whilst the president is the head of the executive branch of government and maintains significant powers, the world's most mature democracy has checks and balances in place in order to prevent an abuse of such power.

Following Congress' opposition to President Nixon's desire to reduce the budget deficit by not spending funds that Congress had allocated in 1972 the Budget and Impoundment Act of 1974 was passed, which aimed to strengthen Congress' budget authority and reduce the president's impoundment ability. In order to pass the federal budget, it has to pass various committees and subcommittees in the House of Representatives and the Senate. Once through these stages, the president is required to sign the budget into law or veto it. If the president decides to veto, this decision can be overturned by a two-thirds majority in each legislative chamber and becomes law without presidential ratification.[2]

Combined with the empirical evidence of the president manipulating the allocation of the federal budget discussed in the literature review and now a clear channel of impact, it is fair to say the president does in fact enjoy significant amount of influence over the federal budget.

Furthermore, this passage has so far done little to motivate the idea that the president duly considers and interacts with the state governors and how the role of partisanship can directly affect a president's actions. First, I present some presidential quotes that give an insight into the interactions with the governors. For instance,

[2] Legal scholars argue that the president maintains significant control over federal funds primarily because of the threat of veto (McCarty, 2000).

Donald Trump, speaking at the 2017 Governor's Ball -a chance for the governors to meet the president in an official capacity- said the following: "And tomorrow, we're going to meet, and we're going to discuss things, like perhaps healthcare will come up... Everybody is different, every state is different, and different requirements, but I think I have something that's going to really be excellent... But tomorrow morning, we're going to meet and have some pretty big sessions on healthcare and other things – whatever is on your mind."[3] It appears, from this evidence alone, that the president is indeed acutely aware of the capacity of each governor and their ability to deliver their states support. And secondly, to motivate the idea that co-partisanship favouring occurs from the federal-to-state level, consider the following from Mitt Romney (speaking whilst the incumbent governor of Massachusetts) "For Republican governors, it means I have an ear in the White House, I have a number I can call, I have access that I wouldn't have otherwise had, and that's of course helpful".[4] In essence, this is an explicit admission that co-partisans of the president lobby for federal support and expect to be favoured in some capacity.

4.4 Data and methodology

4.4.1 Data

I use a novel panel dataset of the 48 contiguous US states.[5] The sample spans a period of 59 years from 1950 to 2008. This is a comparatively large dataset in the literature, with each observation being identified on a state-year parallel. The data is

[3] Quotes are taken from the White House's press office. Available from `https://www.whitehouse.gov/the-press-office/2017/02/26/remarks-president-trump-2017-governors-ball`

[4] Mitt Romney speaking after the re-election of George W. Bush in 2004.

[5] As standard in the literature I focus on the 48 contiguous states, dropping Alaska and Hawaii from my sample.

a perfectly balanced panel with a total of 2,832 observations; the period in question covers 15 presidential elections with 6 incumbency shifts between the Republican and Democrat parties, and 839 gubernatorial elections with numerous changes in incumbency.[6]

Variables of interest and controls

In order to integrate this study into the literature on political alignment I use the natural log transformation of real per capita intergovernmental revenue from the federal to state government as my dependent variable ($ln\ Grant_{st}$). The values are deflated by the consumer price index with a base year of 2000. These data are obtained from the *US Census of Governments* and are available from 1950 onwards for all 50 states. The total intergovernmental revenue can be disaggregated into a variety of sub-categories.[7] Given that different states have different needs, there is considerable variation amongst the amount of grants awarded to the states in a given year.

The main independent variable of interest is partisan alignment ($Alignment_{st}$). This takes the value 1 when the state governor and president are of the same party and 0 otherwise. Naturally, the variation in alignment status comes from two sources: a change in the president or a change in the governor.

In addition to these variables of interest, I include a rich vector of control variables. The vector contains: the growth rate of real per capita income ($\Delta ln\ Income_{st-1}$) and a states own tax revenue from their own sources ($\Delta ln\ Revenue_{st-1}$), both

[6] In the sample there are 4 cases of Independent governors.

[7] The main categories are; air transportation; education; employment security administration; general local government support; health and hospitals; highways; housing; and community development; agriculture; other natural resources; public welfare; sewerage. Full for a complete breakdown of what is included and excluded in each category see `https://www.census.gov/govs/www/class_ch7_ir.html`

from the previous year.[8] I also include the growth rate of the states population ($\Delta ln\ Population$) to control for the size of a state. The raw data are obtained from the *US Census of Governments* and manually transformed into growth rates. The political controls are the margin of victory for the incumbent president at the last presidential election ($Margin\ of\ victory_{st}$), an important control to ensure that an alignment effect is separate to that of loyal support.[9] This is obtained from Leip (2008). As a proxy for state checks and balances, I include whether the state government is split ($Split_{st}$), which is taken from Klarner (2013).[10] This occurs when one party controls the executive branch of the state government and the other party controls at least one of the state legislative chambers. Following Brollo and Troiano (2016) I introduce governor specific traits, namely age (Age_{st}), gender ($Female_{st}$) and whether the governor is a lame duck ($Lame\ duck_{st}$).[11] These are manually gathered from the *National Governors Association* (NGA), which details each and every state governor. An example of a governor's profile is found in Figure 4.1. More detail on all variables is provided in section 4.A.

Descriptives

The variation in alignment status comes from two sources: presidential and gubernatorial elections. For instance, alignment can occur because a new Democratic governor has been elected under an already Democrat president, causing Democratic alignment. Or, a new Republican president has been elected, causing all current Republican governors to become aligned. Given that not all state's guber-

[8] Single lags are used as it likely that this data will be used when determining the economic situation to set grants for a state in time t.

[9] Because this is measured as the two-party margin of victory, in the 1962 presidential election in Alabama, the President Lyndon Johnson received 0 votes thus the margin of victory in that state is -100. Results remain the same when removing outliers here.

[10] I also cycled this for a unified ($Unified_{st}$) government and the results remain unaffected. See Table 4.13

[11] I include age in levels but have tried various other specifications –a quadratic and in natural logs– and the results remain unaffected, see Table 4.13.

natorial electoral cycle follows the presidential pattern, changes in alignment status occur in various years. There are 1,359 observations (approximately 48% of the sample) where president and governor are aligned, with ample variation within states. Given the length of the period available and two sources of variation, there are no states that have never been aligned nor any states that have always been aligned.[12] Because of these two sources of variation, and the fact that voters vote according to very different narratives, alignment is not on the minds of voters when casting their vote at either election (Atkeson and Partin, 1995).[13] Therefore, the effect of partisan alignment is identified by exploiting the quasi-random nature of alignment in the US. To further assess this assumption, I examine whether observable characteristics that are correlated with transfers also determine whether certain states become aligned. If the states and governors are similar across alignment status, then differences in transfers can be attributed to partisan alignment. Table 4.2 presents the summary statistics split by alignment status. Overall, the distributions of these variables are very similar across aligned and non-aligned states.

Figures 4.2 and 4.3 presents the alignment status of states over time. Figure 4.2 shows Democratic alignment in blue, when there is a Democrat governor in place creating partisan alignment with the incumbent Democrat president, Bill Clinton. Figure 4.3 shows the Republican version in red, when the governors are co-partisans of president Ronald Reagan in 1971. Changes of the executive party at the state

[12] The governor for each year is defined as the governor who has served the majority of the year in office.

[13] At this point it is prudent to attempt a brief answer to the question; why, for example, do 'deep red' states in presidential elections not always elect a governor from the GOP? Kansas has returned electoral college support for the Republican party since 1968. In the same period of time the state has had 7 different governors, 4 of which were Democrats. In short, this is because federal elections are more nationalised and voters take into account a range of national and international issues. At the state level governors can break from the party line on issues, allowing pro-choice Republicans to be elected into a blue state, for instance. Atkeson and Partin (1995) use survey data to show that in gubernatorial elections, governors escape from national-level evaluation of presidential performance but are judged on state economic conditions. Personal competence and changing political sentiment are also important factors. A specific individual example of this is from California where Arnold Schwarzenegger –a Republican– was elected as a two-term governor of a deeply liberal state because of his "socially moderate" stances. The prevalence of alignment in these 'purple' states is demonstrated in Figure 4.5 and 4.6.

level occur frequently given that not all states executive cycle follows the president's cycle.

In terms of the federal transfers, the average growth rate is 4.12% per annum. When federal-state executives are aligned the average growth rate is higher, 4.23%. In contrast when there is no alignment the average growth rate is smaller, now 4% per year. Alaska and Hawaii are typically very dependent on the transfers due to their small revenue raising power. Richer states and those with a larger domestic populous tend to receive less federal money. California, for example, has an average transfer growth rate of 3.6% and the largest in-sample population. The transfers are conventionally used to alleviate fiscal difficulty and to increase welfare of the state, however, it is entirely plausible to ask how much political manipulation can explain the differences across states.

4.4.2 Empirical strategy

To test the hypotheses developed in section 4.2.2, I employ a fixed effects model. I start with the following econometric specification to test hypothesis 1:

$$ln\ Grants_{st} = \alpha_0\ ln\ Grants_{st-1} + \alpha_1\ Alignment_{st} + \beta\ Z_{st} + \eta_t + \mu_s + \epsilon_{st} \quad (4.1)$$

where $Grants_{st}$ is the intergovernmental transfer from the federal government to state s at time t. $Alignment_{st}$ is the dummy variable for partisan alignment, Z_{st} contains the sets of control variables detailed in the preceding section, namely; $\Delta ln\ Income_{st-1}$; $\Delta ln\ Revenue_{st-1}$; $\Delta ln\ Population_{st}$; $Margin\ of\ victory_{st}$; $Split_{st}$; Age_{st}; $Female_{st}$; and $Lame\ duck_{st}$. η_t and μ_s are time and state fixed effects, respectively and ϵ_{st} is the i.i.d. error term. Following most previous literature, a lagged dependent variable is included to capture the persistence of grants. Moreover, if Eq. 4.1 is estimated the lagged dependent variable approaches unity, indicating a unit

root.[14] Therefore I conduct Maddala and Wu (1999) and Choi (2001) stationarity tests, which confirm the null hypothesis of a unit root i.e. non-stationarity (see Table 4.3). The first difference of $Grants_{st}$ allows us to reject the null of a unit root indicating stationarity, thus I proceed with the first difference of the dependent variable, which is now the growth rate of the federal-state revenue.

I now specify a version of Eq. 4.1 to be estimated:

$$\Delta ln\ Grants_{st} = \alpha_0\ \Delta ln\ Grants_{st-1} + \alpha_1\ Alignment_{st} + \beta\ Z_{st} + \eta_t + \mu_s + \epsilon_{st} \quad (4.2)$$

where $s = 1, ..., 48$ and $t = 1950, ..., 2008$. The prefix 'Δ' indicates the first difference has been taken. By employing a fixed effects estimator, the time-invariant state specific effects are 'differenced' out, but not time specific effects. Hence, δ_t is included to control for specific year-fixed effects.

With regards to the econometric specification, the inclusion of a lagged dependent variable introduces a potential bias to my estimates by not satisfying the strict exogeneity assumption of the error term ϵ_{st}. One possible solution would be to adopt a GMM estimator (see, e.g., Arellano and Bond, 1991), however, this method only yields consistent estimates in small T, large N panels. Given that the estimated bias is $1/T$, where T is the total time length of the panel, the bias becomes negligible in my long T sample (Nickell, 1981). Thus, I proceed to estimate Eq. 4.2 by employing the linear fixed effects estimator. To test the other 3 hypotheses, I consider augmented versions of Eq. 4.2.

[14] See Table 4.5 column (1).

4.5 Results

4.5.1 Transfer allocation and partisan alignment

Table 4.5 shows the test results for hypothesis 1. In columns (2) to (5) I report parsimonious specifications that show partisan alignment is positively related to intergovernmental transfers at the 5% level of significance. The proceeding column includes the economic controls and the margin of victory for the president in the last presidential election, allowing us to control for the strength of support for the president. Column (7) introduces the governor characteristics, which appear statistically insignificant at the conventional levels. Column (8) includes a state-specific linear time trend to soak up any unobserved heterogeneity specific to each state. Reassuringly the results remain unaffected. In all columns *Alignment* remains positively related to grants at the 5% level. Qualitatively, the findings imply that the growth rate of transfers increases by about 0.8 percentage points when state governors are aligned to the president. Tentatively, this effect can be interpreted as causal due to the quasi-random nature of alignment. Furthermore, *Margin of victory*, the margin of victory for presidential support at the last election, exerts a positive and significant influence on the dependent variable, which suggests that there is some reward for a governor delivering their states vote. Moving from 0 support to full support increase the growth rate of federal-state transfers by an additional 1.6 - 1.7 percentage points. As Migueis (2013) argues, it is important to distinguish between alignment and popular support to be sure the effects are independent from one another. Little evidence is found for the governor's characteristics at this stage, which is not so dissimilar to the related literature (Dreher et al., 2009).[15]

The findings are so far consistent with hypothesis 1. That is, that after controlling for the margin of victory, a state with a governor who is aligned to the president

[15] The results are subject to a set of further robustness checks, which are available in Table 4.13.

is rewarded with more intergovernmental revenue because the governor is a co-partisan. This is also in accordance with Rodden (2006), who suggests that this happens because of vertical spill-over effects; the president will favour co-partisans at lower tiers to ensure voters have a positive view of the government at all levels.

In this setting, a dichotomous variable of interest and year fixed effects, I can evaluate the common trend assumption that is necessary for a causal interpretation of the coefficient. While using a state-specific trend alleviates endogeneity concerns, there still may exist some bias. I can test this assumption by examining whether pretreatment or posttreatment (to use the language of causal inference) trends exist for treated and untreated states that would indicate non-random selection. Given that partisan alignment should only affect transfers when the governor and president are aligned simultaneously, significant lead-variables would cast doubts on the interpretation of the results thus far. Significant lag-variables are not necessarily a violation of the assumption as transfers may be contract based and take time to reverse.

To test this assumption I follow Gehring and Schneider (2018). I create two lead variables, taking the value 1 only in the year $(t-1)$ and two years $(t-2)$ before alignment takes place, and 0 otherwise. I code four lag variables taking the value 1 for the year after alignment has been 'switched off' in $(t+1)$ and up to four years later $(t+4)$, and 0 otherwise. The estimated specification remains the same as that in Table 4.5 column (8), which includes all controls, state-specific time trends and year fixed effects.[16] Table 4.6 depicts the results including different leads and lags.

In column (1), both the lead variables are insignificant, whereas the coefficient for *Alignment* remains statistically significant at the 5% level. Column (2) replaces leads for lags. Here all the lagged terms are insignificant and *Alignment* increases

[16] The estimating equation is: $\Delta ln\ y_{st} = \alpha_0 + \Delta ln\ y_{st-1} + \alpha_1 Alignment_{st} + \sum_{\gamma=-2}^{4}(\alpha_{1t+\gamma} Alignment_{st+\gamma}) + \beta\ Z_{st} + \eta_t + \mu_s + \epsilon_{st}$

to 0.012 and is now significant at the 1% level. Finally, column (3) includes both leads and lags. The coefficient for *Alignment* becomes 0.015, again significant at the 1% level. All leads and lags are insignificant, giving no indication of any pre- or posttreatment trends, whilst *Alignment* remains significant throughout. This is critical for a causal interpretation of the identified relationship. The coefficients of the leads and lags for *Alignment* is illustrated graphically in Figure 4.4.

4.5.2 Transfer allocation and partisan alignment around elections

As a next step in the analysis I consider what role elections play in the tactical allocation of fiscal resources to co-partisans, hypotheses 2a and 2b.. Here, by exploiting the exogenous variation in the electoral calendar, I can causally identify the effect of the electoral period in triggering more funds being allocated to co-partisans. This follows from the notion that the president will attempt to re-allocate to co-partisans around presidential election times in order to keep the presidents' party in office and whether the president seeks to keep states aligned around gubernatorial elections. By analysing whether the president seeks to keep states aligned, this is essentially a placebo test of the president's power given that governor's have no direct discretion over the allocation of transfers. In order to test this I interact my alignment indicator with the (pre-) election variable, therefore I augment Eq. 4.2 in the following manner:

$$\begin{aligned} \Delta ln\ Grants_{st} = \alpha_0\ \Delta ln\ Grants_{st-1}\ +\ \alpha_1\ Alignment_{st} + \alpha_2\ Pres\_elec_{st} + \\ \alpha_3\ Alignment_{st} * Pres\_elec_{st} + \beta\ Z_{st} + \eta_t + \mu_s + \epsilon_{st} \end{aligned} \tag{4.3}$$

The results from the estimates seeking to test these hypotheses are presented in Table

4.7. Columns (1) - (4) present estimates for the president election cycle, here I cannot use year fixed effects as the election occurs in each state at the same time. Columns (1) - (3) includes the interaction term and a introduce progressively more controls. The first thing to note is that the interaction term, $Alignment * Pres\_elec_{st}$, is positive and significant at the 5% level of significance in all 3 columns. Qualitatively, the results suggests that the growth rate of intergovernmental trsnfers in presidential election years, when the governor is aligned to the president, increases by around 1.7 percentage points on average. In columns (4), I introduce a pre-election variable to capture whether any pre-electoral manipulation takes place. This acts as an improvement in the identification of the effect of the electoral cycle (Akhmedov and Zhuravskaya, 2004, Shi and Svensson, 2006). Again, when the governor of the state is aligned to the president and it is the year before a presidential election, there is a positive manipulation of resources towards these states. This incremental effect is significant at the 10% level and slightly smaller than in an election year. This is in accordance with the results found in Fouirnaies and Mutlu-Eren (2015), that more manipulation takes place around election times in order to increase the probability of the national executives party's re-election. In particular, the impact is specific to co-partisans because of the spill-over and credit claiming effects associated with the voters assessment of the state government.

In columns (5) - (8) I test whether the president seeks to keep states aligned by allocating more transfer in gubernatorial election times, which is an implicit placebo test of presidential power. In these regressions I can include year fixed effects as elections occur at different times in different states. Across all columns the coefficient for Gub_elec_t is positive and significant at the 5% level indicating that the president does indeed allocate more to all states in gubernatorial election years. This may reflect the political ability of incumbent governors to strategically lobby for federal resources to increase their re-election chances. This is political experience argument is discussed in more detail later. No statistically significant effect is found for the interaction with the alignment variable, however, the partisan alignment

variable itself, *Alignment*, is highly significant and positive. This is an acceptance of hypothesis 2a and rejection of hypothesis 2b.

4.5.3 Partisan alignment and swing states

In order to further support the argument that partisan favouring occurs partly because of electoral incentives, I examine the role of swing states. This is testing hypothesis 3. So called 'swing' states are key battleground states for the US presidential election, in so far as one party does not have overwhelming support. As state's electoral college votes are nearly all a winner-takes-all game, therefore, it is often vital that a potential president win these key states on their way to the presidency. To construct my variable *Swing* I use information on state-level margins of victory in presidential elections (Glaeser and Ward, 2006; McLaren and Ma, 2016; Conconi et al., 2017). Therefore, I most closely follow Abramowitz and Saunders (2008), Conconi et al. (2017) and McLaren and Ma (2016) and dichotomously define a swing state if and only if the two-party vote share from the previous presidential election was within 5 percentage points (151 states). As a robustness check I push this out to 6 (187 states) and 10 percentage points (283). As in the previous section, I now interact *Swing* with my alignment variable to analyse whether more resources are awarded to aligned governors who are located in electorally critical swing states. Hence, Eq. 4.2 is augmented in the following manner:

$$\begin{aligned} \Delta ln\ Grants_{st} = \alpha_0\ \Delta ln\ Grants_{st-1}\ +\ \alpha_1\ Alignment_{st} + \alpha_2\ Swing_{st} + \\ \alpha_3\ Alignment_{st} * Swing_{st} + \beta\ Z_{st} + \eta_t + \mu_s + \epsilon_{st} \end{aligned} \tag{4.4}$$

The results from these regressions are reported in Table 4.4. The first 5 columns report regressions using the 5 percentage point definition of the *Swing* variable. The interaction term is positive and statistically significant at the 5% level. The

estimates show coefficients of around 1.2 - 1.4 percentage points. Qualitatively, this suggests that the growth rate of federal transfers to governors who are aligned and located in swing states increases by around 1.3 percentage points. Column (5) removes observations where the margin of victory was greater than 40 percentage points; cases where the margin is larger than this value are clearly outliers. For instance, in the 1948 election, the Democratic candidate did not appear on the ballot paper in Alabama so the margin of victory was 100 points. Columns (6) and (7) expand the definition of a swing state to 6 and 10 percentage points, respectively. By doing so I include an extra 142 and 521 observations as swing states. The interaction term remains significant level at the conventional level for the 6 percentage point definition and is insignificant for the 10 point definition. However, this is not unexpected given that 10 percentage point difference between the two parties in a given state would not necessarily be thought of as a swing state per se.

When the $Alignment * Swing$ coefficients are compared to that of $Alignment$, (obtained in Table 4.5) the aligned governors located in swing states are recipient to a larger growth of federal resources, which is in line with hypothesis 3. This results confirm that the growth of federal resources to aligned governors is driven by electoral incentives at the presidential level - which is consistent with the findings from section 4.5.2.

4.5.4 Partisan alignment with experienced politicians

As another tranche to my analysis, I ask whether a more politically experienced politician may also receive more intergovernmental resources as bridged in Section 4.2.2, this is to test hypothesis 4. The plausibly random assignment of experience across governors is useful as it allows us to discern the impact of political ability of

the governors who can lobby the president for more resources.[17] A governor who has more experience prior to taking office is likely to be better at lobbying the president for him to award more funds to their state. In conjunction with this, it is possible that the supposedly 'better' politicians produce larger vertical spill-over effects for their party. Therefore, it may be that co-partisans with more experience receive disproportionately more transfers to produce a better assessment of the president's party. In order to test this hypothesis I include a dummy variable for whether a given governor has ever served as a member of Congress prior to taking office as state executive, denoted *Congress member*. This differs from the Dreher et al. (2009) definition of a 'political career' in so far as I use a specific type of political experience rather than simply any political career. The *Congress member* variable is also interacted with *Alignment* to test the co-partisan spill-over argument. I am able to fully discern the status for each governor using information from the *National Governors Association* (NGA). There are 65 governors who are ex-members of Congress, which equates to 319 state-year observations. The equation to estimate now becomes:

$$\Delta ln\ Grants_{st} = \alpha_0\ \Delta ln\ Grants_{st-1}\ +\ \alpha_1\ Alignment_{st} + \alpha_2\ Congress\ member_{st} + \alpha_3\ Alignment_{st} * Congress\ member_{st} + \beta\ Z_{st} + \eta_t + \mu_s + \epsilon_{st} \tag{4.5}$$

The results from these regressions are presented in Table 4.9. Across columns (1) to (6) both *Alignment* and *Congress member* appear positive and statistically significant. This supports the idea that governors, who have been ex-members of Congress, receive more money from the president. The magnitude implies that

[17] Unforuntaley, I do not have data on whether the candidates runing in a gubernatorial election have been Congress members. So I cannot detrmine whether a prior member of congress is more likely to be elected into a governorship.

prior members of Congress get 0.8 - 1 percentage points more on the growth rate of federal transfers. The mechanism behind this effect is that executives with more political nous are more successful at lobbying for more funds. When the interaction term is included in columns (4) to (6) it is not significantly different from 0. The insignificance of the interaction terms signifies that whilst all ex-members of congress are allocated more, there is no disparity by being a co-partisan of the president. Thus, I can reject the argument that more experienced politicians create larger spill-over effects (hypothesis 4) in favour of the notion that ex-politicians are more effective, and successful, at lobbying for federal resources. The effect is likely driven by the skills obtained whilst the governor was a member of Congress. That is, cross-party co-operation on the legislative and fiscal agendas.

4.6 Falsification and robustness tests

So far I have shown that the positive relationship between alignment and the growth rate of federal-state transfers is robust to various specifications and shown the underlying electorally driven mechanism. I now conduct some validation tests that lend support to the identification strategy and ensure the results are not due to spurious inference. Given the variety of data available, I conduct a placebo test of the effect of alignment upon a state's own tax revenue. This is revenue that can only be raised by the state government, therefore, independent of federal expenditure. Thus, the impact of alignment of the state-federal executives should be unrelated. I check whether *Alignment* is unrelated to state's own revenue by employing the growth rate of per capita own state revenue as the dependent variable.

Table 4.10 panel A reports the results from the set of placebo tests. Column (3) introduces year fixed effects, (4) and (5) include extra controls and the state-specific time trend, respectively. The estimate of *Alignment* is insignificant across all specifications. The strong significance of the margin of victory variable is interesting

and furthers support for my identification strategy. That is, states that voted for the winning party are rewarded with lower state taxes. Hence, I can be confident in my identification of an alignment effect and that my results are not driven by a systematic error.

I apply the method developed by Altonji et al. (2005) to assess how much larger the selection bias based on unobserved factors relative to observed factors would have to be to fully explain away the result. I compare the coefficient of *Alignment* from two kinds of OLS regression models: one which contains only state and year fixed effects to one which contains a full set of controls in addition to the fixed effects, β_L and β_F, respectively. Panel B of Table 4.10 depicts the selection ratio (SR) the ratio of selection-of-unobservables to obseravbles necessary to fully explain away the *Alignment* effect. The SR indicates that the selection-on-unobservables would have to be 7 times as large as the selection-on-observables to fully wash out the positive relationship.[18] In the same set-up, Altonji et al. (2005) estimate a SR of 3.55, a much smaller SR which they interpret as evidence that unobservables are unlikely to explain away their entire effect.

4.7 Extensions

4.7.1 Democrats versus Republicans

Given the dominance of the two parties in the US, I am able to disentangle the behaviour of each party in order to elucidate further differences in their actions. By using the US case, it not only highlights cross party heterogeneity, but differences across the ideological spectrum. Some regressions from the previous sec-

[18] When including a linear state-specific time trend into the full model, the SR remains at 7.

tion are now re-estimated except now the alignment variable is split between the Democrats and Republicans, or the left and right-wing parties. I create two new variables $D\_Alignment$ and $R\_Alignment$ to separate the alignment and replace $Alignment$. The former, $D\_Alignment$, represents Democratic alignment of the state-federal executives and takes the value 0 otherwise, i.e. when Republicans are aligned, or there is no alignment. $R\_Alignment$ reflects the same situation but for Republican alignment. There are 524 and 835 cases of Democratic and Republican alignment, respectively. This disparity is expected given the dominance of the Republican party at the national level over the period – holding office for 36 of the 59 years in the sample. By creating these two variables it allows us to separate out which party is driving the alignment effect and to distinguish which party engages more often in rent seeking behaviour i.e. the manipulation of federal transfers. I modify Eq. 4.2 in following manner:

$$\Delta ln\ Grants_{st} = \alpha_0\ \Delta ln\ Grants_{st-1} + \alpha_1\ D\_Alignment_{st} + \alpha_2\ R\_Alignment_{st} + \beta\ Z_{st} + \eta_t + \mu_s + \epsilon_{st} \tag{4.6}$$

The results from these split alignment regressions are presented in Table 4.11. In columns (1) - (3) I include the two alignment variables with progressively more controls. The results show that the effect of alignment on transfers is stronger, and statistically significant, when the Republican executives are aligned. In contrast, there is no effect, at this stage, for Democratic alignment. Hence, what emphasises the combined alignment effect is driven by the right-wing party. This is not to say that right-wing parties prefer larger governments as the federal transfer system is not necessarily an indicator of government size, but rather that right-wing parties actively target their co-partisans with extra funds.

In addition, I can examine how the distribution of funds is affected around election

times. Once again, I interact my alignment variables with the (pre) election years and re-estimate my specifications. These models are presented in columns (4) - (6). First, what is intriguing, is that both parties favour their co-partisans in presidential election years by similar magnitudes. Qualitatively, this means that when the state executive is aligned to the president, and in a presidential election year, the growth rate of federal transfers increases by approximately 2.3 percentage points. There is also some evidence that if not affiliated, the opposing parties 'ties-the-hands' of the unaffiliated governors. On the whole, this suggests that both Democrats and Republicans engage in electoral cycle behaviour with federal transfers toward their co-partisan governors, in order to increase the probability of re-election of the president's party at the national level. This happens by supporting their co-partisans to create a positive vertical assessment of the presidents' party by voters. Moreover, by taking into account the pre-electoral manipulation the results are also interesting. In the pre-election years, states receive less, however, when the president and governor are Republicans there is a comparably large net gain. When these effects are considered together, it appears that the in pre-election years less funds are awarded, however, when there is Republican alignment, there is a significant net gain.

In the final columns, (7) - (9), I report the whether either party seeks to keep states aligned and the implicit placebo test. Again, $R\_Alignment$ appears positively and highly significant, and as in Table 4.7 the gubernatorial election year variable is significant. However, no heterogeneous effect is found between the two parties, which suggests that neither party seeks to keep their co-partisans in office during gubernatorial elections via the manipulation of transfers.

4.7.2 Variation across different time periods

The long time period for which data is available prompts a brief exploration into the period of time where the alignment effect may be being driven from. Over the

period, there have been few major reforms to budgetary power, the exception being the Congressional Budget and Impoundment Control Act, which was enacted in July 1974. The focus of this Act was to prevent the president impounding federal funds thus shifting the balance of power toward Congress. In light of this, one may expect the alignment effect to be more pronounced in the years prior to the reform as the president has more power to allocate resources to co-partisans. However, it may be that the alignment effect is driven by the years after the reform. The reform shifted the balance of budgetary power back toward Congress, which constrained the president's ability to allocate funds, this therefore increases the incentives to reward co-partisans. Moreover, the president is seen to maintain power over the budget after the reform due to the threat of veto. This is in line with the argument put forward by Abramowitz and Saunders (2008), that ideological polarization in the US has been increasing since the 1970's for both voters and political elites. In order to show some initial evidence of this reform on co-partisan biases, I split the sample either side of this reform date and re-estimate Eq. 4.2 on each time period.

The results of this exercise are presented in Table 4.12. The table shows that *Alignment* is only statistically significant in the years after the reform in 1974. Here, in column (2), the coefficient shows that the growth rate of transfers to a state increases by 0.7 percentage points when the president and governor are of the same party, *ceteris paribus*. This fits with the argument that the president began to favour his partisans only, once he felt that his budgetary setting power was threatened by Congress.

4.8 Conclusions

In this chapter, I have used a novel data set of federal grants to states, elections and political careers from 1950 - 2008 to investigate the role of partisan alignment

in allocating a portion of the federal budget. My analysis depicts a causal relationship between alignment and the allocation of federal money by exploiting the quasi-randomness of alignment status. More specifically, I show that federal grants are targeted towards states whose governor is aligned to the president, in order to increase the re-election probability of the president's party at the national level. I also find evidence in favour of a bi-partisan effect when accounting for political experience.

The results are consistent with prior work on the US that show a positive alignment bias in federal expenditure (Larcinese et al., 2006; Ansolabehere and Snyder, 2006). Moreover, when getting into the mechanism that is driving this effect, my analysis suggests that co-partisans are favoured because of electoral incentives. This is first shown by following the PBC literature and introducing (pre) election year indicators. And secondly, I show that co-partisans, that are located in electorally critical swing states, are favoured to a greater extent. I am also able to present evidence that governors who have previously been members of Congress receive more federal money, which occurs on a cross-party basis. That is, that more politically experienced governors are recipient of more funds due to their political know-how and lobbying efforts, which have been honed in their prior political incumbency.

Moreover, I can split my sample by Democratic and Republican alignment in order to shine light on the party which is potentially driving my results. I show that the pure alignment effect is driven primarily by the Republican party. However, by splitting my alignment variable into two, I am able to show that in election times, presidents of both parties favour their aligned governors to a similar extent.

The implications of this chapter are multifaceted, although, the primary focus is on the role of partisanship. Whilst there are significant checks and balances in place to limit an abuse of presidential power, the results show an implicit contradiction to the founding principles of the US. Whereby George Washington abhorred partisanship and thought it detrimental to the US on the whole. Whilst I show that co-partisans

are favoured, one may ask whether this favouring actually produces a worse outcome for society.

One other potential avenue for future research could be to further disaggregate political experience into specific job types. For instance, in this setup, one could examine the impact of governors who have progressed through the state government offices. This is also not necessarily constrained to the study of experience and transfers, there is significant scope to develop the literature and analyse spending or revenue patterns amongst governors – along a similar line of enquiry to Alesina et al. (2016) and Beland and Oloomi (2017).

Chapter Appendices

4.A Chapter 4 variable definitions

Dependent variables
ln Grants – The natural logarithm of total per capita grants to state governments, deflated by the consumer price index, $2000 prices.
Δ ln Grants – The growth rate of total per capita grants to state governments, deflated by the consumer price index, $2000 prices.

Key independent variables
Alignment – Political alignment. Takes the value 1 when the president and governor are of the same party, 0 otherwise. When there is a mid-year switch in the party of the governor, the party of the governor for the majority of the year is used.
D_Alignment – Democratic alignment. Takes the value 1 when the president is a Democrat and the governor of a given state is a Democrat, 0 otherwise.
R_Alignment – Republican alignment. Takes the value 1 when the president is a Republican and the governor of a given state is a Republican, 0 otherwise.
Pres_elec – Takes the value 1 in a presidential election year.
Gub_elec – Takes the value 1 in a gubernatorial election year.
Swing – Takes the value 1 if the two-party state presidential vote margin of victory was less than 5 percent at the last presidential election.

Governor characteristics
Congress member – Takes the value 1 when the governor has previously served as a member of the House of Representatives or the Senate, 0 otherwise.
Age – The age of the governor in office.
Female – Takes the value 1 when the governor is a female, 0 when male.
Lame duck – Lame duck last year. Takes the value 1 when a governor is in their last year before a binding term limit, 0 otherwise.

Other control variables
Δ ln Income – The growth rate of real per capita income, deflated by the consumer price index, $2000 prices.
Δ ln Revenue – The growth rate of the total amount of per capita state revenue from a state's own sources, deflated by the consumer price index, $2000 prices.

Δ ln Population – The growth rate of state population. $pop = Ln(Population/1000)$.
Margin of victory – The two-party margin of victory for the incumbent president at the last presidential election.
Split – Takes the value 1 when one party controls the executive and the other party controls at least 1 of the two state legislatives chambers.
Trend – Linear trend term for each state.

4.B Chapter 4 Figures

Figure 4.1: Example of a governor's profile on the National Governors Association website

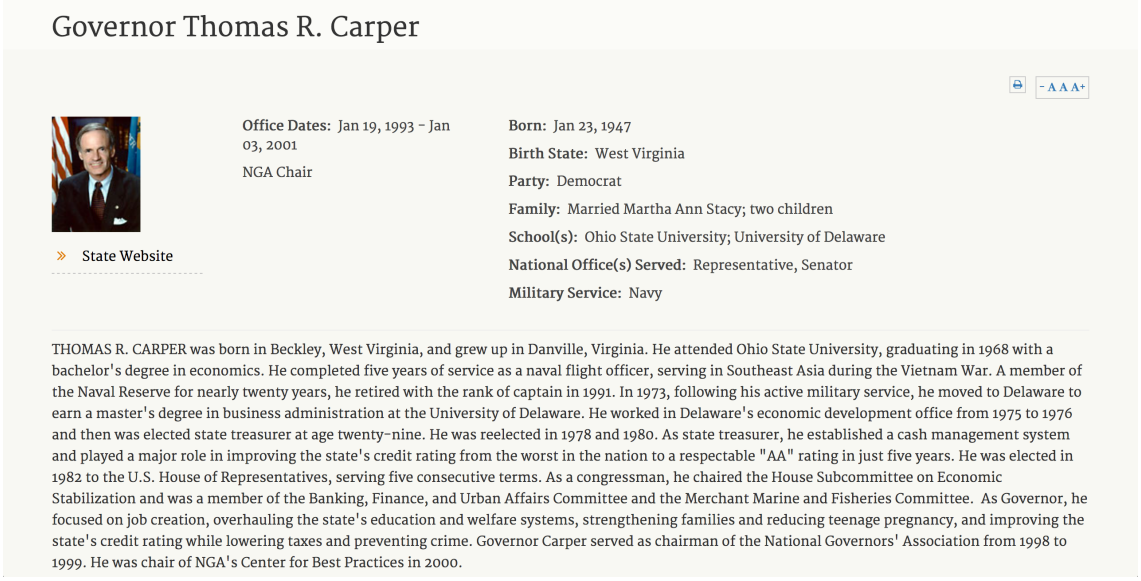


Figure 4.2: Democrat alignment in 1994

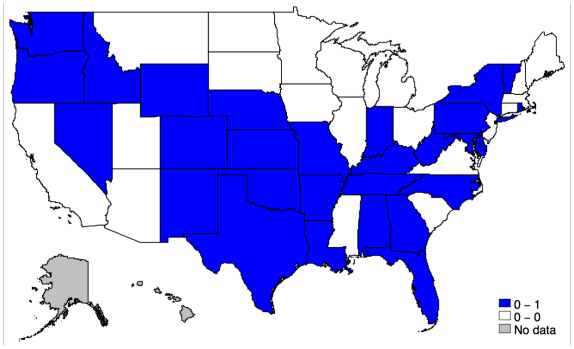


Figure 4.3: Republican alignment in 1971

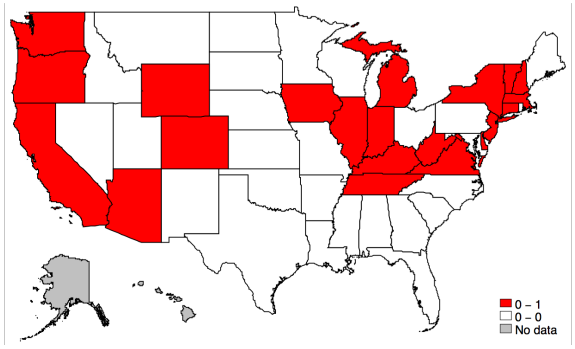


Figure 4.4: Leads and lags. Regression coefficients and confidence intervals correspond with Table 4.6 column (3).

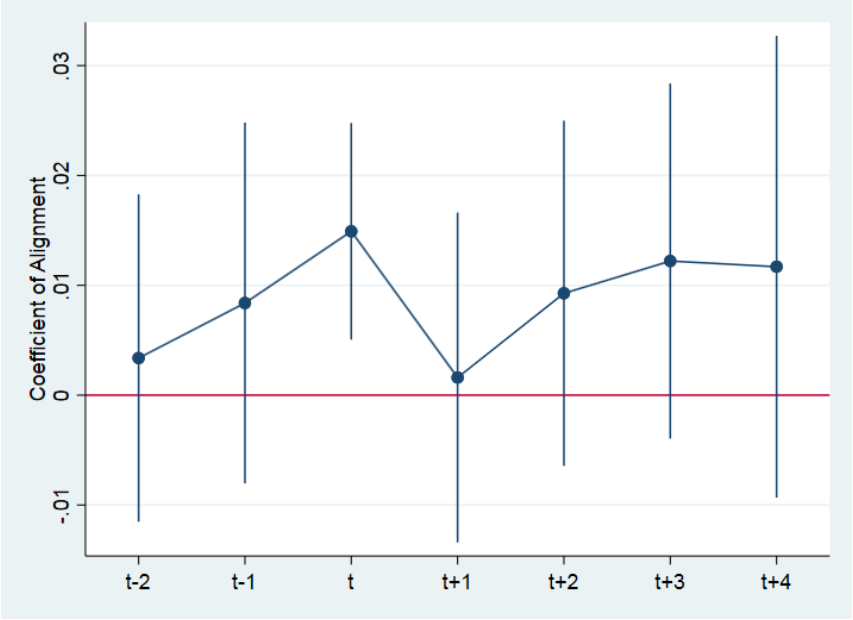


Figure 4.5: 'Purple' aligned states in 1993: Rep. voting states in the 1992 presidential election, with a Dem. governor and a president who is a Dem.

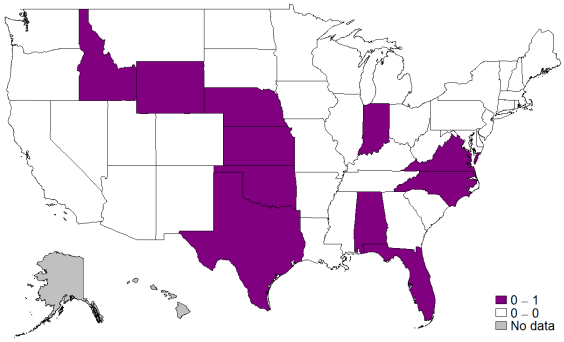


Figure 4.6: Democratically aligned states in 1993.

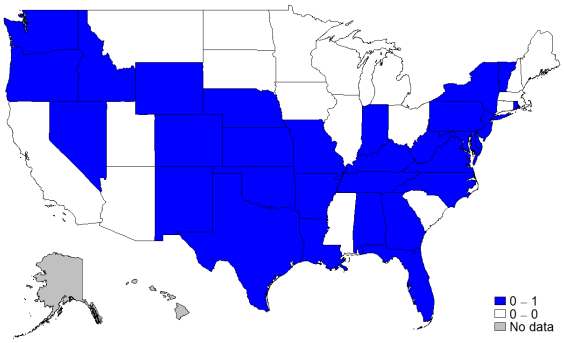


Figure 4.7: Governors who are ex-members of Congress in 1993

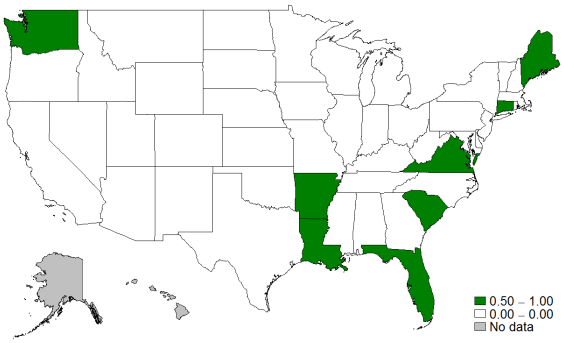
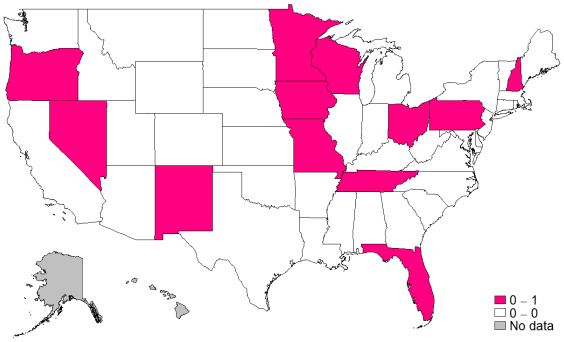


Figure 4.8: Swing states from the 2000 presidential election



4.C Chapter 4 Tables

Table 4.1: Summary statistics 1950 - 2008

	(1) No. of obs	(2) Mean	(3) Std. Dev.	(4) Min	(5) Max	(6) Source
Δln Grant (per capita)	2,784	0.0413	0.104	-0.431	0.574	US Census of Govts.
Alignment	2,832	0.480	0.500	0	1	Author's elaboration
D_Alignment	2,832	0.185	0.388	0	1	Author's elaboration
R_Alignment	2,832	0.295	0.456	0	1	Author's elaboration
Δln Income (per capita)	2,784	0.0220	0.0291	-0.152	0.331	BEA
Δln Revenue (per capita)	2,784	0.0319	0.0993	-0.985	0.743	US Census of Govts.
Δln Population	2,784	0.0120	0.0291	-0.0586	0.331	BLS
Margin of victory	2,832	9.236	18.34	-100	100	Leip (2008)
Split	2,832	0.487	0.500	0	1	Klarner (2013)
Age	2,832	52.312	8.040	33	78	Author's elaboration from the NGA
Female	2,832	0.0364	0.187	0	1	Author's elaboration from the NGA
Lame duck	2,832	0.0703	0.256	0	1	Klarner (2013)
Congress member	2,832	0.113	0.316	0	1	Author's elaboration from the NGA
Pres_elec	2,832	0.254	0.436	0	1	Author's elaboration
Gub_elec	2,832	0.296	0.457	0	1	Author's elaboration
Swing	2,832	0.207	0.405	0	1	Author's elaboration from Leip (2008)
Trend	2,832	30	17.03	1	59	Author's elaboration

Notes: **BEA:** Bureau of Economic Analysis. **BLS:** Bureau of Labor Statistics. **NGA:** National Governors Association.

Table 4.2: Summary statistics by alignment status

	Alignment = 1				Alignment = 0			
	N	mean	min.	max.	N	mean	min.	max.
Δln Income	1,340	0.022	-0.152	0.331	1,444	0.022	-0.121	0.271
Δln Revenue	1,340	0.029	-0.985	0.612	1,444	0.034	-0.789	0.743
Δln Population	1,340	0.013	-0.033	0.120	1,444	0.011	-0.059	0.099
Margin of victory	1,359	9.849	-100	50.24	1,473	8.671	-72.94	100
Split	1,359	0.504	0	1	1,473	0.47	0	1
Age	1,359	52.54	33	74	1,473	52.12	33	78
Female	1,359	0.027	0	1	1,473	0.046	0	1
Lame duck	1,359	0.077	0	1	1,473	0.064	0	1
Congress member	1,359	0.119	0	1	1,473	0.107	0	1
Swing	1,359	0.204	0	1	1,473	0.210	0	1

Table 4.3: Panel unit root tests

	(1) ADF - Fisher χ^2	(2) ADF - Choi Z-stat
ln Grants	49.051	3.013
	(1.000)	(0.989)
Δln Grants	544.768***	-18.111***
	(0.000)	(0.000)

Notes: H0: unit root. The prefix 'Δ' indicates the first difference has been taken. Unit root tests are carried out with a constant, a maximum of 3 lags and a trend term. Figures without parentheses are test statistics and figures in parentheses are p-values where * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 4.4: Pairwise correlation matrix

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
(1) $\Delta ln\ Grant$	1												
(2) *Alignment*	0.00971	1											
(3) $\Delta ln\ Income$	0.0665	0.00145	1										
(4) $\Delta ln\ Revenue$	0.0426	-0.0245	0.204	1									
(5) $\Delta ln\ Pop$	-0.0247	0.0586	-0.0150	-0.00502	1								
(6) *Margin of victory*	0.0502	0.0321	-0.0202	-0.0172	0.0624	1							
(7) *Split*	-0.00846	0.0329	-0.0502	-0.0299	-0.00291	0.0542	1						
(8) *Age*	-0.0103	0.0261	-0.0725	-0.0424	-0.00263	-0.0110	0.0435	1					
(9) *Female*	-0.0258	-0.0507	-0.0234	-0.0423	-0.00405	-0.0347	0.0409	0.119	1				
(10) *Lame duck*	0.0414	0.0263	0.0276	0.00293	-0.00228	-0.0586	-0.0771	0.0584	-0.0387	1			
(11) *Congress member*	0.0213	0.0199	-0.0681	-0.00997	-0.00174	0.0171	-0.00521	0.0223	-0.0334	-0.0237	1		
(12) *Pres_elec*	0.0263	-0.0511	0.105	0.0657	0.0510	-0.00910	-0.000962	0.0168	0.00352	0.00446	0.01000	1	
(13) *Gub_elec*	0.0242	0.0579	-0.00323	0.0222	-0.0130	-0.00572	0.00226	0.0427	-0.0104	0.424	-0.0257	0.0918	1

Table 4.5: Transfer allocation and partisan alignment

	(1) *ln Grants*	(2) *Δln Grants*	(3) *Δln Grants*	(4) *Δlln Grants*	(5) *Δln Grants*	(6) *Δln Grants*	(7) *Δln Grants*
y_{t-1}	0.957***		-0.253***		-0.264***	-0.266***	-0.266***
	(0.007)		(0.024)		(0.023)	(0.023)	(0.023)
Alignment	0.005*	0.006**	0.007**	0.007**	0.008**	0.008**	0.008**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
$\Delta ln\ Income_{t-1}$						-0.150**	-0.148**
						(0.061)	(0.062)
$\Delta ln\ Revenue_{t-1}$						0.029	0.030
						(0.027)	(0.027)
Δln Population						-0.476**	-0.480**
						(0.221)	(0.220)
Margin of victory						0.016*	0.017*
						(0.009)	(0.009)
Split						-0.001	-0.001
						(0.003)	(0.003)
Age							0.000
							(0.000)
Female							-0.002
							(0.009)
Lame duck							0.008
							(0.005)
No. of states	48	48	48	48	48	48	48
Year FEs?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-specific trend	No	No	No	No	No	No	No
R-squared	0.989	0.351	0.390	0.353	0.396	0.400	0.401
Observations	2,784	2,784	2,736	2,784	2,736	2,736	2,736

Notes: Fixed effect regressions (except for columns (1), (2) and (3), which are pooled OLS) using annual data for US states between 1950 and 2008. y_{t-1} in column (1) and $\Delta grants_{t-1}$ in all other columns. Variables are at time t unless stated otherwise. Robust standard errors, clustered at the state level * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 4.6: Pretreatment and posttreatment trends

	(1)	(2)	(3)
Alignment $(t-2)$	0.008		0.008
	(0.008)		(0.008)
Alignment $(t-1)$	0.002		0.003
	(0.007)		(0.007)
Alignment	0.010**	0.012***	0.015***
	(0.004)	(0.004)	(0.005)
Alignment $(t+1)$		0.000	0.002
		(0.007)	(0.007)
Alignment $(t+2)$		0.009	0.009
		(0.007)	(0.007)
Alignment $(t+3)$		0.011	0.012
		(0.008)	(0.008)
Alignment $(t+4)$		0.011	0.012
		(0.010)	(0.010)
No. of states	48	48	48
All controls?	Yes	Yes	Yes
Year FEs?	Yes	Yes	Yes
State-specific trend?	Yes	Yes	Yes
R-squared	0.401	0.402	0.402
Observations	2,736	2,736	2,736

Notes: The dependent variable, in all columns, is $\Delta ln\ Grant_{st}$. Fixed effect regressions using annual data for US states between 1950 and 2008. Variables are at time t unless stated otherwise. Robust standard errors, clustered at the state level, in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 4.7: Partisan alignment around elections

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
y_{t-1}	-0.022	-0.021	-0.041	-0.040	-0.263***	-0.265***	-0.265***	-0.265***
	(0.025)	(0.025)	(0.025)	(0.026)	(0.023)	(0.023)	(0.023)	(0.024)
Alignment	-0.001	-0.002	-0.003	-0.008	0.010**	0.010**	0.010**	0.014***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)	(0.005)
$Pres\_elec_{t-1}$				-0.012**				
				(0.005)				
$Alignment * pres\_elec_{t-1}$				0.015*				
				(0.009)				
$Pres\_elec_t$	-0.003	-0.005	-0.004	-0.008				
	(0.005)	(0.005)	(0.005)	(0.005)				
$Alignment * pres\_elec_t$	0.017**	0.017**	0.016**	0.021**				
	(0.008)	(0.008)	(0.008)	(0.008)				
Gub_elec_{t-1}								0.003
								(0.005)
$Alignment * Gub\_elec_{t-1}$								-0.010
								(0.008)
Gub_elec_t					0.015**	0.015*	0.015*	0.016**
					(0.006)	(0.008)	(0.008)	(0.007)
$Alignment * Gub\_elec_t$					-0.006	-0.006	-0.006	-0.010
					(0.008)	(0.008)	(0.008)	(0.009)
No. of states	48	48	48	48	48	48	48	48
No. of elections$^{\alpha}$	720	720	720	720	839	839	839	839
All controls?	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Year FEs?	No	No	No	No	Yes	Yes	Yes	Yes
State-specific trend?	No	No	Yes	Yes	No	No	Yes	Yes
R-squared	0.002	0.013	0.043	0.044	0.398	0.402	0.402	0.402
Observations	2,736	2,736	2,736	2,736	2,736	2,736	2,736	2,736

Notes: The dependent variable, in all columns, is $\Delta ln\ Grant_{st}$. Fixed effect regressions using annual data for US states between 1950 and 2008. Variables are at time t unless stated otherwise. All controls includes $\Delta ln\ income_{t-1}$, $\Delta ln\ revenue_{t-1}$, $\Delta ln\ pop$, *margin of victory*, *split*, *age*, *female* and *lame duck*. $^{\alpha}$ number of total presidential elections = 48 states * 15 elections, gubernatorial elections are specific individual elections. Robust standard errors, clustered at the state level, in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 4.8: Partisan alignment and swing states

	Swing=5pp margin					Swing=6pp	Swing=10pp
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Alignment	0.008**	0.006	0.006	0.006	0.006	0.005	0.005
	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)
Swing	-0.000	-0.006	-0.005	-0.005	-0.005	-0.003	0.001
	(0.004)	(0.004)	(0.005)	(0.005)	(0.004)	(0.004)	(0.005)
*Alignment * Swing*		0.014**	0.013**	0.013**	0.012*	0.011*	0.008
		(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
No. of states	48	48	48	48	48	48	48
No. of swing states	151	151	151	151	151	187	283
All controls?	No	No	Yes	Yes	Yes	Yes	Yes
State-specific trend?	No	No	No	Yes	No	Yes	Yes
Year FEs?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.396	0.397	0.401	0.401	0.397	0.397	0.401
Observations	2,736	2,736	2,736	2,736	2,602	2,736	2,736

Notes: The dependent variable, in all columns, is $\Delta ln\ Grant_{st}$. Columns (6) and (7) define swing states as states whose margin of victory was 6 and 10 percentage points, respectively. Fixed effect regressions using annual data for US states between 1950 and 2008. All controls includes $\Delta ln\ income_{t-1}$, $\Delta ln\ revenue_{t-1}$, $\Delta ln\ pop$, *margin of victory*, *split*, *age*, *female* and *lame duck*. Robust standard errors, clustered at the state level, in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 4.9: Partisan alignment with experienced politicians

	(1)	(2)	(3)	(4)	(5)	(6)
y_{t-1}	-0.264***	-0.266***	-0.266***	-0.264***	-0.266***	-0.266***
	(0.023)	(0.023)	(0.023)	(0.023)	(0.023)	(0.023)
Alignment	0.008**	0.008**	0.008**	0.009**	0.008**	0.008**
	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)
Congress member	0.008**	0.008*	0.008*	0.010**	0.009*	0.009*
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)
Alignment $*$ *Congress member*				-0.004	-0.003	-0.003
				(0.008)	(0.008)	(0.008)
Δln *Income*$_{t-1}$		-0.146**	-0.146**		-0.146**	-0.146**
		(0.062)	(0.062)		(0.062)	(0.062)
Δln *Revenue*$_{t-1}$		0.030	0.030		0.030	0.030
		(0.027)	(0.027)		(0.027)	(0.027)
Δln *Population*		-0.477**	-0.477**		-0.475**	-0.475**
		(0.218)	(0.218)		(0.218)	(0.218)
Margin of victory		0.016*	0.016*		0.016*	0.016*
		(0.009)	(0.009)		(0.009)	(0.009)
Split		-0.001	-0.001		-0.000	-0.000
		(0.003)	(0.003)		(0.004)	(0.004)
Age		0.000	0.000		0.000	0.000
		(0.000)	(0.000)		(0.000)	(0.000)
Female		-0.001	-0.001		-0.001	-0.001
		(0.010)	(0.010)		(0.010)	(0.010)
Lame duck		0.008	0.008		0.008	0.008
		(0.005)	(0.005)		(0.005)	(0.005)
No. of states	48	48	48	48	48	48
State-specific trend?	No	No	Yes	No	No	Yes
Year FEs?	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.397	0.401	0.401	0.397	0.401	0.401
Observations	2,736	2,736	2,736	2,736	2,736	2,736

Notes: The dependent variable, in all columns, is $\Delta ln\ Grant_{st}$. Fixed effect regressions using annual data for US states between 1950 and 2008. Variables are at time t unless stated otherwise. Robust standard errors, clustered at the state level, in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 4.10: Falsifaction tests and selection-on-observables

	(1)	(2)	(3)	(4)	(5)
Panel A					
y_{t-1}		-0.168***	-0.300***	-0.306***	-0.306***
		(0.019)	(0.022)	(0.021)	(0.021)
Alignment	-0.005	-0.005	-0.004	-0.002	-0.002
	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)
$\Delta ln\ Income_{t-1}$				0.300***	0.300***
				(0.066)	(0.066)
$\Delta ln\ Population$				-0.072	-0.072
				(0.232)	(0.232)
Margin of vitory				-0.024***	-0.024***
				(0.008)	(0.008)
Split				-0.003	-0.003
				(0.003)	(0.003)
Age				0.000	0.000
				(0.000)	(0.000)
Female				-0.004	-0.004
				(0.007)	(0.007)
Lame duck				-0.003	-0.003
				(0.005)	(0.005)
No. of states	48	48	48	48	48
Year FEs?	No	No	Yes	Yes	Yes
State-specific trend?	No	No	No	No	Yes
R-squared	0.001	0.021	0.556	0.563	0.563
Observations	2,784	2,736	2,736	2,736	2,736
Panel B					
Controls in the limited set	State FE Year FE	$\beta_L =$	0.007		
				$SR = \lvert\beta_F/(\beta_L - \beta_F)\rvert$	$= 7$
Controls in the full set	State FE Year FE Control variables	$\beta_F =$	0.008		

Notes: In Panel A the dependent variable, in all columns, is $\Delta ln\ Revenue_{st}$. Fixed effect regressions using annual data for US states between 1950 and 2008. Variables are at time t unless stated otherwise. Panel B reports OLS regression coefficients for *Alignment* and SR (selection ratio) based on the depicted formula. β_L refers to the coefficient of *Alignment* from a model that contains only state and year fixed effects, β_F refers to the coefficients of *Alignment* from a model that contains all control variables in addition to these fixed effects. The selection ratio indicates the extent of remaining selection bias due to unobservables relative to the observable variables in the model that would be necessary to drive treatment effect down to 0. Robust standard errors, clustered at the state level, in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 4.11: Democrat versus Republican alignment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
y_{st-1}	-0.264***	-0.266***	-0.266***	-0.018	-0.018	-0.018	-0.263***	-0.265***	-0.265***
	(0.023)	(0.023)	(0.023)	(0.026)	(0.026)	(0.026)	(0.024)	(0.023)	(0.023)
$D\_Alignment$	0.005	0.005	0.005	-0.002	0.002	0.002	0.004	0.005	0.005
	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
$R\_Alignment$	0.010**	0.010**	0.010**	-0.009	-0.013*	-0.013*	0.013***	0.012**	0.012**
	(0.004)	(0.004)	(0.004)	(0.007)	(0.007)	(0.007)	(0.005)	(0.005)	(0.005)
$Pres\_elec_{t-1}$				-0.016***	-0.012**	-0.012**			
				(0.005)	(0.005)	(0.005)			
$D\_Alignment * Pres\_elec_{t-1}$				0.001	-0.003	-0.003			
				(0.010)	(0.011)	(0.011)			
$R\_Alignment * Pres\_elec_{t-1}$				0.028***	0.028***	0.028***			
				(0.009)	(0.010)	(0.010)			
$Pres\_elec_t$				-0.008	-0.008*	-0.008*			
				(0.005)	(0.005)	(0.005)			
$D\_Alignment * Pres\_elec_t$				0.026**	0.022*	0.022*			
				(0.011)	(0.011)	(0.011)			
$R\_Alignment * Pres\_elec_t$				0.021**	0.023**	0.023**			
				(0.009)	(0.010)	(0.010)			
Gub_elec_t							0.015**	0.015*	0.015*
							(0.006)	(0.008)	(0.008)
$D\_Alignment * Gub\_elec_t$							-0.000	-0.001	-0.001
							(0.010)	(0.010)	(0.010)
$R\_Alignment * Gub\_elec_t$							-0.009	-0.009	-0.009
							(0.008)	(0.009)	(0.009)
No. of states	48	48	48	48	48	48	48	48	48
All controls?	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Year FEs?	Yes	Yes	Yes	No	No	No	Yes	Yes	Yes
State-specific trend?	No	No	Yes	No	No	Yes	No	No	Yes
R-squared	0.396	0.401	0.401	0.006	0.017	0.017	0.398	0.402	0.402
Observations	2,736	2,736	2,736	2,736	2,736	2,736	2,736	2,736	2,736

Notes: The dependent variable, in all columns, is $\Delta ln\ Grant_{st}$. Fixed effect regressions using annual data for US states between 1950 and 2008. There are 524 and 835 cases of Democratic and Republican alignment, respectively. Variables are at time t unless stated otherwise. Robust standard errors, clustered at the state level, in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 4.12: Sub-sample analysis

	<=1974	>1974
	(1)	(2)
Alignment	0.008	0.007**
	(0.007)	(0.003)
No. of states	48	48
All controls?	Yes	Yes
Year FEs?	Yes	Yes
State-specific trend?	Yes	Yes
R-squared	0.398	0.372
Observations	1,104	1,632

Notes: The dependent variable, in all columns, is $\Delta ln\ Grant_{st}$. Fixed effect regressions using annual data for US states between 1950 and 2008. Variables are at time t unless stated otherwise. Robust standard errors, clustered at the state level, in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 4.13: Other robustness checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
y_{t-1}	-0.266***	-0.266***	-0.266***	-0.266***	-0.266***	-0.266***	-0.266***
	(0.023)	(0.023)	(0.023)	(0.023)	(0.023)	(0.023)	(0.023)
Alignment	0.008**	0.008**	0.008**	0.008**	0.008**	0.008**	0.007**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
$\Delta ln\ Income_{t-1}$	-0.148**	-0.148**	-0.148**	-0.149**	-0.148**	-0.149**	-0.148**
	(0.062)	(0.062)	(0.062)	(0.061)	(0.062)	(0.062)	(0.062)
$\Delta ln\ Revenue_{t-1}$	0.030	0.030	0.030	0.030	0.030	0.030	0.030
	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)
$\Delta ln\ Population$	-0.479**	-0.480**	-0.481**	-0.487**	-0.479**	-0.470**	-0.482**
	(0.220)	(0.218)	(0.220)	(0.221)	(0.220)	(0.230)	(0.221)
Margin of victory	0.016*	0.017*	0.017*	0.017*	0.017*	0.017*	0.016*
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Split	-0.001	-0.001	-0.001	-0.001		-0.001	-0.003
	(0.003)	(0.004)	(0.003)	(0.003)		(0.003)	(0.004)
Age	0.000	0.000		0.000	0.000	0.000	0.000
	(0.000)	(0.002)		(0.000)	(0.000)	(0.000)	(0.000)
Female	-0.002	-0.002	-0.002	-0.003	-0.002	-0.002	-0.002
	(0.009)	(0.009)	(0.009)	(0.010)	(0.009)	(0.009)	(0.009)
Lame duck	0.008	0.008	0.008	0.008	0.008	0.008	0.008
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Pres birth state	0.003						
	(0.006)						
Age^2		0.000					
		(0.000)					
ln Age			0.008				
			(0.012)				
Military				-0.004			
				(0.003)			
Unified					0.001		
					(0.003)		
Senators						-0.002	
						(0.002)	
Southern Democrat							-0.009*
							(0.005)
No. of states	48	48	48	48	48	48	48
All controls?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-specific trend?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.401	0.401	0.401	0.401	0.401	0.401	0.401
Observations	2,736	2,736	2,736	2,736	2,736	2,736	2,736

Notes: The dependent variable, in all columns, is $\Delta ln\ Grants_{st}$. Fixed effect regressions using annual data for US states between 1950 and 2008. Variables are at time t unless stated otherwise. Column (1) controls for the president's birth state. Columns (2) and (3) alternatively model the impact of governor's age, first by including a square term and then by using the natural log transformation. Column (4) controls for military experience of the governor, *Military*. Column (5) replaces the state checks and balances variable to *Unified*, (6) controls for over representation of small states in the Senate by including the number of senators per capita and (7) accounts for Democratic governors located in the South. Robust standard errors, clustered at the state level, in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Chapter 5 Conclusions

This thesis analyses three distinct scenarios that are motivated by three major political economy events in the recent past, namely the 2014 Scottish independence referendum, the 2016 EU referendum and the 2016 US presidential election. By taking cues from these events it allows this thesis to contribute to the existing literature in the most meaningful way possible. The first empirical chapter examines why countries tend to be more decentralized than others, which has implications in explaining what drives the decentralization of fiscal powers, like in Scotland. The following empirical chapter is prompted by the Brexit vote. By examining the impact of the government's pro-remain impact leaflet across assorted demographic groups, the evidence shows results in line with expectations. This includes a distinction across partisan groups i.e. separate analysis on Conservative and Labour supporters. The subsequent chapter, chapter 4, looks at whether a centralized policy maker can exploit a decentralized system of governance for partisan gain at elections. By using evidence from the US, this chapter provides insights into how the president may seek to consolidate power for his party.

5.1 Summary of findings

Chapter 2 presents analysis on the drivers of fiscal decentralization with a focus on the role of ethno-linguistic diversity. Using a new dataset of decentralization measures, that better capture the true degree of sub-national decision making authority within a given country, I show that increasing the level of diversity leads to a increase in more fiscal decentralization. When accounting for the depth of cleavages between

ethno-linguistic groups I find that is the more superficial cleavages that are behind this relationship. Whereas the perennial cleavages bear no impact on the degree of decentralization. In order to overcome endogeneity concerns, mainly because of reverse causality and possible omitted variables, I employ an instrumental variable methodology. I use the origin of anatomically modern human life in a given country as an instrument for long-run diversity.

In chapter 3 I investigate how the government's leaflet impacted voting behaviour in the 2016 EU referendum. The leaflet was sent to all UK households, however, not every individual was exposed to this source of information. Using individual-level data from the British Election Study, this chapter implements matching and difference-in-difference identification strategies to identify a causal effect of leaflet exposure. I find that exposure caused an individual to become 3 percentage points more likely to vote remain. I split the sample between those with high and low exposure to other sources of referendum information. Here, I distinguish between several demographic groups that differ in their susceptibility to persuasion bias. I find that the result is driven by groups more susceptible to persuasion bias who had a low level of exposure to other information. Also, this chapter shows that Conservative partisans who were exposed with a high amount of other referendum information are even more likely to vote remain, which is because they observed splits in their party and interpreted the leaflet as a signal to vote remain from the Conservative government. There is also evidence that shows the leaflet was a persuasive tool changing leave votes into remain ones, rather than increasing the remain vote turnout. The mechanism behind this persuasion argument is based on exposed individuals becoming more informed about likely scenarios in the event of a leave vote.

The final empirical chapter, chapter 4, explores partisan alignment in the US over the period 1950 – 2008. In particular, partisan alignment is when the president and state governor are simultaneously in office and of the same party. I exploit the quasi-randomness of alignment in the US to identify the alignment effect. I show

that when aligned, the president increases the growth rate of grants to a co-partisan state by 0.8 percentage points. When examining why this occurs, I focus on an electoral incentives argument. By interacting the alignment variable with the electoral calendar, I find that funds are disproportionately increased in election years. To further this argument, the paper examines the role of swing states. There are electorally critical states in presidential elections. Here, funds are again increased to a greater extent when a co-partisan governor is in place. As another facet to this chapter, and contribution to the literature, I analyse the role of governor's personal traits in determining the amount of grants received and find that those governors with prior political experience in congress receive more, irrespective of their partisanship. This is a result of their superior lobbying skills. All in all, the results are in accordance with the president increasing funds to co-partisans in order to benefit electorally from positive vertical spill-overs in the performance of government.

5.2 Policy implications, limitations and future research

The results of chapter 2 indicate that a more ethno-linguistically diverse population positively impacts the level of fiscal decentralization in a given country. The findings are not only of interest to social scientists, but to the general population because they offer a reasoned projection of how the world will develop. As the world continues to become a more globalised place, where groups of people are able to migrate with increased fluidity, there should be a continued push toward more regional autonomy. This reaction to increasing diversity is necessary for local authorities to integrate those out-groups into a modern society (Alesina and Ferrara, 2005). In this sense, the work is limited by looking only at the cross-country level. More information on the impacts of diversity may be revealed by using a more disaggregated within country approach. For instance, Montalvo and Reynal-Querol (2017) use the case of Africa to reveal that city-level diversity has a positive effect on economic growth, whereas

no relationship is found when using a country as the unit of analysis. Moreover, more work can be done to disentangle the channels of impact that diversity affects decentralization.

The following chapter finds that exposure to the UK government's leaflet caused individuals to become more likely to vote remain in the 2016 EU referendum. All previous work focuses on correlations and associations of why people voted leave or projections of the impact of leaving the EU. This chapter, however, offers the first evidence from a quasi-natural experiment in context of the referendum. The implications of this research are particularly pertinent due to the novel setting studied. Future political, or referendum, campaigns may be more focused on those demographics that are more liable to persuasion bias in order to elicit their votes. The drawbacks of this research are that it considers only a single source of referendum information. Throughout the campaign there was a wealth of sources arguing different things about what leaving the EU may mean for the UK. Portions of the news were considered to be inaccurate and would therefore warrarnt further work to examine the impact of this. New research in this area about who believes 'fake news' (defined as news stories that are intentionally and verifiably false) in context of the 2016 US election has been explored by Allcott and Gentzkow (2017). In terms of other areas of future research, given that the literature is in the very early stages there is ample scope for future work. Other interventions such as the televised debates or the murder of Jo Cox MP may prove to be fruitful avenues in contributing to the understanding of how a leave vote prevailed.

The third empirical chapter reveals that, in the US, the president shows a bias towards co-partisan governors in order to benefit from vertical spillovers of performance and increase his party's chances of re-election. Naturally in this case, the connotations of this scenario are a malapportionment of federal monies, which creates an inefficient allocation of resources. As a remedy to this, policy should focus on a re-structuring of presidential power. Perhaps a blind system that continues to allow governors to lobby for federal transfers but through a channel which hides all

information that may reveal who is applying for the funds. The blind system should allow funding to reach projects based on merit alone. However, as this research focuses solely on the case of the US, the implications may not be applicable to other countries. Not only may this work not apply to other mature democracies, but it is unlikely to be correct for autocracies or even less mature democracies due to the lower level of constraints placed on the executive branch. It would also be interesting to assess the welfare implications of a misallocation of resources because of alignment. Are funds diverted from one states welfare budget into an aligned states welfare budget? Or, are the extra funds channelled to the states in one particular classification of transfer? Furthermore, one may ask whether the alignment affect is still present at lower tiers of governance. To use the US case again, it would be interesting to investigate whether state governors award more funds towards co-partisan city mayors to benefit their party in gubernatorial elections.

In summary, each chapter within this thesis separately contributes to the various relevant strands of literature in topics such as state development, electoral cycles and political preferences. There are also implications for behavioural economics and the mass media literature. In its entirety, this thesis builds upon and develops existing scholarly research on the political and public economics literatures. Whilst there are limitations to the research presented in this thesis, several future avenues have been discussed for the literature to develop understanding further. The implications are of this research are important for policy makers and social scientists alike, due to the motivation and framing of each chapter.

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