

# When politicians collude with firms

The impact of elite networks on inequality

**Vuk Vukovic**

Pembroke College

Department of Politics and International Relations

University of Oxford

Thesis submitted in partial fulfilment of the requirements for the degree of DPhil  
in Politics in the Department of Politics and International Relations at the  
University of Oxford

Under the supervision of  
Prof Ben Ansell  
Nuffield College, Oxford

[Word count: 78204]

January 2019



## Abstract

This thesis examines the effect of elite networks on inequality. Elite networks are informal social networks between politicians in power and top executives of politically connected firms where personal ties and long-term interactions build trust and loyalty between involved actors. Both groups draw benefits from these interactions. Politicians stay in power and may extract bribes and other favours, while firms are rewarded with exclusive government contracts, favourable regulation, and direct subsidies. Top corporate executives that are successful in acquiring these rents, as a consequence of their elite network interactions, are rewarded by their firms with higher salaries. This consequentially widens the dispersion of earnings between the top 1% of income earners (most of which are corporate executives) and everyone else. The contribution of this thesis to the political economy literature is twofold: (1) it examines, on an individual level, the consequences elite network interactions have on its members; increasing re-election chances for politicians and securing rents for firms, and (2) it examines the consequences elite networks impose on society, notably on increasing income inequality. It presents a thus far unrecognized factor that may help explain the rise of income inequality in the US and the UK. The thesis is organized into three stand-alone papers. The first two papers show the benefits of collusion for politicians and firms, while the third one links the impact of elite networks on inequality. The first paper shows that politicians which distribute fraudulent procurement contracts increase their chances of re-election. The second paper shows that politically connected financial firms were favoured in the TARP allocation process during the US financial crisis. The third paper shows that politically connected senior executives have significantly higher earnings than non-connected ones.

*Keywords:* Elite networks, politicians, firms, inequality



# Contents

<b>Contents</b>	<b>i</b>
<b>Acknowledgements</b>	<b>iv</b>
<b>1 Introduction</b>	<b>1</b>
1.1 Elite networks: informal mutually-beneficial interactions between political and business elites . . . . .	1
1.2 The logic of elite networks . . . . .	17
1.3 Why should elite networks have an effect on income inequality? A historical argument . . . . .	26
1.4 Methodological approach to measuring the impact of elite networks	32
1.4.1 Structure of the thesis . . . . .	35
1.4.2 Corruption and re-election . . . . .	37
1.4.3 The politics of bailouts . . . . .	40
1.4.4 Elite networks and inequality . . . . .	42
<b>2 Literature review</b>	<b>45</b>
2.1 How firms and politicians benefit from collusion? . . . . .	46
2.2 Economic reasons behind the rise of inequality . . . . .	52
2.3 Political factors that could help explain the rise of inequality . . . . .	58
<b>3 Corruption and re-election</b>	<b>68</b>
3.1 Introduction . . . . .	69
3.2 Do voters punish corruption? . . . . .	73
3.2.1 Elite networks, corruption, and non-linearity . . . . .	77
3.2.2 Why Croatia? . . . . .	81
3.3 Data . . . . .	83

3.4	Measuring corruption in public procurements . . . . .	86
3.4.1	Defining the proxies . . . . .	88
3.4.2	Independent validation of the proxies . . . . .	96
3.5	Does corruption affect re-election? . . . . .	99
3.5.1	Results . . . . .	103
3.5.2	Justifying the functional form . . . . .	109
3.5.3	Placebo tests and robustness checks . . . . .	111
3.5.4	Solving endogeneity issues . . . . .	114
	Background: council size, corruption, and the exclusion re-	
	striction . . . . .	116
	Estimation with 2SLS . . . . .	122
3.6	Conclusion . . . . .	128
	Appendix . . . . .	131
	Bibliography . . . . .	145
<b>4</b>	<b>The politics of bailouts</b>	<b>160</b>
4.1	Introduction . . . . .	161
4.2	Political connections and bailouts in the context of the 2008-09 US	
	financial crisis . . . . .	168
4.2.1	The allocation of the TARP . . . . .	168
4.2.2	The role of personal connections . . . . .	172
4.3	Data and variables . . . . .	175
4.3.1	Bailout data and covariates . . . . .	175
4.3.2	Measuring lobbying and political connections . . . . .	179
4.4	Empirical strategy and results . . . . .	183
4.4.1	Selection on observables: OLS and matching . . . . .	184
	Matching and OLS results . . . . .	187
4.4.2	Imposing randomization: regression discontinuity design . . . . .	190
	RDD results . . . . .	197
4.4.3	Instrumenting for connections using winners of close elections	202
	IV validity checks . . . . .	205
	IV estimation and results . . . . .	208
4.4.4	Discussion of results and different estimated effects . . . . .	212
4.5	Conclusion . . . . .	215
	Appendix . . . . .	217

Bibliography . . . . .	225
<b>5 Elite networks and inequality</b>	<b>233</b>
5.1 Introduction . . . . .	234
5.2 Theory: elite networks and rent-seeking firms . . . . .	240
5.3 Data and variables . . . . .	246
5.3.1 Measuring networks and connections of individuals and firms	249
Network size . . . . .	249
Political connections . . . . .	250
Firm-level political connections . . . . .	254
Correlation between political connections and network size .	255
5.3.2 Inequality in top executive incomes . . . . .	257
5.4 Empirical strategy . . . . .	263
5.5 Results . . . . .	268
5.5.1 United States . . . . .	268
5.5.2 United Kingdom . . . . .	277
5.6 Conclusion . . . . .	286
Appendix . . . . .	288
Bibliography . . . . .	294
<b>6 Conclusions and implications</b>	<b>302</b>
<b>Bibliography</b>	<b>310</b>

# Acknowledgements

I want to thank, first and foremost, my mentor and supervisor Prof Ben Ansell for his invaluable advice, comments, criticisms, and the many suggestions that help made this thesis into what it is today. I would also like to thank a number of people whose advice and detailed comments helped me with various parts of the thesis: Andrew Eggers, Robin Harding, Elias Dinas, Jacob Nystrup, Josip Glaurdic, Alan Krueger (†), Joshua Angrist, Dejan Kovac, Jacob N. Shapiro, Jurica Zrnc, and Victor Menaldo. All the papers in the thesis have been presented at various conferences, such as the 2017 and 2018 American Political Science Association Annual Meetings in San Francisco and Boston (panels on political economy), the 2018 PIIRS Conference at Princeton University, the 2017 Nuffield Politics Graduate Student Conference at Oxford, etc. I thank all the participants at these conferences and panels for valuable comments and insights. Also, the findings of the first paper on corruption and local government in Croatia have been presented at the Croatian Parliament, in front of the National Council for Monitoring the Anti-Corruption Strategy in November 2017. I thank the members of the Council for expressing interest in the research that came as a result of this thesis.

I would also like to thank Mile Sikic and Marko Rakar for extracting parts of the original procurement database for the first paper, as well as to all the interviewees who kindly agreed to talk about their experience in tracking corruption in the public procurement process in Croatia. For the second paper I would like to thank the Center for Responsive Politics for access to their online database on lobbying and campaign donations. Finally, a special thank you to Damon Silver from the TARP Oversight Subcommittee as well as to other interviewees involved in oversight or decision-making regarding the allocation of TARP funds, all of which wished to remain anonymous. I am indebted to all of these individuals and institutions for their valuable and indispensable help in various parts of the thesis. All errors and mistakes remain mine.



# Chapter 1

## Introduction

### **1.1 Elite networks: informal mutually-beneficial interactions between political and business elites**

What does the collusion between political and corporate elites look like? Consider the example of the infamous Democratic Party political machine Tammany Hall that dominated New York politics during the 19th century. This group of politicians did not just collude with business interests — they merged politics and business into one, benefiting vastly from their accumulated position of political power. Tammany Hall was an organization notorious for its patronage systems and outright corruption. It built a massive organization of loyal supporters, making sure that its members were well-placed within the system to obtain full control. Their actions included everything from direct bribery, extortion, budgetary mis-

appropriation, vote-buying, nepotism, even directly buying judges and thwarting any sign of reform of its deeply corrupt system of governance (Riordan, 1995).

The most famous leader of this organization was William “Boss” Tweed. Although he was only a mere member of the New York State Senate, his tenure as Tammany boss landed him immense political power, giving him direct control over the New York job market, as well as the voting process itself. Tweed was a large landowner in the city, director of a railroad company, a bank, a printing company, and a hotel. The law did catch up to him eventually. In 1877 he was convicted for stealing between \$25 million and \$45 million (which in today’s inflation-adjusted terms would be over \$1 billion) (Ackerman, 2005). Other notorious examples of Tammany Hall political-made millionaires were John Kelly and his successor Richard Croker, who were running the machine like a typical criminal organization, or the case of George W. Plunkitt who admittedly bought land he knew the city was planning to buy, and sold it back to the city for an inflated price. It was, in his own words, “honest graft” (Riordan, 1995).

On the corporate side, a good example would be the 19th century US industrialists, or pejoratively — robber barons, infamous for using political protection and corruption to build massive monopolies (in oil, steel, transportation, banking, etc.). Rockefeller, Carnegie, Vanderbilt, Gould, Fisk, Sage; they all used similar practices of extortion, dumping pricing, stock market frauds, and forming cartels to crush competitors and fortify their monopoly status. In fact, many of them,

living in New York at the time, required close collaboration with Tammany Hall bosses to achieve their goals. For example investors and speculators Gould and Fisk teamed up with Tweed to buy judges and legislators in order to get special legislation passed that favoured their interests or bankrupt the companies they betted against. There was hardly any difference between the modus operandi of Tammany bosses and any robber baron during America's Gilded Age.

Examples such as these should be impossible to find in modern-day United States. However, the recent case of Robert Rizzo, a city manager of a small town Bell in California suggest otherwise. Rizzo was in power for 17 years in this poor, mainly Hispanic community with a population of slightly over 35,000 people. During this time he managed to consistently misappropriate public resources for his own benefit and for the benefit of his small group of business and political cronies. He paid himself a legal salary almost twice as high as that of the US President (\$787,000 per year), and four times as much as the state's governor, in addition to a series of other corrupt activities, earning him a 12 year prison sentence (Bueno De Mesquita and Smith, 2011). A similar case of political bribery and misuse of power to benefit a small group of people was that of former Illinois governor Rod Blagojevich, who ended up with a 14 year prison sentence. His fellow party member and longest serving Chicago mayor Richard Daley avoided prison but his reign was marred with significant controversies of allocating city contracting jobs to personal friends and political allies.

Similar examples can be found on a much higher level. The connections of Wall Street executives and politicians in power are much more subtle, but that does not make them any less malign than a relationship between a corrupt mayor and his cronies, or a kleptocratic ruler and his oligarchs in any post-communist society. In fact, lobbying activities, super PACs, or the revolving door practice of former staffers working for industry giants are all completely legal and even fully legitimate ways for various individuals and organized groups to pursue their interest. In this pursuit some groups will fare better than others. According to Olson (1971) because of their inability to solve the collective action problem the latent (unorganized, dispersed) groups will lose out at the expense of the privileged (organized, narrow) ones, thus skewing the distribution of resources towards groups with more power and with a more narrowly defined set of interests. The question is does the pursuit of one's interests hamper in any way market outcomes, benefiting the few at the expense of the many?

In this thesis I seek to make a contribution to the political economy literature in examining to what extent can interactions between political elites and various corporate interests aligned with them — interactions that lead to the formation of *elite networks* — explain the dispersion of earnings among top income earners and hence offer a novel perspective on the implications behind higher inequality in selected Western democracies. The aforementioned cases, all coming from the world's most prominent example of an institutionally well-defined democratic order,

suggest that even the richest and most advanced democracies are not immune to the behaviour of elite networks.

## **Hypothesis**

Figure 1.1 sums up the general hypotheses and assumptions made in the thesis: informal social networks are created between politicians in power and top executives of politically-connected firms so as to benefit all agents within the network. Politicians satisfy their interest by staying in power and extracting bribes and other favours, while executives manage to get exclusive government contracts and favourable regulation for their firms. Based on acquiring these rents the firms reward their top executives with higher salaries which consequentially increases the level of income inequality in a country.

Rents for firms correspond to the classic Tullock (1967), Stigler (1971), and Krueger (1974) definitions of rent-seeking; when firms use their connections to the government to gain a significant advantage over its competitors, be it through direct government subsidies and contracts, through favourable legislation and regulation, or by gaining monopoly or oligopoly status in their industries. Rent-seeking by definition reduces the allocative efficiency of the market and reduces wealth-creation.

A theoretical link between rent-seeking and inequality has been established in the literature. Dabla-Norris and Wade (2001) formalize a model where only

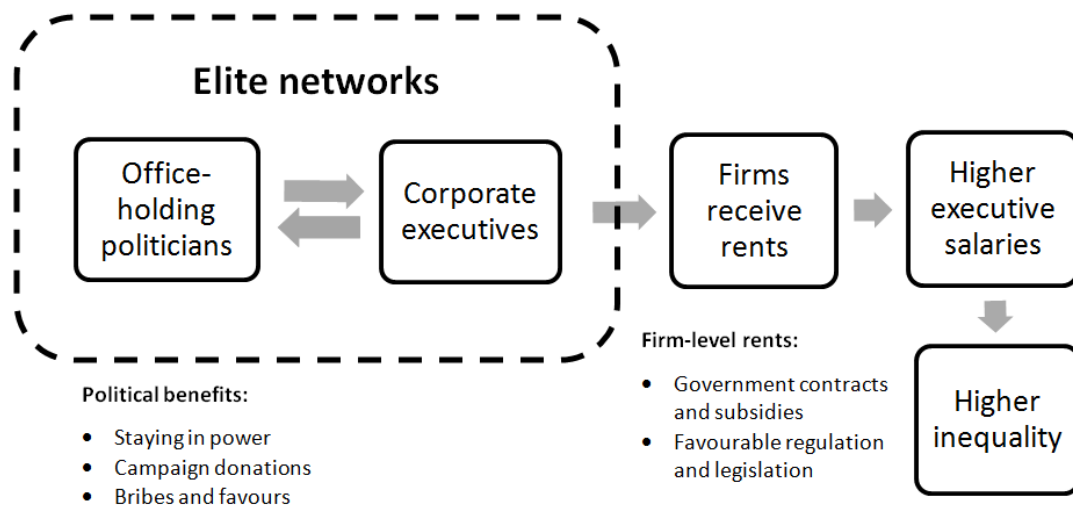


Figure 1.1: The interaction between political and business interests within elite networks provides another, thus far unrecognized, avenue for explaining higher levels of inequality.

wealthy agents engage in rent-seeking to protect their wealth. Stiglitz (2012) argues that the top 1% in the US mostly originate from rent-seeking activities instead of market innovations, while Furman and Orszag (2015) add some empirical backing to this story by finding that the most profitable firms who pay the highest salaries and thus increase between-firm inequality base these high concentrated returns not on innovation but on rent-seeking. Finally, Lindsey and Teles (2017) use case studies of regulatory capture to show how stifling competition only results in a redistribution of wealth towards the elites.

The biggest contribution of the thesis is to observe this relationship empirically on an individual level — the connections between individual politicians and individual corporate executives. The goal is to see if rent-seeking activities, which

arise as a consequence of agents forming informal elite networks, result in higher incomes for the top income earners.

Is this hypothesis justified in the inequality literature? The historical roots of inequality have been documented by Scheidel (2017). His argument is that high levels of inequality are a persistent socio-economic phenomenon that can only be temporarily reduced via violent events like wars, revolutions or epidemics. What we noticed in the post-WWII period was therefore only a temporary decline of what is otherwise permanently high inequality. Things did change however, as Piketty (2014) notes that the wealthy of the second half of the 20th and the 21st century are no longer rentiers living off land and inherited wealth, but corporate executives, or as he calls them “supermanagers”, living off their abnormally high salaries. In this thesis I show that such high salaries can, in part, be explained by the executives’ political connections through which they bring rents to their firms and get rewarded for it.

Levels of inequality, however, differ among countries. Therefore, one prediction arising from this hypothesis is that we should observe higher levels of inequality in every society where corporate interests are heavily interlinked with governments and political power. Societies where firms grow on political rents which they acquire as a consequence of personal relationships between their executives and political decision-makers should have higher levels of inequality (measured as the income share of the top 1%) than societies where this practice is limited.

This prediction, however, will not be tested empirically across countries. Rather the thesis has the goal of explaining how modern elite networks function within countries, how they benefit their inter-connected participants, and finally to measure the extent of their influence on the increasing disparities in earnings.

### **Does it pay off to be a part of the network?**

An elite network is a group connecting political elites to a number of powerful individuals within a specific political jurisdiction, be it the entire country, region, city, or municipality. Individuals within such groups are all bound by their common interest: preservation of their position of power. Elite network members, in addition to politicians, can either be interest groups in the classical sense (represented by lobbyists), wealthy individuals with a specific agenda that are usually main campaign donors to the politician in power, or individual private sector firms (their owners and CEOs, particularly executives that held elected office or other positions in government prior to their corporate career). The focus of this thesis will be on the role corporate executives and their firms play in an elite network.

All of these members have an incentive to be a part of a politician's close network from which they are free to extract non-market rents. Firms get them in terms of exclusive procurement contracts, interest groups get them in terms of favourable industry-wide or group-wide legislation and regulation, and campaign donors get them in terms of personal favours they are free to demand from politi-

cians. This is therefore a network model of cartelization (similar to how Schoenman (2014) describes network creation in post-communist societies) where even though the agents compete against each other for government favours, they are all fully aware of the benefits of cooperating with each other to achieve individualized-level support from the government. In a co-dependent system where support and loyalty are traded for concessions, those linked with the politician, who have a lot to lose from his or her demise, will make sure he or she stays in power for as long as possible.

Politicians also have an incentive to form and maintain such networks. They too can enjoy multiple benefits. They get important campaign contributions, crucial to their political survival, they can take bribes for favourable legislation or exclusive procurement contracts, they can get in-kind benefits (dinners, golf games, sports or theatre tickets, various gifts), or can even earn money by giving speeches at industry events. After their political careers some get rewarded by taking high-level executive or board positions in the companies they used to regulate.

However, there are costs associated with elite network membership. For every agent the most important are reputational costs. If the rent-seeking activities of the network are uncovered, each agent suffers a cost to his or her reputation even if the activity is not illegal per se. If the activities are illegal, there is a potential cost of prison time, which increases the reputational costs before even engaging in the activity. Each agent also has the cost of effort (which includes the costs of

entry). Being part of an elite network means going to a lot of social gatherings and activities, thereby spending time to build the relationship. This may last for several years, even decades, and it may yield physical costs (e.g. for buying gifts) along with the costs of time and effort. Politicians, in addition to all of the above also have the costs of potentially losing an election if any of their shady activities are uncovered.

The potential benefits from joining an elite network to engage in rent-seeking activities have to outweigh all the potential costs for each individual agent in order for them to join. This means that not every person will join an elite network, just the ones who, according to Becker (1968), value the potential benefits more than all the potential costs. This is further discussed in section 1.2 examining the logic behind elite networks.

### **The role of institutions**

Every political system is based on upholding power. Therefore the logic of elite networks should be similar (albeit not identical) across different political systems. However the variation in outcomes in curbing this power differ with respect to differently defined institutions which is conditioned by historical patterns of development and a whole number of other endogenous factors. This is why we notice different consequences of elite network behaviour in different institutional surroundings. But not everything is down to institutions.

Consider the puzzle of why voters sometimes fail to punish corruption (Rundquist, Strom and Peters, 1977; Kurer, 2001; and Winters and Weitz-Shapiro, 2013). The institutional argument fails to explain why corruption tends to exhibit high levels of variation among the highly-developed institutionally stable and inclusive societies. The United States, Japan, France or Ireland for example exhibit higher rates of corruption than the equally institutionally well-endowed Scandinavian countries, New Zealand, Canada, Germany, or the Netherlands. The average gap between the former and the later group of countries is 14 points on the Transparency International CPI index for 2016 (Transparency International, 2016). This is almost the same average gap as between the former group and, for example, Costa Rica, Spain, Georgia, Latvia, and Cyprus (TI, 2016). All of the aforementioned countries fare much better than the low-income countries in terms of corruption; however the variation among the first two groups suggests that institutions are hardly the only explanation.

In order to account for this variation in political economic outcomes even across rich democracies I revisit Olson's (1971; 1982) idea of organized special interests with an important addition: interest groups are not necessarily limited to organized groups in the classical sense (such as labour unions, industry lobbies, the NRA, AARP, etc.) but also as groups of powerful individuals that collude together to preserve their status of power. The stronger the links between them, the more powerful they become. I predict that this accumulation of power among

elite networks will make them more successful than any other group in achieving their goals which could explain the existence (and persistence) of corruption and favourable legislation given to certain industries and/or individuals. An additional negative consequence of elite network behaviour will be that it generates pressures on income inequality. When corporate executives reap competitive advantages for their firms based on their proximity to politics, they get rewarded with higher compensation. Well-defined institutional rules and even a strong legal system might not be enough to prevent such practices and their consequential outcomes in every country. In other words, institutions matter, but institutions are not always fully robust to elite networks.

### **Research questions**

The analysis will revolve around three key research questions:

1. How do politicians use corruption to keep themselves in power? More specifically, how do politicians benefit from being part of elite networks by giving concessions to firms tied to them? If stronger connections with key agents increase their chances of re-election, it is rational for every politician to act accordingly in order to achieve their main goal of political survival. This research question will be examined using the example of local public procurement allocation decisions and how they affect re-election chances of mayors in Croatia, the newest EU member state.

2. How do firms' political connections help them gain favourable policy outcomes? More specifically, how do individual competing agents benefit from being part of elite networks? If more lobbying, campaign donations, and better connections to politicians result in greater benefits in terms of favourable policies and legislation, it too is rational for every firm to engage in such activities and be more closely connected to politicians. This research question will be tested on the example of political connections of the US finance industry during the administration of the bailout (TARP) funds during the 2008-2009 financial crisis in the United States.
  
3. What role do personal connections and social networks have in explaining top executive compensation? The third paper makes the final link towards providing an explanation for higher levels of income inequality in countries where top corporate executives benefit from being members of elite networks through their higher earnings. According to the inequality literature (Piketty, 2014; Atkinson 2015; Atkinson et al, 2017; Milanovic, 2016, etc.) the biggest culprit for the rising levels of inequality in the last part of the 20th century has been the rise in salaries of top managers. I use individual and firm-level data in the US and the UK over a time span of 16 years and show that politically connected executives as well as firms with politically connected executives pay out higher managerial salaries than non-connected executives and non-connected firms.

Answering all three research questions will bring us closer in (1) uncovering the modus operandi of elite networks — the benefits both politicians and firms receive if they are connected together, and (2) examining the consequence of this relationship on the widening disparities in the distribution of income. Each question therefore addresses one important piece of the puzzle that testifies to which extent elite networks matter in explaining the rise of inequality. The thesis is organized into three papers, each tackling one particular issue. After the first two papers uncover the obvious benefits politicians get from being connected to rent-seeking firms, and that firms get from being connected to politicians, the final paper uncovers how the same type of relationship affects income inequality by driving compensations of top corporate managers.

### **Elite theory in sociology**

The definition of elite networks used throughout this thesis is somewhat similar to what classical sociology refers to as *elite theory*. According to elite theory a very small minority of economic and political elites holds the majority of power and influence within a society. The origin of power is derived either from one's position in the corporate hierarchy, or from one's position of control over wealth-creating activities. It is through these positions that elites determine who will hold political power, or at least try to influence policy-making. Accordingly, a democratic system is often criticized by elite theorists as incapable to curtail the

power of the elites. Similar to Olson's (1971) logic of group behaviour, elites are successful because they represent a well-organized group with a narrow interest, whereas the non-elites are disorganized and hence unable to challenge the elites.

The origin of classical elite theory can be found in the works of Max Weber, Vilfredo Pareto, Gaetano Mosca, and Robert Michels. From Weber (1978[1922]) elite theory draws the definitions of power and the three forms of domination — charismatic, traditional, and rational-legal — through which power relations are formed within a society. From Pareto (1935) it determines elite selection: members of an elite originate from proximity to economic or political power, which is enabled by political connections of families and inherited wealth. In other words if social mobility would be unrestricted within a society, elite members would be drawn from a pool of its most competent members. However, because of unfair advantages driven by family wealth and connections, the selection of new elite members is skewed in favour of the current elite. Mosca's (1939) contribution is similar to Olson's interest group theory, in that the elite is a well-organized tiny minority, as opposed to an unorganized majority ("the masses"), and therefore has an advantage in achieving its own interest. He also discusses the intellectual and moral, in addition to economic, superiority of elites over the masses. Finally, Michels (1962[1915]) coined the phrase the "iron law of oligarchy" to define the existence of ruling elites within any democratic organization, be it the state or the firm. He discusses the role such elites play within organizations, where they

control both information and resources, therefore enabling them to concentrate power. Elites have been a focus point of many contemporary research efforts ever since in sociology, anthropology, and political science (e.g. Mills, 1956; Dye, 1976; Gilens, 2012). What most of them have in common was to understand who the elite is (its composition), how does one become a member of an elite (its selection), and more importantly, does the very existence of elites hamper the idea of pluralism and democracy itself (its implications).

My approach differs from the aforementioned sociological definitions in that I do not draw implications of elite behaviour on the functioning of democracy, nor am I focused on explaining within-elite selection. Rather, I am concerned with showing how elite networks benefit the actors within them — how they help politicians stay in power, and how they help firms gain a competitive advantage. Furthermore I find politicians at least equally important (if not even more important) as the corporate elites. The position of political power carries significant impact over the corporate sector, even when politicians originate from or are appointed by the corporate sector, as elite theory would assume. Nowhere is this more obvious than in autocracies, however even in democracies proximity to political power is a crucial part of what makes membership of an elite network worthwhile. Finally, I am interested in the consequences of elite network behaviour on the distribution of top incomes through a very specific mechanism — the effect of political connections, which is a proxy for elite network membership, on executive salaries.

## 1.2 The logic of elite networks

The pursuit of self-interest is rational, legitimate, and expected within a democratic order. When politicians and special interests realize they can both benefit by colluding with each other, their rational and legitimate response is to do so. Every citizen within a democratic order has the right and is encouraged to pursue his or her own interests. Every individual indeed does so, in almost all cases as a part of a wider group: family, friends, school, club, society, religious organization, business organization, political party, etc. The interests can be entirely altruistic where a person gets satisfaction purely from helping others (e.g. religious organizations or charities), or they can be entirely selfish where a person gets satisfaction from furthering his or her own goals, or the goals of those close to them (e.g. winning office, getting a better job/salary, etc.). In such an environment it is natural to expect both cooperation and conflict to occur, with varying rates of success between groups.

A society, therefore, is a complex web of interactions between a multitude of agents organized through various groups all promoting their own interests, whether selfish or altruistic. Some are more some are less successful at doing so, which means that some groups will over time acquire more power and influence than others. People with wealth and power in democracies arise as a consequence of such a dynamic exchange, and once they reach wealth and power, they have every incentive to preserve it. This is an evolutionary behavioural trait of humans: being

well off compared to those around creates an incentive to maintain this relationship. Being worse off creates an incentive for change.

The contemporary divide between the rich and the middle classes is a natural consequence of a society in which individuals and groups fight for their own interests. This ‘fight’ is a euphemism. The fight itself is what Adam Smith (1904[1776]) described as a persistent interaction between self-interested individuals which will all, as if being led by an invisible hand, act to promote the collective interest.

However success and power vary between groups. Successful groups will be able to skew the distribution of resources towards themselves. Successful groups, with a greater stake in maintaining their position of power, will have a greater incentive to shape systemic rules and incentives. A good example of this is given by the elite-competition theory of democratization in Ansell and Samuels (2014). They argue that democratization did not commence as a consequence of fear from uprising by the unorganized and dispersed poor who wanted political rights (as suggested by Acemoglu and Robinson, 2006), but rather, building upon Olson’s logic, from the well-organized and concentrated wealthy who lacked political representation and feared expropriation of their newly acquired wealth by the state. Successful groups emerged to form a new elite.

Have the elites in some way conspired to produce such outcomes? I reject the premise of a conspiracy theory. Elite behaviour is not due to some grand intelligent design. Each self-interested agent or group forms social networks with

others and acts within them to further their interest. Be it businesses, civil society groups, or the government itself. They formulate clusters within a network and interact to determine what is best for each of them. Such clusters spontaneously arise throughout the system and over time congest it. Each group is prepared to vigorously defend its actions under the justification of protecting the interests of its members and/or their ideological worldview. No group will ever consider that the furthering of its interest will in any way harm the rest of society.

This is why there can be no conspiracy and no grand design, just random sets of clusters that are highly connected and interdependent and that have a strong incentive to preserve their interest. There is no ‘enemy’ in the classical sense. People with power are, after all, only temporary. There is a persistent flux and dynamism between those who currently hold power, those who held it 20 years ago, and those who will hold it in 20 years time. However, in each case no matter who holds power and who the elite is they will always express the same behavioural pattern — self-preservation.

The formation of elite networks within a country is therefore entirely spontaneous, serving to promote the narrow interests of its members, and limited in most cases geographically (around the centres of money and power). An elite network is essentially a social network characterized by high levels of positive assortativity — the tendency of highly-connected and powerful individuals to be closely connected with other highly-connected and powerful individuals<sup>1</sup>. Highly-connected

---

<sup>1</sup>In network theory these are called high-degree nodes, where the degree of a node determines the

individuals tend to cluster with each other so that they maximize their influence across the broadest segment of the population. For example, Fortune-500 CEOs, sports and entertainment agents, or high-end venture capitalists all have a strong interest (and is part of their job description) in being connected to a large number of other highly-connected individuals. For them the mere volume of individual connections is irrelevant as long as all of these connections are important. The same logic applies to politicians who, by the very definition of their jobs, get to become connected to a wide network of powerful and well-connected stakeholders, whether locally or nationally.

This makes every elite network in a country highly topocratic (Borondo et al, 2014), meaning that the average compensation of an individual depends on how connected he or she is, and how central he or she is within his or her network. A topocratic network is the opposite of a meritocratic network — instead of what you know it matters more who you know. According to Borondo et al (2014) if a country is dominated by topocratic networks it is more likely to have higher levels of economic inequality. The logic is simple: the majority of people in a country are poorly connected to one another (they do not know many people outside their everyday environment) while a very small number of individuals in key positions of power are highly connected, in most cases to other highly-connected individuals in powerful occupations. This is the very essence of an

---

total amount of other nodes connected to it (Jackson, 2008). Therefore the scope of power for one node is determined by the sum of all other nodes it is connected to.

elite network. One notable advantage of these connections is access to privileged information on various opportunities (e.g. for job listings of top management positions, business opportunities such as private tenders, or stock market trading) which are usually limited to a very narrow social group of trusted individuals. One obvious consequence is an increase in informational asymmetry which skews the distribution of wealth-gaining activities towards a narrow group of highly-connected individuals.

### **Costs and benefits of elite network membership**

Thus far I have mostly discussed the advantages of elite network membership, taking it for granted that it represents a stable equilibrium for each agent to join the network and never deviate from it. However there are real costs of membership, as noted in the previous section. These costs include reputation, effort, the threat of prison time if the activity is illegal, and an additional cost of losing elections for the politician. In order for an individual agent to join an elite network the benefits of membership have to outweigh the costs.

This logic is shown in Figure 1.2, representing the cost-benefit structure of elite network membership with respect to the total amount of units consumed (think of them as money, goods, and services gained through rent-seeking) when part of an elite network. It also shows the marginal costs and marginal benefits functions in the lower panel, while the absolute costs and benefit functions are shown in the

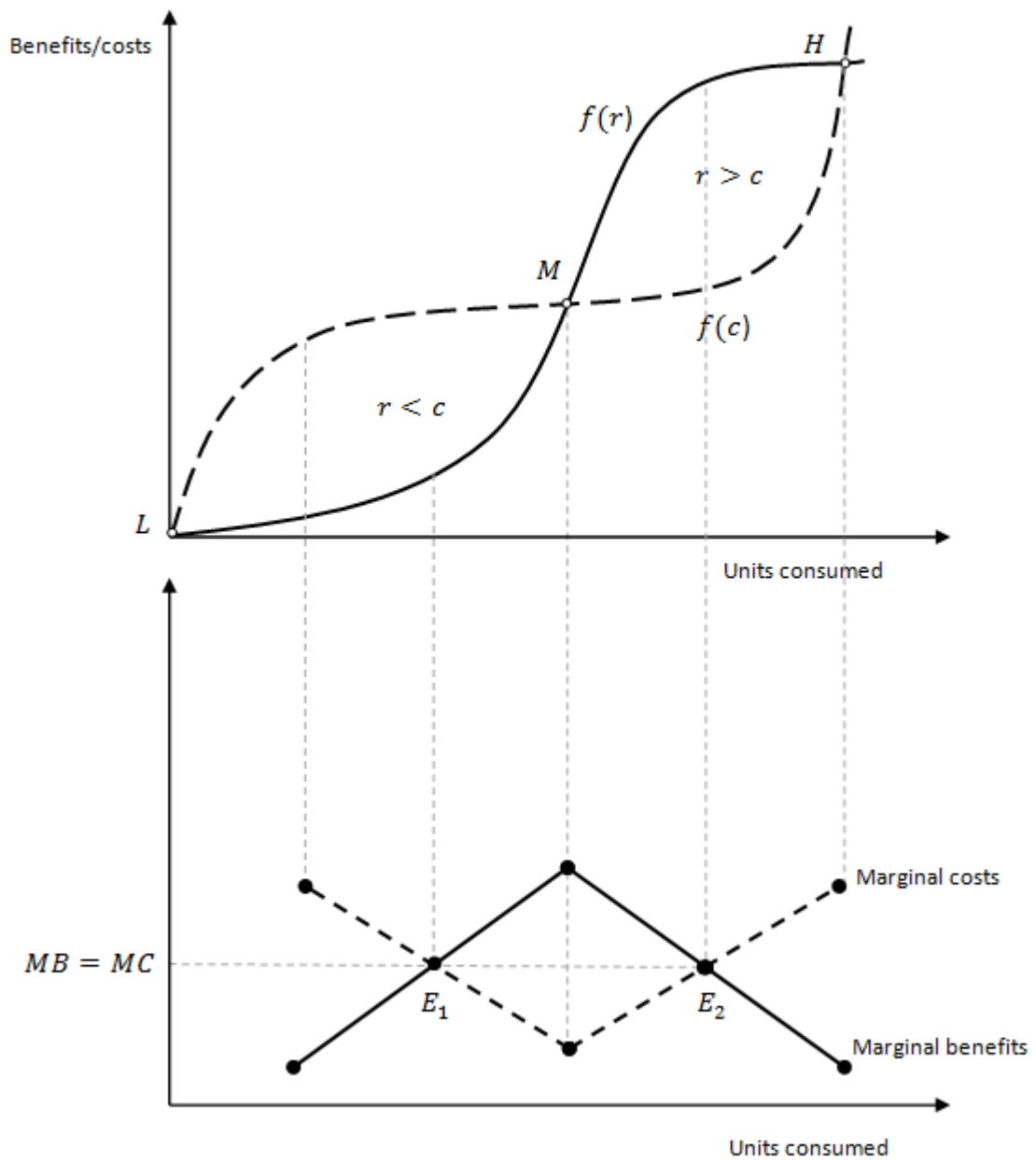


Figure 1.2: Cost-benefit graph of joining an elite network for an individual agent. The decision to join an elite network happens between points  $E_1$  and  $E_2$ , when marginal benefits outweigh marginal costs ( $MB > MC$ ).

upper panel. The cost function for each individual agent  $f(c)$  (dashed line) is a monotonically increasing function that is concave up to the point  $M$  after which it becomes convex. It reflects the idea that costs initially grow at a diminishing rate. Initial effort and reputation are sacrificed to enter the network. These entry costs are high, but once they are realized, further consumption of elite network benefits does not imply great additional effort or threats to reputation, at least until point  $M$ , when by assumption most benefits enjoyed by agents are not (yet) illegal. Hence marginal costs (also dashed line) decline at a steady rate until point  $M$  where they reach their lowest value.

After further greater consumption of rent-seeking units, and after agents start getting involved in illegal activities, costs of elite network membership exponentially increase the threat of reputation, particularly under the possibility of a prison sentence. After point  $H$ , the costs have gone up too high, and this small margin is the level at which an individual is punished (e.g. by going to prison<sup>2</sup>). At point  $H$  is where the marginal costs significantly exceed marginal benefits.

The benefits function  $f(r)$  is convex at first, reflecting the idea that the benefits of elite network membership have to increase exponentially to attract individuals inside (e.g. earning more money to afford a better lifestyle). Once the consumption of units reaches point  $M$ , more exposure to the network still increases benefits, albeit at a diminishing rate. At this point the marginal benefits from elite network

---

<sup>2</sup>Note that the corresponding benefits do not drop. Convicted white collar criminals tend to keep most of their material possessions.

membership are highest. Once the initial thirst for a different lifestyle has been satisfied additional units do not increase marginal utility as fast as before.

The equilibrium conditions are given in the lower panel of Figure 1.2. An agent decides to join an elite network only when his or her marginal benefits exceed his or her marginal costs of membership ( $MB > MC$ ). Because of the specific shapes of the total costs and benefit curves we have two equilibria for which marginal costs equal marginal benefits,  $E_1$  and  $E_2$ . A decision to join an elite network happens only between those two points.

It is interesting to notice that the decision to join an elite network happens even when  $r < c$ , at least after point  $E_1$ . Before point  $E_1$  an individual knows that a substantial effort is necessary to become part of a network as there is a limit to how many units he may consume. In other words an individual is not high enough in the hierarchy to even have an opportunity to join the network, so any costs of effort in trying to get in significantly outweigh the benefits. This may change in a dynamic setting, as agents may expect to climb up the hierarchy once a part of the network.

Therefore after point  $E_1$ , after a certain effort has been spent (going to social gatherings and meeting the ‘right’ people, earning a large sum of money, winning an election, or gaining more experience that enables a person to climb the corporate hierarchy and therefore meet the people with power), marginal benefits start exceeding marginal costs making it attractive for a person to join the network,

even if the total costs temporarily exceed total benefits (e.g. paying a high initial price to cater to a politician, the benefits of which will only be visible later). Soon enough the agent will start gaining marginal utility for which total benefits will exceed total costs,  $r > c$  happening after point  $M^3$ . This situation will last only until point  $E_2$ , after which, despite still receiving benefits from network membership, the marginal costs start to exceed marginal benefits. In colloquial terms, the agent is “in too deep”, as there is an increasing likelihood that the activities of the group might be uncovered and that punishment will occur. Finally, after point  $H$ , costs again outweigh the benefits as an agent is punished for illegal activities. At this point membership in an elite network stops.

An extension to this setting would be to model the within-network relationships of agents. Not all agents are the same, nor are they valued the same way in the network. Some carry more influence than others (although this obviously changes over time), so to adjust for this I would have to draw different cost-benefit curves for different agents depending on their network centrality (how important they are in the system). This is outside the scope of this thesis. I assume all agents that engage in the network are equally important as each of them is driven by an incentive to pursue their interest. Therefore each will only join if his or her expected benefits are high enough.

---

<sup>3</sup>Note that point  $M$  represents an unstable equilibrium as agents have an incentive to move to the right. They realize that one cannot stay below point  $M$  for too long and they expect total benefits to start exceeding total costs after initial time and effort have been spent.

### 1.3 Why should elite networks have an effect on income inequality? A historical argument

In order to understand why elite networks would carry any significant effect on the distribution of income I first provide a brief historical overview of the origins of wealth, power, and its concentration in the hands of the elite. This argument reinforces the logic behind the creation of elite networks and helps understand how they might generate an effect on inequality today.

According to Scheidel (2017) early pre-modern states and empires enabled the concentration of income and wealth within the ruling elite through basic rent-extraction. He refers to these elites as the “original 1 percent” whose purpose was to uphold power in order to seize wealth. The very distribution of income and wealth in early societies and empires was based on proximity to power. Any income generated from labour or capital by the non-elites was quickly expropriated by those with proximity to political power, leaving the vast majority of the population to live on subsistence income. This Malthusian economic system, where technological progress never really improved living standards but only produced population growth (Clark, 2011), was supported by the holders of political power. Having or being close to political power was the only way to become rich in a Malthusian economy.

This argument is further supported by Milanovic et al (2011) who find that pre-industrial societies (ranging from ancient Rome and Byzantium to medieval

England, Holland or France) were characterized by a very high extraction ratio — the rate at which potential inequality is turned into actual inequality. The higher the extraction ratio for a given society, the greater the level of expropriated wealth by the top income groups and consequentially, the more unequal the society. Essentially they confirm the idea that extractive and repressive ruling elites were responsible for creating and maintaining unequal societies. It was a stationary bandit model based on extractive political institutions (Olson, 2000; Acemoglu and Robinson, 2012).

Political inequality, or the inequality in the distribution of power, was therefore the primary source of income and wealth inequality ever since our species evolved from hunter-gather societies. This artefact of societal development was, and still is, a key characteristic of many states and nations — all, in fact, which have had or still have some type of authoritarian rule. After the fall of the Roman Empire, medieval kingdoms replaced the earlier structures and enabled the extractive stationary bandit model to spread out, giving rise to the first nobles as the supporting elite group of the ruler. Their political power gave them the right to own land, (slave) labour, and ultimately amass large wealth. The medieval nobility and the elites that were created at the onset of the Middle Ages, as well as the fortunes that they've amassed, according to Clark's (2014) research on social mobility based on rare elite surnames, are still in the upper tail of the income distribution until this very day. Clark's natural law of social mobility predicts a regression to the mean

over time for elites and impoverished groups, however this regression is very slow, and it can take up to five hundred years for an elite family to regress to the mean. The seeds of today's inequality were therefore planted during times of stationary bandit extraction-based politics. This is the legacy that elite networks have had on the distribution of income and wealth.

Piketty's (2014) seminal contribution testifies to this argument. His long-run verdict is that European inequality of the 18th and 19th century was driven primarily by rentiers and income from capital. This was, in fact, a characteristic of all traditional agrarian societies. The pre-20th century elites lived on rents they extracted from their ownership of inherited capital and land. The upper decile of income earners owned over 90% of total wealth in a country, where the top 1% (kings and nobility) owned about 50-60% of it while the other 9% (landed gentry and clergy) owned the other 30-40%. A vast majority of people owned almost no wealth at all. Gaining wealth — land or capital — was possible either through expropriation, carried out by the rulers themselves, or by inheritance, which is how wealth got transmitted among the nobility and gentry. The income hierarchy of pre-20th century societies was therefore dominated by capital as the main source of income. This means that a person was much better off to inherit wealth rather than to earn it. A top 1% inheritor had about 2.5 times more income than a top 1% labor earner. No job could be as lucrative as owning land or capital.

Piketty's fundamental force for divergence — where the return on capital ( $r$ ) is

bigger than the economic growth rate ( $g$ ) — was a defining force of all agrarian and pre-20th century societies. These were societies which had a high concentration of wealth among the elites in addition to a high level of persistence of wealth in the hands of the elites<sup>4</sup>.

It was not until the beginning of the 20th century when the structure of capital started to change and its value relative to national income started to decline. The two World Wars and the Great Depression between them marked the ultimate downfall of the rentier and made the shift from what Piketty calls a “society of rentiers” to a “society of managers”. In other words the top decile of the income distribution is ever since consisted mostly of individuals who live on wages instead of rents, i.e. from labour instead of capital. Today the top 1%, the new elite, are the top managers (CEOs, presidents, board members) of global companies and entrepreneurs who created entirely new industries. Their income is mainly determined by labour, however they also benefit from exposure to the financial market (e.g. dividend returns on investments) particularly those in the top 0.1% of the income distribution. Bakija, Cole, and Heim (2012) confirm that 60 to 70% of individuals in the top 0.1% of the income distribution are top managers of global

---

<sup>4</sup>Even revolutionary periods failed to change this. Clark (2014) shows this on the example of China’s Communist revolution, which despite being very determined to purge the country of its “class enemies” — the landlords and businessmen — still has these “class enemies”, originating from the same elite surname groups of the late 19th century, overrepresented in the top earner groups in China. They are even present in the highest structures of its Communist Party. In general, Clark’s conclusion for a number of countries (Sweden, USA, UK, China, India, Japan, Korea, and Chile) is that social mobility across all societies tends to be very low regardless of any societal changes, including revolutions, major policy reforms like the introduction of the welfare state, or even groundbreaking events like the Reformation, the Enlightenment, or the Industrial Revolution.

companies. By all means of measurement this group is the new elite.

The structure of the elites had therefore changed in the 20th century. However, their resulting effects remain the same. Atkinson, Piketty, and Saez (2011) attribute the rise of inequality in the United States (but also in other Anglo-Saxon countries) since the 1970s to the rise in incomes of the top 1% and top 0.1% of income earners, where salaries and capital gains from investments in securities represented the largest contributor to this increase. The rise of top executives' salaries, a consequence of a multitude of factors that include an increasing demand for top talent (the "superstar effect"), significantly advanced not only their position in the income hierarchy, but more importantly, their position in the social hierarchy. The supermanagers signal their importance to their organizations by having access to a wide network of influence. Their degree centrality (the positioning of an individual in the network where he or she is highly connected) enables them considerable power in justifying their own abnormal earnings (abnormal in a sense that their total compensations are higher than their productivity).

Historically, elite persistence was always closely correlated with political power. One could not maintain its dominant position in the social hierarchy without being somewhat dependent on the ruler. Today, this relationship, although different in the scope of political power the ruler can use to expropriate its subordinates, is essentially the same as before. The power of a ruler to take away human lives has vanished in democracies (at least in domestic affairs), however the ruler still has

enough power in terms of budgetary redistribution so that his role attracts anyone dependent on these resources, be it various interest groups seeking to gain public resources or regulatory leeway for its members, or firms seeking to gain regulatory, legislative, or other form of advantage over its competitors. Furthermore it has been shown that ‘supermanagers’ often use their political power to lower top marginal income tax rates or finance various NGOs so as to indirectly promote their cause (Hacker and Pierson, 2011; Bonica et al, 2013). This too can affect the rise of top incomes and widen income inequality.

Arguably this political effect — lobbying and using campaign donations to persuade politicians to make legal changes in the tax codes — is part of a much wider network effect. The way a ‘supermanager’ gains access to a politician is not through some crude and well-defined institutional mechanism where more campaign funding automatically transfers into favourable decisions for the giver. No, influence is gradually constructed through a series of informal meetings and gatherings, where favours are exchanged between powerful individuals. This is the foundation of an elite network.

## **1.4 Methodological approach to measuring the impact of elite networks**

In order to prove the underlying theory I need a good way of measuring an elite network. Empirical efforts of recognizing who is and who is not a member of an elite group have usually been limited to occupational studies where all leaders of business, political or military organizations have been classified as the elite (e.g. Mills, 1956; Guttsman 1965; Useem 1984; Ruostetsaari 2006; Maclean et al. 2010; Griffiths et al, 2014). In this thesis I am primarily focused on outcomes that are supposed to result from an elite network-type of relationship, rather than listing and analyzing its individual members. The relationship that leads to an outcome is hidden (deals done behind closed doors), but the outcome itself is visible. For example, a politician whose district is riven with fraudulent procurement contracts keeps winning elections and stays in power for a long period of time. Or a firm whose top management has a clear connection to politics and receives a beneficial treatment from the government, where the decision is made by the very same politicians they are connected to. Finally, I somewhat follow the empirical literature and try to construct my own indicator of elite membership by looking at how certain top managers utilize their political connections and their relative position within a social network in order to drive up their compensation packages. Essentially there is no precise way of measuring an exact elite network relationship, mostly because their interactions are hidden from the general public, but it

is possible to apply a number of approximations by making inferences from direct outcomes that have the highest probability to signal an elite network-type of relationship.

As defined earlier, the reason why someone would enter into an elite network is to preserve his or her position of power. All elite networks throughout recorded human history depend on preserving power and wealth. Modern elite networks, at least in democracies, are comprised of politicians whose desire is to preserve power, and rent-extracting firms and their executives whose desire is to uphold wealth. The goal of the thesis is therefore twofold: first I seek to establish that both parts of an elite network benefit from proximity to each other, and second, to establish how their relationship impacts the distribution of top incomes. In other words, I wish to uncover how both types of powerful individuals benefit from being included in this network of influence, and what unwanted consequences their relationship entails.

What are the typical outcomes that arise from an elite network relationship? Corruption is one. In particular, a specific form of corruption from procurement contracts that are allocated on a suspicious and hence potentially fraudulent procedure. The focus is on office-holding politicians, and the potential benefit they can incur if they are part of informal groups to which they distribute rents and are free to take rents from. There is no way, apart from criminal investigations, to directly prove that the owner or CEO of the firm received a contract because

of their connection to a politician, however in many cases based on the firms' relative performance and status it is possible to deduce a potentially suspicious relationship that implicates the inner dealings of an elite network.

Another outcome that testifies to an elite network-type of behaviour is when firms that are directly and undoubtedly connected to politicians (via lobbying, campaign donations or via their key personnel) get favourable or better deals from the government compared to similar firms that are not connected to politics. Although the outcome need not be a direct result of elite network membership, any causal effect of political connections on a government subsidy again implicates the existence of an informal relationship through which the decision-making process was moulded.

Finally, a good way of quantifying an elite network is to look at individual-level data of top corporate executives on memberships in various organizations as well as their career paths to see if at any point they could have formed a relationship with the political decision-makers. For example, a corporate CEO might have been working at a top position in government prior to his/her position in the firm, which would grant them preferential access to former colleagues if a firm needs a government favour. Also, corporate bosses and politicians can be members of the same country clubs or various societies, can be tied together through think tanks and NGOs, and can often meet in informal gatherings where they have direct access to privileged information.

### 1.4.1 Structure of the thesis

The dissertation is structured as a three-paper thesis. After the introductory chapter, the literature review starts by surveying what the economic theory of regulation and the standard political economy literature have to say on the connections between firms and politicians. How do firms benefit, how do politicians benefit, and most importantly what is the motivation to enter into such relationships? The second part of the literature review examines what the thesis posits is one of the consequences of elite network relationships — a higher level of income inequality. There are a number of factors that the economic and political science literatures recognize as the causes of rising income inequality, so this part reviews these findings in order to uncover the gaps in the literature and establish how this thesis attempts to close those gaps. After the literature section the next three chapters are individual papers, each tackling one of the three main research questions: (1) how politicians benefit from giving concessions to firms; (2) how firms benefit from being connected to politics; and (3) do political connections of top managers affect income inequality through their earnings? The final concluding chapter connects the implications of all three papers, verifies whether the main hypothesis has been proven, discusses the external validity limitations of the thesis, and draws motivation for further research. The rest of this section presents the overall motivation behind each paper, and briefly touches upon the data and methodological approaches used in each paper.

Before detailing the logic and methodology of each individual paper, a few words on the chosen empirical cases. The first paper looks at the allocation of suspicious public procurement allocations in Croatian municipalities, the second paper looks at the distribution of bailout funds to the US finance industry during the 2008 financial crisis, while the third paper takes a panel of top managers of publicly listed companies in the US and the UK and looks at how their political connection impacts their earnings.

The reason why I connect these two very different economic systems (Eastern European vs. Anglo-Saxon) is to draw a parallel between the political economic outcomes in a transitional society with those of the developed West. In other words I seek to emphasize the similarities of how political elites and corporate interests benefited from collusion at the expense of the rest of society in both settings. Croatia serves as a case in point of a failed transition from socialism to capitalism, trapped in cronyism through which corporate and political elites usurp institutions of the democratic order. I then uncover that the same logic of narrow political and corporate networks is at place in institutionally stable democracies like the United States or the United Kingdom. The narrow group of beneficiaries from the process of transition in Croatia is very similar to the narrow network of rent-seeking beneficiaries in the US for example. The difference in outcomes between the two countries lies in the historical development of institutional rules. In the US such institutions do enable fair winners on the market based on their

competitive strength and innovative capacities. In Croatia the very process of transition shaped the initial rules of democracy and tainted its outcomes ever since. However, despite clear differences in the levels of development, income, and the institutional foundations of each society, there are similarities in the types of elite rule and special interest state capture, as history suggests that there should be. Similarities arise in the way elite networks have captured the democratic decision-making process and have consequentially shaped similar economic outcomes facing both the highly developed West with a long tradition of stable institutions and transitional countries which have never even erected stable institutions to begin with.

#### **1.4.2 Corruption and re-election**

In the first paper I test the hypothesis that mayors win elections without decreasing the scope of their corrupt behaviour. Corruption is generated through interactions within elite networks between politicians and rent-seeking firms, and is approximated through potentially fraudulent procurement contracts. Measuring corruption this way enables me to directly implicate the mutually beneficial relationship through which procurement contracts are traded for electoral support. I find that mayors have a greater probability of survival if they engage in such elite network relationships and produce higher corruption. However there is a limit to this kind of behaviour, as mayors get punished for too high levels of potentially

fraudulent procurements.

In order to measure corruption generated through elite networks I create several corruption proxies using a unique dataset of Croatian local public procurements consisting of more than 35,000 contracts over a four-year electoral cycle from 2009 to 2013, with a total value of more than 3.7bn euros. The dataset is built using online sources of official data from the publicly available registry of procurements published in the Official Gazette of the Republic of Croatia. I single out various cases which present a reasonable doubt in the regularity of the procurement procedure. Examples include cases in which firms with no employees and as a single bidder on the tender received procurement contracts worth millions, in which firms signed contracts vastly exceeding their capabilities, or when firms with substantial losses received indirect subsidies through lucrative deals. The intuition behind the formation of each of these corruption proxies is based on qualitative evidence using interviews with key stakeholders in the procurement process: special police investigators, civil sector anti-corruption watchdogs, local government officials, public sector bureaucrats, and entrepreneurs. Observing fraudulent public procurements is the easiest way to establish a link between corrupt politicians and rent-seeking firms, given that the allocation of procurements in Croatia depends entirely on discretionary power of the mayors. This is the first major contribution of the paper.

The second contribution is focused on showing that the relationship between

corruption and re-election is non-linear. In particular, corruption entails a concave effect on the chances of re-election. Higher corruption increases the probability of re-election but only until an upper limit after which corruption is too high and politicians get punished by the voters. This helps explain why the literature finds both examples of when voters punish and do not punish corruption — corruption is punished but only after it becomes too large. However given that the hypothesized relationship might be subject to selection bias and omitted variable bias I use a ‘fuzzy’ regression discontinuity (RD) approach based on population thresholds that determine the size of the local council. Council size is determined exogenously by law, and is conditioned on population size of a city or municipality with clearly defined legal boundaries. It therefore exhibits no direct impact on re-election chances of a politician (re-election is independent of population size and hence the size of the local assembly). It does however exhibit a strong positive relationship with local corruption, given that mayors, in order to keep their assembly majority, need to divert more resources towards corruption and use this to bribe assembly members for support. The more members a mayor needs to buy, the more corruption he or she engages in (this is further justified by citing cases of local bribery of assembly members to achieve narrow majorities in the legislature). The intensity of the treatment (size of corruption) increases with higher council size thus justifying the ‘fuzzy’ design, as the jump after each threshold represents an as good as random assignment of local cities and municipalities just above and

just below the threshold.

### **1.4.3 The politics of bailouts**

In the second paper I examine the impact of firm-level political connections on policy favours using the case of government bailout (TARP) funds allocated to the US finance industry during the 2008-2009 financial crisis. I look at how financial institutions that received TARP funds used lobbying, campaign spending during the 2008 cycle, and direct links their corporate management had with the decision-makers (Congress, Treasury, Fed) to get a better bailout deal for themselves. I find that politically connected firms were clearly favoured over unconnected ones during the bailout allocation process. This however does not imply that the government decision-makers broke the law in any way by explicitly giving more money to banks they had a connection with, but it does suggest that being part of a Congressmen's social network benefited the TARP recipients. The idea rests upon what Acemoglu et al (2016) call the "social connections in a crisis hypothesis" which suggest that every person in power has a close social network they rely upon. Often, as in the case of Treasury Secretary Henry Paulson and New York Fed Chairman (and next Treasury Secretary) Timothy Geithner, the close social network is comprised of chief executives of the biggest banks. Similarly, individual Congressmen also get influenced by firms which donate them money and by chief executives they informally frequent. In many cases this can morph into cultural capture of the

regulators by the industry (Kwak, 2014). In each case however, social connections play a key role in the decision-making process, exemplifying the existence (and consequences) of elite networks. This type of relationship need not be illegal or even immoral — it is a spontaneous self-preservation instinct that guides behaviour of elite network members to pursue their goals, particularly in high-intensity and high-stress events such as the post-Lehman panic of 2008. The conclusion of the paper is that, for a firm, it always pays off to be politically connected, but in times of crisis, it pays off even more.

The methodological problems with empirical studies of lobbying and political connections are twofold. First, there is a self-selection problem — firms are more likely to lobby and be connected if they expect successful outcomes. And second there is a serious omitted variable bias. The hypothesis posits that political connections among TARP recipients resulted in greater bailout funds. However, it could be that the driver of this result is an unobservable factor such as the relative risk of the lender. Riskier firms lobby more because they are more risk prone to begin with, implying they will also have a greater chance of getting bailout funds. Exposure to risk, not political connections, can thus be the main driver behind the bailout allocation.

In order to solve the endogeneity issues I exploit close electoral races in the November 2008 federal elections as sources of as-good-as random assignment of politicians connected to TARP recipients. I use a regression discontinuity (RD)

design and an instrumental variables (IV) approach, both under the assumption that in close races, decided within a 1% margin of victory, a random element such as luck is most likely to decide an election (e.g. rain on election day that affects turnout), meaning that the bare winners and losers are supposed to be statistically interchangeable. The RD analysis narrows the focus only on connected firms whose congressmen won close elections (units of analysis are firm-politicians), and finds that a narrow victory of a connected congressman increased the relative bailout allocation for the congressman's connected firm. The IV analysis focuses on firms as the unit of analysis, and finds that firms with a higher proportion of connected congressmen who won the 2008 election in a closely contested race received more TARP funds. These are different effects but they point to the same conclusion: being politically connected made a difference in the allocation of TARP funds to its recipients.

#### **1.4.4 Elite networks and inequality**

In the third paper I come closest to quantifying an elite network in order to observe its consequence on an economic outcome. This paper delivers the final argument in support of the main hypothesis of the thesis. I examine whether corporate executives derive higher earnings by being politically connected. A political connection implies any channel through which a top ranked corporate executive could form an informal interaction with an office-holding politician, be it through their previous

careers, or through membership in the same organizations such as country clubs, foundations, charities, church or religious groups, professional organizations, etc. Once such an informal relationship is established, favours can be exchanged; the executive gets beneficial regulation or government contracts, and the politician gets campaign donations or direct bribes. Once the executive successfully extracts rents for his firm, he demands a higher compensation as a reward for his efforts. If this theory holds we would notice a clear difference in earnings between executives who carry some type of political connection and those who do not. An elite network member should always be compensated more than a non-member.

The empirical findings of the paper confirm this theoretical intuition. Politically connected executives, by assumption elite network members, tend to have higher salaries and total earnings than non-connected executives. They are also better connected within the corporate world, however the network effect does not offset the political connection effect — if anything, it reinforces it. The implication this carries for income inequality is striking. It suggests that one of the drivers of top wages of today's supermanagers are their political connections. Being a member of an elite network directly implicates earnings of such individuals and hence drives up income inequality.

The findings are confirmed using US and UK individual and firm-level data over a time period of 16 years, from 2000 to 2015. Given the panel structure of the dataset I use firm fixed effects and time fixed effects to estimate the effect

of political connections and network size on total executive earnings. Firm fixed effects imply that I estimate the effect of being politically connected vs. being politically unconnected within firms over time. I also apply an instrumental variable (IV) estimation on a firm level that takes average levels of connections (when defined as a continuous variable) across each industry and use it as an IV for firm-level connections within that industry. The assumption is that any variation across industries is not likely to be driven by unobserved firm-specific factors, but rather by industry-level characteristics that are likely to be exogenous to firm-level choices over its wage structure. However a disadvantage of this approach is that the IV only varies across industries, rather than across firms, meaning that I use it primarily as a robustness check for the initial results. Each estimation method delivers the same conclusion — being a politically connected corporate executive yields higher earnings and hence widens the income disparities between the top income earners and the rest.

## Chapter 2

# Literature review

The main hypothesis of the thesis is to uncover how elite networks of political and business interests affect the rise of income inequality. Given the scope of the thesis I survey three sets of literature in order to reveal gaps in our current understanding of the hypothesized mechanism. I start by examining the literature on the benefits firms and politicians receive from colluding together. This literature is rooted in the economic theory of regulation and the public choice definition of rent-seeking, and is in most cases focused on showing two things: how does the interaction look like and what are its social welfare implications. I then focus on the hypothesized consequences arising from this collusion — higher income inequality, or to be exact, the thus far recognized causes of rising inequality. I start by listing all the potential economic factors that are attributed to the increase in income inequality over the past four decades, ranging from skill-based technological progress theories to the

explanations behind the extraordinary growth of top incomes in many countries. Finally, I examine all the political factors that could influence inequality in order to uncover the gaps in the political science and political economy literatures, and establish how the theory presented in this thesis potentially fills those gaps.

## **2.1 How firms and politicians benefit from collusion?**

The seminal theoretical contributions explaining the link between firms and politicians come from economic theories of regulation. Stigler's (1971) initial contribution, resting upon Olson's (1965) logic of collective action, was a demand-side explanation showing the effect of cartelization: large, well-organized firms (small, homogenous groups) will lobby most effectively and get the benefit of favourable regulation, which will always come at the expense of consumers (large, dispersed groups which are unable to organize). Peltzman (1976) expanded that model to include the role of legislators who are decision-making agents that seek to remain in power, and will guide their decisions to fulfil that goal. The electoral concerns of legislators should explain why big businesses can sometimes be punished, or why protectionism can sometimes be relaxed. Becker (1983) then expands Peltzman's model by introducing competition between interest groups where size and within-group free riding determine success in receiving government transfers.

A more precise definition of these activities is encapsulated by Tullock's (1967) and Krueger's (1974) concept of rent-seeking; the process of gaining private ben-

efits for firms or individuals by misusing the political process (by using lobbying, campaign spending, personal connections, or even bribes to influence legislators). Rent-seeking is usually linked to gaining protection for a particular firm or industry, which can vary from achieving monopoly status to barriers to entry for newcomers that reduces competition (e.g. via licensing); or from imposing tariffs on foreign goods to protect domestic incumbents, to picking winners via government subsidies. In each case rent-seeking activities hamper market outcomes by reducing its allocative efficiency and only end up destroying wealth. In the context of this thesis, rent-seeking is the crucial ingredient linking collusion between firms and politicians to the negative consequences of that collusion.

Following these seminal theoretical contributions, the next step was to explain how exactly the interaction between firms and politicians looks like and, more importantly, what would be its social welfare implications? An interesting concept that expanded on the economic regulation and rent-seeking literature was McChesney's (1997) "money for nothing" model of political extortion. According to this model firms and interest groups that employ lobbyists pay money to prevent legislative outcomes that could hurt them. The "money for nothing" implies paying the price of political extortion. Politicians propose legislations and regulations that would hurt a given industry which is then forced to pay the rent in order for a politician to withdraw this piece of legislation. McChesney argues that many regulatory wealth-reducing threats are only announced for this particular purpose

— to extract rents from private sector firms. He shows this empirically for the US and calls upon the evidence from Beck, Hoskins and Connolly (1992) of a similar rent-extraction practice in Canada.

In similar fashion Shleifer and Vishny (1998) describe a clear-cut set of grabbing hand government models that encapsulate the full benefits of the elite network relationship: each agent pursues their own objectives, none of which promote the public's interest. Their analysis is particularly suitable to explain the negative social outcomes following dubious privatization schemes in post-communist countries. Politicians privatize profitable public firms by giving them to their cronies in return for continued bribes. They keep less profitable public firms in the government's hands and continue to demand the pursuit of political goals from these firms in exchange for subsidies (Shleifer and Vishny, 1994).

North, Wallis, and Weingest (2009) describe such grabbing hand environments as limited access orders in which governing institutions are personalized and economic outcomes are manipulated by the ruling elites which cater to privileged interests. All political and economic outcomes are merely a consequence of interpersonal relationships among agents within the ruling elite networks. Bueno De Mesquita et al (2005) go a step further to show how the collusion between political interests and the interests of a politician's close group of supporters mutually benefits both: politicians maximize their time in power, while the loyal supporters get rewarded either directly through private goods, or indirectly through public goods

(depending on the institutional environment). Their analysis is focused almost entirely on the motivation of the politician to remain in power. To do so a politician creates a powerful group of dependent supporters which is called a *winning coalition*. The smaller the size of the winning coalition (i.e. the less interests a politician needs to caress), the greater the chance of political survival. The crucial effort is to build a winning coalition of key powerful players that can generate both constant rents to the politician and deliver enough votes to keep him in power.

In other words whenever loyal political support is conditioned on various material and non-material benefits the political system is likely to deliver subpar outcomes, as they tend to favour the interest of the few (usually the elite) over those of the many. In his seminal 1982 book “The Rise and Decline of Nations” Mancur Olson warned of the negative implications of the rising power of cartels and lobbies over time. Their actions, fully legitimate outcomes of the democratic process, have resulted in the rising power of lobbyists, lawyers, and bureaucrats who exercise control over the redistributive flow of resources. As the struggles for redistribution and prone regulation between interest groups surpass those of the productive sectors of the economy, economic growth becomes undermined and decline is imminent.

These sets of theories present an accurate depiction of how the elite network mechanism works and it clearly recognizes a negative effect of collusion between politicians and firms on social welfare. What are the empirical implications? And

what is the size of the distortion generated by such networks? Bellettini et al (2013) directly confirm the theoretical intuition from Stigler (1971) and Peltzman (1976) on a panel of 62 countries. They find that the negative impact of political longevity on economic growth can be explained through the actions of politically-connected firms which use their power to mitigate regulatory costs, prevent entry of higher quality competitors, and preserve their monopoly positions. Fisman (2001) shows, on the case of Indonesia, that a large part of a connected firm's value can be explained by its political connections. In fact 30% of Indonesia's GDP depends on politically-connected firms. Faccio (2006), using a panel of 47 countries, also confirmed that greater political connections increase firm value. She finds that connected firms (firms with a large shareholder either in office or connected to the ruling party) represent almost 8% of the world's market capitalization. Not surprisingly, connected firms are more common in countries with greater corruption and lower levels of transparency (e.g. in Russia they amounted to 87% of all market capitalization during the 1990s).

Goldman et al (2009) confirm a similar finding in the US where announcements of politically connected individuals in the board of directors resulted in abnormal market returns. Eggers and Hainmueller (2009) find that Conservative MPs in the UK benefit personally from industry connections after their time in office. Jayachandran (2006) and Blanes i Vidal et al (2012) make the same inference but for the opposite effect of when the political connection is lost (i.e. when a politician

exists politics). When this happens, both connected firms and connected lobbyists are faced with losses. Furthermore, Faccio et al (2006), on a group of 35 countries, show that politically connected firms tend to receive more government bailouts during times of crisis than non-connected firms, Sapienza (2004) and Dinc (2005) find that government-owned banks increase lending in election years to help party-affiliated borrowers, while Khwaja and Mian (2005) show that politically-connected firms get better loan deals from public banks.

Institutions can matter as well. Gehlbach, Sonin and Zhuravskaya (2010) use the case of Russia to show that weak institutional environments serve to promote partial interests of firms, and that in environments which fail to demand political accountability businessmen themselves run for office to reduce their lobbying costs and directly promote policies that favour their interests. Earle and Gehlbach (2015) on the case of the Ukrainian Orange Revolution similarly find that political turnovers exercise a significant impact on politically-connected firms in weak institutional environments. In a comprehensive study of political-business networks in the cases of Poland, Bulgaria, and Romania, Schoenman (2014) shows that the impact of such networks on the formation of post-communist institutions was critical to the success of a country's path through transition. The key implication is that broad networks in combination with greater political competition and hence greater political uncertainty generate better institutions. Narrow networks of elite interests will, on the other hand, result in weak institutions and worse economic

outcomes.

The current literature therefore provides ample evidence on how and why networks (or connections) between corporate and political interest form and what consequences they entail to society. What is missing is a link between these groups and income inequality, or in other words, how their relationship inhibits a more equal distribution of income. The literature extending from the economic theory of regulation and public choice theory is mostly concerned with the impact of corporate and political collusion on economic growth or some general indicator of economic activity (like productivity). There *is* a literature linking the misuse of politics by special interests to inequality, but before I touch upon that evidence it will be useful to survey the economic literature on the causes of income inequality.

## **2.2 Economic reasons behind the rise of inequality**

According to Atkinson, Piketty, and Saez (2011) annual average real income growth for the bottom 99% of income earners in the United States was only 0.6% over a period of 40 years (from 1976 to 2007). In the same time incomes for the top 1% grew at an annual rate of 4.4%. Most of this income growth did not however come from salaries (even though their growth was also substantial), but from things like capital gains and business income (investments, dividends, business profits, etc.). This long-run trend led to a massive accumulation of wealth at the top, where the top 10% of income earners earned 47% of all income in the US, while the top 1%

earned about 19% of all income (their share has slightly declined as an immediate consequence of the financial crisis). Even though the US is an outlier among the rich democracies, similar trends of rising incomes at the top and stagnating incomes at the middle and the bottom happened all across the West.

Atkinson (2008) offers the most detailed summary of inequality trends across 20 OECD countries. He finds that over a period of 25 years, from the 1980s to 2014, only France and Ireland have seen a significant (over 10%) growth in incomes of the bottom earners that has outpaced the growth of the top decile earners<sup>1</sup>. All other countries have experienced high growth rates in top decile incomes coupled with either modest or declining growth rates of the bottom deciles. In the US for example top decile incomes increased by 15%, while bottom decile incomes fell by 5%. Similar trends, although with a lower magnitude in both directions, occurred in the UK, Netherlands, Sweden, New Zealand, and Australia, while other countries all experienced much higher growth rates in earnings of the top decile compared to growth in earnings of the bottom decile. Atkinson (2008) also points out that inequality in the dispersion of earnings mostly originated because of changes in top incomes, a trend that coincided in almost all observed countries. Data on the Gini index suggest a very similar conclusion across the same sample of 25 OECD countries: inequality started going up since the 1980s, with the average

---

<sup>1</sup>If the growth of income among the top 10% of earners outpaces the growth of the median income in the population, inequality goes up. Atkinson (2008) suggests that the distribution (or dispersion) of earnings is the most precise measure of income inequality, as it captures only the differences in earned incomes (as opposed to capital, rents, transfers, etc.), and it focuses on the relative performances of top income earners compared to the rest of society (this way it accounts for the fact that some people deserve higher wages due to their greater job responsibility or risk).

increase being around 5 percentage points (excluding the US and the UK the average increase is still high - around 4 p.p.) (Atkinson et al, 2017).

Taking all the trends into account the first question is why have real wages stagnated particularly among the lower income groups and the middle classes, while income earnings at the top grew rapidly?

Economists have provided a number of explanations. The most usual ones are the impact of education, technology and globalization. Technological progress in the past 30 years was attributed to the IT revolution (which many call the “Third Industrial Revolution”), and has been tilted in favour of those with greater skills and better education. Economists call this the “skills-based technological progress” hypothesis, fully capturing the creative destruction impact of technology on jobs and wages. The supply side has adjusted to this change. More and more people started getting a better education across the developed world. University enrolments as well as the share of people with a university degree are sky-high in every rich democracy compared to the pre-WWII levels. The skills premium, according to which greater education implies better paying jobs, has made a further dent in the divergence between top and bottom incomes, which are becoming clustered with respect to attained education levels.

Globalization was another important factor. The rise of China and the supply-chain economies of East Asia have created an outsourcing trend where low-skilled manufacturing jobs in the West were being shifted to low labour cost Asian coun-

tries. Free trade created clear winners and losers from globalization. Domestically, immigration took the rest of the low-skilled jobs, pushing the labour market equilibrium towards lower wages. The low-skilled, poorly educated blue collar workers who could not adjust faced either long-term unemployment or a significant dent in their incomes by accepting jobs in the emerging services industries, which paid less than their former manufacturing jobs<sup>2</sup>.

Economic theories do recognize the importance of certain political factors that depressed low and medium-income earnings. Minimum wage laws, always a controversial political issue, have not followed inflation trends, thus further depressing real wages of the poor. Changes in tax code became less progressive over time, and have opened up new opportunities for the wealthier citizens to shift their wage incomes into things like capital gains. This consequentially increased the share of capital and decreased the share of wages in total national income. On the other hand, transfers to the poor became more streamlined towards groups with more political power (such as the elderly, see Deaton, 2013). Atkinson (2015) also cites various cuts in welfare state programs throughout the 1980s and 1990s, leading to a scaling back of the welfare state, rising levels of long-term unemployment, rising interest rates on student loans and the role of capital markets, and of course the declining bargaining power of labour unions (membership in unions, particularly

---

<sup>2</sup>Another factor that is often overlooked in the story of stagnating real wages, at least in the US, is the rising share of non-cash benefits in the incomes of lower and middle income workers. Mainly, health insurance premiums and pensions savings. Such benefits will not be reported as taxable income in the official statistics, however they should show up in gross wages data. So the data on total income inequality could be slightly biased upwards, but the data on earnings dispersion should remain robust to this.

in the private sector, fell drastically since the mid-1970s). Deaton (2013) adds to this the gender wage gap, which particularly affected single-mother households, and the fact that poorer, non-voting immigrants are not politically represented well enough to successfully fight for more redistribution for themselves.

On the other hand of the spectrum, positively affected by most of these changes, were the top income earners. As emphasized earlier, the rise in earnings dispersion came primarily as a result of higher growth of top incomes, mainly through higher gains from capital investments, dividends, and business profits (for innovative entrepreneurs). The high-skilled, innovative part of the economy received their just rewards. Emerging billionaires in the IT sector created new jobs and entirely new industries. The problem was that such industries required a high-skilled workforce and could not replace the jobs lost due to outsourcing, at least not for the same people. While the IT sector captured most of the innovation-led growth, other industries turned towards trade and managed to achieve large economies of scale operating multinationally. This explains the exponential growth of CEO incomes (both their salaries and their capital gains), the competition for which was no longer local or national, but global. Top talent could have been attracted from all across the globe. This can explain even the within-firm differences where workers get paid the local rate of their city labour market while CEOs get paid the global rate in the market for top talent. A similar logic is at hand for earnings of other superstars, either in sports or show-business. A high demand for top quality talent,

by either the spectators or the shareholders, and extremely scarce skills will make sure that earnings of superstars keep on rising. However, in some industries the superstar effect seems to be clouded by luck (Taleb, 2008). In finance the best example are bonuses given to high-performance employees as a consequence of high stock market returns. Kahneman (2011) and De Bondt and Thaler (1985) showed that there was no consistency in year-to-year rankings of top performers in the stock market within financial firms. In other words, finance companies were rewarding luck instead of skills. A similar phenomenon occurs with CEO rewards in the oil industry (Bertrand and Mullainathan, 2001). An oil shock that pushes oil prices up and has nothing to do with individual skills of a CEO (usually it is triggered by political factors) will still deliver a large bonus to that CEO.

To sum up, the economic culprits for higher inequality can be found in the interaction of several factors. Rapid technological progress in the past 30 years resulted in a typical creative destruction process where new jobs and careers made certain types of old jobs obsolete (automated work). Some of these obsolete jobs were outsourced to Asia, others either did not re-emerge or were limited to low-skilled immigrants. The low-skilled workers failed to adapt to the changes and were left stranded either at lower paid jobs or became long-term unemployed. Poor education played an important role, while stagnating wages in the “dying” sectors only widened the gap. On the other hand, the innovative part of the equation was working quite well taking advantage of the new technological wave,

thus further raising the income of the top 10% and the top 1% (hence the great disparity between college and non-college degree workers). It's not hard to imagine how these two forces, one pulling the low-skilled downwards, the other pulling the high-skilled upwards, widened the income inequality gap in the West.

### **2.3 Political factors that could help explain the rise of inequality**

In addition to the innovative and superstar part of the top 1% (and a few lucky ones), there were others who instead of working for their wealth either inherited it or have benefited from the political system of capturing rents. Stiglitz (2012, pg. 32) refers to them as rent-seekers, those who receive “*income not as a reward for creating wealth but by grabbing a larger share of the wealth that would have otherwise been produced without their effort.*” The problem therefore is that even though the economic pie is growing, an even bigger amount of that pie is being captured by rent-seekers instead of wealth generators. Furman and Orszag (2015) and Lindsey and Teles (2017) examine this further by observing concentrated returns of firms and cases of regulatory capture, suggesting that such outcomes arise from rent-seeking and only end up redistributing wealth towards the elites.

While all these authors are referring to the United States, Svejnar and Bagchi (2015) examine the hypothesis worldwide. They find that in countries where billionaires made their fortune thanks to political connections allowing them to

control and build monopolies, the effect of their accumulated wealth and the consequential income inequality on economic growth is extremely negative. In cases where the billionaires weren't politically connected there is no effect between inequality and growth. Also, more unequal countries, where individual fortunes are conditioned on political connections, are all characterized by high levels of corruption. Corruption and political connections tend to go hand in hand. And they seem to be pulling inequality with them (a similar finding was confirmed by Gupta et al, 2002; Glaeser et al, 2003; and You and Kagham, 2005).

Hence the importance of politics in explaining the phenomenon of rising inequality. If the political system is captured by crony rent-seeking elites which make sure that the gains from economic growth are limited to the selected few, and if the majority of the population believe that the system is skewed against them ("rigged") and that even in their greatest effort they still cannot move up the social ladder or even escape poverty, this will fuel their anger and create political discontent. One direct and noticeable consequence of this across the West has been the ascent of extremist populist political parties, feeding on the justified anger of disenfranchised voters.

Consider the example of the finance industry. It is no surprise to learn that the finance industry has been very effective in using campaign expenditures, lobbying, and their accumulated political influence to guide regulatory decisions, block unfavourable and get favourable legislation passed in Congress, and as a consequence

achieve abnormal market returns (Gehlback, 2006; Hockberg et al, 2009; Mian, Sufi, and Trebbi, 2010; Mian and Trebbi, 2011; de Figueiredo and Richter, 2014). In fact, banks with greater lobbying expenditures were shown to be taking on more risk, particularly prior to the recent mortgage crisis (Igan, Mishra, and Tressel, 2012; Blau, Brough, and Thomas, 2013), while greater political influence clearly helped some banks secure much better bailout deals (Duchin and Sosyura, 2012; Thomas, Blau, and Brough, 2013; Calomiris and Khan, 2015), a finding that will be further examined in the second part of the thesis.

The main issue with such activities of the finance industry is that lobbying done by banks to save themselves imposed a huge cost on taxpayers. This happened in Europe as much as it did in the US. The consequence was a sovereign debt crisis as European countries massively accumulated debt to save systemically important banks from bankruptcies, thus imposing a double cost on taxpayers when it came time for harsh austerity measures to address the debt and deficit issues. These activities have increased both inequality and government spending.

The problem therefore is that lobbying activities of one powerful group (even if done on an individual firm-by-firm basis) have imposed direct costs on the rest of society. In the fight for government redistribution the well-organized and the powerful groups will prevail. This goes beyond individual industry examples. A number of papers confirm this effect where greater lobbying expenditures enable interest groups to get a large part of the budget in several countries: Evans (1996),

Kelleher and Yackee (2009), and de Figueriedo and Silverman (2006, 2007) for the US; Helland (2008) for Norway; or even to avoid paying taxes (Richter et al, 2009 for the US, and Schone et al, 2013 for France).

To illustrate how this works, consider the example set out by Deaton (2013, pg. 180-181) on poverty rates in the United States. The poverty trends have genuinely been favourable in the US ever since the 1960s. They have experienced steady declines for all social groups, the old, the young, the African Americans, and all minorities. However in the past decade, even prior to the crisis, poverty rates for all groups have started to slowly increase, except for one group — the elderly (to be exact, all those over 65 years of age). The reason the elderly were the only group that experienced a consistent decline in poverty rates, down to 10% according to the official Census data<sup>3</sup>, is entirely attributed to the success of the Social Security (Medicare) program for senior citizens. And why has the Social Security program been so successful for citizens over 65? Because their interests are being protected by one of the most powerful lobby groups in Washington, the AARP (formerly the American Association of Retired Persons). The AARP has consistently been ranked among the top 3 most powerful interest groups in America by the Fortune’s “Power 25” interest groups ranking (Fortune, 2005), and it has spent over \$260 million on lobbying to Congress since 1998 (Center for Responsive Politics, 2016). Its lobbying efforts have certainly paid off as its members have

---

<sup>3</sup>For comparison, the child poverty rate in the US is 21%, the African American poverty rate is 26%, for Hispanics 24%, for Asians 12%, and for whites 10% (all data for 2014). The overall poverty rate for 2015 in the US was 13.5%. Source: US Census Bureau (2016).

enjoyed a better standard of living and lower poverty rates compared to all other groups. Lowering poverty and improving living standards of senior citizens is certainly a commendable effort; however, the problem is that by having one group successfully diverting budgetary resources towards their own interests, it manages to do so at the expense of all the other groups dependent on public funding. In other words, successful political lobbying from the AARP for its members lowers the funds available for all other groups that are becoming more exposed to the risk of poverty.

Political scientists have uncovered a myriad of ways special interests tend to capture the political system. Some of them link this directly to inequality, others simply point out an indivertible logic of a system gone wrong. Findings have however mostly been limited to the United States, so there is a necessity to provide a compelling, overreaching theory that will be able to explain why the political system tends to get captured by special interests regardless of a specific country's institutional strength<sup>4</sup>.

Bonica et al (2013) tackle the issue of inequality and democracy, by asking why democratic forces haven't lowered inequality. They stress five possible reasons as to why the US political system failed to address the issue of rising income inequality. The first is ideological support for free market capitalism that lowered support for transfers, taxation, and financial regulation. The second was immigration and low

---

<sup>4</sup>Arguably in countries with inclusive institutions and a well-functioning constitutional order, when such practices get uncovered, the actors get punished.

turnout among the poor, in addition to administrative measures that made it more difficult for the poor to vote. The third was that rising wealth of the population made a larger fraction of them less supportive of social transfers (e.g. difference between Social Security, Medicare and Obamacare). Fourth is that the wealthy have used their resources to influence legislative and regulatory decisions via lobbying and campaign contributions, and the fifth is that the political process has further been undermined and distorted by things like gerrymandering, filibusters and political polarization resulting in gridlocks, all designed to maintain the status quo of American politics which clearly benefits the few at the (indirect) expense of the many. Bermeo (2009) however elaborates that democratic mechanisms may even prevent the formation of broad-based support for more redistribution. Democracy is not about achieving economic but political equality, a desire for the state to offer protection against violence and of property rights. Therefore many voters do not see inequality as an issue of high enough salience for it to be directly tackled by their governments. In other words, democracies can survive with high inequality.

Hacker and Pierson (2011) directly attribute the rise of inequality in the US to political causes, rather than globalization or the skills-based technological change hypothesis. According to them the most important change since the 1970s was the rise in organizational capacity of various special interest groups representing the wealthy elites (big business organising to reduce regulation and taxes). The decisions on resource redistribution was not being made by the voters as suggested

by the Meltzer and Richard (1981) median voter hypothesis, but rather by a myriad of special interests, as suggested by Olson (1982). The elites have blocked reforms that could hurt them and have promoted beneficial tax and regulatory policies, tilting the gains from globalization in their favour (and hence causing the huge increase in top incomes which drove income inequality up). The status quo favoured the elites so they lobbied to preserve it, as McChesney (1997) has shown describing this relationship as “money for nothing”.

While Hacker and Pierson (2011) assign the blame for rising inequality to both political parties in the US, Bartels (2016) finds that political partisanship is the main culprit. He too claims that inequality is, in most of its part, a political phenomenon but links its rise to the policy choices made during the terms of Republican presidents. He finds that real incomes of middle classes have doubled during a Democratic President compared to a Republican one, while incomes of the poor and working classes have increased tenfold under Democratic presidents compared to Republican presidents, controlling for all other factors. Bartels even goes so far to suggest that income inequality would have stagnated during the past 60 years had there always been a Democratic President in power, and that it would have been twice as high than it currently is had the Republicans been in power for all this time. These claims hardly constitute a causal link between Republican presidents and inequality, as the statistical analysis is overly simplified, suffers from endogeneity issues, and is based on a very small sample to be able to

adequately convince the reader in the validity of the argument. However, there are clear policy differences between the two main US political parties that arguably have made different impacts on inequality in the past several decades (e.g. tax cuts, minimum wage legislation, labour union power, etc.). His explanation is therefore that inequality was driven by partisan policy decisions.

He also finds evidence of politicians in power being more likely to represent the preferences of the wealthy rather than the general population, a conclusion that has further been examined and confirmed by Gilens (2012) and Gilens and Page (2014). These findings testify of a significant increase in *political* inequality in the US, suggesting the lack of representation of the poor and middle classes whose preferences are almost always subverted to those of the rich. This too could have contributed to the rise of income inequality as the wealthy (roughly approximated and underrepresented by the top 10% income earners) consistently get the policies they prefer (economic, social, religious or foreign), whereas policy preferences of the median income voters and those in the lowest 90th income percentile are consistently ignored. The same finding is confirmed for interest group policy preferences. The poor and the middle classes don't always lose — they get the policies they prefer only if their preferences align with those of the wealthy or with a given interest group (however interest groups tend to represent very narrow groups of citizens in most cases, so the likelihood of their alignment with the general population is low). Accordingly US democracy became the service

of the wealthy and organized few instead of the disorganized many. Moss (2008), through a series of case studies and interviews with Washington insiders, delivers a similar finding, suggesting that policymaking decisions in the US are not driven by what the voters want, but are a consequence of diligent interactions between politicians and special interests who both achieve their goals at the expense of the rest of society.

Overall such findings are striking. Not only do they imply that when it comes to affecting policy change the elites and special interests fare most successfully, they also indirectly imply that such groups could have an important say in setting the agenda that, in most cases, preserves the status quo. This means that the institution of majority rule in America is to a large extent disturbed. Citizens do not get to influence policy. Their preferences only get represented if they happen to align with those of the elites and special interests. Such a democratic order, skewed in favour of elite power explains the status quo bias of American politics and why is it so hard to implement policy change.

This literature gets us closer in being able to attribute rising inequality to elite networks which distort the political process to service their private interests, but there is still one step missing — a coherent individual-level empirical proof on how the mechanism works. More precisely, we need to establish a mechanism on the level of individual politicians and individual corporate executives through which their distortion of the political process widens the gap between the top income

earners and the rest. This thesis hopes to do just that: define (and approximate) elite networks on an individual level to see (1) how both sets of individuals — politicians and executives — benefit from colluding together, and (2) whether their interaction can explain the earnings differential at the top of the income distribution. Each subsequent paper addresses one part of the hypothesis of the thesis and overall they bring us one step closer in understanding how political forces shape the distribution of income.

## Chapter 3

# Corruption and re-election

## How much can politicians steal before getting punished?

### Abstract

Can corruption be used to improve re-election chances of politicians in office? And what is the optimal level of corruption for doing so? In this paper I first make a methodological contribution of using suspicious public procurements in local government as a unique proxy for corruption. I then tie suspicious procurements to re-election probabilities of mayors in Croatian cities and municipalities from 2009 to 2013, and find that due to a rent-extracting relationship formed within elite networks between firms and political elites, local politicians can engage in corruption

and still win elections. There is an optimal level of suspicious procurements for which politicians maximize their re-election chances. When a mayor surpasses the cut-off level of around 20% of suspiciously allocated funds from public procurement his or her probability of re-election starts to decline, while he or she loses office for at least one half of all procurements allocated suspiciously. In order to address potential endogeneity issues I apply a fuzzy regression discontinuity design based on population thresholds that determine the size of the local council, where the intensity of the treatment (corruption) increases with increasing council size. The results overall confirm a concave relationship between corruption and re-election.

**Keywords:** corruption, re-election, suspicious procurement, elite networks, local government

### 3.1 Introduction

Political economy theory posits that corrupt politicians holding power have every incentive to design a system that maximizes their probability of re-election as well as their rent-seeking behaviour (Brennan and Buchanan, 1980; Ferejohn, 1986; Persson and Tabellini, 2000; Besley, 2006). This is usually carried out through informal networks of interests — which I refer to as *elite networks* — where distributional benefits (jobs, exclusive contracts, favourable legislation, protectionism) are shared to actors within the network who in return promise political loyalty. The key to political survival lies in the effectiveness of such networks to preserve

the systemic corruption that enables all the connected actors to keep extracting rents (Bueno De Mesquita et al, 2005; North, Wallis and Weingest, 2009). In practice this means that politicians holding power will engage into numerous deals with various special interests from which they can expect to receive electoral support (Coate and Morris, 1995; McChesney, 1997; Grossman and Helpman, 2002; Gilens, 2012; Gilens and Page, 2014). In a co-dependent system based on close personal ties where concessions are traded in return for votes, bribes, or other kind of support, all members of an elite network have every incentive to preserve their status quo and keep corrupt politicians in power for as long as possible.

In this paper I examine whether corruption formed through such elite networks has an effect on re-election chances of local politicians, and to which extent do voters punish corruption. Corruption is defined by observing suspicious public procurement contracts administered by local mayors to private firms. Focusing on suspicious procurements allows me to directly implicate the relationship of mutual benefit in which rigged contracts are exchanged for electoral support. The hypothesis is that due to personal connections and mutual dependence between rent-extracting firms and the political establishment, politicians at a local level of governance can engage in corruption without having to sacrifice their chances of winning elections. Moreover local politicians have a greater probability of staying in power if they create an informal network of interests that enables and encourages corruption. There is a limit to corruption however, as politicians get punished for

very high levels of suspicious procurements.

I make two important contributions to the existing literature on corruption and political survival, one methodological and one empirical. First, I reinforce the idea of measuring grand-scale corruption directly through suspicious public procurement contracts (as done by Fazekas et al, 2016, Ferwerda et al, 2017, and Fazekas and Kocsis, 2017). I use data on local public procurements in 556 Croatian cities and municipalities, conducted in the period of a single local government mandate from 2009 to 2013, from which I create several proxy variables of corruption. From the database on over 35,000 public procurement contracts I single out various cases which present a reasonable doubt in the regularity of the procurement procedure, defining them as *suspicious procurements*. I conducted interviews with key stakeholders in the procurement process, including special criminal investigators, to validate my definitions on how to recognize potential corruption from the data. Examples include cases in which firms with no employees and as a single bidder on the tender receive multiple contracts worth millions, cases in which firms sign contracts that vastly exceed their capabilities, and cases when firms with substantial losses receive indirect subsidies through lucrative deals. As I clarify in the methodology section not all such contracts necessarily represent corruption, however each procurement administered this way raises a red flag of suspicion over the allocation process. The methodological contribution, despite its own caveats, attempts to provide a blueprint for researchers to move away from

perception indices and try to quantify corrupt behaviour more directly.

Second, I find evidence of a clear effect of corruption on re-election, a relationship I show to be non-linear. Corrupt practices entail a concave effect on re-election chances, meaning that corruption can increase the probability of re-election, however only until a certain cut-off after which corruption is too high and politicians gets punished. I successfully bridge the gap between two competing hypotheses of whether voters punish corruption — they do, but not until it becomes too large and too noticeable. I calculate an optimal level of corruption to keep a politician in power. Probability of re-election is maximized for about 20% of all suspiciously allocated funds from public procurement. If more than 50% of all procurement is allocated suspiciously, a politician very likely loses power. Because this relationship could potentially be subject to selection bias I apply a fuzzy regression discontinuity (RD) design based on population thresholds that determine the size of the local council. The fuzzy design is used since the intensity of the treatment — proxy level of corruption — increases with increasing council size. The jump after each threshold represents an as good as random assignment of local units just above and just below the threshold. The results of the RD estimation confirm the conclusion of a concave relationship between corruption and re-election.

The following sections present these contributions in greater detail. The second section examines the literature on why voters fail to punish corruption, how political networks of key supporters give rise to corruption incentives, and why

Croatia offers a good empirical test of this theory. After describing the data and the measurement procedure behind six different proxy variables of corruption in the third and fourth sections, the empirical section presents the full set of results on the relationship between corruption and re-election. The final section concludes and presents implications for further research.

### **3.2 Do voters punish corruption?**

There is one puzzle that appears to characterize electorates regardless of their institutional background and strength of democratic institutions. The impact of corruption on chances of re-election should by any reasonable assumption be negative. However a large portion of the literature suggests otherwise, leaving an open question of why voters do not punish corruption at the polls, at least not as much as one should expect.

A famous study by Peters and Welch (1980) found that US Congressmen from 1968 to 1978 were more likely than not to get re-elected following a corruption charge against them. Also for the US, Rundquist, Strom and Peters (1977) conducted a survey experiment and found that voters would trade-off corruption for other things they value in a candidate. Similar findings were confirmed in Italy (Chang and Golden, 2004; Golden, 2004), Greece (Dobratz and Whitfield, 1992; Patrikios and Karyotis, 2008) and Japan (Reed, 1999; Nyblade and Reed, 2008), to name only a few developed countries. Even when studies do find that cor-

ruption negatively affected re-election (e.g. Ferraz and Finan, 2008, 2011 and de Figueiredo, Hidalgo, and Kasahara, 2012 for Brazil; Dimock and Jacobson, 1995 for the effect of the House banking scandal on US voters in 1992; or a repeated study of US Congressmen from 1982 to 1990 by Welch and Hibbing, 1997) the overall pattern still suggests that politicians are either not punished at all or that they are not punished enough<sup>1</sup>.

The underlying assumption beneath this puzzle is that the vast majority of voters are honest people who dislike corruption. Accordingly they should punish corruption when they become aware of it. But being aware of corruption in a world of informational asymmetries is not straightforward. Politicians have every incentive to hide their illegal activities and particularly the strength of their powerful networks from the public eye. This has led to the origination of two competing hypotheses explaining the conundrum of why voters fail to punish corruption, even when they know it is likely to be present.

According to Rundquist, Storm and Peters (1977), Kurer (2001) and Winters and Weitz-Shapiro (2013) these can be defined as the *information hypothesis* and the *trade-off (material benefits) hypothesis*. The information hypothesis follows the Downsian (1957) logic of rationally ignorant voters. Voters either rationally choose to stay misinformed or they suffer from informational asymmetries. There are two ways to overcome the information asymmetry: (1) voters receive information from

---

<sup>1</sup>The literature examining this relationship in developing and emerging countries found evidence both in support of the idea that corruption does not get punished at the polls (Manzetti and Wilson, 2007; Chang and Kerr, 2017), and against it (Klasnja, 2015; Klasnja, Tucker, and Deegan-Krause, 2016).

sources they consider to be credible and trustworthy (the media would be one mechanism for closing the informational asymmetry, however voter trust in media is selective at best), or (2) if this information is easily accessible to all voters (Ferraz and Finan, 2011 showed that this mechanism worked in Brazil — an independent audit on political corruption closed the informational asymmetry and the voters punished corrupt mayors).

The trade-off, or material benefits, hypothesis on the other hand claims that voters are fully aware of political corruption in the country, but tend to overlook it due to personal benefits they might receive (like patronage or pork spending) and/or ideological proximity to their candidate. This hypothesis envisions politicians in a quasi-Robin Hood fashion where voters know that politicians steal, but “at least they are giving some of it back to the people”. Furthermore, due to uncertainty regarding the challenger the voters often fear replacing the incumbent as they are not sure that the challenger will keep delivering the same benefits as the incumbent, nor are they certain that the challenger will not be even more corrupt than the incumbent (this is particularly relevant in electorates with negative selection into politics where the voters perceive all politicians as bad — as in Caselli and Morelli, 2004).

In each scenario, however, if corrupt politicians deliver successful policies (infrastructure, growth, etc.), i.e. if they are perceived to be of high competence, corruption is irrelevant to the voters and politicians can avoid punishment for

years. This is one mechanism by which corruption can become entrenched in democracies. Podobnik, Vukovic, and Stanley (2015) show that democracies do not cause corruption but can serve as a mechanism that preserves it. If voters rationally decide to be ignorant about political corruption, or if they rationally support a known corrupt candidate, then the mechanisms of political competition do not work, and democratic institutions are inefficient in combating corruption (Kurer, 2001; Helland and Sorensen, 2012).

With this in mind, the given two hypotheses are not necessarily mutually exclusive. For example, core party supporters choose to remain rationally ignorant about corruption and dismiss any evidence against their candidates as politically motivated accusations from the opposition or the biased media. In this case a candidate's ideology and his perceived competence keep him safe from punishment. If a politician lacks competence or likeability, or if he failed to deliver the expected benefits, then the voters will punish him at first implication of corruption. The combination of the two hypotheses suggests that voters find both of these factors important: competence (in terms of likeability, ideological proximity, and delivered benefits) and corruption, the importance of which is triggered only if competence is low. Voters thus react to corruption only if the candidate is of low quality, or ideologically distant.

### 3.2.1 Elite networks, corruption, and non-linearity

There is another factor that can increase informational asymmetries among voters — the existence of elite networks: informal social networks between politicians and politically connected firms where personal ties and long-term interactions solve the collective action problem and build trust between involved actors.

In this paper I expand the classical definitions of rent-seeking (Tullock, 1967; Krueger, 1974), clientelism (Stokes et al, 2013), and patronage (Piattoni, 2001) to include deals between politicians and politically connected private sector firms which instead of competing on the market for customers compete for political support. Many firms are handed government contracts but only a subset of those classify as politically connected. Specifically the ones whose primary source of income is conditional not on government contracts, but on political connections to get those contracts. Such firms are defined to be *rent-extractors*, where rent represents any benefit from corruption, as is its standard definition in most political agency literature (Brennan and Buchanan, 1980; Ferejohn, 1986; Persson and Tabellini, 2000; Besley, 2006)<sup>2</sup>.

Combining insights from several theories — Shleifer and Vishny (1998), Verdier (1995), Root and Nellis (2000), Fisman (2001), Bueno De Mesquita et al (2005), Faccio (2006), North, Wallis and Weingest (2009), Schoenman (2014), and Fazekas

---

<sup>2</sup>In this paper I use rent-extraction and corruption interchangeably despite the obvious differences between them (not every rent is necessarily gained through corruption, nor does every corruption end with rents). For example Ades and Di Tella (1999), defining rents in the classical sense as forms of protectionist policies, find that in countries where firms are able to secure higher rents, corruption is higher. There is a clear correlation between the two although one does not imply the other.

and Toth (2016) — I present the hypothesis that close ties of politicians with rent-extracting firms, created through the formation of elite networks, can increase a politician's chances of re-election, and keep him or her in power for a long period of time. Any activity that includes generating rents from a personal interaction between politicians and the private sector is considered to be political corruption in the broader sense (Rose-Ackerman, 1978; Shleifer and Vishny, 1993; Knack and Keefer, 1995, Gardiner, 2002). Corruption defined this way enables me to measure it directly by examining misallocations of public procurement contracts and test for its impact on re-election. As will be discussed in the methodology section suspicious procurements are not necessarily clear-cut evidence of corruption, however they are useful in portraying the relationship of interest.

The motivation to be a part of an elite network is threefold. Verdier (1995) and Root and Nellis (2000) claim that such relationships reduce risk and uncertainty for the connected members. Being part of an embedded network of interest encourages all parties to stay in, be loyal, and protect the patron, knowing that his demise might endanger one's entire enterprise and its expected revenues. The obvious outcome is that the politically-connected firms find it easy to obtain monopoly power and are very successful in lobbying their governments for greater protectionism. An equally important motivation derived from risk-aversion is the ability to extract rents (Shleifer and Vishny, 1998; Besley, 2006). Politicians receive rents in the form of bribes or campaign funding that helps them get re-elected. Rent-extracting

firms receive their rents either in the form of favourable legislation (e.g. monopoly rights or lax regulation) or an exclusive procurement contract. Finally, being part of the network breeds nepotism as it relies on a number of “small favours” that vary from playing golf and getting tickets to sporting events (McChesney, 1997), to directly hiring friends and relatives of the politician in the firm as a favour to the patron (Robinson and Verdier, 2013).

The more people involved within a network of a politician’s key supporters, the more likely that corruption will be noticed by outsiders, rendering thus a non-linear effect on political survival. How? Given that corruption is hidden, the mechanism through which the process works could be the following: voters are unaware of actual corruption and are only aware of rumours and media reports. These rumours and media reports increase in intensity as the mayor gets involved in more corrupt practices (e.g. allocates more fraudulent procurement contracts). In other words, the more corrupt activities there are, the more difficult it is to hide them from the public’s eye. The voters and the reporters however do not see the full extent of corruption. They only see signals if other people talk more about them. A mayor who is corrupt manages to deliver a substantial amount of public goods through his cronies, and this is perceived to be beneficial by the voters (and rewarded with re-election), as they do not notice any direct corruption. However as more and more jobs are distributed to a narrow group of core supporters, voters (and reporters) do start to notice and the information spreads.

This is why I assume a non-linear relationship between the corruption and re-election probabilities — I measure procurement-based corruption, which is not observed by the majority of the public at lower levels. A mayor easily gets away with some small(er) level of corruption without antagonizing too many constituents. As more jobs are distributed within the elite network, more and more local constituents tend to notice that the distribution of procurement contracts (or jobs in the public sector) is unfairly skewed towards the same people, the people with closer ties to the mayor. This triggers media reports and spreads the information to a wider number of voters, who, upon receiving this new information, update their beliefs over the mayor's competence and behaviour, and decide whether the level of corruption is indeed too high and that it should be punished. When too much corruption becomes visible, mayors have a lower probability of survival.

Intuitively, there is no justification of why corruption should entail a linear effect on the probability of winning (so that it is either a strictly increasing or decreasing function of corruption). It is much more likely that the relationship depends on the specific situation the incumbent is in. For example, is the incumbent electorally safe, or does he have to fight for his or her seat? Nyblade and Reed (2008) use the argument that the effect of corruption on re-election depends on the uncertainty surrounding the electoral race. If a politician expects a close race he is less likely to engage in corruption (to "loot") but more likely to try and disturb the election process to win (to "cheat"). In other words, the behaviour of a politician depends

on how good he performs on previous elections and to which extent does this allow him to cheat.

To sum up, an elite network type of relationship which reinforces the creation of corruption also entails a concave effect on a politician's re-election chances. Voters see the creation of public goods and reward the politician without knowing whether he delivered the public goods with some corruption. This partially corresponds to Besley's (2006) model of political agency, where political types (good or bad) are hidden, so voters must infer type based on action — whether or not the politician provides enough public goods and under what cost. Public goods can be provided under a high cost by a bad politician who extracts rents (and still gets re-elected) if the voters believe he faced a high cost shock. Extending this logic further, a bad politician can fool the people only a few times before such practices get uncovered. Hence the concave effect.

### **3.2.2 Why Croatia?**

Croatia is particularly suitable to be analysed under the given theoretical framework. First, the country is riven with endemic corruption. According to Transparency International (TI) it ranks 55th out of 175 countries (TI, 2016), but with an average score of 49 out of a 100, defining it as a mostly corrupt country. It also ranks at the bottom of the EU Member States, with only Greece, Italy, Bulgaria, and Romania having higher corruption levels. The European Commission

(2014) anti-corruption report states a similar conclusion, reporting that Croatia is particularly vulnerable to corruption in its public procurement procedure and its legal framework. According to the report 64% of domestic entrepreneurs claim that corruption is extremely high in the public procurement process, while 89% say it is impossible to win a tender without “pulling strings”.

Furthermore, a few recent cases of political corruption trials testify of a worrying relationship between politics and rent-seeking firms. Croatia’s former Prime Minister (served six years in office, winning two elections) was accused and sentenced to nine years in prison on corruption charges for forcing state-owned enterprises to hand a private sector firm exclusive procurement contracts<sup>3</sup>. His party, the conservative HDZ, was also found guilty on corruption charges in 2014, and was ordered to return the money they’ve acquired illegally for campaign financing (30 mil kunas; about 4 mil euros). However the Supreme Court brought down the entire sentence in 2016 due to a technicality, returning the process back to the city court in Zagreb, raising public outcry. Other political parties were not immune to the same type of behaviour. In the past several years five mayors from large Croatian cities (Dubrovnik, Vukovar, Varazdin, Sisak, and the capital Zagreb), originating from parties in the centre-left coalition (SDP and HNS; social-democrats and liberal-democrats), were all arrested on corruption charges for abusing power by favouring private sector companies and extracting bribes for

---

<sup>3</sup>He used the money flowing to this company to take rents for himself and to finance his party’s campaigns. This is only one of the six major cases against the former PM (Petrusic, 2014).

themselves. One of these mayors was sentenced, served his one year probation sentence, and came back to win a local by-election in 2015 (Tomicic, 2015). Two other mayors were in jail for six months on corruption charges, and their ongoing court trials did not prevent them from winning regular local elections in 2017. The country's electoral institutions support such outcomes, given that there are no term limits for local office, meaning that local mayors may stay in power indefinitely (some of them have been holding local office for more than 20 years, and a great deal have held office for at least 12 years). Croatia is thus an ideal case-study to examine the impact of political corruption on long-lasting re-election chances.

Finally, the Croatian case is comparable with two groups of countries: Post-Communist transitional economies, given the similar historical and institutional background, and Mediterranean countries of the European South such as Greece, Italy, Spain, or Portugal, which share similarly high levels of corruption persistence. Any extension to the empirical contributions of this paper seeking its external validity should first attempt to test the given contributions on these two groups of countries.

### **3.3 Data**

The complete database on public procurements contains over 80,000 contracts administered between 2009 and 2013, worth in total around 88bn kuna (about

11.7bn euros)<sup>4</sup>. From this large dataset I managed to single out 35,724 contracts signed by the local public administration for the duration of a single mayoral mandate, i.e. from May 2009 to May 2013. The total value of these contracts was 28.765 billion kunas (about 3.7 billion euros). This includes all public companies under direct ownership of the local government such as communal services, but excluding local health and education procurements, since these are not necessarily under direct control of the local mayor. The benefit of focusing only on local government procurements is that their administration is under full discretion of the local mayor.

The source of procurement data was the Official Gazette of the Republic of Croatia (2015), and its online registry of public procurement. One part of the database, for the years 2009 to 2011, was extracted from the same source by independent non-government organizations, Vjetrenjaca (2011) and Integrity Observers (2012). The second part of the database, for the years 2012 and 2013 was extracted directly from the Official Gazette, while simultaneously checking the validity of the data extracted by the two NGOs for the previous period. The procurement database contained information on who proscribed the legal tender (which unit of government), location (which municipality), date, total value of the contract, the firm that won the tender, its financial identification number, and a brief description of what the tender was for. The precision of the data depends

---

<sup>4</sup>2009 is the first year the government started to publish all procurement contracts in a single online database in an effort to increase transparency.

solely on the quality of entry in the Official Gazette.

The second step was to combine the official data on each procurement contract with the financial statements for companies that were engaged in the procurement procedure. Each contract was linked to a firm via its financial identification number (OIB), and clustered across local units (city or municipality) where it was completed. The source of data on firm financial statements was extracted from the government's Financial agency (FINA), the official public database on business entities, for all four years during which the tenders have been assigned. The financial statements data includes size of revenues and expenditures, total profits and losses after taxes, tax liabilities, total assets and liabilities, total capital, and number of employees for all firms that won tenders in the observed period. This way it was possible to define suspicious procurement contracts by observing which firms were potentially favoured in the procurement procedure. For example, the data shows how many tenders a firm has won over the years, and what the competition for the tender was, which made it possible to single out firms that were the only bidder, and that won multiple contracts in a single year. The next section describes the various corruption proxies that were made using some of these indicators from the database. It should be noted that around 10% of all procurements could not be assigned to any company due to errors in reporting the unique financial numbers of a firm, or other missing data. This raises a slight concern with regards to potential measurement error, however as the next section explains, the ways in which

corruption indices were defined reduces this concern.

The data on electoral results was extracted from the State Electoral Commission of the Republic of Croatia (DIP, 2014) for local elections in 2009 and 2013, and for parliamentary elections in 2011, on a municipal level for 556 cities and municipalities. The sources of the many economic and demographic controls were the Croatian Bureau of Statistics (DZS, 2013, 2014) and its 2011 Census, the Croatian Unemployment Bureau (2014), and the Ministry of Finance (2014). The Electoral Commission was the source of data on various political, but also mayoral and municipal specific characteristics, such as turnout, vote shares in various elections, data on political experience, political polarization, size of governing coalition, size of local council, and mayor gender.

### **3.4 Measuring corruption in public procurements**

Measuring corruption precisely is immensely difficult. The primary reason is obvious: participants of corrupt activities successfully hide their transactions from any methodological coverage. Unless there is a criminal investigation followed by a court hearing, these transactions will remain hidden from the public eye. However, grand-scale political corruption manifested through close relationships between politicians and firms can be approximated by carefully overseeing the flow of public resources. In particular the price, quantity, and the end quality of the public good, provided as a result of a dubious procurement process. The Euro-

pean Commission (EC, 2014), in their corruption report on 28 EU member states, reported that the lack of transparency in public procurement is the main source of political corruption in all of its member states.

This section describes the main methodological contribution of this paper. It describes the logic behind classifying certain contracts as suspicious and lists all of the six proxy variables used to measure corruption. The way corruption is defined in the paper is by no means subjective; it follows the literature and uses established indicators of fraudulent procurement practices (e.g. Fazekas et al, 2016 use large procurement datasets to uncover “red flags” such as cases of restricted competition and multiple contracts repeatedly awarded to the same firm; while Ferwerda et al, 2017 use data from criminal investigations to develop risk indicators of corruption based on lack of transparency, the size of the tender, and collusion of bidders)<sup>5</sup>, and it validates the given measures by conducting interviews with key stakeholders in the procurement process: special police investigators, civil sector anti-corruption watchdogs, local government officials, public sector bureaucrats, and entrepreneurs<sup>6</sup>. The interviews served the purpose of external validation but also in recognizing and including other sources of suspicion. After establishing how suspicious procurements were quantified I perform an additional validity check using independent evidence from state audit reports on procurement irregularities

---

<sup>5</sup>Many research efforts use specific indicators to quantify corrupt behaviour of politicians. Reinikka and Svensson (2004) study the misappropriation of education spending in Uganda, Olken (2007) the quality of roads in Indonesia, Ferraz and Finan (2011) the misappropriation of local government budgets via public procurements in Brazil, Bandiera, Prat and Valletti (2009) public procurement of civil service in Italy, and Di Tella and Schargrotsky (2003) public procurement of hospitals in Argentina.

<sup>6</sup>Full summaries of interviews available in Appendix B

and a local government transparency indicator.

Despite best efforts and despite rigorous cross-referencing of contracts and validation of the created proxies, there is always room for doubt in whether the given definitions truly capture corruption. Because corruption can never entirely be uncovered by the data alone (i.e. without criminal investigations), I cautiously define such practices as *suspicious procurements*. The paper does offer readers the benefit of doubt in the empirical sections and in its general phrasing by using the word *corruption*, however the way corruption is classified in the paper follows the theoretical logic of measuring some form of collusion between rent-extracting firms and local politicians. Potentially fraudulent procurements are only one part of the story in how mayors extort their influence in order to gain benefits, but they do capture the essence of a rent-extracting relationship within an elite network environment. Because it would not be entirely precise to classify suspicious procurements as standard political corruption, the indices should therefore only be considered as *proxies for corruption* defined through a rent-extracting relationship between firms and politicians.

### **3.4.1 Defining the proxies**

What do suspicious procurements look like? Any contract that raises reasonable doubt that factors other than the official tender requirements were used to determine who gets the contract, or that the tender requirements were designed to

favour a particular firm over all others is classified as suspicious. These include cases in which firms with zero employees receive multiple contracts as the only bidder in the procurement, firms that win tenders in which the value of the procurement contract is larger than their average annual revenues, and firms with large losses which are indirectly subsidized by the local government. From these categories six proxy variables for measuring corruption are assembled, with two additional variables that measure corruption in public infrastructure investments (according to Tanzi and Davoodi, 1997, and Mauro, 1998). The reason I focus only on local procurements is because the local government does not use procurements to buy highly-specialized technical equipment (hospital or military) which usually only a handful of companies in the whole country can do. The vast majority of local government contracts include random construction work, maintenance jobs, or a number of services from driving to printing. These are jobs that are highly unlikely to have only one suitable firm to perform them. An additional benefit is that according to Law of local government (Croatian Parliament, 2013, NN 36/09) the allocation decisions are at the mayor's full discretion. This makes it useful to operationalize and test the theoretical model of how mayors' discretionary decisions in procurement allocations affect their relationship with connected firms and how this in turn affects their prospects of re-election.

For each category of suspicion a corruption proxy is calculated as the ratio of total resources (the monetary amount) allocated to suspicious procurements to

the total value of all procurements in the observed period in a single municipality (methodologically similar to Ferraz and Finan, 2011):

$$k_i = \frac{\sum_i^n s_i}{\sum_i^n u_i}, \forall s_i \subseteq u_i \quad (3.1)$$

The proxy variable in all cases is the share of the total monetary sum of suspicious procurement contracts ( $s_i$ ) in total contracts ( $u_i$ ), determined for the entire observed period for a given municipality  $i$ .

A procurement contract is labelled as suspicious ( $s_i$ ) in the following three cases:

- (1) If the total value of a single contract given to a particular firm exceeds 70% of its total average annual revenues for the four years observed (large tenders)<sup>7</sup>
- (2) Cases in which multiple contracts were allocated to firms with zero employees and which were the only bidder at the tender (fake firms)
- (3) Cases in which multiple contracts were given to companies which were operating at a net loss prior to the tender, but made a profit in the following year after signing the contract (as a measure of indirect subsidies gained through political connections).

---

<sup>7</sup>Following the formula in 3.1,  $k_i$  will be the sum of all contracts allocated by the municipality in which a particular firm wins a tender with a value at or in excess of 70% of its average revenues, divided by the total value of contracts from all public procurement contracts in the municipality. The same reasoning is applied to variables (2) and (3).

The detailed explanations behind each indicator are presented in Appendix B, under the summary of interviews with key stakeholders in the procurement process, and in Appendix C, which presents the summary statistics of corruption proxies in addition to a few examples for each indicator. This section briefly clarifies the logic behind each indicator, summarizing the information from the appendices.

The first proxy, (1) *total value of contracts exceeding 70% of average annual four-year revenues*, focuses on firms which had little or no sources of revenues other than contracts received from the local government. The puzzling thing about the majority of such firms is that they received contracts worth several millions, but their revenues remained minuscule in the following years (about 73% of the over-70 subsample includes such firms; and they represent about 17% of the entire local procurement sample). This suggests a clear example of accounting fraud as there was no sign of the allocated funds in their annual reports. According to the information from the interviews (Appendix B) such cases do happen occasionally. They represent clear examples of money laundering schemes usually performed by fake firms, where a firm with almost no revenues would win a tender at a dumping price, and then sell it to another (often sister) firm at a higher price.

In addition to firms whose contract value exceeded their revenues, there were others that did exist on the market, but who were over-reliant on government support (about 27% of the subsample). The 70% cut-off for the entire group was chosen as the first marginal point above having two thirds of one's revenues

received from public procurement, and it was the estimate that correlated highest with other used measures of corruption. A sensitivity analysis was performed for other values of the cut-off. Anything above the 100 % cut-off exhibits the same effects as the 70% cut-off, while anything below the 50% cut-off exhibits no significant effect. Values between 50% and 70% could have also been used, however the 70% cut-off was chosen to have a conservative estimate of potential corruption. In other words I prefer to underestimate rather than overestimate the approximation for corruption, making sure I only capture firms that have the highest probability of being politically connected. This is further reinforced by the information from special police investigators who state that every corrupt firm they were investigating had an over-reliance on government procurement contracts (see Appendix B), and furthermore that most large tenders given to firms with otherwise low revenues always raise red flags of suspicion. In addition, having income come only (or mostly) from public procurement contracts suggests a high level of dependence on politics and the allocation of public budgets, which raises the probability that a firm would have to maintain good political connections in order to be awarded a contract. Particularly since the local contracts almost never included highly-specialized jobs that only a single firm can do.

The second proxy (2) looks at *multiple contracts given to firms with zero employees where the firm was the only bidder in the auction* — the so-called ‘fake firms’ (see Appendix B). Similar to the first proxy indicator this was coded as

suspicious because firms with zero employees that won the tender almost never performed the tender by themselves, and have always served as a proxy company for another bidder who was unable to compete at the tender due to a conflict of interest (personal connection to the decision-maker). This indicator captures corruption with a high degree of confidence and has been labelled as almost certainly a case of corruption. However, such firms did not grab a large portion of the procurement allocation, only around 3% in total, but this still amounted to almost 800 million kunas received from the local budgets.

The third proxy (3) looks at *how much the local government indirectly subsidized firms with large losses*, which after signing the procurement contract several times higher than their annual revenues, realized a profit in their next business year. Giving money to firms operating with losses is not a corrupt activity per se, but it constitutes bad practice according to the procurement administrators, and is highly indicative of a political connection (see Appendix B). It also raises outrage among non-connected entrepreneurs as firms with large losses acquire political connections which ensure that they are ‘bailed out’ during times of economic hardship.

The overall conclusion that can be drawn from the use of the aforementioned proxies is that even though not every single case can be labelled as outright corruption, all of them raise red flags of suspicion with the authorities, and most of them implicate some form of political connections. Despite this evidence, there is obviously still room for caution in interpretation and a possibility of unintentionally

labelling some regular procurements as corrupt or a result of political connections. The margin of error therefore certainly exists, however given the volume of contracts and the fact that each proxy only looks at the relative level of suspicious contracts within a city or municipality, the error most likely does not undermine the validity of the definition.

Using the given definitions of suspicion I am able to design several indices of corruption proxies, in order to capture all possible suspicious procurements in a given municipality. These indices are intended to be an even better indication of a rent-extracting relationship because they capture all cases where political connections could have played a role in the allocation process within a single local unit. Each index is a weighted average of the aforementioned variables along with several others taken out from the database and described below<sup>8</sup>:

- (4) *Index 1*: cases in which the value of contracts exceed 100% of the total revenues of the firm, contracts in excess of 100% of the firm's total losses (i.e. procurement contracts that completely cover the firm's loss), and firms with zero employees which were the only bidder at the tender;
- (5) *Index 2*: cases where firms received multiple contracts as the only bidder (but not only those with zero employees), firms that received contracts in excess to 70% of their average annual revenues, and all firms that overturned losses into profits for the full value of the tender;

---

<sup>8</sup>All three indices are assembled in the same way as the first three proxies, described by equation 3.1.

- (6) *Total Index*: weights all of the aforementioned indicators of suspicious procurement equally, capturing all the potential frauds into a single, common corruption indicator. None of the individual cases overlap, meaning that, for example, a firm that had both zero employees and contracts in excess to 70% of revenues was included only once. Because the Total Index takes into account all suspicious procurements within a single city or municipality it is the main corruption proxy used throughout the empirical part of the paper.

Finally, I assemble two additional robustness-test variables. They both rest on the assumptions of Mauro (1998) and Tanzi and Davoodi (1997) that public infrastructure investment spending is the type of spending most vulnerable to corruption and resource manipulation. In the EU statistical framework such variables are defined as expenditures on non-financial assets. The Croatian Bureau of Statistics (2013) defines them as expenditures for purchase of produced assets (building structures, machinery and equipment, and other fixed assets such as inventories) and non-produced assets (land, subsoil assets, patents, concessions, licenses, etc.). Any spending on such assets must be made via the official public procurement procedure, which is why it represents a good robustness measure to the existing proxy indicators. Usually such expenditures are observed in relative terms, either in per capita or as a percentage of GDP. Unfortunately, there are no GDP figures for local municipalities in Croatia as they are too small of a unit for which GDP can be aggregated and calculated. This is why I use (7) *the share of non-*

*financial assets in total local budget expenditures* and the standard (8) *expenditures for non-financial assets per capita*.

The summary statistics of all 8 corruption proxies are presented in Table 5.1. It shows the mean values for each corruption proxy by city, municipality and overall. As expected, instances of corruption tend to be higher in cities than in municipalities, except for the final two robustness variables. This makes sense since municipalities have smaller financial capabilities and thus every infrastructure spending is likely to be higher in relative terms.

Table 3.1: Summary statistics of the 8 corruption proxies

Corruption proxy	(1)	(2)	(3)	Index 1	Index 2	Total Index	NFA to exp	NFA pc
City	0.286	0.077	0.041	0.177	0.117	0.147	0.177	676.1
Municipality	0.091	0.055	0.02	0.078	0.046	0.062	0.345	731.7
Overall	0.135	0.059	0.024	0.101	0.062	0.082	0.195	719.1

Note: The standard deviation and the range of each proxy is given in the summary statistics Table A1 in Appendix A.

### 3.4.2 Independent validation of the proxies

An ideal example of external validation of my corruption proxies would be to use actual arrests and indictments of mayors, however since such cases are rare and happen infrequently (and are often correlated with electoral cycles), I seek other independent evidence about potential corruption. I perform a validity check using audit reports on either the violation of the procurement procedure or any other kind of budgetary irregularities, in addition to a local government transparency

indicator. I use two sources for this validation exercise: the official state audit that each year audits local budgets and expresses positive or negative opinions regarding the budget process and the allocation of procurements (State audit, 2014, 2013, 2012, 2011), and a local budget transparency index, created by the Croatian Institute for Public Finance (IJF, 2016).

The State audit issues a passing grade to cities and municipalities which show no irregularities in its budgetary allocation, its accounting processes, its procurement allocation, or in managing its assets. It also publishes a list of local units which have had irregularities and violations of the law in either one of the aforementioned procedures. Such irregularities are not defined lightly. The irregularities and violations recognized by the State audit are often used in cases of criminal indictment against corrupt politicians, in the rare cases when such arrests are actually made. Irregularities in the budget allocation process usually imply fraudulent bookkeeping and financial statements, deliberate misrepresentation of the value of the unit's assets like land and property, missing important documents and contracts, favouritism in funding selected budgetary recipients (e.g. for schools, cultural institutions, health facilities, sports, etc.), allocating funds without underwriting procurement tenders, irregular compensation of city or municipal assembly-members, and various other errors and omissions. In public procurements irregularities are mostly related to allocating contracts without tenders, but even when such tenders were underwritten in many cases the negotia-

tion process was highlighted as troublesome (by favouring a single bidder) and not abiding to the law. I separate the cases of procurement irregularities with cases of any irregularity in order to design two indicator variables that are supposed to measure the extent of potential corruption in local government. The indicator is coded as 1 if the state audit issued a negative grade in any of the four years during the mayor's tenure, and 0 otherwise.

In addition to these official indicators of irregularities and violations, the budget transparency index published by the Institute for Public Finance is another useful robustness indicator. It ranks cities and municipalities based on whether or not they made the entire decision-making process behind its budget accessible online (including planning for the next fiscal year, previous year execution, revision, details of the current budget and guides for the general public). Each of these is required by law to be published online and easily accessible to the public. Although the transparency indicator in itself does not measure corruption or any illicit activity, it creates a decent measure of transparency that should, according to the literature (e.g. Lindstedt and Naurin, 2010), be highly correlated to corruption.

All three indicators based on independent evidence of suspicious behaviour should therefore offer a good validation of the corruption proxies. Table 3.2 below uses the Total Index of suspicious procurements and compares it to the three designed indicators: State audit irregularities, State audit procurement irregularities

(both coded as an indicator variable), and Budget transparency index (coded on a scale from 0 to 5). The corresponding t-tests are reported below. The table shows only cross-correlations with the Total Index, however in the case of all other proxies the results point to the same conclusion: there is a statistically significant correlation between independent evidence on irregularities and transparency and the indices defining suspicious procurements.

Table 3.2: Corruption correlation matrix

	Total index	Audit irregularities	Audit procurement irregularities	Budget transparency
Total index	1.00			
Audit irregularities	0.258 (0.00)	1.00		
Audit procurement irregularities	0.213 (0.00)	0.484 (0.00)	1.00	
Budget transparency	0.112 (0.008)	0.127 (0.002)	0.166 (0.00)	1.00
Corruption index by all irregularities	Corruption index by procurement irregularities			
	0.0623*** (0.00)			0.0662*** (0.00)

Note: p-values reported in parentheses. The final two rows report t-tests for the differences in corruption indices by all irregularities (left) and by procurement irregularities (right).

### 3.5 Does corruption affect re-election?

The main question of interest is whether corruption approximated via suspicious procurements within elite networks affects re-election chances. The theoretical implications suggest that it should, and that the relationship between the two

should be non-linear. Before examining the treatment effect using RD estimation I first establish the conditionality for the right functional form, and examine the relationship with respect to all corruption indices available, including the two robustness-test indicators of infrastructural corruption.

The first step is to test directly how corruption gets turned into votes. The following binary model is evaluated, anticipating the effect of the independent variable which represents a specific measure of corruption on the probability of electoral success of the incumbent mayor:

$$P(R_i = 1|k_i, \epsilon_i) = G(\beta_0 + \beta_1 k_i + \beta_2 k_i^2 + V_{t-1} + \xi \mathbf{I}_i + \varphi \mathbf{E}_i + \mu \mathbf{M}_i + \vartheta \mathbf{D}_i + \epsilon_i) \quad (3.2)$$

Where  $G$  is the standard cumulative distribution function (c.d.f.) defined between 0 and 1,  $0 < G(z) < 1$ , for all real numbers  $z$ . Both a probit model and a standard linear probability model (LPM) are used for estimation.

The dependent variable,  $R_i$  is a binary variable which takes the value of 1 if the politician is re-elected in the 2013 elections and 0 if he is not<sup>9</sup>. The independent variable  $k_i$  represents one of the six proxy measures of corruption. The  $\beta_1$  and  $\beta_2$  parameters jointly measure the total effect of corruption on re-election. The

---

<sup>9</sup>The variable looks at electoral fortunes of both mayors and parties, whilst favouring mayors over parties. If the mayor got re-elected but changed parties (usually by becoming independent), the value assigned is 1. If the party won re-election but with a different mayor (when the old left for higher office or retired), the value assigned is also 1. However, a direct mayoral defeat, even if the party managed to stay in power as a minority partner in a governing coalition, is assigned a value of 0.

square value of  $k_i^2$  should indicate either the concavity or the convexity of voter preferences towards corruption (depending on whether  $\beta_2$  is positive or negative).

I draw a series of political (I), economic (E), mayoral or municipal (M), and demographic (D) covariates from the economic voting literature empirically proven to have an effect on the probability of re-election (Lewis-Beck and Paldam, 2000; Duch and Stevenson, 2008, and for Croatian national elections Glaurdic and Vukovic, 2016). The political variables (I) include the following:  $V_{t-1}$ , vote share the incumbent received in the previous election (in this case in 2009) as a measure of the incumbency effect. I further included size of the governing party coalition in the local council for the entire mandate (2009-2013) and the size of the total mayor majority for the entire mandate. These are included to evaluate the effect of voter anticipation of post-electoral coalitions on the probability of electoral success (Armstrong and Duch, 2010). The next was turnout at the 2013 election which usually carries a negative effect on re-election probability, and the alignment indicator variable of whether or not the mayor was from the same party that was in power on a national level at the beginning of the term (denoted as Mayor Gov), which usually boosts the local candidate's electoral chances (Glaurdic and Vukovic, 2017). Other political controls include political experience which is simply total number of years in power for the current incumbent, and the electoral polarization index between two major electoral blocks (left and right). A higher value of the index indicates greater ideological distance between the centre-left and

the centre-right party in a municipality, regardless of which of them is in power. Basically a high EPI signals that one party dominates in a given municipality and wins all elections virtually uncontested because its voters are very ideologically biased towards either the left or the right.

Economic covariates (E) also follow the standard literature on economic voting (Lewis-Beck and Paldam, 2000; Duch and Stevenson, 2008): the local tax rate, average four-year unemployment rates, income per capita, and intergovernmental grants received all act as measures of economic strength of a local unit. Mayoral and municipal (M) controls include an indicator of male gender, council size, and the log wage of the mayor, while demographic (D) controls include the percentage of Croats in the total population in a given municipality, average years of education for municipality population over 15 years of age, share of war disabled per 1000 inhabitants, the settlement size of a municipality, and a regional indicator variable for Istria. The share of Croats in a municipality controls for the ethnic category (in Croatia there are 63 municipalities in which at least one vice-mayor must be a representative of minorities). Size of municipality represents a proxy variable for the urban-rural cleavage and is measured as the logarithm of the weighted average of settlement size<sup>10</sup>. The variable war disabled per 1000 inhabitants quantifies to which extent a municipality was exposed to the Croatian war for independence. It represents the number of people (civilians and military) that were made disabled from 1991 to 1995 due to the war devastation. This variable represents the most

---

<sup>10</sup>The methodological explanation is available in Glaurdic and Vukovic, 2016.

important determinant of voter preferences in the last four national elections in Croatia (according to Glaurdic and Vukovic, 2016). Finally, the regional dummy for Istria is necessary due to a specific electoral situation in this region where a local political party dominates the majority of local elections for the past 25 years.

The control variables used were tested for multicollinearity by calculating the variance inflation factors (VIF) for all independent variables across all regressions. In neither case does a single VIF exceed the allowed value of 10 (the average values are around 3). The analysis of the correlation matrix implies the same conclusion. Therefore, it is safe to conclude that there is no multicollinearity problem.

### **3.5.1 Results**

Table 3.3 presents the initial results of Eq 3.2 using the probit regression model and building the relationship from a simple to a full model. It uses the *Total Index* of corruption to examine the relationship of interest, given that *Total Index* equally weights all the proxy variables used (defined in the previous section). All other corruption proxies produce the same implications, as shown in Tables 3.4 and 3.5 below. I use the probit regression and the linear probability model (LPM), both of which show almost identical results.

The results presented in Tables 3.3, 3.4, and 3.5 are in line with the theoretical predictions. Six different proxies for corruption all imply a statistically significant effect on re-election. Adding covariates in Table 3.3 does not affect statistical

Table 3.3: Corruption and re-election, simple and full models

Dependent variable: Re-election ( $R_i$ )	(1)	(2)	(3)
Corruption ( $k_i$ )	0.739 (0.33)**	0.857 (0.33)**	0.856 (0.36)**
Corruption squared ( $k_i^2$ )	-1.99 (0.69)***	-2.07 (0.69)***	-2.04 (0.70)***
Vote share ( $t - 1$ )		0.465 (0.14)***	0.507 (0.144)***
Years in power		-0.003 (0.005)	-0.002 (0.005)
Mayor Gov		0.032 (0.05)	0.027 (0.05)
Turnout		-0.132 (0.197)	-0.213 (0.219)
Size of governing coalition		-0.027 (0.018)	-0.024 (0.019)
Mayor majority		0.41 (0.146)***	0.368 (0.151)**
Polarization index		-0.143 (0.144)	-0.15 (0.155)
Local tax rate		-0.917 (0.483)*	-0.868 (0.476)*
Council size		0.006 (0.006)	0.005 (0.008)
Unemployment			0.259 (0.33)
Income p/c			$2.0 \times 10^{-5}$ ( $8.1 \times 10^{-5}$ )
Grants p/c			$-1.5 \times 10^{-5}$ ( $7.6 \times 10^{-6}$ )**
Croats			-0.078 (0.151)
Education			0.0005 (0.04)
War disabled			-0.001 (0.002)
Settlement size			-0.019 (0.06)
Istria			0.076 (0.10)
Male mayor			0.126 (0.064)**
Observations	556	549	549
R squared	0.012	0.084	0.094
Wald Chi2 (p-value)	8.8 (0.01)	52.8 (0)	57.7 (0)

Notes: All estimates are marginal effects calculated using probit regressions. Standard errors are shown in parentheses and are robust to heteroskedasticity. \*\*\* denotes significance at 1%, \*\* at 5% and \* at 10%.

significance but it does increase the magnitude of the effect. Altogether, it can be inferred that by increasing the proxy level of corruption (i.e. increasing the share of suspicious public procurements in total procurement) the probability of re-election of the local politician increases. The negative value of estimated parameter  $\widehat{\beta}_2$  across all regressions in all three tables implies the concavity of voter preferences with respect to corruption. This suggests that too high levels of suspicious procurements decrease voter utility and imply a punishment. There is a maximum (marginal level) of the probability function for which the voters find corruption acceptable.

Before unpacking this further, there are a few control variables in Table 3.3 that deserve further explanation. Greater vote share in the previous election increases a mayor's probability of winning in the next election as does the size of his overall majority during his time in office. This is not surprising as both of these variables depict the mayor's durability and strength. Higher local tax rates generate a negative effect on re-election as expected, however more grants received from the national government also reduces electoral chances which is a somewhat counter-intuitive finding (usually more intergovernmental grants help mayors increase their chances of political survival). Finally, having a male mayor increases chances of re-election, which is not surprising in the Croatian context given that over 80% of all mayors are men.

It order to quantify the total effect of corruption on re-election and calculate

Table 3.4: Corruption and re-election, probit estimates for all proxies

Dependent variable: $(R_i)$	(1) Value of contract revenues > 0.7	(2) of Only bid-der & zero employees	(3) From loss to profit	(4) Index 1	(5) Index 2	(6) Total Index
Corruption $(k_i)$	0.67 (0.34)**	0.637 (0.35)*	1.125 (0.64)*	0.663 (0.301)**	0.558 (0.337)*	0.856 (0.36)**
Corruption squared $(k_i^2)$	-0.73 (0.36)**	-1.34 (0.61)**	-1.38 (0.72)*	-1.47 (0.56)***	-1.21 (0.597)**	-2.04 (0.70)***
Controls	YES	YES	YES	YES	YES	YES
Observations	549	549	549	549	549	549
Pseudo R squared	0.088	0.089	0.088	0.094	0.086	0.094
Wald chi2 (p-value)	54.88 (0)	53.70 (0)	54.26 (0)	56.41 (0)	53.12 (0)	57.73 (0)

Notes: All regressions are probit estimates as specified in Eq 3.2, reporting average marginal effects. Controls include all the variables used in the full model version of Table 3.3 (column 6 in this table is equivalent to column 3 in Table 3.3). Standard errors are shown in parentheses and are robust to heteroskedasticity. The Pseudo R-squared used is the McFadden R-squared. \*\*\* denotes significance at 1%, \*\* at 5% and \* at 10%.

Table 3.5: Corruption and re-election, OLS estimates for all proxies

Dependent variable: $(R_i)$	(1) Value of contract revenues > 0.7	(2) of Only bid-der & zero employees	(3) From loss to profit	(4) Index 1	(5) Index 2	(6) Total Index
Corruption $(k_i)$	0.638 (0.291)**	0.535 (0.27)**	0.993 (0.48)**	0.629 (0.25)**	0.736 (0.317)**	0.897 (0.34)***
Corruption squared $(k_i^2)$	-0.700 (0.328)**	-1.12 (0.34)***	-1.29 (0.59)**	-1.42 (0.40)***	-1.5 (0.608)**	-2.24 (0.70)***
Controls	YES	YES	YES	YES	YES	YES
Observations	549	549	549	549	549	549
R squared	0.0949	0.0961	0.0954	0.1013	0.1134	0.1032
F-test (p-value)	3.90(0)	5.05(0)	3.82(0)	4.73 (0)	4.95(0)	4.16(0)

Notes: All regressions are OLS estimates as specified in Eq 3.2. Controls include all the variables used in the full model version of Table 3.3. Standard errors are shown in parentheses and are robust to heteroskedasticity. \*\*\* denotes significance at 1%, \*\* at 5% and \* at 10%.

the cut-off point (maximum of the re-election probability function) after which higher corruption adversely affects electoral chances, I use the following formula:

$$\widehat{k}_i^* = \left| \frac{\widehat{\beta}_1}{2\widehat{\beta}_2} \right| \quad (3.3)$$

Where  $k_i^*$  is the marginal cut-off value of the proxy for corruption while  $\widehat{\beta}_1$  and  $\widehat{\beta}_2$  are estimated regression coefficients from Eq 3.2.

Take for example the values from the first column of Table 3.4 where the proxy variable measures procurement contracts in excess of 70% of total average firm revenues. If we take  $\beta_1 = 0.67$ ,  $\beta_2 = -0.73$ , then the marginal value of  $k_i^*$  according to Eq 3.3 is  $k_i^* = 0.67/2(0.73) = 0.4589$ . This implies that a mayor will maximize the probability of re-election, according to the probit estimation, when the share of suspicious in total procurements is 46%, if suspicious procurements are defined as contracts worth more than 70% of the firm's average revenues. If the mayor increases the share of suspicious procurements above 46% this does not necessarily imply he will lose the election, but that his or her probability of re-election decreases. The mayor can, without any constraint, allocate almost half of total public procurement tenders in a suspicious way without jeopardizing his or her position in power. For the same proxy measure of corruption (value of contract at or above 70% of average revenues), Table 3.5 shows the LPM regression estimates (again the first column). The coefficients are of a similar magnitude and direction, and the value of the cut-off is almost the same, 45.6%. All other

statistically significant proxy measures of corruption show similar results.

It is perhaps better to focus on the cut-off value of corruption according to the *Total Index*. The cut-off value  $k_i^*$  of Total Index (6) is 20.98% for the probit and 20.02% for the LPM. The other two indices (Index 1 and Index 2) show similar values, and in both cases the effects are lower than in the case of the first proxy variable (1). Overall the results make intuitive sense since the voters in a local environment will notice too frequent instances of suspicious procurement contracts being handed around. A politician therefore maximizes his or her time in office if the weighted average of all suspicious procurement contracts is roughly a fifth of total procurements. Once again this does not imply that the politician cannot allocate even more resources to suspicious procurements, however after about 20% of procurements allocated this way his or her probability of re-election decreases. Figure 3.1 shows the estimated probability of winning function against the Total Index of corruption.

According to the estimated values of the dependent variable 're-election' from Figure 3.1, it can be inferred that the probability of re-election increases to 80% with optimal levels of corruption ( $k_i^* = 20.87\%$ ), other things held equal. However if the level of corruption increases by two standard deviations (jump from a municipality where the total index is 20% to a municipality where this index is around 50%) the probability of re-election drops below 50%, other things held equal.

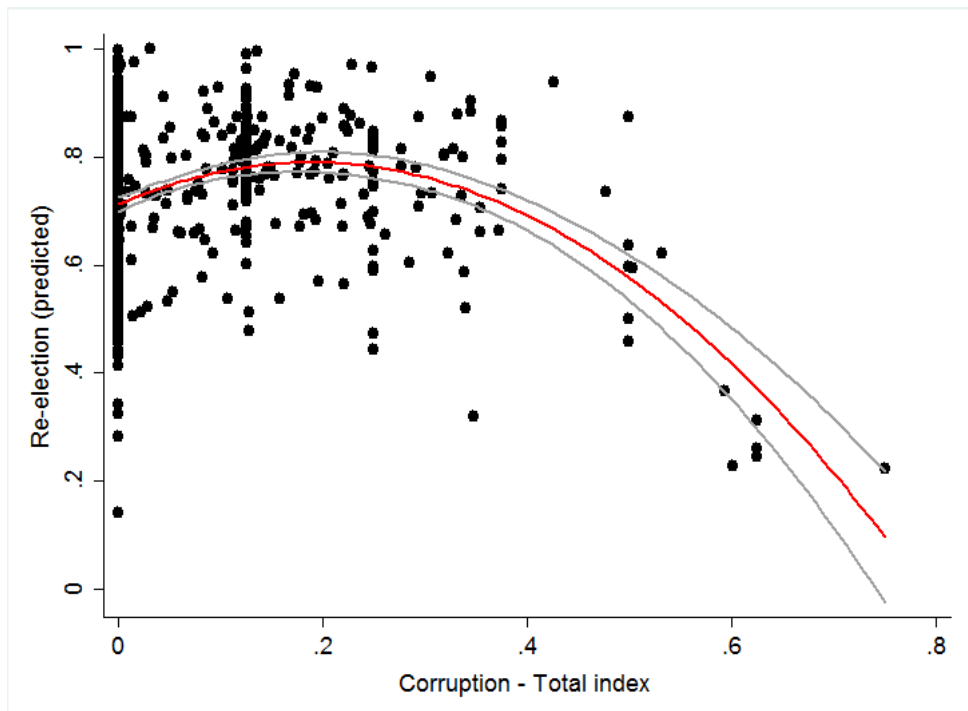


Figure 3.1: Corruption and re-election. The Total Index was used as the corruption proxy, while the y-axis shows predicted values of the re-election indicator variable. The probability of re-election function is fitted using the probit regression estimates with 95% confidence intervals.

### 3.5.2 Justifying the functional form

The functional form imposed by the earlier analysis seems to be crucial in evaluating the total effect of corruption on re-election. This section will therefore present the results for several other functional forms. In particular it will examine which functional form fits the data better, by comparing the quadratic function used earlier to the linear, 3rd degree and 4th degree polynomials. This has been performed for all corruption proxies, however only the estimated coefficients for the *Total Index* are reported. For the other five indicators I report only the Bayesian Information Criterion (BIC) and the Akaike Information Criterion (AIC), while underlying the model that has the lowest value for each criteria. Table 3.6 presents the results.

The smaller the value of the AIC or the BIC, the better the fit of the model. In most cases the quadratic model provides the best fit. For every proxy variable it has the lowest AIC and BIC, except for two cases when one of these values is second lowest. In each case the absolute differences for the different values across the model show positive or strong support for the quadratic form over all others. Additionally, out of all four models used, only the quadratic model shows statistically significant results for the effect of corruption on re-election.

Table 3.6: Functional form tests

Functional form:	(1) Linear	(2) Quadratic	(3) 3rd degree	(4) 4th degree
Total Index	-0.07 (0.15)	0.856 (0.36)**	-0.093 (0.72)	0.55 (1.34)
Total Index <sup>2</sup>		-2.04 (0.70)***	3.54 (3.68)	-3.38 (13.1)
Total Index <sup>3</sup>			-6.95 (4.45)	14.4 (39.5)
Total Index <sup>4</sup>				-19.46 (35.8)
Controls	YES	YES	YES	YES
Observations	549	549	549	549
Loglikelihood	-292.5	-288.8	-287.7	-287.5
AIC	625.2	<u>619.6</u>	<u>619.4</u>	621.1
BIC	711.3	<u>710.1</u>	714.2	720.2
Proxy (1)				
AIC	667.1	<u>663.7</u>	665.7	665.5
BIC	763.7	<u>762.6</u>	768.9	773.1
Proxy (2)				
AIC	666.8	<u>664.5</u>	666.5	667.3
BIC	764.5	<u>763.4</u>	769.7	774.8
Proxy (3)				
AIC	665.6	<u>664.0</u>	664.9	666.5
BIC	<u>760.2</u>	762.9	768.1	774.0
Index (1)				
AIC	667.3	<u>662.5</u>	664.3	664.9
BIC	761.9	<u>761.4</u>	767.5	772.4
Index (2)				
AIC	667.2	<u>664.1</u>	665.5	667.5
BIC	763.0	<u>761.8</u>	768.7	774.9

Notes: All estimates are calculated using probit regressions. The dependent variable in each case is re-election. Controls are the same as in the full model in Table 3.3. Standard errors are shown in parentheses and are robust to heteroskedasticity. \*\*\* denotes significance at 1%, \*\* at 5% and \* at 10%.

### 3.5.3 Placebo tests and robustness checks

The relationship between corruption and re-election presented in Tables 3.3 to 3.5 has been estimated using the re-election indicator for the 2013 elections, and the procurement allocations during the political term prior to those elections, from May 2009 to May 2013. To further strengthen the implications from the aforementioned estimations I perform two ‘placebo’ (falsification) tests where I regress the corruption proxies from the 2009-2013 term against the probability of re-election in 2009, and then against the probability of re-election in 2005. The results for both placebo tests should show no effect of corruption on re-election, as I do not expect future levels of corruption to affect election performance in the past. Table 3.7 shows the results. The dependent variable for the first two columns is the re-election indicator for 2009, and for the last two columns the re-election indicator for 2005. *Total Index* is used as the main independent variable in each case. The first and third columns report the estimates of the probit model, whereas the second and fourth column report the estimates of the LPM.

As expected both placebo tests show no statistically significant relationship between the level of corruption in the final term and re-election chances in the two previous elections. This finding adds further strength to the previously estimated relationship between corruption and re-election. Suspicious procurements performed from 2009 to 2013 have only made an impact on the upcoming election in 2013, and have not had any impact on past elections. In other words, being

Table 3.7: Corruption and re-election, placebo tests

Dependent var:	2009		2005	
Re-election ( $R_i$ )	(1) Probit	(2) LPM	(3) Probit	(4) LPM
Corruption ( $k_i$ )	0.262 (0.27)	0.308 (0.261)	0.528 (0.363)	0.523 (0.381)
Corruption squared ( $k_i^2$ )	-0.195 (0.62)	-0.301 (0.49)	-0.968 (0.743)	-0.965 (0.77)
Controls	YES	YES	YES	YES
Observations	546	546	546	546
R squared	0.4948	0.4787	0.1610	0.1797

Notes: All regressions are either probit or OLS estimates as specified under Eq 3.2. The *Total Index* is used as the main independent variable in each case. The dependent variable for the first two columns is the re-election indicator for 2009, and for the last two columns the re-election indicator for 2005. Controls are the same as in the full model in Table 3.3. Standard errors are shown in parentheses and are robust to heteroskedasticity. \*\*\* denotes significance at 1%, \*\* at 5% and \* at 10%.

re-elected does not imply a mayor will engage in corruption, but corruption can improve chances of re-election.

Finally, Table 3.8 provides a robustness check for the used proxy measures of corruption. It evaluates how precise the proxies were with respect to similar indicators usually applied in the literature measuring infrastructure spending which tends to be more sensitive to corruption and fraudulent procurement than any other part of the budget (Tanzi and Davoodi, 1997; Mauro, 1998). Infrastructural indicators are good robustness checks for procurement-based corruption given that each infrastructural expenditure needs to be administered using a procurement tender. Both variables, non-financial assets as a share of total expenditures and non-financial assets per capita, indicate the same results as do the other proxy variables, in both size and magnitude of the effect (even though the quadratic

term is not significant for the first variable, in columns 1 and 2). The more funds a mayor allocated towards infrastructure projects, the higher his or her probability of re-election.

However, as before, there is a limit to how much expenditures on non-financial assets helps re-election chances. The cut-off point in this case was 0.78 for the probit and 0.60 for the LPM. Initially these values seem high, however when looking at the distribution of the variable of non-financial assets in total expenditures (Table A1 in Appendix A) it varies from 0.007 to 2.62, where there are a number of cities and municipalities in Croatia (22 municipalities and 4 cities to be exact<sup>11</sup>) which had expenditures on non-financial assets higher than or close to average total expenditures in the years observed. The reason for including these extreme values is precisely due to potential frauds in this form of budgetary expenditures. It is very likely that a few extreme values would bias the total results upwards. However the point was to show how exactly such forms of corrupt budgetary practices affect the probability of re-election of a mayor who uses discretionary measures to pick winners and approve projects in his or her municipality or city.

### **3.5.4 Solving endogeneity issues**

The effect of corruption on local re-election as defined in Eq 3.2 and presented in the previous sections can potentially be biased due to many unobservable confounders. Even though I control for quite a few municipal-specific political, eco-

---

<sup>11</sup>In 19 out of 26 local units (73%) the mayor stayed in power.

Table 3.8: Robustness checks, measures of infrastructural corruption

Dependent var:	NonFin Assets to revenues		NonFin Assets per capita	
Re-election ( $R_i$ )	(1) Probit	(2) LPM	(3) Probit	(4) LPM
Corruption ( $k_i$ )	0.902 (0.343)***	1.02 (0.342)***	$1.4x10^{-4}$ $(3.6x10^{-5})$ ***	$1.25x10^{-4}$ $(3.0x10^{-5})$ ***
Corruption squared ( $k_i^2$ )	-0.578 (0.583)	-0.841 (0.506)*	$-1.06x10^{-8}$ $(3.3x10^{-9})$ ***	$-9.8x10^{-9}$ $(2.7x10^{-9})$ ***
Controls	YES	YES	YES	YES
Observations	549	549	549	549
R squared	0.1134	0.1211	0.1077	0.1130

Notes: All regressions are either probit or OLS estimates as specified under Eq 3.2. Controls are the same as in the full model in Table 3.3. Standard errors are shown in parentheses and are robust to heteroskedasticity. \*\*\* denotes significance at 1%, \*\* at 5% and \* at 10%.

conomic and demographic factors, the estimated effects could still be driven by things that cannot be measured. For example, a politician's intrinsic competence or ability may increase re-election probability and at the same time enable him to be more corrupt. It is thus necessary to find a way to control for such unobserved factors by exploiting some kind of randomization on the impact of corruption.

In order to solve potential endogeneity issues and estimate the treatment effect of corruption on re-election I use a 'fuzzy' regression discontinuity (RD) design. Unlike the regular or 'sharp' RD design where treatment is a deterministic and discontinuous function of a given covariate after some cutoff level, the fuzzy design exploits a discontinuity in the *probability* or *intensity* of treatment around an arbitrary threshold. In such cases the discontinuity can be treated as an instrumental variable for treatment status. In this paper I use population thresholds determin-

ing the size of local councils as defined by law (Croatian Parliament, 2013, NN 36/09) as the main source of discontinuity (similar to Pettersson-Lidbom, 2012 for Sweden and Finland, and Egger and Koethenbueger, 2010 for Germany). The design exploits the fact that council size increases in increments of 2 (or 4 for larger cities) following an arbitrary rule on population size for each local unit. The jump generated after passing each threshold represents the as good as random assignment of local units into treatment (just above cutoff) and control groups (just below cutoff). Because we are comparing units just below or just above a narrow threshold, we are operating under the standard regression discontinuity assumption that such units are similar in all unobservable covariates except for the change in council size, making it possible to infer a local treatment effect by comparing their outcomes.

Council size does indeed carry a positive and significant effect on corruption<sup>12</sup>, or in other words, corruption increases in intensity as council size increases in increments of 2 or 4. This could be explained as a proxy impact of local coalition formation. In other words, the more sitting members in the council, the more likely for a mayor to engage in corruption (the greater the intensity of corruption) to satisfy key individual members (the swing representatives) and thus maintain his or her political support. The following section summarizes the logic behind this relationship.

---

<sup>12</sup>This can be seen in the first stage estimates of Table 3.10.

**Background: council size, corruption, and the exclusion restriction**

The number of council representatives for each local unit is determined on a national level by law (i.e. exogenously imposed) and is contingent on the total population of a given city or municipality. The lowest requirement determined by the Law of local and regional government (Croatian Parliament, 2013, NN 36/09) is to have at least 7 representatives, which is allocated to municipalities under 500 inhabitants. The law then defines the population values for each next number of representatives, increasing in increments of 2 or 4 (in order to always have an odd number). For example, between 501 and 1000 inhabitants, the total number of council members is 9, between 1001 and 2500 it is 11, between 2501 and 5000 it is 13, and so on (see Table 3.9). As the population of a city or municipality changes, these numbers automatically adjust, going up or down by 2 (or 4) council members. The population size is determined nationally based on census data. The rule is therefore exogenously imposed and cannot be manipulated by any local actor, making it as good as randomly assigned.

How does council size exert its indirect influence on corruption? Or in other words why should the intensity of corruption increase with increasing council size? One hypothesis is that more council members can induce a mayor to be more corrupt, as in order to keep his council majority he needs to divert more resources towards corrupt activities and from this bribe the council members for support. Bribe is only one method, this can also include patronage where mayors have the

Table 3.9: Council size with respect to population threshold

Population threshold	Council size below	Council size above	No of units 5% below threshold	No of units 5% above threshold
500	7	9	2	3
1,000	9	11	5	10
2,500	11	13	29	24
5,000	13	15	25	16
10,000	15	17	8	6
20,000	17	21	2	2
35,000	21	25	0	1
60,000	25	31	3	1
100,000	31	35	0	1
200,000	35	45	0	0
300,000	45	51	0	0
Total			74	64

Notes: The final two columns represent the total number of cities and/or municipalities within the 10% bandwidth from the threshold.

power to employ representatives' family members in city or municipal companies, or in politically-connected private firms. The evidence for both of these practices is ample. The mayor of Zagreb is infamous for employing whole families of city council members in public firms (N1 news, 2016). The most famous example of bribery of council members was the case of the convicted mayor of Vukovar who was audio-taped offering 50,000kn to an opposition council member to switch to his side (Vecernji list, 2013). He was sentenced to one year and four months in prison (Patkovic, 2014). Anecdotal evidence suggests that the mayor of Vukovar was the "only one sentenced for something that everybody does in local politics" in Croatia (Litvan, 2014). Because of this it makes a big difference if a mayor is trying to get a majority in the local council of 25 members and in a local council of 9 or 11 members. Less people to corrupt implies lower demand for corruption.

Additional evidence to support the link between corruption and council size is based on the reports of the independent Committee for Determining Conflict of Interest, which has jurisdiction to investigate and conclude whether a public official is in conflict of interest. According to numerous reports from 2013, 2014, and 2015 on local mayors and their council members the Committee has found that the most usual cases of conflict of interest arise when a mayor allocates a procurement contract to the firm run by one of his or her council members (from the mayor's coalition, and even more often to a member of the local opposition party). According to the Committee this practice is a regular occurrence in local

politics in Croatia (Committee for Determining Conflict of Interest, 2016, 2015, 2014). It is obvious therefore that the best mechanism for a mayor to build a coalition of supporters is to allocate procurement contracts to them.

Recall however that the corruption indicator at hand is only a proxy for actual corruption, approximated through suspicious procurement. It therefore assumes that cities and municipalities where the mayor engages in more suspicious procurements also carry a higher probability of other corrupt activities like bribes and nepotism. In other words higher levels of suspicious procurements administered by the local government are likely to be correlated with incidences of bribes and patronage. This operates purely on assumption and anecdotal evidence, however the assumption itself is not entirely unimaginable (according to the report from the State Attorney Office, in cases when arrests of either mayors or former ministers were made they contained incidences of all three — bribery, patronage, and fraudulent procurement; DORH, 2015).

If mayors bribe members to keep their council majority, will this affect their re-election chances as well? Not directly. Since 2009 elections for local mayors and local council have been separated. Voters vote for parties on one ballot and for the mayor on the other ballot. A mayor has an incentive to have a majority in the council, but his or her primary concern is to be voted in directly by the voters. Therefore the realization of council majority comes *ex post* and independently of the mayor's electoral victory. If a mayor bribes council members to achieve

this majority the act itself has no effect on voters directly (unless uncovered by a criminal investigation, obviously).

An additional feature of Croatian laws is that the only policy change that occurs at the population thresholds as specified in Table 3.9 is the size of the local council (Croatian Parliament, 2013, NN 36/09). The status of the mayor does not change, nor does any category of budgetary revenues or spending. The allocation of intergovernmental grants is based on development indicators and has nothing to do with population or council size (Glaurdic and Vukovic, 2017). Electoral rules also remain the same regardless of population size, while rules on public officials' salaries and the maximum local tax rate follow a different pattern than council size rules<sup>13</sup>. Other rules that determine some of the aforementioned categories are in no case contingent on population size or local council size. This makes the argument that an increase in council size does not carry any policy or spending effect other than corruption more persuasive. It reduces the possibility of compound treatment — when different policies change in the same population threshold (Eggers et al, 2016).

The fact that there is no compound treatment increases the confidence of satisfying the exclusion restriction — the idea that local council size after a particular population threshold has no other way of affecting the outcome (re-election) other

---

<sup>13</sup>In cases of the local tax law, all municipalities have the same maximum rate, while cities are divided based on a different population threshold — at 30,000 inhabitants (Croatian Parliament, 2016, NN 115/16). The same is true with public official's salaries, they are defined based on the 3,000 inhabitant threshold, and therefore should not affect any change at the council size threshold (Croatian Parliament, 2010, NN 28/10)

than through the treatment. In the specific local Croatian context no other policy directly changes with the size of the population threshold. There is still room for caution given that places with higher council sizes and larger populations obviously have larger budgets meaning that any spending category is bigger and can entail its own specific effect on re-election. In other words what I see as a corruption effect might simply be disguised as a spending effect. It is for this specific reason that I only compare cities and municipalities with similar size, i.e. within a narrow bandwidth around each population threshold (see Figure 3.2). Places with 5% higher and 5% lower population size from the threshold (which can be anything between 25 people, 250 people, or for a few big cities a 1000 people) are indeed not too different from each other in terms of budgetary spending or fiscal capacity, particularly when controlling for geographical areas and war-affected areas. The downside is that I can only estimate the local average treatment effect which applies for a limited sample size of cities and municipalities. I cannot prove causality between corruption and re-election for the entire sample, but this estimate is the next best alternative given the available data and empirical strategy.

### **Estimation with 2SLS**

The setup is a two stage least squares (2SLS) procedure in estimating the impact of corruption  $k_i$  and corruption squared  $k_i^2$ , instrumented by changes in council size and a dummy indicating the jump in council size  $D_p$ , on the outcome —

re-election. The running variable ( $p_i$ ) is the difference in population of each unit from its population threshold (so that the middle value is 0). This type of non-linear 2SLS estimation, according to Angrist and Pischke (2009, pp. 192) and Wooldridge (2002, pp. 235-36), requires the usage of two first-stage equations. It treats both  $k_i$  and  $k_i^2$  as endogenous while instrumenting them with the regular instrument — council size ( $cs_i$ ) and its squared value ( $cs_i^2$ ).

The first stages are therefore:

$$E [k_i | D_p, cs_i] = \alpha + \pi D_p + \beta_1 cs_i + f(p_i) + X_i \gamma + \epsilon_i \quad (3.4)$$

$$E [k_i^2 | D_p, cs_i^2] = \alpha + \pi D_p + \beta_2 cs_i^2 + f(p_i) + X_i \gamma + \epsilon_i \quad (3.5)$$

Where  $f(p_i)$  represents the non-linear functions of the running variable, while  $X_i$  is the vector of all covariates used in Eq 3.2. The second stage estimates the following equation:

$$E [R_i | k_i] = \alpha + \tau_1 \widehat{k}_i + \tau_2 \widehat{k}_i^2 + f(p_i) + X_i \gamma + \eta_i \quad (3.6)$$

Where  $\widehat{k}_i$  and  $\widehat{k}_i^2$  are first-stage estimates of corruption and corruption squared from equations 3.4 and 3.5. The estimated coefficients  $\tau_1$  and  $\tau_2$  represent the final treatment effects of corruption on re-election.

A non-parametric version of the same approach is to perform the 2SLS es-

timation within a narrow bandwidth around the threshold, thus removing the non-linear trend element. I impose bandwidths of 5 and 10 percent above or below the threshold. All the thresholds are pooled into one and the estimation is performed using a standard 2SLS estimation where the reduced form equation is:

$$E [R_i | p_0 \leq p_i < p_0 + \Delta] - E [R_i | p_0 - \Delta < p_i < p_0] = \rho\pi \quad (3.7)$$

while the first stage for  $k_i$  is<sup>14</sup>:

$$E [k_i | p_0 \leq p_i < p_0 + \Delta] - E [k_i | p_0 - \Delta < p_i < p_0] = \pi \quad (3.8)$$

The resulting estimate ( $\rho\pi/\pi = \rho$ , also called the Wald estimator) is the local average treatment effect (LATE) of corruption on re-election (local because it is defined within the narrow bandwidth between units just above and just below the given population threshold). Figure 3.2 presents the graphical evidence of this relationship suggesting clear jump in corruption levels after the zero threshold. The data is scarce (only 138 observations that fall within the 5% bandwidth) but it is suggestive of a discontinuity around the threshold.

Balance checks were performed on each covariate, in order to see whether or not the treatment and control groups were in fact similar in all pre-observed covariates. Table 3.13 in Appendix A reports the t-tests for each covariates and

---

<sup>14</sup>There are again two first-stage estimates, as before, one for a linear parameter of corruption and the other for the squared parameter.

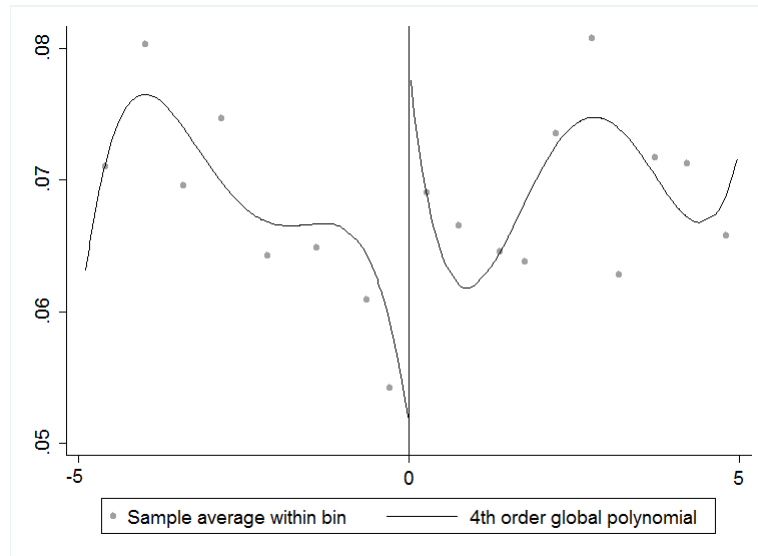


Figure 3.2: RD plot with the 5% bandwidth

finds no evidence of any significant differences between the pre-observed covariates of observations within the 5% level above or below the zero threshold. This makes the reported effects for the non-parametric approach even more plausible. Appendix A also reports the McCrary (2008) density plot and histogram of the running variable, the difference in population from the zero threshold. According to the figures 3.3 and 3.4 there seems to be no evidence of sorting around the threshold that could have biased the results of the non-parametric estimation.

Table 3.10 summarizes the main results, as before looking at the joint effect of  $k_i$  and its squared value. The first stage results all show an expected positive and significant effect of council size on corruption. The results of the parametric estimation, either linear or the quadratic, both suggest a concave effect of corruption on re-election. The estimated coefficients are larger than the original quadratic

Table 3.10: Fuzzy regression discontinuity results

	(1) Parametric (linear)	(2) Parametric (quadratic)	(3) Non- parametric (5%)	(4) Non- parametric (10%)
Corruption	4.04 (1.03)***	2.09 (0.05)***	6.29 (1.76)***	3.58 (0.93)***
Corruption <sup>2</sup>	-15.57 (4.43)***	-7.12 (1.74)***	-10.25 (1.76)***	-9.85 (4.1)**
Population difference	$2.2x10^{-5}$ ( $1.1x10^{-5}$ )**	$-4x10^{-5}$ ( $7.7x10^{-6}$ )***	$-1.1x10^{-6}$ ( $1x10^{-5}$ )	$3.7x10^{-5}$ ( $7.4x10^{-5}$ )
First stage $k_i$	0.028 (0.007)***	0.029 (0.007)***	0.005 (0.002)**	0.01 (0.002)***
First stage $k_i^2$	0.005 (0.002)**	0.006 (0.001)***	0.0001 ( $2.6x10^{-5}$ )***	-0.001 (0.0006)
Controls	YES	YES	YES	YES
Observations	547	547	136	247
R-squared	0.4913	0.7459	0.6955	0.7221

Notes: The dependent variable in each case is the indicator value of re-election. For the parametric approach estimations were done up to the 7th polynomial, all giving the same result. Only the linear and the quadratic are reported. The non-parametric estimation presents the results in the 5% and 10% bandwidths from the threshold. Estimated coefficients represent the  $\hat{\tau}$  coefficients from the 2SLS estimation of equations 3.4 to 3.8. The covariates used are the same as used in Table 3.3. Standard errors are reported in parentheses and are robust to heteroskedasticity. \*\*\* denotes significance at 1%, \*\* at 5% and \* at 10%.

estimates, however the magnitude of the effect is slightly smaller across both estimates, but it still reinforces the implications from Tables 3.3 to 3.5. The upper cut-off after which the probability of re-election starts to decline is now between 13% and 15% for the parametric estimates.

The non-parametric estimation results estimated within 5% and 10% bandwidths above and below each population threshold are reported in columns (3) and (4). Unfortunately there isn't enough data at lower margins of the bandwidth (e.g. 2.5%) to produce consistent estimates, meaning that estimates at the 5% bandwidth are the closest we can get to estimating the local treatment effect. The non-parametric estimates also confirm the previous findings of a concave relationship between corruption and re-election, however the magnitudes of the effect slightly differ across bandwidths. For the 5% bandwidth the upper cut-off of corruption for which the mayor maximizes re-election probabilities is around 30%, while for the 10% bandwidth the estimate is more in line with the parametric results and is around 18%. This simply implies that the treatment effect of corruption on re-election gets stronger as we move closer to the zero population cut-off, i.e. where the comparison of units is more likely to be randomized. Overall the OLS and fuzzy RD estimates suggest a quite substantial level of corruption the mayors are allowed to get away with.

Table 3.11 reports the results of several placebo tests. The first four instances, reported in columns (1) to (4) follow the Imbens and Lemieux (2007) approach of

Table 3.11: Fuzzy regression discontinuity results: placebo tests

	(1)	(2)	(3)	(4)	(5)	(6)
	Threshold at -10	Threshold at -5	Threshold at +5	Threshold at +10	Placebo 2009	Placebo 2005
Corruption	4.7 (6.1)	-9.11 (6.98)	0.86 (0.63)	-50.2 (276.1)	0.37 (1.85)	-3.64 (7.38)
Corruption <sup>2</sup>	-13.19 (9.46)	47.2 (26.5)	-4.89 (4.18)	53.1 (224.3)	13.06 (15.7)	32.8 (56.1)
Controls	YES	YES	YES	YES	YES	YES
Observations	72	116	108	47	544	544
R-squared	0.0078	0.0125	0.0085	0.0096	0.0001	0.0058

Notes: Thresholds defined from -10% to +10% from the 0 population threshold. In each case the 5% bandwidth was used. The dependent variable in each case is the indicator value of re-election. The placebos for 2009 and 2005 use the linear parametric estimation. The covariates used are the same as used in Table 3.3. Standard errors are reported in parentheses and are robust to heteroskedasticity. \*\*\* denotes significance at 1%, \*\* at 5% and \* at 10%.

testing discontinuities by taking values to the left and to the right of the original zero threshold and thus generate placebo thresholds around which the estimated effects should be non-significant. The columns (1) to (4) apply a non-parametric estimation again taking the 5% bandwidth around these newly created placebo thresholds (which are defined from -10% below 0 to +10% above zero). As expected in each case there is no significant effect, meaning that in absence of the treatment there is no effect of council size on corruption and subsequently on re-election. Finally, the last two columns, (5) and (6) report the results of the linear parametric estimation regressing corruption against the probability of winning in 2009 and 2005. The logic is the same as done in Table 3.7: if the effect of council size on re-election is conditioned only by municipal corruption in the final term, then

there should not be any effect of council size on the probability of winning in the previous two terms (for 2009 and 2005 elections). As expected, there is no effect of later period corruption on previous period(s) probability of winning.

### **3.6 Conclusion**

The central implication of the paper is that long-term political survival depends on how successful politicians are in creating mutually dependent networks of interests with rent-extracting firms where electoral support is exchanged for favourable procurement contracts. The empirical section confirms a positive, yet concave relationship between corruption and re-election, where the probability of re-election is maximized for around one fifth of all procurements allocated in a potentially fraudulent way. When about half of all procurements are allocated this way, a mayor, on average, loses elections. The voters therefore do punish corrupt behaviour, but only when corruption becomes too rampant and too obvious. The concave impact of corruption on re-election has been further confirmed by applying a fuzzy regression discontinuity approach, using the exogenously assigned size of local council as a successful instrument for increasing intensity of corruption.

The methodological contribution attempts to provide a blueprint for researchers to move away from perception indices and try and find a more robust and more precise measure of corrupt behaviour. It builds upon the recent literature that attempts to do the same thing by gaining access to much better datasets on public

procurements (Fazekas et al, 2016, Ferwerda et al, 2017, Fazekas and Kocsis, 2017). Even with such efforts there is always a concern of committing a type I error (falsely coding a regular procurement as suspicious). In addition, using only procurement contracts might also be underestimating the overall level of corruption. There are various other corrupt activities the local government is prone to, such as converting agricultural land to residential land, or changing local legislation to favour partial interests. All these practices also suggest a close relationship between local political elites and local rent-extracting firms, however they are much harder to measure and quantify than is the case with procurement contracts. Future research efforts should be going in this direction as well, in addition to generating even better efforts at recognizing corruption in public procurements.

Finally, a normative implication can be drawn from the paper's main results, specific to the Croatian electoral framework analysed in the paper. The suggestion is to introduce a term limit electoral rule for the maximum of two terms. Two terms in power (a total of eight years) can often be enough for a politician to create a powerful network from which he or she could extract rents. Research on the effects of term limits on political behaviour (e.g. Besley, 2006; Ferraz and Finan, 2011) have found that politicians steal more in their last and final term, knowing they will lose office with certainty. However, having no term limit is arguably worse than having a two term limit, as there is virtually no constraint imposed on the politician either on the amount of theft he can do, or for how long he can do it.

Resting upon the assumption that political corruption prevents the progress of local communities making them dependent on interpersonal relationships between powerful interests and political elites, it is crucial to first reduce political power. The introduction of term limits is the first step towards achieving that goal.

## Appendix A

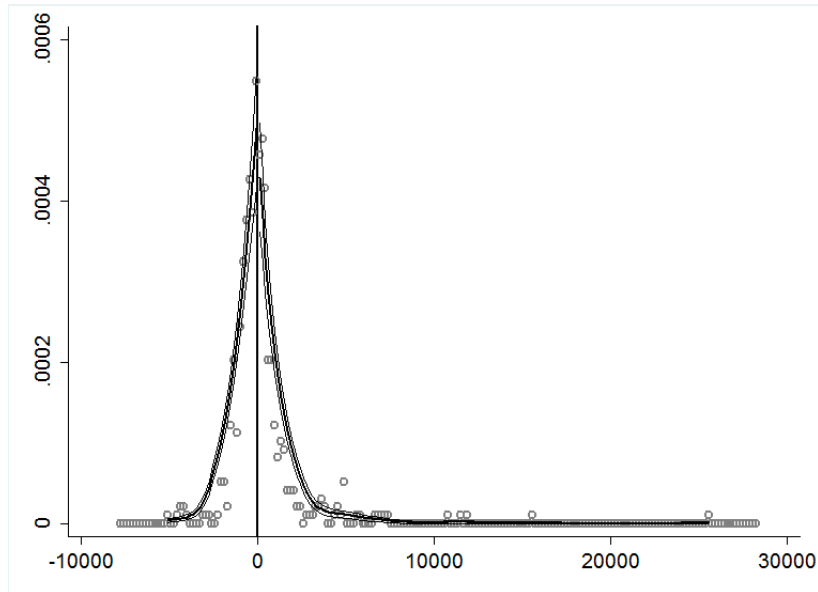


Figure 3.3: The McCrary (2008) density plot of population difference shows no sign of sorting

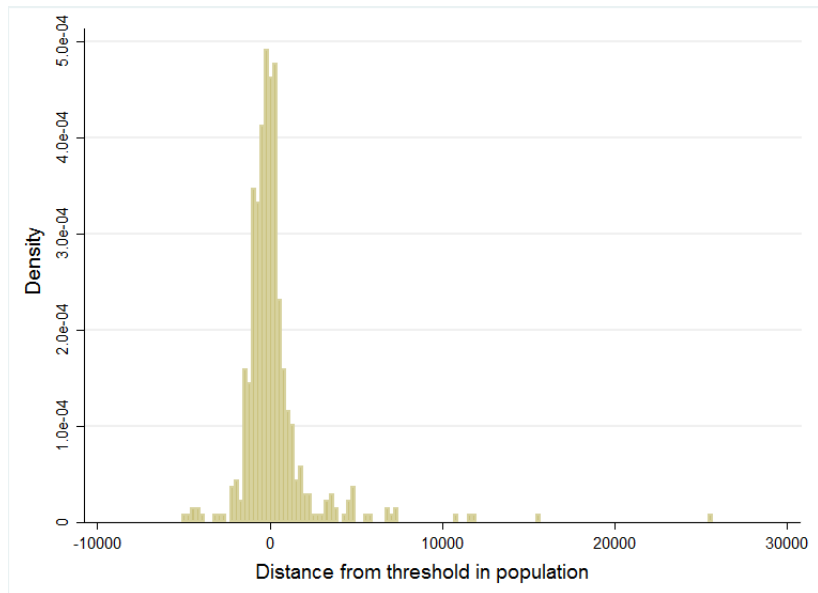


Figure 3.4: Histogram of population difference

Table 3.12: Summary statistics

Variable	Mean	Std. Dev.	N
Re-election 13	0.736	0.441	556
Re-election 09	0.742	0.438	546
Re-election 05	0.676	0.468	546
Corruption proxy 1	0.135	0.294	556
Corruption proxy 2	0.06	0.145	556
Corruption proxy 3	0.024	0.134	556
Corruption index 1	0.101	0.152	556
Corruption index 2	0.062	0.126	556
Total Index	0.082	0.126	556
Non financial assets in total spending	0.195	0.141	556
Non financial assets p/c	719.014	1093.962	556
State audit irregularities	0.583	0.494	556
State audit procurements	0.246	0.431	556
Budget transparency	1.674	1.473	556
Incumbent vote share 2009	0.517	0.158	549
Incumbent vote share 2013	0.506	0.157	556
Years in power	8.635	3.511	556
Mayor from national party	0.223	0.417	556
Turnout	0.534	0.098	556
Size of gov coalition	2.165	1.136	556
Mayor majority	0.566	0.146	556
Polarization index	0.837	0.152	556
Local tax rate	0.037	0.041	556
Unemployment	0.199	0.092	556
Income p/c	1647.506	432.645	556
Grants p/c	650.306	1808.381	556
Croats	0.889	0.171	556
Years of education	10.149	0.855	556
War disabled per 1000	8.494	8.034	556
Settlement size	3.002	0.544	556
Istria	0.074	0.262	556
Male mayor	0.930	0.256	556
Council size	13.505	3.93	556
Total population	7651.836	35739.373	556
Population difference	218.854	2095.006	554
$D_p$	0.471	0.5	554

Table 3.13: Balance tests for all covariates

Covariate	Control mean ( $D_p = 0$ )	group Treatment group mean ( $D_p = 1$ )	t-value
Vote share (t-1)	0.504 (0.017)	0.491 (0.019)	0.507 (0.613)
Years in power	8.27 (0.417)	8.80 (0.461)	-0.852 (0.395)
Turnout	0.522 (0.012)	0.533 (0.011)	-0.684 (0.495)
Mayor Gov	0.144 (0.041)	0.258 (0.056)	-1.67 (0.096)
Coalition size	2.052 (0.128)	2.209 (0.149)	-0.802 (0.424)
Mayor majority	0.551 (0.019)	0.555 (0.017)	-0.174 (0.862)
Polarization	0.846 (0.017)	0.839 (0.018)	0.268 (0.788)
Tax rate	0.037 (0.004)	0.038 (0.005)	-0.104 (0.917)
Unemployment	0.212 (0.01)	0.191 (0.01)	1.444 (0.151)
Income	1592.9 (43.8)	1670.9 (52.1)	-1.152 (0.251)
Grants	487.2 (90.2)	660.9 (172.4)	-0.939 (0.349)
Croats	0.874 (0.023)	0.904 (0.019)	-0.988 (0.324)
Education	10.11 (0.079)	10.12 (0.108)	-0.085 (0.932)
War disabled	8.82 (0.855)	8.405 (1.105)	0.299 (0.765)
Settlement size	3.03 (0.063)	2.99 (0.071)	0.416 (0.677)
Istria	0.065 (0.028)	0.064 (0.031)	0.029 (0.976)
Male mayor	0.921 (0.031)	0.967 (0.022)	-1.165 (0.246)
Observations	76	62	

Notes: The first two columns report standard errors in parentheses, while the third column reports p-values for the t-test.

## Appendix B

### Summary of interviews on corruption

In order to approximate and quantify corruption as precisely as possible I conducted a series of interviews during September, October, and November 2014 with agents that were to some extent involved in either the procurement process (as applicants or administrators) or in tracking corruption from procurement records.

Each interviewee suggested improvements of how to classify a given procurement contract as suspicious, and each interviewee confirmed that my initial classification was indeed correct and “on the right track”. I benefited from in-depth experience in tracking corruption from an interview held with two special police investigators from the official Croatian anti-corruption agency (USKOK), under the jurisdiction of the Croatian Attorney General (DORH). I also conducted an interview with a civil sector anti-corruption watchdog, who pointed out very similar conclusions as did the police investigators. I then interviewed two local government officials, one was a former deputy mayor of a local municipality, the other was a former city council-member and later a government official at the Ministry of Finance. I also interviewed two national-level government bureaucrats working at the Ministry of the Economy and Entrepreneurship, both with experience in public procurement law and practice. Finally I spoke with five entrepreneurs who competed for procurement tenders at a national or at a local level.

Given the controversial and delicate nature of the topic I will keep the entire

Appendix anonymized and will only paraphrase each of the interviews (I hold all the original notes from each interview). I will describe the logic behind each indicator of suspicious procurement separately, adhering to the suggestions and comments given to me by the interviewees. It should be noted that none of the used indicators is a 100% accurate depiction of corruption. Each indicator measures only the extent to which a procurement contract raises a reasonable doubt in how it was allocated. Furthermore, as the interviewees have confirmed, not every contract classified by my definitions is necessarily corruption, but all of the contracts that were, in fact, corrupt do satisfy the given definitions.

**First indicator of suspicion: value of procurement contracts with respect to annual revenue**

*“The first proxy, (1) total value of contracts exceeding 70% of average annual four-year revenues, focuses on firms which had little or no sources of revenues other than contracts received from the local government.”*

According to the police investigators every corrupt firm they were investigating had an overreliance on government support, either through direct subsidies or rigged procurement contracts. They had strong reason to believe that every such firm whose revenues consisted mainly of funds from government procurement contracts had some form of connection to the politicians who made the procurement decisions, in either local or national office. As they clearly emphasized not

every firm that has such a structure of revenues is corrupt (some are simply highly specialized), but every corrupt firm they have uncovered had this characteristic — a large share of revenues coming from government contracts. It is for this reason that I only focus on local government procurement, which tends to avoid having highly specialized expenditures like medical equipment or defence spending.

Both the investigators and the anti-corruption watchdog pointed out that the practice of awarding contracts to firms with otherwise miniscule revenues meant that they were usually cover-up firms that would get the deal and then outsource it to another firm that would actually do the work. This was done for two purposes: one was to deliver the contract to a firm that would otherwise not qualify for the procurement (usually because hiring that firm would entail a direct conflict of interest to the politician), so the owner, or his/her friend would open a new company for the sole purposes of getting the deal, after which it would be free to outsource it to the original firm (this is not prohibited by law). The second purpose involved a variety of “money laundering schemes” where a firm would get the deal for a low price and then sell it to another firm for a much higher price. All of the interviewed entrepreneurs confirmed this practice. They believe that a vast majority of procurement contracts are fraudulently allocated, and that in order to secure the deal one needs to have some form of political connection (e.g. cultivating a long-term relationship with a local or national politician by taking them to lunches and dinners, buying them presents, etc.).

In addition, the investigators said that the usual practice is that many firms would deliberately offer a dumping price just so that it beats its competitors, and would then perform the job at a much higher price, usually because the job had been outsourced (to a partner company) and they claimed unexpected expenses.

**Second indicator of suspicion: firms with no employees and a single bidder**

*“The second proxy (2) looks at multiple contracts given to firms with zero employees where the firm was the only bidder in the public auction.”*

Similar conclusions were given for the second indicator of suspicion. The case of firms with zero employees was said to be a similar example to the aforementioned firms that just exist to get the contract in order to outsource it to a partner firm, or perform various money laundering schemes. In addition, all the interviewees argued that a firm with zero employees that receives multiple tenders as a single bidder is almost certainly a case of corruption. They referred to them as “fake firms”. Public officials are usually unable to react because the decision is out of their hands, but they have noticed that such companies with zero employees that keep winning tenders are almost never the ones who actually perform the job, and have emphasized all such deals as “shady”.

The interviewed entrepreneurs have confirmed that the same “fake firms” consistently keep showing up and winning tenders even when they are not the single bidder. The entrepreneurs have reacted by reporting each such case to the anti-

corruption agency (USKOK). However, the police investigators have said that the problem with arresting the participants of such deals is that it is difficult to find a judge to convict them, even though it does happen if the participants are “small fry”. Usually in a process against a well-connected entrepreneur or a politician, a judge would, in addition to witness statements, require direct evidence of marked bills to make a conviction. The explanation of the investigators was that such judges must have been bought off. The relationship between the judiciary system, politics, and the private sector, as important as it may be for an even better quantification of corrupt networks, is unfortunately beyond the scope of this paper.

**Third indicator of suspicion: firms with large losses**

*“The third proxy (3) looks at how much the local government indirectly subsidized firms with large losses, which after signing the procurement contract several times higher than their annual revenues, realized a profit in the next business year.”*

Finally, labelling firms with losses that get indirect government subsidies is not necessarily corruption, but it definitely qualifies as evidence of political connections between a given firm and the local politician. All of the interviewed entrepreneurs have confirmed this. They expressed their outrage that a government would indirectly subsidize connected companies, but have said that unfortunately this is common practice. In their experience politicians “bail out” connected firms on a regular basis. Whenever an entrepreneur with good political ties finds himself in

financial difficulties he can “count on his friend in power to bail him out”.

The public officials also expressed concern saying that it was “bad practice” to give out jobs to firms with large losses, particularly if the procurement contract itself was enough to more than cover the size of loss. They prefer to give it to either companies with good market performance and stature, so that even if a firm happens to be operating at a loss, the value of the procurement contract is not what makes the firm overturn the loss into a profit overnight.

Finally, public officials from both local and national-level government have confirmed that the administration of contracts is done by the bureaucracy, not the politicians, however the administrators themselves do not decide who gets a contract. They do have a set of formal rules they follow “to the letter”, however they have no power to override the decisions of their superiors (in this case mayors).

## **Appendix C**

### **Summary statistics of corruption proxies**

The first proxy for corruption captures firms whose total value of procurement contracts received exceeded 70% of their average annual four-year revenues. 824 firms (17 % of the total sample) fall within this category, 634 of which received contracts greater than their average annual revenues (see Table 3.14). In fact, 240 firms which reported zero or minuscule average 4-year revenues received lucrative procurement deals that were not reported in their balance sheets. For example, a

firm which reported revenues between 10,000 and 20,000 kunas for four years in a row received 13 million kunas in one year to build a local sports hall. Another construction firm in a near-by municipality received a 57 million kuna contract to perform road maintenance works whilst reporting less than 100,000 kunas of average annual revenues for the entire observed period. A few firms across the country received between 29 and 73 million kunas for forestry services from the local municipalities, all of which reported very low, close to zero average revenues. The record is held by a firm selling unidentified equipment reporting zero revenues for the entire four year period, while receiving a total of 100 million kunas.

Table 3.14 observes the distribution of public funds through the ratio of contract value to annual firm revenue. Firms that signed procurement contracts whose value exceeded 50% of their average annual revenues received in total 12bn kunas (1.55bn euros) in the observed period, which constitutes roughly 40% of the total value of all procurement contracts in the database. More than 6bn kunas (790m euros) was allocated to 404 firms which had between 100% to 500% higher value of contracts to total revenues. Over 1.4bn kunas (180m euros) was allocated to 131 firms that received a procurement contract which was between 10 and 100 times greater in value than their annual revenues. All these firms are included in corruption proxy (1).

The second proxy for corruption captures contracts given to firms with zero employees if the firm was the only bidder in the public auction. One such firm

Table 3.14: Value of procurement contract with respect to annual firm revenues

	Total value of contracts	Number of companies
less than 10 %	3,367,726,636	2403
from 11 to 20%	1,985,008,957	577
from 21 to 30%	2,012,790,672	333
from 31 to 50%	5,733,244,517	410
from 51 to 70%	2,094,285,071	234
from 71 to 100%	1,963,941,872	190
from 101 to 200%	3,292,071,338	214
from 201 to 500%	3,171,321,851	99
more than 501%	1,388,130,539	81
Firms with no revenues	149,844,158	109
Firms with 10 times greater contract value than revenues	753,636,598	58
Firms with 50 times greater contract value than revenues	492,341,134	40
Firms with 100 times greater contract value than revenues	169,969,464	33

Note: All values in kunas, 1 euro  $\approx$  7.6 kunas. Sources and description of data available in the Data section.

with zero employees was hired 36 times as the only bidder over a two year period to perform some very generic construction works in the city of Dubrovnik, and received 21 million kunas (2.8 milion euros). Another firm with zero employees performed IT maintenance worth 23 million kunas for the city of Zagreb, receiving 21 exclusive contracts as the only bidder. There were also a number of single-bidder firms with the description of procurement undefined, which received between 10 and 14 million kunas in the observed period.

Table 3.15 decomposes the total value of all procurement contracts with respect to the number of employees. It shows that in total 783 million kunas (around 100 million euros) was given to firms without a single employee (327 companies). After receiving the bid, firms that remained having zero employees received 506 million kunas. In total 117 firms without any employees won procurement bids (many of them multiple times) where they were the only bidder. Additionally, around 2.275 billion kunas (around 300 million euros) was allocated to 524 companies which were the only bidder at the procurement auction.

Table 3.15: Value of procurement contracts with respect to the number of employees

Number of employees	Total value of contracts	Type of company	Number of companies
0	783,737,024	No employees	327
1 to 9	2,150,542,380	Micro	1787
10 to 49	6,005,155,171	Small	1737
50 to 249	8,560,869,289	Medium	728
250>	9,088,785,126	Large	266
Total	26,589,088,990		4845

Note: All values in kunas, 1 euro  $\approx$  7.6 kunas. Sources and description of data available in the Data section.

The third proxy for corruption captures contracts given to firms who had large losses in one year, but after signing the procurement contract which more than offset the size of their loss, finished the next business year with a profit. One medium-sized firm won a tender for supplying office equipment to the city of Zagreb worth 30 million kunas, which completely covered their 14.8 million loss in one year and enabled them a 15 million profit in the next. Another firm with reported 25 million losses in one year signed 30 procurement contracts for the supervision of IT networks worth 43 million, and made a profit in the subsequent business year of 20 million kunas. Furthermore a medium-sized construction firm with 6.3 million losses in one year signed a contract to build boats in a small coastal municipality worth 9 million kunas, and ended up with 2 million profits in the next year. A small-sized construction company turned their 4 million loss into a 4 million profit after signing two 9 million contracts for undefined construction works for the local city. Another company gained a 30 million contract to perform printing services for the local University, funded by the city, which enabled them to overturn their 13 million loss into a 26 million profit. A firm in the same city won a 52 million tender for technical inspection of motor vehicles helping them overturning a 5 million loss into a 20 million profit. A record was set by a company overturning a 35 million loss into a 45 million profit in a single year after winning an exclusive 100 million tender for performing transportation services for local primary and high school students. Needless to say, many of these companies had

Table 3.16: Indirect subsidies to firms with losses

Value of contract with respect to total loss in the year the procurement was won	Total value of contracts	Number of companies
0 to 10%	108,152,031	104
from 11 to 50%	50,014,015	96
from 51 to 100%	146,302,052	57
from 101 to 200%	285,914,219	58
from 201 to 500%	479,519,327	74
more than 501%	2,557,218,139	118
Total	3,627,119,783	507
Only firms with losses that made a profit after winning the bid		
Total value of contracts	1,874,643,400	202
Total loss (t = 0)	-1,211,965,362	
Total profit (t = 1)	437,639,572	

Note: All values in kunas, 1 euro  $\approx$  7.6 kunas. t = 0 is the year the procurement contract was won by a firm, while t = 1 is the subsequent business year.

almost no other sources of revenues except public procurement contracts.

Table 3.16 shows how the local government indirectly subsidized firms with losses, channelling a total of 3.6bn kunas (470m euros) to such firms, from which 3.3bn was allocated to 250 companies which received contracts whose value exceeded or just about covered their loss. Out of the given sample, 202 firms reported a profit immediately in the following year after signing the contract. They received 1.8bn kunas (230m euros) during the observed period.

## Bibliography

- [1] Ades, A., and Di Tella, R., 1999. “Rents, Competition and Corruption.” *American Economic Review*, 89(4): 982-993.
- [2] Angrist, J. D., and Pischke, J.S., 2009. *Mostly Harmless Econometrics. An Empiricist’s Companion*. Princeton: Princeton University Press.
- [3] Armstrong, D.A., and Duch, R.M., 2010. “Why can voters anticipate post-election coalition formation likelihoods?” *Electoral Studies*, 29: 308-315.
- [4] Attorney General of the Republic of Croatia (DORH), 2015. *Report of the Attorney General in front of Croatian Parliament for the year 2014*. Report number A-464/14. Zagreb, April, 2015. Available at: <http://www.dorh.hr/IzvjesceDrzavnogOdvjetnistvaRepublike>. [Accessed 20th February 2017]
- [5] Bandiera, O., Prat, A., and Valletti, T., 2009. “Active and Passive Waste in Government Spending: Evidence from a Policy Experiment.” *American Economic Review*, 99(4): 1278-1308.
- [6] Besley, T., 2006. *Principled Agents? The Political Economy of Good Government*. New York: Oxford University Press.
- [7] Besley, T. and Prat, A., 2006. “Handcuffs for the Grabbing Hand? Media Capture and Government Accountability.” *The American Economic Review*, 96(3): 720-736.

- [8] Brennan, G., and Buchanan, J., 1980. *The Power to Tax: Analytical Foundations of a Fiscal Constitution*. Cambridge: Cambridge University Press.
- [9] Bueno de Mesquita, B., Smith, A., Siverson, R., and Morrow J., 2005. *The Logic of Political Survival*. Cambridge: MIT Press.
- [10] Caselli, F., and Morelli, M., 2004. "Bad Politicians." *Journal of Public Economics*, 88: 759-782.
- [11] Chang, E.C. and Golden, M.A., 2004. "Does corruption pay? The survival of politicians charged with malfeasance in the postwar Italian Chamber of Deputies." Unpublished paper, Michigan State University and the University of California at Los Angeles.
- [12] Chang, E. C.C. and Kerr, N. N., 2017. "An Insider Outsider Theory of Popular Tolerance for Corrupt Politicians." *Governance*, 30(1): 67-84.
- [13] Coate, S. and Morris, S. 1995. "On the Form of Transfers to Special Interests." *Journal of Political Economy*, 103, 1210-1235.
- [14] Committee for Determining Conflict of Interest (2016, 2015, 2014). Annual report for the year 2015, 2014, 2013. Zagreb. Available at: <https://www.sukobinteresa.hr/hr/annual-report-of-the-commission>. [Accessed 29th June 2017]
- [15] Croatian Bureau of Statistics, 2013. "Census of Population, Households and

- Dwellings 2011.” [online] DZS, Zagreb. Available at: [www.dzs.hr/default\\_e.htm](http://www.dzs.hr/default_e.htm) [Accessed 22nd June 2014]
- [16] Croatian Bureau of Statistics, 2014. “Statistical Yearbook 2013.” [online] DZS, Zagreb. Available at: [www.dzs.hr/default\\_e.htm](http://www.dzs.hr/default_e.htm) [Accessed 30th June 2014]
- [17] Croatian Employment Bureau, 2014. “Employment bureau yearbook 2013, 2012, 2011, 2010, 2009.” [online] HZZ, Zagreb. Available at: [www.hzz.hr/default.aspx?id=10055](http://www.hzz.hr/default.aspx?id=10055) [Accessed 22nd June 2014]
- [18] Croatian Institute for Public Finance (IJF), 2016. “Proracunska transparentnost zupanija, gradova i općina: studeni 2016. do ožujak 2017.” *IJF Newsletter* 115 (2018). Institut za javne financije, Zagreb, 2016.
- [19] Croatian Parliament, 2010. “Law on wages in local and regional government.” NN 28/10, Narodne Novine, Zagreb. Available at: [https://www.zakon.hr/z/290/Zakon-o-plaC487ama-u-lokalnoj-i-podruC48Dnoj-\(regionalnoj\)-samoupravi](https://www.zakon.hr/z/290/Zakon-o-plaC487ama-u-lokalnoj-i-podruC48Dnoj-(regionalnoj)-samoupravi).
- [20] Croatian Parliament, 2013. “Law of local and regional government.” NN 36/09 (earlier issue), 19/13, Narodne Novine, Zagreb. Available at: [https://narodne-novine.nn.hr/clanci/sluzbeni/2013\\_02\\_19\\_323.html](https://narodne-novine.nn.hr/clanci/sluzbeni/2013_02_19_323.html) [Accessed 3rd November 2016]

- [21] Croatian Parliament, 2016. "Law on income tax." NN 115/16, Narodne Novine, Zagreb. Available at: [https://narodne-novine.nn.hr/clanci/sluzbeni/2016\\_12\\_115\\_2525.html](https://narodne-novine.nn.hr/clanci/sluzbeni/2016_12_115_2525.html)
- [22] de Figueiredo, M.F.P, Hidalgo, D.F, and Kasahara, Y. 2012. "When Do Voters Punish Corrupt Politicians? Experimental Evidence from Brazil." Working paper.
- [23] Di Tella, R. i Schargrodsky, E., 2003. "The Role of Wages and Auditing during a Crackdown on Corruption in the City of Buenos Aires." *Journal of Law & Economics*, 46(1): 269-292.
- [24] Dimock, M.A. and Jacobson,G.C., 1995. "Checks and Choices: The House Bank Scandal's Impact on Voters in 1992." *The Journal of Politics*, 57(4): 1143-1159.
- [25] Dobratz, B.A., and Whitfield, S., 1992. "Does Scandal Influence Voters' Party Preference? The Case of Greece During the Papandreou Era." *European Sociological Review*, 8(2): 167-180.
- [26] DORH, 2015. "Izvjesce Drzavnog Odvjetnistva Republike Hrvatske za 2014. godinu." Office of the State Attorney General for Croatia A-464/14, Zagreb, April 2015.
- [27] Downs, A., 1957. *An Economic Theory of Democracy*. New York: Addison-Wesley.

- [28] Duch, R.M. i Stevenson, R.T., 2008. *The economic vote: How political and economic institutions condition electoral results*. Cambirdge: Cambridge University Press
- [29] Eggers, A., Freier, R., Nannicini, T., and Grembi, V. 2016. “Regression Discontinuity Designs Based on Population Thresholds: Pitfalls and Solutions.” Forthcoming in *American Journal of Political Science*.
- [30] Egger, P. and Koethenbuerger, M., 2010. “Government Spending and Legislative Organization: Quasi-Experimental Evidence from Germany.” *American Economic Journal: Applied Economics*, 2: 200-212.
- [31] European Commission, 2014. “EU Anti-Corruption Report.” [online] Bruxelles, 03.02.2014. Available at: [http://ec.europa.eu/dgs/home-affairs/e-library/documents/policies/organized-crime-and-human-trafficking/corruption/docs/acr\\_2014\\_en.pdf](http://ec.europa.eu/dgs/home-affairs/e-library/documents/policies/organized-crime-and-human-trafficking/corruption/docs/acr_2014_en.pdf) [Accessed March 1st 2014]
- [32] Faccio, M., 2006. “Politically Connected Firms.” *American Economic Review*, 96(1): 369-386.
- [33] Fazekas, M., Toth, I.J., and King, L.P., 2016. “An Objective Corruption Risk Index Using Public Procurement Data.” *European Journal on Criminal Policy and Research*, 22(3): 369-397.
- [34] Fazekas, M., and Toth, I.J., 2016. “From Corruption to State Capture: A New

Analytical Framework with Empirical Applications from Hungary.” *Political Research Quarterly* 69(2): 320-334.

- [35] Fazekas, M., and Kocsis, G., 2017. “Uncovering High-Level Corruption: Cross-National Objective Corruption Risk Indicators Using Public Procurement Data.” *British Journal of Political Science*, published online 24 August 2017.
- [36] Ferejohn, J., 1986. “Incumbent Performance and Electoral Control.” *Public Choice*, 50, 5-25.
- [37] Ferraz, C. and Finan F., 2008. “Exposing Corrupt Politicians: The Effects of Brazil’s Publicly Released Audits on Electoral Outcomes.” *Quarterly Journal of Economics*, 123(2): 703-745.
- [38] Ferraz, C., and Finan, F., 2011. “Electoral Accountability and Corruption: Evidence from the Audits of Local Governments.” *American Economic Review*, 101(4): 1274-1311.
- [39] Ferwerda, J., Deleanu, I., and Unger, B., 2017. “Corruption in Public Procurement: Finding the Right Indicators.” *European Journal on Criminal Policy and Research* 23(2): 245-267.
- [40] Fisman, R. 2001. “Estimating the Value of Political Connections.” *The American Economic Review*, 91(4): 1095-1102.

- [41] Fisman, R., and Gatti, R., 2002. "Decentralization and corruption: evidence across countries." *Journal of Public Economics* 83(2002): 325-345.
- [42] Gardiner, J.A. 2002. "Defining Corruption." In Heidenheimer, A. and Johnston, M. (eds.), 2002. *Political Corruption: Concepts and Contexts. Third edition*. New Brunswick: Transaction Publishers.
- [43] Gilens, M., 2012. *Affluence and Influence. Economic Inequality and Political Power in America*. Princeton: Princeton University Press.
- [44] Gilens, M., and Page, B., 2014. "Testing Theories of American Politics: Elites, Interest Groups, and Average Citizens." *Perspectives on Politics* 12(3): 564-581.
- [45] Glaurdic, J., and Vukovic, V., 2016. "Voting after war: Legacy of conflict and the economy as determinants of electoral support in Croatia." *Electoral Studies*, 42 (June 2016): 135-145.
- [46] Glaurdic, J., and Vukovic, V., 2017. "Granting votes: Exposing the political bias of intergovernmental grants using the within-between specification for panel data" *Public Choice*, 171(1): 223-241.
- [47] Golden, M. A., 2004. "International economic sources of regime change: How European economic integration undermined Italy's postwar party system." *Comparative Political Studies*, 37(10): 1238-1274.

- [48] Golden, M.A., and Picci, L., 2005. "Proposal for a new measure of corruption, illustrated with Italian data." *Economics and Politics*, 17(1): 37-75.
- [49] Grossman, G.M. and Helpman, E. 2002. *Special Interest Politics*. Cambridge, MA: MIT Press.
- [50] Helland, L. and Sorensen, R., 2012. "Persistent rent extraction." *Public Choice*, 153(1): 205-213.
- [51] Imbens, G., and Lemieux, T. 2008. "Regression discontinuity design: A guide to practice." *Journal of Econometrics*, 142(2): 615-635.
- [52] Integrity observers, 2012. Statistical data on public procurement. [online] Zagreb. Available at: [www.integrityobservers.eu](http://www.integrityobservers.eu) [Accessed January 25th 2014]
- [53] Kaufman, D. and Vicente, P., 2011. "Legal Corruption." *Economics and Politics*, 33(2): 195-219.
- [54] Klasnja, M., 2015. "Corruption and the Incumbency Disadvantage: Theory and Evidence." *The Journal of Politics*, 77(4): 928-942.
- [55] Klasnja, M., Tucker, J.A., and Deegan-Krause, K., 2016. "Pocketbook vs. Sociotropic Corruption Voting." *British Journal of Political Science*, 46(1): 67-94.
- [56] Knack, S. and Keefer, P., 1995. "Institutions and economic performance:

- Cross-country tests using alternative institutional measures.” *Economics and Politics*, 7(3): 207-227.
- [57] Krueger, A., 1974. “The Political Economy of the Rent Seeking Society.” *American Economic Review*, 64(3): 291-303.
- [58] Kurer, O., 2001. “Why do voters support corrupt politicians?” In Jain, A.K. (ed.) *The Political Economy of Corruption*. London: Routledge.
- [59] Lewis-Beck, M.S., and Paldam, M., 2000. “Economic voting: An introduction.” *Electoral Studies*, 19: 113-121.
- [60] Lindstedt, C. and Naurin, D. 2010. “Transparency is not Enough: Making Transparency Effective in Reducing Corruption.” *International Political Science Review*, 31(3): 301-322.
- [61] Litvan, G. 2014. “Samo Zeljku Sabi zatvor za uobicajeno kupovanje vijecnika.” [online] Lider, published 06th September 2014. Available at: <https://lider.media/aktualno/biznis-i-politika/hrvatska/samo-zeljku-sabi-zatvor-za-uobicajeno-kupovanje-vijecnika/>. [Accessed 02nd May 2015]
- [62] Manzetti, L. and Wilson, C.J., 2007. “Why Do Corrupt Governments Maintain Public Support?” *Comparative Political Studies*, 40(8): 949-970.

- [63] Mauro, P., 1998. "Corruption and the Composition of Government Spending." *Journal of Public Economics*, 69(2): 263-279.
- [64] McChesney, F., 1997. *Money for Nothing: Politicians, Rent Extraction, and Political Extortion*. Cambridge: Harvard University Press.
- [65] McCrary, J. 2008. "Manipulation of the Running Variable in the Regression Discontinuity Design: A Density Test." *Journal of Econometrics*, 142(2): 698-714.
- [66] Ministry of Finance, Republic of Croatia, 2014. Archives of local budgets 2009-2011. [online] Zagreb, 2014. Available at: <http://www.mfin.hr/hr/lokalni-proracuni> [Accessed June 24th 2014]
- [67] N1 news, 2016. "Bandic masovno zaposljava podobne u Zagrebacki holding." [online] N1 info, published 22nd November 2016. Available at: <http://hr.n1info.com/Vijesti/a163540/Sindikati-Bandic-masovno-zaposljava-politicki-podobne.html>. [Accessed 30th September 2017]
- [68] North, D.C., Wallis, J.J., and Weingast, B.R., 2009. *Violence and Social Orders. A Conceptual Framework for Interpreting Recorded Human History*. Cambridge: Cambridge University Press.
- [69] Nyblade, B. and Reed, S.R., 2008. "Who Cheats? Who Loots? Political

Competition and Corruption in Japan, 1947-1993.” *American Journal of Political Science*, 52(4): 926-941.

- [70] Official Gazette of the Republic of Croatia, 2015. Electronic register of public procurement in the Republic of Croatia. [online] Zagreb. Available at: [eojn.nn.hr/Oglasnik/](http://eojn.nn.hr/Oglasnik/) [Accessed January 18th 2015]
- [71] Olken B.A., 2007. “Monitoring Corruption: Evidence from a Field Experiment in Indonesia.” *Journal of Political Economy*, 115(2): 200-249.
- [72] Patkovic, N. 2014. “Zbog podmicivanja vijećnika osuđen na godinu i 4 mjeseca.” [online] Jutarnji list, published 02nd September 2014. Available at: <https://www.jutarnji.hr/vijesti/hrvatska/zeljko-sabo-proglasen-krivimzbog-podmicivanja-vijecnika/693781/>. [Accessed 10th May 2015]
- [73] Patrikios, S. and Karyotis, G., 2008. “The Greek parliamentary election of 2007.” *Electoral Studies*, 27(2): 356-359.
- [74] Persson, T., and Tabellini, G., 2000. *Political Economics. Explaining Economic Policy*. Cambridge: MIT Press.
- [75] Peters, J.G. and Welch, S., 1980. “The Effects of Charges of Corruption on Voting Behavior in Congressional Elections.” *American Political Science Review*, 74(3): 697-708.

- [76] Petrusic, Z. 2014. "Svi su krivi: Sanaderu devet godina zatvora. Osudenima se oduzima 55 milijuna kuna" [online] Jutarnji list, published 11th March 2014. Available at: [www.jutarnji.hr/fimi-media--sanaderu-devet-godina-zatvora/1172471/](http://www.jutarnji.hr/fimi-media--sanaderu-devet-godina-zatvora/1172471/). [Accessed 10th May 2015]
- [77] Pettersson-Lidbom, P., 2012. "Does the size of the legislature affect the size of government? Evidence from two natural experiments." *Journal of Public Economics*, 96: 269-278.
- [78] Piattoni, S., 2001. "Clientelism in Historical and Comparative Perspective." In Piattoni, S. (ed.) *Clientelism, Interests, and Democratic Representation*. Cambridge: Cambridge University Press.
- [79] Podobnik, B., Vukovic, V., and Stanley H.E., 2015. "Does Corruption Encourage the Wage Gap between Private and Public Sectors?" *PLoS One*, 11(8): e0158782.
- [80] Reed, S. R., 1999. "Punishing corruption: The response of the Japanese electorate to scandals." In O. Feldman (Ed.) *Political Psychology in Japan: Behind the Nails Which Sometimes Stick out (and Get Hammered Down)*. Commack, N.Y.: Nova Science.
- [81] Reinikka, R., and Svensson, J., 2004. "Local capture: evidence from a central

- government transfer program in Uganda.” *Quarterly Journal of Economics*, 119(2): 679-705.
- [82] Robinson, J.A. and Verdier, T., 2013. “The Political Economy of Clientelism.” *Scandinavian Journal of Economics*, 115 (2): 260-291.
- [83] Rose-Ackerman, S., 1978. *Corruption: a study in political economy*. New York: Academic Press.
- [84] Root, H.L. and Nellis, N., 2000. “The Compulsion of Patronage: Political Sources of Information Asymmetry and Risk in Developing Country Economies.” In Bueno De Mesquita and Root (eds.) *Governing for Prosperity*. New Haven: Yale University Press.
- [85] Rundquist, B.S., Strom, G.S. and Peters, J.G., 1977. “Corrupt Politicians and Their Electoral Support: Some Experimental Observations.” *The American Political Science Review*, 71(3): 954-963.
- [86] Schoenman, R. 2014. *Networks and Institutions in Europe’s Emerging Markets*. Cambridge: Cambridge University Press.
- [87] Shleifer, A., and Vishny, R., 1993. “Corruption.” *Quarterly Journal of Economics*, 108(3), 599-617.
- [88] Shleifer, A., and Vishny, R., 1998. *The Grabbing Hand*. Cambridge, MA: Harvard University Press.

- [89] State Electoral Commission of the Republic of Croatia, 2014. "Information on election results on county and municipal level 2009, 2013." [online] DIP, Zagreb. Available at: [www.izbori.hr/ws/index.html?documentId=A758299505B80F02C1257C8400606C3B](http://www.izbori.hr/ws/index.html?documentId=A758299505B80F02C1257C8400606C3B) [Accessed 12th May 2014]
- [90] Stokes, S.C., Dunning, T., Nazareno, M., and Brusco V. 2013. *Brokers, Voters, and Clientelism. The Puzzle of Distributive Politics*. Cambridge: Cambridge University Press.
- [91] Tanzi, V., and Davoodi, H., 1997. "Corruption, Public Investment and Growth." *IMF Working Paper* No.97/139, International Monetary Fund.
- [92] Transparency International, 2016. "Corruption by country: Croatia." [online] Available at: [www.transparency.org/country#HRV\\_PublicOpinion](http://www.transparency.org/country#HRV_PublicOpinion) [Accessed 18th February 2015]
- [93] Tomicic, L. 2015. "Biracima korupcija ne smeta: Zasto nikoga ne brinu pravomocne presude kandidatima?" [online] Novi list, published 15th March 2015. Available at: [www.novilist.hr/Vijesti/Hrvatska/Biracima-korupcija-ne-smeta](http://www.novilist.hr/Vijesti/Hrvatska/Biracima-korupcija-ne-smeta) [Accessed 10th May 2015]
- [94] Tullock, G., 1967. "The Welfare Costs of Tariffs, Monopolies and Theft." *Western Economic Journal*, 5(3): 224-232.
- [95] Vecernji list, 2013. "Poslusajte cijelu snimku razgovora u kojem Sabo nudi 50.000 kn mita!" [online] Vecernji list, published

03rd August 2013. Available at: <https://www.vecernji.hr/vijesti/poslusajte-cijelu-snimku-razgovora-u-kojem-sabo-594038> [Accessed 10th May 2015]

- [96] Verdier, D., 1995. "The politics of public aid to private industry: The role of policy networks." *Comparative Political Studies*, 28(1): 3-42.
- [97] Vjetrenjaca, 2011. Public procurement register. [online] Published November 28th 2011, Zagreb. Available at: <http://nabava.vjetrenjaca.org/index> [Accessed January 25th 2014]
- [98] Welch, S. and Hibbing, J.R., 1997. "The Effects of Charges of Corruption on Voting Behavior in Congressional Elections, 1982-1990." *The Journal of Politics*, 59(1): 226-239.
- [99] Winters, M.S., and Weitz-Shapiro, R. 2013. "Lacking Information or Condoning Corruption: When Do Voters Support Corrupt Politicians?" *Comparative Politics*, 45(4): 418-436.
- [100] Wooldridge, J.M., 2002. *Econometric Analysis of Cross Section and Panel Data*. Cambridge, MA: MIT Press.

## Chapter 4

# The politics of bailouts

## Estimating the causal effects of political connections on corporate bailouts during the 2008-2009 US financial crisis

### Abstract

In 2008 as the financial crisis unfolded in the United States, the banking industry increased their lobbying and campaign spending activities. By the end of 2008 and during 2009 the biggest political spenders on average received the largest bailout packages. Is this relationship causal? In this paper I examine the effect of political connections on the allocation of the Troubled Asset Relief Program (TARP) to the United States finance industry during the 2008-2009 financial crisis. I find that

TARP recipients which lobbied the government, donated to political campaigns, or had direct connections to politics via top executives received a better bailout deal. I run a regression discontinuity design and an instrumental variable estimation to uncover how losses of politicians in close races affected the distribution of bailout funds for a connected firm. This does not imply that some banks were deliberately favoured over others, just that banks that were favoured benefited because of their proximity to the right people in power. If being politically connected matters in general, in times of crisis it matters even more.

**Keywords:** Bailouts, political connections, lobbying, campaign spending, US financial crisis, TARP

## 4.1 Introduction

The biggest financial crisis since the Great Depression started with the collapse of the US housing market in 2007 which subsequently exposed the systemic risk of the US finance industry. The bankruptcy of Lehman Brothers in September 2008 triggered a downward spiral of financial institutions in desperate need for refinancing to avoid the same scenario. The policymakers responded and in October 2008 drafted a Troubled Asset Relief Program (TARP) that would over the next year provide liquidity to the finance industry. Total investment from the government through TARP to the finance industry was \$372bn (2.55% of US GDP).

The eight biggest banks by size of assets<sup>1</sup> and their subsidiaries received almost half of that amount — \$174bn (1.19% of US GDP). At the same time, in 2008 and 2009, lobbying expenditures and campaign contributions of TARP recipients increased significantly. Conventional wisdom would suggest a connection between TARP distribution and lobbying and donations. The literature that links lobbying to the distribution of TARP funds (e.g. Duchin and Sosyura, 2012; Igan, Mishra, and Tressel, 2012; Blau, Brough, and Thomas, 2013; Calomiris and Khan, 2015; Blau, 2017), mostly using some type of multivariate analysis, claims that greater lobbying increased the probability of bailouts. However these conclusions could be misleading as they are not robust to a number of endogeneity issues such as self-selection or the omitted variable bias.

Fortunately this episode of US history provides an opportunity for a compelling research design that is able to address the endogeneity issues and estimate the causal effect of political connections on the distribution of TARP funds, at least among its politically connected recipients. As the crisis was unfolding and the politicians needed to make a succession of decisions on how to deal with the failing banking system, the United States was in the midst of a political campaign for the next President and the new 111th Congress. This context, having a federal election right in the middle of the biggest banking panic since the 1930s, allows me to exploit close electoral races as sources of as-good-as random assignment of

---

<sup>1</sup>In order by size of assets in 2008: JP Morgan Chase, Bank of America, Citigroup, Wells Fargo, US Bancorp, Bank of New York Mellon, Goldman Sachs, and Morgan Stanley.

politicians connected to TARP recipients, and thus enable me to answer the main question of interest: did political connections matter in the distribution of TARP funds.

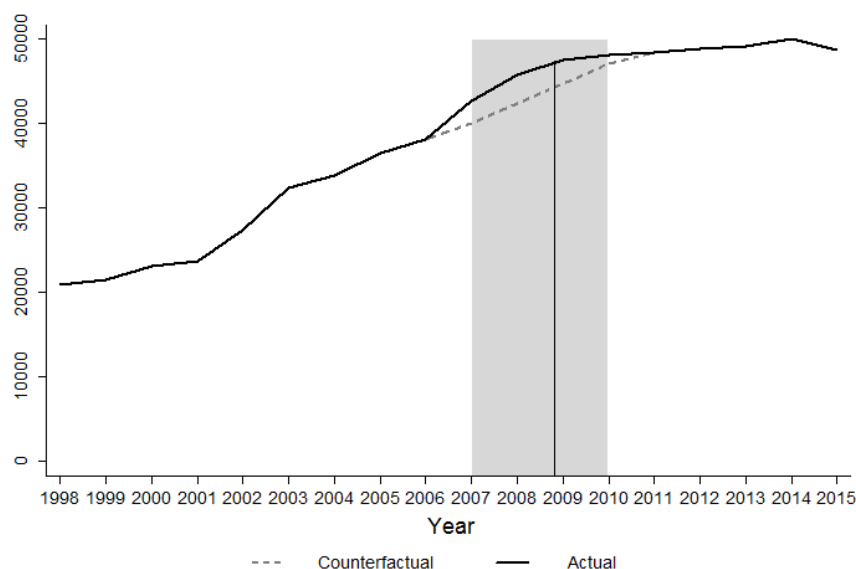


Figure 4.1: Actual (black) and counterfactual (red) lobbying spending of finance firms (y-values in millions of dollars). Counterfactual lobbying is the estimated size of lobbying expenditures had it not been for the crisis. It is estimated using simple exponential smoothing, according to equation 4.1 described in Section 4. The difference between actual and counterfactual lobbying in 2008 and 2009 is what is defined to be abnormal lobbying. The straight vertical line denotes the start of the bailout allocation process (28th October 2008).

Consider Figures 4.1 and 4.2 that show a clear peak in both total lobbying and the total number of financial firms that lobbied during the distribution of TARP funds in 2008 and 2009. The figures reveal that the upward trend of lobbying started several years before the crisis, raising doubt over the potential causal mechanism suggested by conventional wisdom. Furthermore, there was obviously large within-industry variation. Some firms lobbied and spent on campaigns more

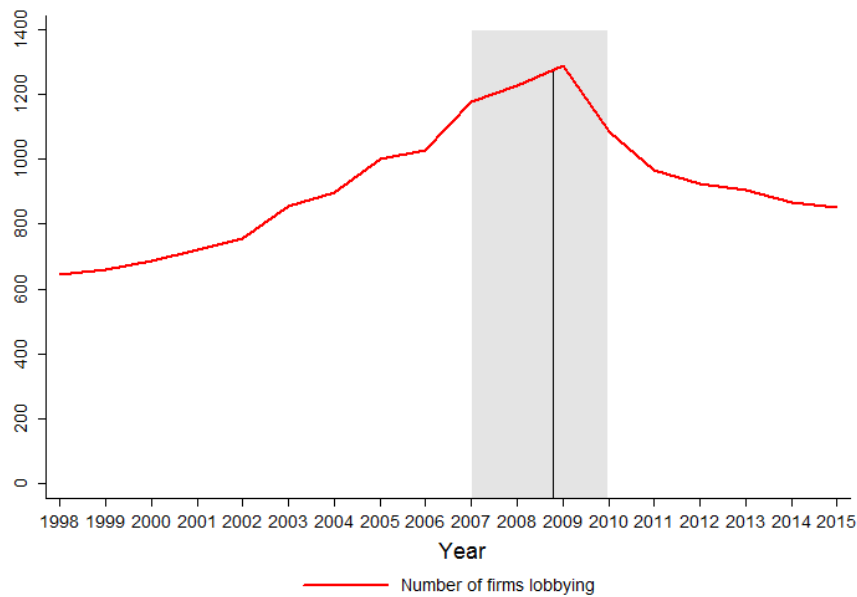


Figure 4.2: Total number of finance industry firms lobbying, peaking in 2008 and 2009. The straight vertical line denotes the start of the bailout allocation process (28th October 2008).

than others, some had greater political connections, but some were also much more exposed to risk to begin with. Saying that firms that lobbied more got more money from bailouts does not tell us anything about whether political connections *caused* better bailout deals. There may be a whole number of potential confounders that could have affected the distribution of TARP funds but that had nothing to do with political connections, lobbying, or campaign financing. Therefore any research attempt to uncover the true causal implication of this relationship must solve two basic methodological problems. First, a self-selection problem — firms are more likely to lobby and engage in campaign donations if they expect successful outcomes and a return of the favour from the politician. Second, there is a serious

omitted variable bias. The driver of the variation that simultaneously increased lobbying spending and the likelihood of getting a bailout could be risk exposure of banks. Riskier firms lobby and donate more because they are more prone to risk, implying they will also have a greater chance of getting bailout funds. Exposure to risk, not political spending, can thus be the main explanation behind the bailout allocation.

In this paper I apply several methodological approaches in attempting to resolve these endogeneity issues and uncover the causal mechanism of political connections on policy outcomes in the context of the US financial crisis. Political influence is approximated by looking at financial firms which either lobbied the government, made campaign donations in the 2008 cycle, or had upper-management executives who used to have high-level positions in government at least five years prior to the crisis. This type of political influence could have indirectly affected the allocation decisions of the Treasury and Congress and swayed some funds towards the better-connected firms. I start the empirical analysis using selection on observables approaches; OLS and matching. Neither of them necessarily solve the omitted variable bias problem, but they do indicate a strong and very large positive effect. The initial results offer motivation for more robust methods to resolve endogeneity issues, in particular regression discontinuity (RD) design and an instrumental variables (IV) approach. I look at the allocation of bailout funds to firms whose connected politicians lose a very close election (decided within a 1%

margin of victory). Both of these approaches draw randomization from the assumption that in such close races a random element such as luck is most likely to have tipped the election, meaning that the bare winners and losers are supposed to be statistically interchangeable.

The sample is consisted only of firms that received TARP funds, separated into connected and unconnected firms, meaning that all of the estimated effects are necessarily local treatment effects. The research question is thus very specific; among firms that received TARP funds did political connections result in a larger relative bailout? The matching and OLS estimates make a direct comparison between connected and unconnected firms, and find that being politically connected increased a firm's received bailout funds. The RD analysis restricts the sample only to connected firms whose politicians won a narrow election, and finds that a close electoral victory of an additional connected politician increased the relative bailout allocation for that politician's connected firm. Finally the IV approach finds that firms with a higher proportion of politicians who won the 2008 election in a closely contested election received more TARP funds. These are three different effects but they all suggest a similar conclusion: being politically connected clearly made a difference in the allocation of TARP funds among those who received it.

It may seem strange to conclude that personal connections to politicians mattered in policy decisions of such high importance in a country with robust and strong institutions like the United States. Usually the literature recognizes policy

distortions based on personal connections in developing countries and emerging markets (Fisman, 2001; Faccio, 2006; Faccio et al, 2006; Khwaja and Mian, 2005; Desai and Olofsgard, 2011), whereas in developed countries these things happen on a local rather than a national level. However, the conclusions of this paper do not imply that policy-makers deliberately made concessions to specific banks. It suggests that in making the decision political connections inadvertently favoured some banks over others simply due to their proximity to the people in power. Acemoglu et al (2016) call this the “social connections in a crisis hypothesis” exemplifying how firms connected to the new Treasury Secretary Geithner experienced a sharp increase in market returns after his announcement. According to them every person in power has a limited social network, a group of friends and acquaintances whose expertise and advice they rely upon. This influence of the social network can even extend into cultural capture (Kwak, 2014), where regulators, particularly at the highest positions, tend to side with the industry view instead of maintaining an unbiased impartial approach. Social connections can thus prove to be the crucial explanatory factor behind enacting legislation, promoting regulations, and even hiring decisions. In delicate and high-stress situations such as the latest financial crisis, these connections matter even more. It generally pays off to be politically connected, and in times of crises — it pays off even more.

The main contribution of this paper is empirical — it uses a natural experiment design to establish a link between political connections of TARP recipients and

the allocation of bailout funds during the 2008-2009 US financial crisis. By doing so it adds to the literature on the empirical verification of the influence of political connections on policy outcomes, as well as to the explanation of the role of politics in the allocation of TARP funds during the crisis. The next section describes the context of the US financial crisis and the TARP allocation process. To fully understand the process I used information from interviews I held with former employees of the Office of the Special Inspector General of the TARP, the US House Oversight Subcommittee on TARP, and the US Treasury. The third section describes the sources of data and defines all the variables while the fourth section outlines the empirical strategies used to examine the relationship of interest, and presents the results. The final section concludes, discusses the limitations of the paper, and offers guidelines for further research.

## **4.2 Political connections and bailouts in the context of the 2008-09 US financial crisis**

### **4.2.1 The allocation of the TARP**

The context of the 2008 US financial crisis is more complex than what the standard economic theory of regulation could explain. The panic that started with the bankruptcy of Lehman Brothers in September 2008 threatened to disrupt the entire financial system and send the US, and consequently the global economy into

another Great Depression. To avert disaster policy-makers needed to react quickly with new pieces of legislation to prevent further panic and hopefully restore confidence to a shaken industry. The response came in form of the Emergency Economic Stabilization Act (EESA) of 2008, designed by Treasury Secretary Henry Paulson with an aim to recapitalize US financial institutions in case of systemic collapse. The so-called bailout proposal was initially voted down in Congress on September 29th 2008, but after a few amendments by the Senate, the House enacted the bill into law on October 3rd 2008. The Troubled Asset Relief Program (TARP) was the key part of the EESA, allowing the Treasury sweeping powers in purchasing troubled assets (mainly mortgage-backed securities) from ailing financial institutions to the initial total sum of \$700bn (this was later reduced to a maximum of \$475bn). On October 14th the Treasury announced that instead of purchasing troubled assets it would purchase preferred stock and equity in financial institutions, for which it established a new plan called the Capital Purchase Program (CPP). The CPP had \$250bn at its disposal, 125bn of which was to be spent to recapitalize the eight biggest banks (all of which were automatically included in the CPP, having been declared “too big to fail”). In total \$204.9bn was spent under the CPP, and \$426bn was spent under the TARP. All of the funds were allocated by the end of 2009, and were repaid by December 2014, when the Treasury closed the program after getting back in total \$441bn by selling the equity it was holding (as banks were gradually exiting the program). For the vast majority of financial

institutions the TARP was essentially a loan (in the form of cheap capital) from the government that helped them recover during times of great distress. The full timeline of the bailout allocation process during this turbulent period is available in the Appendix.

The selection criteria for the TARP was based on the CAMELS rating system, which ranked all US financial institutions based on how likely they would be to need recapitalization using a weighted index of their various risk and performance indicators<sup>2</sup>. The decisions over eligibility and who gets selected for funding were made by the Treasury using the CAMELS rating, in consultation with the Federal Reserves. Firms that were eligible could apply and the Treasury had the option of accepting or rejecting the application. Entering the TARP had its downsides as well — it was subject to limitations of executive pay, bonuses, and golden parachutes, in addition to losing some tax benefits. However there was mounting perception that the selection criteria were not clear enough and that too much power was given to the Treasury (Veronesi and Zingales, 2010); that it favoured those institutions that would survive anyway and that were too big to fail (Dash, 2008); and even that some congressmen were funnelling resources to banks in their own districts (Pana and Wilson, 2012).

According to Duchin and Sosyura (2012) out of 537 public firms that were

---

<sup>2</sup>The CAMELS rating system uses Capital adequacy, Asset quality, Management quality, Earnings, Liquidity, and Sensitivity to risk as categories from which it creates a single index. The Treasury has not made it public, however in this paper I re-create it using the same indicators. See further information in the data section.

eligible for the CPP investment (the total number was over 900 institutions, but the authors only focus on the subsample of public firms), 80% applied for the program, most of which were approved and received the funding (only 15% rejected it after being approved). Participating in the TARP was therefore attractive for most firms, despite the limitations on executive compensation. Some banks even went so far to falsify financial statements in order to be eligible for the program, which was later uncovered by the Special Inspector General of the TARP in his report to Congress (SIGTARP, 2011). Even though some financial institutions wanted to get out of the TARP already in mid-2009 — to remove the stigma associated from participating in the program — the motivation to be included in the TARP was to mitigate the uncertainty at the height of the crisis. During uncertain times when industry-wide panic sent shock-waves to the rest of the economy, requesting to be a part of the TARP was the best available strategy a bank could use to remain solvent.

Mian, Sufi, and Trebbi (2010) have shown that campaign contributions from the finance industry in 2008 strongly predict the vote of an individual congressmen in favour of the EESA. Therefore, despite their later push-back, the majority of banks were eager to participate, which induced them to activate their connections to politics, either through abnormal lobbying, campaign spending, or direct connections to their former colleagues at the Treasury, Congress, or the Fed. It is for these reasons that I only focus on firms that received the bailout funds, instead of

comparing recipients and non-recipients. The majority of the non-recipients did not even have an incentive to participate so it would be misleading to compare their outcomes to the recipients, not to mention that this would bias the estimates.

Finally, prior research clearly suggests that individual members of Congress exerted some sort of influence over the entire process (Mian, Sufi, and Trebbi, 2010; Pana and Wilson, 2012). This implies that even though the final decision over who gets the funds was made by the Treasury following a clear-cut formula, the decision itself was subject to influence by a number of actors. Financial institutions had two ways of trying to influence the allocation of funds: they lobbied or donated campaign funds to their Congressmen, or they used their personal connections to anyone inside the Treasury, the Fed, or Congress to try and get a better deal for themselves. Influencing Congressmen was most likely motivated by encouraging them to vote for the TARP, but given that some Congressmen were seen as favouring their districts, this means that they too had an influence over the decision-making, even though they were not directly involved in the allocation process. How personal connections were used to make this happen is described in the following subsection. The empirical logic of the paper relies on this decision-making mechanism, where the process itself was subject to potential outside influence. The interviews that I conducted with former members of the SIGTARP confirm that this mechanism was indeed plausible. They did not investigate personal connections between Congressmen or Treasury officials to bank

executives, so there is no direct evidence of misconduct, but they did conclude that Congressmen had an incentive to use their influence to favour some recipients over others.

#### **4.2.2 The role of personal connections**

The context of the crisis was important as it attests to a very specific way the policy-makers interacted with industry giants in order to resolve the crisis. Lobbying and political spending of the finance industry at that time were examples of both intra-industry and inter-industry competition for political favours. During the height of the crisis in September and October 2008, while the TARP was being drafted and conceived, all chief executives of the eight biggest financial institutions in the country held regular formal and informal meetings with New York Fed Chairman Timothy Geithner and Treasury Secretary Henry Paulson (Becker and Morgenson, 2009; Stewart, 2009). The very connections the banks had with these individuals (Paulson was a CEO of Goldman Sachs before becoming Treasury Secretary, while the New York Fed Chairman is selected by a board containing chief executives of all the biggest banks in the city) suggest a cartelisation model of industry-wide influence over the policymaking process. The personal relationships forged between the industry giants and its key regulators exemplify an elite network that exists to promote partial interests of its members.

There is nothing inherently illegal in using connections, lobbying, or campaign

spending to get a better deal from the government. Even criticism from an ethical perspective is doubtful in a time when systemic risk threatened to undermine the entire economy. Acemoglu et al (2016) examined the impact of connections of firms to Treasury Secretary Timothy Geithner during the crisis. They found that firms which were connected to Geithner achieved abnormal market returns immediately after his nomination by the President-elect. They called this effect the “connections in a crisis hypothesis” — during crisis times it is natural for decision-makers to seek advice from within their own social networks<sup>3</sup>. If Congressmen or the Treasury Secretary himself tapped their own networks in an attempt to get better information or simply get an idea on how to approach a given issue, then it should not be surprising to see that well-connected firms received a better bailout deal. Mere persistent interactions with the decision-makers are enough to establish the effect. Just like the concept of cultural capture in financial regulation<sup>4</sup> (Kwak, 2014), the decision-makers at the Treasury and Congress were influenced by their own connections.

The joint efforts of Paulson, Geithner, and the big bank CEOs to find a solution to the liquidity and solvency crisis created a lifeline for many other financial institutions down the line who then utilized their own political connections to get a better part of the bargain. The political influence of the eight biggest banks created

---

<sup>3</sup>Similarly, Querubin and Snyder (2013) find that rent-seeking among politicians tends to be higher during episodes of great political or economic turmoil. This happens because in those times government expenditures increase rapidly, while media oversight is less effective.

<sup>4</sup>When regulators, under persistent influence of lobbyists, start thinking like the industry they regulate.

a positive externality for the free-riding 900 other banks and financial institutions that received bailout funds. This is where the inter-industry competition for the allocation of bailouts started. The stage was set for massive campaign spending and lobbying by an unprecedented number of financial institutions (see Figure 4.2) to ensure that each gets their own piece of the pie. The personal connections of the biggest banks' CEOs with the top regulators (Treasury Secretary and NY Fed Chairman) set the deal in motion, after which lower-level personal networks were activated by individual banks to gain a short-run government stimulus. Individual firms which cultivated their own relationships with politicians got a better deal than firms which failed to do so.

## **4.3 Data and variables**

### **4.3.1 Bailout data and covariates**

The dataset was assembled using three separate sources. I first gathered the data on total bailouts received from the US Department of Treasury (2013) via the Office of Financial Stability and its information on the total distribution of the TARP funds from November 2008 until December 2009. These include the Capital Purchase Program worth in total \$200bn, as well as all other spending allocated to banks (commercial and investment), mortgage lenders, credit unions, insurance companies, financial services companies, and the rest of the finance industry. The total bill was \$623bn, however I exclude the Government Sponsored Enterprises

(Fanny Mae and Freddy Mac) and the automobile companies (GM and Chrysler). This brings the sample down to \$372bn allocated in the stipulated time period for 962 financial institutions.

This initial dataset was then merged with the data on bank balance sheets from the Federal Deposit Insurance Corporation (FDIC, 2016) Quarterly Uniform Bank Performance Reports (Call reports). The merge was performed by name and by state, however I manually cross-referenced most of the entries due to different names of banks in the two datasets and given that the former dataset from the Treasury did not have bank addresses or postcodes. The finally assembled dataset enabled me to extract for each bank its total assets, total liabilities, total deposits, net loans and leases, Tier 1 capital, net risk-weighted assets, total equity, cash holdings, net income, earnings assets, allowance for loans and lease losses (ALLL), total debt, employee salaries and benefits, and total number of employees. From this I managed to construct the main dependent variables: the natural logarithm of total bailout funds (*Log bailouts*), and the *ratio of bailout funds to total assets* for each firm, in addition to a number of financial indicators useful in evaluating bank business performance and more importantly their exposure to risk.

The following performance indicators are included in the analysis: *Leverage* (total debt divided by total assets), *Deposit-to-asset ratio* (deposits divided by assets), *Liquidity* (cash holdings divided by assets), *Earnings ratio* (earnings assets divided by total assets), *ROA* (net income divided by total assets), and *Tobin's*

$Q$  (book value of assets minus book value of equity plus market value of equity divided by book value of assets). In order to quantify the relative risk of a financial institution I follow the approach used by Duchin and Sosyura (2012) who approximate the CAMELS rating system to evaluate individual risk of a bank (because the official CAMELS ratings records are not publicly available). The CAMELS rating system has six categories which form one common indicator of risk. These include: Capital adequacy, Asset quality, Management quality, Earnings, Liquidity, and Sensitivity to market risk. I calculated *Capital adequacy* as the ratio of Tier 1 capital to net risk-weighted assets, *Asset quality* as the ratio of net losses to total loans and leases, Earnings as *ROA*, defined above, *Liquidity* as cash over deposits, and *Sensitivity to market risk* as the sum of short term assets and liabilities divided by earnings assets. I could not get any data on management quality so I omit this variable, but I still manage to construct a suitable index of risk for each institution (I cross-checked my grades with Duchin and Sosyura's (2012) published grades and the numbers are almost identical). The index is calculated as a weighted average of the five indicators multiplied by 100, and designed so that the higher the value of the index, the lower the exposure to risk of a particular financial institution.

For each absolute value variable (such as total assets or total deposit) I take its natural logarithm. In the final dataset I therefore include *Log total assets*, *Log total deposits*, *Log risk-weighted assets*, *Log ALLL* (allowance for loans and lease losses),

and *Log salary*. I only take *Number of employees* at face value. I use the Log ALLL variable as an additional robustness indicator for credit risk, given that this indicator measures how much reserves a bank keeps for bad debts. Finally I also included the *average foreclosure rate* (% of loans to start foreclosure in the past 18 months) and the *average subprime loan rate* (subprime loans to total loans) for the state where the financial institution was headquartered. This data was taken from the US Department of Housing and Urban Development (HUD, 2010) and their Neighbourhood Stabilization Program data.

The FDIC data however only covered bank balance sheets, which means that the initial sample still did not have data on mortgage companies, insurance companies, investment funds, or other financial service companies, all of which received considerable sums as part of the TARP. In order to acquire the data for other companies that participated in the bailout, I used the official reports from the Securities and Exchange Commission (SEC, 2017) for each institution that was a publicly listed company. This was done manually for 52 institutions. The final sample, after the merging and manually cross-referencing included a total of 685 financial institutions that received bailout funds in the total sum of \$329bn, or 88% of the original financial institutions sample (excluding GSEs and automobile companies).

I created two main datasets for three different estimation strategies. The first was created by merging bank balance sheet data from the FDIC to the bailout data

from the Treasury, as well as the lobbying, campaign contribution, and political connection variables for each bank. This sample is used to perform OLS and matching, and is later used to derive a new treatment variable and a corresponding instrument for 2SLS estimation. The second dataset takes a subset of firms that had political connections (264 of them) and connects them to all the politicians they donated money to in order to perform the regression discontinuity strategy. Given that many firms exhibited connections to more than a single politician, this increased the sample size to 2629 observations. I manually cross-referenced all the politicians who had connections to an individual firm to avoid any mistakes. After this I allocated each politician to his or her 2008 election results (whether they won or lost, and by what margin), their districts, the rate of foreclosures in each district, and other personal characteristics (their party, their age, etc.). This data was obtained from the Federal Election Commission (FEC) for the 2008 House and Senate elections, while the foreclosure data was taken from the HUD. The full summary statistics for all variables used are available in the Appendix.

### 4.3.2 Measuring lobbying and political connections

The explanatory variables were designed using two separate sources. The lobbying and campaign spending data was downloaded from the online database at the Center for Responsive Politics (CRP, 2017)<sup>5</sup>. It includes total *Lobbying expenditures* and *Campaign spending* for each firm in the finance industry for a given year, as

---

<sup>5</sup>All this data is publicly available at [www.opensecrets.org](http://www.opensecrets.org).

well as a directory of all politically connected lobbyists. For the political influence variable I used the BoardEx database, accessed via the Wharton Research Data Service (WRDS, 2017)<sup>6</sup>. The two datasets were used to form a unique political connections (*PolCon*) variable attempting to capture several levels of potential political influence: lobbying just before the bailout, campaign spending in the 2008 cycle, and personal connections of top firm executives to the decision-makers. I describe the intuition behind the inclusion of each sub-variable.

The BoardEx database allowed me to access career histories and trajectories of over one million senior corporate executives and examine which of them had direct connections to either the Treasury, Congress, or the regulators (the FED and the SEC in this case). A bank executive — which included any upper management director position, CEOs, CFOs, COOs, Chairmen, Presidents, and members of the Board — is coded to be connected if he or she held a position at the Treasury, a relevant banking regulator (the Fed or the SEC), or had worked in Congress, either as a formerly elected politician or their staffers, at least five years prior to the crisis. The justification of using such a narrow time limit is that anyone holding a senior (top-level management) position at a financial institution is highly unlikely to have been a low-level civil servant recently before his or her current job. The senior bank executives which had experience in government were in each case previously employed at decision-making public sector positions before arriving at their current executive jobs. During their time in public office they

---

<sup>6</sup>Also publicly available at <https://wrds-web.wharton.upenn.edu/wrds/>.

were highly likely to have engaged in personal relationships with people who were decision-makers during the bailout process. This sub-variable therefore captures if a financial institution had anyone in upper management who could have fostered close relationships with decision-makers in Congress or the Treasury.

In addition to direct political connections I also look at bank lobbying and political campaign expenditures. Unlike some of the literature that focused on the total amount of lobbying in the years before the crisis — Igan et al (2011) use lobbying expenditures from 2000 to 2006, while Blau et al (2013) use lobbying expenditures from 2004 to 2008 — I look at the immediate response of the lenders to the crisis shock. This way I eliminate any prior bias of lobbying for various legislations that may have increased risk-taking for a particular bank (since this effect would then spread out across all firms in the industry as well), and look only at the immediate response of similar firms to the shocks in 2008 and 2009, i.e. throughout the period of the bailout allocation.

Using the CRP (2017) databases on lobbying and political contributions I design the following two sub-variables: the 2008 cycle *Campaign spending* and *Abnormal lobbying*. The *Campaign spending* variable is self-explanatory: it looks at firms that made political contributions in the 2008 election cycle. The *Abnormal lobbying* variable however requires further explanation. The logic is based on Jayachandran's (2006) calculation of abnormal market returns where *Abnormal lobbying* represents the difference between the *expected mean* of lobbying expen-

ditures (the counterfactual level of lobbying — shown as the red line in Figure 4.1) and the *mean of actual lobbying* expenditures in 2008 and 2009. The expected (counterfactual) level of lobbying for 2008 and 2009 was predicted using simple exponential smoothing<sup>7</sup>. The formula used to calculate *Abnormal lobbying* is then:

$$AbnL_{it} = L_{it} - E(L_{it}) \quad (4.1)$$

The main explanatory variable *PolCon* is coded as an indicator variable, where *PolCon* = 1 if a firm (a) engaged in abnormal lobbying during 2008 and 2009; (b) funded any political campaigns during the 2008 election cycle; or (c) had any senior executives that used to work in the Treasury, in Congress, or for a finance industry regulator in the five years prior to the crisis. In total there are 264 such firms in the sample. There is a potential conceptual problem here as these are three different variables (lobbying and campaign spending are not equivalent) which are pooled into one binary indicator. However I ran all the regressions using each individual variable, both as an indicator and as relative and absolute values of lobbying and spending, and they all suggest the same conclusion, so the decision to pool them together is out of mere simplicity. The estimations of heterogenous treatment effects are available upon request from the author.

Table 4.1 provides descriptive statistics of the distribution of the share of

---

<sup>7</sup>The level of expected lobbying in 2008 was estimated using the lobbying series from 1998 to 2007 and applying the following exponential smoothing formula in the final stage:  $E(L_{2008}) = \mu L_{2007} + (1-\mu)L_{2007}^P$ , where  $\mu = 0.2$ ,  $L_{2007}^P$  is the predicted time series value of lobbying in 2007, and  $L_{2007}$  is the actual value of lobbying in 2007. The same calculation was done for 2009.

bailouts in total assets as well as the log of total bailouts<sup>8</sup>, both with respect to the indicator variable of political connections *PolCon*. It also shows a t-test for the comparison in means between connected and unconnected firms for both dependent variables. There is a clear difference in the distribution of bailouts for firms that were connected and firms that were not. A simple mean comparison shows that politically connected firms had a 2.4 percentage point higher share of bailouts in total assets than non-connected firms, which constitutes a large effect as can be inferred from the first row of Table 4.1. However, this still tells us nothing about any other firm-specific characteristics that could have driven the allocation of bailouts. In order to make these comparisons more realistic and in order to draw some inference from them I perform several empirical strategies described in the following section.

#### 4.4 Empirical strategy and results

The goal of the paper is to uncover the extent to which political connections mattered in the allocation of bailout funds among the recipients of TARP funds. The focus is only on TARP recipients, i.e. only banks and financial institutions that needed and applied for a bailout. The counterfactual outcome is how much a TARP recipient would have received had it not been politically connected, while the question of interest is whether being politically connected helped a TARP

---

<sup>8</sup>The histograms for both outcome variables are available in the Appendix.

Table 4.1: Descriptive statistics of the main dependent variables with respect to political connections

	Mean	Min	25th perc	Median	75th perc	Max	N
Entire sample							
Bailouts to assets	0.04	0	0.02	0.024	0.028	0.96	685
Log bailouts	16.4	7.4	8.47	16.2	17.3	24.9	679
Politically connected = 1							
Bailouts to assets	0.055	0.0006	0.02	0.025	0.03	0.96	264
Log bailouts	17.9	11.3	16.67	17.54	18.8	24.9	264
Politically connected = 0							
Bailouts to assets	0.031	0	0.19	0.024	0.027	0.9	421
Log bailouts	15.46	7.4	14.9	15.6	16.3	18.7	415
T-test connected vs unconnected (Bailouts to assets)				T-test connected vs unconnected (Log Bailouts)			
0.0239*** (0.0063)				2.435*** (0.138)			

recipient secure a better deal for themselves. However this is not as straightforward as it seems, primarily because the allocation of the bailout was not random, nor did firms randomly assign into those who are connected and those who do not. In order to establish the existence of any potential causal relationship between political connections and bailouts and thus address the endogeneity concerns I apply several methodological approaches. Before turning to as-if randomization of the sample that will address the two biggest concerns, self-selection and the omitted variable bias, I first run the conventional selection on observables approaches: OLS regression followed by a matching strategy using the same dependent and independent variables but with a covariate-adjusted sample to control for any imbalance in key firm-level confounders.

#### 4.4.1 Selection on observables: OLS and matching

The baseline OLS regressions estimate whether the allocation of bailouts among TARP recipients was to some extent affected by political connections. The following equations are estimated, where in 4.2 the outcome variable is the *Share of bailouts in total assets*, while in 4.3 the outcome variable is *Log bailouts*. In both cases the main explanatory variable is the indicator of political connections — *PolCon*(as defined in the data section).

$$\frac{B_i}{A_i} = \delta_0 + \tau_1 PolCon_i + \gamma \mathbf{C}_i + \varepsilon_i \quad (4.2)$$

$$\log B_i = \delta_0 + \tau_2 PolCon_i + \gamma \mathbf{C}_i + \varepsilon_i \quad (4.3)$$

for firm  $i$  for all bailouts received during 2008 and 2009. The vector of controls  $\gamma \mathbf{C}_i$  contains all the variables mentioned in the data section that measure an individual financial institution's market performance (leverage, deposit-to-asset ratio, earnings ratio, ROA, Tobin's Q), exposure to risk (the Camels rating), its size (total assets, deposits, number of employees, employee salary), and the foreclosure and subprime loan rates in their states.

The next step is to utilize a matching method where firms are matched into treatment and control groups based on their pre-treatment observable covariates. In order to confirm the reasoning behind Table 4.1 that greater political connec-

tions imply more bailouts, I balance the dataset according to the available covariates (observed before the treatment) in order to make the connected (treatment) and non-connected firms (control) as similar as possible and therefore comparable. Any difference in outcomes between the two groups should be a result of political connections only, and no other covariate like size or exposure to risk. Matching as a method is not robust to the omitted variable bias problem because it depends on finding a balance only within the set of observable covariates, but it does help in establishing counterfactual inference when we are unable to exploit any natural randomization or generate an experiment. It is thus conceived as a useful first step to uncovering the relationship of interest.

The treatment, as in the case of OLS regressions, is a binary indicator of whether a firm was politically connected in the observed period (Political connection,  $PolCon_i$ ). The outcome is the share of bailouts in total assets, where  $B_i(0)$  and  $B_i(1)$  represent the potential outcomes — how much bailout funds would have a firm received conditional on it being politically connected. Given that the counterfactual potential outcomes are obviously unobservable, the matching strategy allows me to assume unconfoundedness with respect to all of my observed covariates which makes it possible to estimate the treatment effect  $\tau_{ATE} = E[B_i(1) - B_i(0) | PolCon_i = 1]$ . The dataset I have assembled contains a sufficient number of useful covariates on bank performance and their exposure to risk which makes it possible to condition for a lot of potential differences between con-

nected and unconnected firms. For example, larger firms (by size of assets) with more employees and with greater exposure to risk are by definition more likely to get a larger bailout (this can be seen in Figure 4.7 in the Appendix). Without balancing the treatment and control groups for these pre-existing characteristics, which are very likely to be correlated to bailout funds received, it is difficult to establish which factor was the main driving effect behind higher bailout allocations. I use all the control variables from the baseline regressions and seek to balance the two groups using several different matching algorithms, the details of which are described in the Appendix under section Matching strategy.

### **Matching and OLS results**

Table 4.2 shows the results for the regressions performed after matching the sample using entropy balancing, propensity score and kernel matching, and also reporting the OLS estimates in the final column. The upper row shows the results for the average treatment effect of political connections on the share of bailouts in total assets. The effect is similar across all estimates and suggests a large impact of political connections on the relative size of bailouts. On average a politically connected firm receives around 5 percentage point greater share of bailouts to total assets compared to a politically unconnected firm. The entropy balancing estimated coefficient is slightly smaller but it still finds that a switch from an unconnected to a connected firm increases the share of bailouts in total assets by

3.4 percentage points. This effect is almost twice the size of the t-test estimate from Table 4.1, and is considerably large, particularly when taking into account that the mean value of bailouts to total assets is 4%, with a standard deviation of 8% (see Appendix Table 4.9). In other words, a firm that is politically connected roughly doubles the share of bailouts in total assets (up by between 86 and 127%).

Table 4.2: Matching results

Matching algorithm:	Matching estimates			
	Entropy balancing	Propensity score	Kernel	OLS
Effect of political connections on share of bailouts in total assets	0.0344*** (0.007)	0.0511*** (0.016)	0.048*** (0.012)	0.047*** (0.012)
Size of effect	86%	127%	120%	117.5%
Controls	Yes	Yes	Yes	Yes
N	671	208	671	671
R2	0.2534	0.6194	0.5259	0.2076
Effect of political connections on log bailouts	2.175*** (0.319)	1.276*** (0.438)	0.917*** (0.207)	0.893*** (0.176)
Size of effect	780%	258%	150%	144%
Controls	Yes	Yes	Yes	Yes
N	665	208	665	665
R2	0.4543	0.2767	0.2116	0.6086

Matching and OLS estimates for the average treatment effect of political connections on bailouts. The independent variable in all cases is the indicator of political connections (PolCon), while the dependent variable is bailouts in share of assets for the upper row, and log bailouts for the lower row. Control variables include the following: log total assets, CAMELS rating, ROA, Tobin's Q, earnings to assets ratio, ALLL, leverage, deposits to assets ratio, log salaries, number of employees, foreclosure rate and subprime mortgage rate. Standard errors shown in parentheses and robust to heteroskedasticity. \*\*\* denotes significance at 1%.

The *Log bailouts* estimates reported in the lower part of the table suggest a very large effect as well. Take the entropy balancing estimated coefficient. A

correct interpretation would be that a switch from an unconnected to a connected company yields a 780% increase in the allocation of bailout funds (calculated as  $100 \cdot e^{2.175} - 1$ ), while a switch from a connected to a non connected firm decreases the allocation of bailouts by 88% (calculated as  $100 \cdot e^{-2.175} - 1$ ). The entropy balancing coefficient yields by far the largest effect size compared to all other estimates used. It suggests that being politically connected resulted in as much as 8 times higher bailout funds received than not being politically connected. Given that entropy balancing estimates the average treatment effect for two identical groups there is no other observable factor that is driving this huge effect.

Overall, the results in Table 4.2 show that not only does there exist some correlation between political connections and the allocation of bailouts, but that this correlation is huge in magnitude, and that being politically connected and giving money to campaigns was a viable strategy in securing a better bailout deal during the allocation of TARP funds in 2008 and 2009. Furthermore the effect size is indeed quite large. This is most likely because the absolute values of bailouts allocated during the crisis were themselves considerably large, implying that it did not take a lot of money in terms of campaign donations and lobbying to secure such an enormous short-run return on investment. This finding adds to the puzzle of “why there is so little money in US politics” (Ansolabehere, de Figueriedo, and Snyder, 2003) as US firms can spend only a few million dollars in order to achieve considerable benefits either through legislation, regulation, or in this case direct

government investment (even though this particular investment was essentially a loan at very favourable rates). It pays off to be politically connected.

However, these results, although highly suggestive and precise in estimating the average treatment effect of political connections on the allocation of bailouts are still not enough to infer a causal relationship between the two. The matching method, even if performed perfectly, still implies conditioning on observable covariates, which means that there could be unobservable factors that could have impacted our results. In other words, regardless of how well we have balanced our treated and control units there could still be unobservable factors that would render these results as biased. In order to solve this I engage in as-if randomization of my sample to estimate the overall causal effect.

#### **4.4.2 Imposing randomization: regression discontinuity design**

One way to solve the endogeneity problem is to exploit some arbitrary or rule-based threshold that allows as-if random assignment of units into treatment and control around the threshold. Fortunately the majority of the bailout funds were distributed after the 2008 national elections (see Timeline in the Appendix), meaning that the discontinuity that could be exploited in this case is the 50% vote share that determines winners or losers of electoral races. Much of the literature uses the fact that very narrow races around the 50% voting threshold (someone winning by 51% to 49%) are good sources of as-good-as random variation given that in such

close races luck is very likely to impact the outcome (see e.g. Lee, Moretti and Butler, 2004; Lee, 2008; Dal Bo, Dal Bo and Snyder, 2009; Eggers and Hainmueller, 2009; Ferraz and Finan, 2011; Brollo and Nannicini, 2012; and Boas, Hidalgo, and Richardson, 2014).

As-good-as random variation of electoral outcomes within a narrow bandwidth of the 50% cut-off implies that winners and losers of such races should be similar in all unobservable characteristics (ability, appearance, successful campaign, etc.) as the electoral outcome itself was more a result of luck than their ability, skills, or any other factor that we *can* measure like campaign funding or incumbency status.

The unit of analysis necessary for this strategy will no longer be just firms, but rather firm-politicians. I attach all the *connected* congressmen to each firm as new individual observations thus increasing the overall sample size (to 2650 observations; see Data section and Appendix for summary statistics) and consequently the variation in the number of politicians winning or losing close races. Connected congressmen in this sample are only those who received campaign donations from firms in the 2008 cycle. Defining the sample this way allows me to exploit the as-if random assignment of winners and losers in close congressional races to see how a win or a loss of a *connected* congressman affected the size of bailouts to the connected firm. If a congressman barely won his seat all firms that donated money to that congressman should receive larger relative bailouts compared to

those firms that donated to a congressman that barely lost.

Obviously firms donate to multiple candidates in a given electoral cycle, which means that the dependent variable does not vary within firms raising the possibility of an error across observations. To address this I cluster the standard errors within firms. The estimated coefficient should therefore be interpreted as the effect of an additional recipient candidate on relative bailouts for a firm holding all other candidates' electoral performance constant. The outcome variables are as before: *Share of bailouts to total assets* and *Log bailouts* given to all firms connected via campaign donations to an individual politician.

Given that elections were held on the 4th of November 2008 and that the vast majority of bailout allocations were awarded after the election (see Timeline in the Appendix), this reduces the probability of post-treatment bias. The EESA was voted into law by the previous Congress, before the election, but the allocation decisions on who gets funds weren't made until after the election for over 95% of all awarded financial institutions. The exception to this were AIG which received help on several occasions, and the eight largest banks<sup>9</sup> which had already agreed initial bailout packages on 14th of October immediately after the announcement of the Capital Purchase Program (and received the funds on 28th of October). In order to directly account for this I exclude campaign donations of these eight big banks except for AIG, Bank of America, and Citigroup which all received additional funds

---

<sup>9</sup>Bank of America, Bank of New York Mellon, Citigroup, Goldman Sachs, JP Morgan, Morgan Stanley, State Street, and Wells Fargo. Citigroup received an addition \$20bn on 23rd November, while Bank of America received an additional \$20bn on 16th January.

after the election. Their outcome variables are adjusted to account for only the newly received bailout funds. This represents my *reduced* sample, whose results are presented alongside the *full* sample ones. For a deeper discussion of why these big banks should be included see more in the ‘Notes on sample selection’ section in the Appendix.

As specified the RD approach depends on exploiting an arbitrary rule to create a quasi-experimental setting. In case of two-party elections the cut-off rule is the aforementioned 50% vote share, distinguishing between electoral winners and losers. The running variable is the margin of victory ( $M_i$ ) which is defined as the difference between the winner vote share and the 50% cutoff. The following model is estimated:

$$\frac{B_i}{A_i} = \beta f(M_i) + \tau R_i + \delta R_i M_i + \mu \mathbf{C}_i + \eta_i \quad (4.4)$$

$$R_i = 1[M_i \geq 0]$$

Where  $M_i = V_i c$  is the difference between the congressman vote share and the 50% cutoff (or the distance between the first and second candidate when more than two candidates competed in a race),  $f(\cdot)$  is a smooth continuous function of the margin of victory,  $R_i$  is the indicator variable of whether or not the congressman won the election (representing a deterministic function of victory), and  $R_i M_i$  is the interaction term.  $C_i$  represents the same controls as before. The functional form

of  $f(\cdot)$  is both linear and quadratic for the parametric estimator, as I follow the suggestions of Gelman and Imbens (2018) not to use higher-order polynomials<sup>10</sup>. The same estimation equation is applied for the *Log bailouts* outcome variable. Given that the as-if random assumption rests upon observing the effect in narrow races, I use several bandwidths around the zero margin of victory threshold. The default is the  $\pm 5$  percentage point margin above and below the threshold, however I also use the  $\pm 1$  p.p. margin, the  $\pm 3$  p.p. margin, and the  $\pm 10$  p.p. margin. Finally, for an additional robustness check I use the Calonico, Cattaneo, and Titiunik (2014) bias-corrected nonparametric estimator which calculates its own optimal bandwidth (henceforth CCT).

### **RDD validity checks**

An often made criticism of RD design using close elections is that bare winners and bare losers are not necessarily statistically exchangeable. For example Caughey and Sekhon (2011) find that bare winners raise more campaign money, while Snyder et al (2015) find an electoral advantage for incumbents and candidates whose party controls the state legislature or the governorship even in very close elections. This means that the results could be systematically biased if the sample of bare winners had more incumbents or candidates coming from parties that controlled the legislature or had the governor seat. An additional concern raised by the lit-

---

<sup>10</sup>The quadratic function is similar to equation 4.4 and it looks like this:  $\frac{B_i}{A_i} = \tau R_i + \beta_1 M_i + \beta_2 M_i^2 + \delta_1 R_i M_i + \delta_2 R_i M_i^2 + \mu C_i + \eta_i$

erature is that in these cases we would observe non-random sorting around the threshold which questions the key RD assumption that non-treatment covariates are continuous around the threshold.

In order to check for this I perform several validity tests on the balance of the sample with respect to incumbency status, state-level party control, and campaign funding.

In cases of close elections decided within the 5% margin, if a party held either the governor office or the state legislature, only in 47.5% did this help the congressmen or senator win a close election. In 52.5% of cases the candidate won a close election despite not having his or her party control the state office or legislature. There is therefore no bias arising from the candidate's party controlling the legislature or governorship. The same thing can be said of the distribution of campaign funding to close election winners and losers. Even though electoral winners got more money on average (3.2m compared to 2.7m) I ran a t-test and the difference in campaign funding between bare winners and losers was not statistically significant under any close election margin. I ran a separate t-test just for incumbents. Incumbents winning close elections within the 5% margin got less money than non-incumbents on average (2.5m compared to 3m), and the t-test did not point to any systematic difference between candidates caused by campaign funding.

Incumbents however did have a slight electoral advantage. When looking at the

5% threshold, out of all candidates who won a close election 55% were incumbents. In particular 33 incumbents out of 59 winners won a close election, while only 18 incumbents out of 62 losers lost a close election. In other words if an incumbent was running for races decided in a 5 p.p. margin in 33 out of 51 cases they won (64.7%), and in 18 out of 51 they lost (35%). This implies that there is indeed a systematic bias in favour of incumbents even in close electoral races. However things change as we get closer to the 50% cut-off. For races decided within a 3 p.p. margin the same thing happens, 65% of all candidates won, and 35% lost, however for races decided at a 1 p.p. margin, exactly half, 50% of all incumbents won, and 50% of all incumbents lost. Therefore the best as-if randomization occurs at the most narrow bandwidth we can observe. For the 1% margin we can truly say that there is no systematic bias between bare winners and bare losers, not driven by incumbency effects, state-level party control, or campaign spending.

In conclusion, I find no systematic bias in favour of candidates coming from the party that controls the state-level legislature or the governorship, or in favour of candidates with more campaign spending. I do find a small bias in favour of incumbents at the  $\pm 5\%$  and  $\pm 3\%$  electoral margin, but this bias vanishes at the most narrow  $\pm 1\%$  margin, which represents the clearest indicator of as-if randomization. Therefore the most unbiased results presented in Tables 4.3 and 4.4 are the ones reported for the 1% margin of electoral victory. I also perform the McCrary (2008) density test to show that there was no sorting around the

threshold that might have biased the results. The density graph is shown in the Appendix.

### **RDD results**

Before presenting the results of the estimation I plot the margin of victory against the share of bailouts as total assets for a 5 p.p. and a 10 p.p. margin of victory in Figures 4.3 and 4.4. Both figures show a clear discontinuity at the zero threshold making it possible to estimate the local average treatment effect of a connected politician victory on the likelihood of receiving a higher bailout. The given discontinuity suggests that in narrow races, where politicians are statistically similar to one another, victory of a connected politician entails a positive effect on receiving a higher bailout for a connected firm.

Table 4.3 presents the results for equation 4.4 for both the full sample and the reduced sample. Both panels show that there is a positive causal effect of an additional connected candidate victory on the share of bailout in total assets for connected financial firms, holding all other recipients' electoral performance constant. The results are robust to functional form, bandwidth size, choice of estimation method, and the definition of the main dependent variable, as in each case the main intuition of the paper's theory is confirmed.

For the most narrow  $\pm 1$  p.p. bandwidth, across both functional forms (linear and quadratic), the total effect of an additional connected candidate winning a

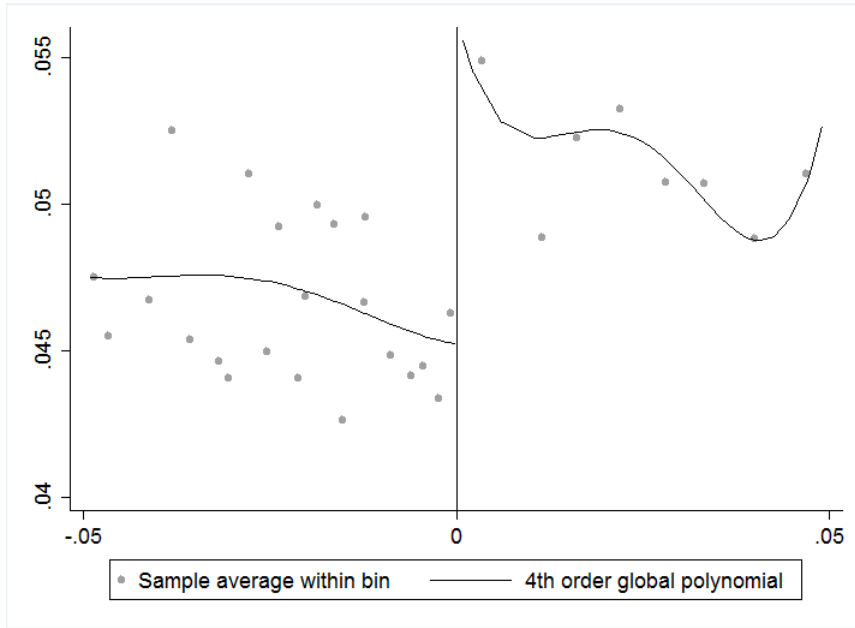


Figure 4.3: The effect of a connected political victory on the share of bailouts to total assets (y-axis) for close races decided within a  $\pm 5$  percentage point margin of victory. The fitted lines are a 4th order polynomial.

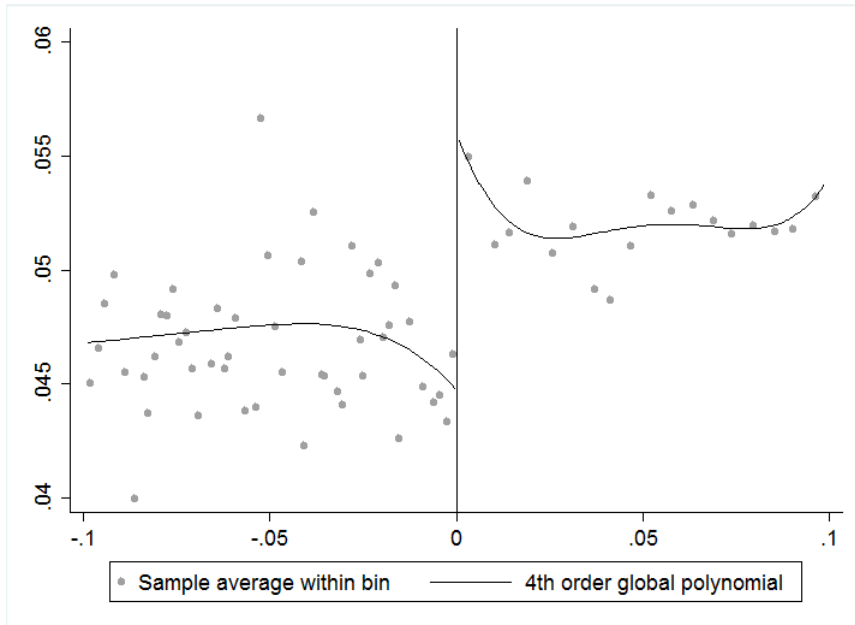


Figure 4.4: The effect of a connected political victory on the share of bailouts to total assets (y-axis) for close races decided within a  $\pm 10$  percentage point margin of victory. The fitted lines are a 4th order polynomial.

Table 4.3: Regression discontinuity design results

	Linear	Linear	Quadratic	Non-parametric
Full sample				
Effect of political connections on share of bailouts in total assets	0.0078*** (0.002)	0.0065*** (0.0012)	0.0072*** (0.0017)	0.0065*** (0.001)
Size of effect	19.5%	16.25%	18%	16.4%
Bandwidth	± 1	± 5	± 1	± 8.7
Controls	Yes	Yes	Yes	Yes
N	56	488	56	960
Effect of political connections on log bailouts	0.016 (0.029)	0.035 (0.021)	0.065*** (0.022)	0.1136** (0.045)
Size of effect	n.s.	n.s.	6.7%	12%
Bandwidth	± 1	± 5	± 1	± 10
Controls	Yes	Yes	Yes	Yes
N	56	488	56	1090
Reduced sample				
Effect of political connections on share of bailouts in total assets	0.0041 (0.0025)	0.0049*** (0.001)	0.0052** (0.002)	0.0035*** (0.001)
Size of effect	n.s.	12.25%	13%	8.75%
Bandwidth	± 1	± 5	± 1	± 9
Controls	Yes	Yes	Yes	Yes
N	34	314	34	670
Effect of political connections on log bailouts	0.044** (0.017)	0.041*** (0.013)	0.071*** (0.017)	0.101 (0.057)
Size of effect	4.5%	4.2%	7.4%	n.s.
Bandwidth	± 1	± 5	± 1	± 8
Controls	Yes	Yes	Yes	Yes
N	34	314	34	685

RD estimates for the marginal effect of victories of connected politicians in narrow races on the allocation of bailouts, either as shares in total assets or in log terms. Upper panel presents the results for the full sample, while the lower panel presents the results for the reduced sample where the recipients of funds before the election were excluded. For the non-parametric estimation the optimal bandwidths are calculated using the CCT (2014) approach for bandwidth selection. Controls are the same as in Table 4.2. Standard errors shown in parentheses and are clustered by firm. n.s. stands for non-significant, so the effect size is not calculated. \*\*\* denotes significance at 1%, \*\* at 5%.

narrow race is 18 to almost 20% higher share of bailouts to total assets for the connected firm in the full sample, and about 13% for the reduced sample. The effect on log bailouts is between 4.5% and 7.4% higher bailouts for connected firms in the reduced sample, and 6.7% higher bailouts in the full sample. At first glance these effects might seem considerably smaller than the ones previously estimated for matching and OLS, however this is because they measure different things. Table 4.3 presents candidate-level marginal effects, not firm-level averages.

Even though the identification strategy for races decided within a 1% margin most adequately satisfies the as-if randomization assumption the number of observations in this subsample suggests that we are only observing a few races across several firms (given that the unit of analysis is firm-politician). There is an inherent trade-off to be made here. Races decided within a 5% margin have a slight bias towards the incumbent candidate (described in the Appendix) however they produce a better sample with more races and more firms. Despite not being an ideal strategy it still provides a somewhat plausible identification for the total effect of connections on bailouts. In terms of which effect more accurately describes the relationship of interest, the trade-off is between a very robust identification strategy with a smaller sample, and a less robust identification strategy but with a larger sample, which seems to be a general problem in RDD inference.

When applying the non-parametric approach (using the CCT approach for optimal bandwidth selection), the effects are similar: an additional connected

politician victory entails a 16% higher share of bailouts in total assets, and 12% higher total bailouts received for the full sample, and slightly lower, 8.75% higher share of bailouts for the reduced sample. The chosen margin of victory bandwidths by the CCT approach are  $\pm 8.7$  p.p. and  $\pm 10$  p.p. for the full sample and  $\pm 9$  p.p. and  $\pm 8$  p.p. for the reduced sample. These are considerably higher than the initially chosen  $\pm 1$  and  $\pm 5$  p.p. bandwidths for the parametric approaches, so the next step is to verify the results by applying different sets of bandwidth sizes for the margin of victory. These results are presented in Table 4.4.

Table 4.4: RDD estimates for different bandwidth sizes

Bandwidth	Full sample			Reduced sample		
	Bailouts to assets	Log bailouts	N	Bailouts to assets	Log bailouts	N
$\pm 1$	0.0078*** (0.002)	0.016 (0.029)	56	0.0041 (0.002)	0.044** (0.017)	34
$\pm 3$	0.0074*** (0.001)	0.046*** (0.016)	313	0.0063*** (0.001)	0.042** (0.02)	201
$\pm 5$	0.0065*** (0.001)	0.035 (0.021)	488	0.0049*** (0.001)	0.041*** (0.013)	314
$\pm 10$	0.0051*** (0.001)	0.053*** (0.007)	1090	0.0044*** (0.001)	0.044*** (0.008)	675

Each row represents the estimates from a separate regression where only the main coefficient of interest is reported. Standard errors shown in parentheses and are clustered by firm. \*\*\* denotes significance at 1%.

The size of the effect changes with respect to different bandwidths, but it does not alter the overall conclusion<sup>11</sup>. Each row represents the main average treatment effect for both dependent variables across four different chosen bandwidths and for both samples. In the case of bailouts to total assets the size of the effect increases

<sup>11</sup>I have also used  $\pm 2$ ,  $\pm 2.5$ ,  $\pm 4$ , and  $\pm 7.5$  and they all yield the same conclusion.

in magnitude as we move closer to the zero threshold, which is expected given the necessary identification assumption of the RD approach (this is also suggested in Figures 4.3 and 4.4). The effect also increases in magnitude as we approach the zero threshold for log bailouts in the reduced sample. It is not surprising to see a highly significant effect regardless of the chosen bandwidth, but it is reassuring that the effects remain strongest for the narrow races where the as-good-as random assumption is most likely to hold despite having less observations.

Another good robustness test for RD design is suggested by Imbens and Lemieux (2008) where instead of changing bandwidths, we should change the value of the threshold, i.e. move it to the left and right of the original zero threshold to generate a set of placebo thresholds around which there should be no effect. The RDD estimates in this paper rest upon the idea of utilizing the 50% vote share as a clear cutoff rule determining a win or loss. Redefining the cutoff at 55% or 60% (or 45% and 40%) should not yield any effect on bailouts, at least not as a consequence of a narrow political race. Table 4.5 shows that this is indeed the case. In none of the four chosen placebo thresholds do I find any statistically significant effect of the margin of victory on the allocation of bailouts. This further strengthens the original findings.

Table 4.5: Placebo tests: RDD estimates across different thresholds, around the default zero threshold

Threshold	Bailouts to assets	Log bailouts
-10	-0.00005 (0.002)	-0.112 (0.063)
-5	-0.0005 (0.003)	0.032 (0.096)
<b>0</b>	<b>0.0066***</b> (0.0009)	<b>0.049***</b> (0.011)
+5	0.0028 (0.017)	0.009 (0.05)
+10	-0.001 (0.002)	-0.057 (0.07)

Estimation performed only for the full sample (results for the reduced sample yield an identical conclusion). The bandwidth in each case is the  $\hat{\Delta}5$  p.p. The results for the zero threshold are bolded. Each row represents the estimates from a separate regression where only the main coefficient of interest is reported. Standard errors shown in parentheses and are clustered by firm. \*\*\* denotes significance at 1%.

#### 4.4.3 Instrumenting for connections using winners of close elections

The close election setting allows for one more identification strategy. The firm is once again the unit of analysis, and the dependent variables are, as before, *Bailouts as share of assets* and *Log bailouts*, but the treatment is now the number of politicians connected to the firm, instead of firm board members connected to politics. This new treatment variable captures the total number of politicians an individual firm donated money to in the 2008 cycle. The initial relationship of interest looks at the effect of the size of political connections (total number of connected politicians for each firm) on relative bailouts received by the firm. Estimating this relationship with a simple OLS is very likely to produce biased

results given that larger banks donated to more politicians and also received a larger bailout package. For example, each of the 5 biggest banks (Citigroup, Bank of America, JP Morgan, Morgan Stanley, and Wells Fargo) had over 200 politicians they were connected to in the 2008 cycle.

For this reason I use an instrument (IV) which looks at the proportion of the firm's connected politicians who won in 2008 conditional on having won a close election. This variable is much more randomly distributed across banks, as it eliminates the upward bias of the biggest banks who had only about 10-13% of their connected politicians win a close election. The IV captures the extent to which a firm's donations and connections to politicians fall short of expectations on the politician winning their race, due to randomness in outcomes of close elections. According to the hypothesis of this paper a financial firm that invests into a politician expects a return on their investment in the form of a better bailout deal. However there is still uncertainty regarding the politician's electoral outcome, particularly for those in close races. A close race therefore provides an as-good-as random source of variation for the explanatory variable, the number of connected politicians for each firm. The instrument drawn from this relationship is then the fraction of connected politicians in each firm that won a close election. I follow an approach similar to Clots-Figueras (2011, 2012) who uses the fraction of women who won close elections as an IV for the share of women in local legislatures in India, and Hyytinen et al (2018) who use within-party variation in close elections

in Finland as an IV for the share of municipal employees in a local legislature.

I design the IV in the following way. First, I take the firm-politician RDD dataset and define three bandwidth sizes for close election margins, the 5%, 3%, and 1% bandwidth. I then look at which politician won an election and multiply it by the binary indicator of the narrow electoral margin,  $C_j$ , defined as:

$$C_j = \begin{cases} 1, & \text{if } v_j \leq |\Delta|, \\ 0, & \text{if } v_j > |\Delta| \end{cases} \quad (4.5)$$

Where  $v_j$  is the electoral margin of victory for politician  $j$ , and  $\Delta$  is defined as either  $\pm 1\%$ ,  $\pm 3\%$ , or  $\pm 5\%$ . At such small electoral thresholds candidates are assumed to be similar to each other in all unobservable characteristics. The close election setting emphasizes the impact of uncertainty on the outcome, meaning that either one of the candidates could have won, but a random factor such as luck played a role in tipping the seat from one to the other.

Finally, I divide the number of connected politicians for firm  $i$  who won a close election within each of the three margins against the total number of connected politicians for firm  $i$ :

$$Z_i = \frac{\sum_i (C_j W_j)}{\sum PolCon_i} \quad (4.6)$$

Where  $W_j = 1$  if  $v_j \geq 50$ , and 0 otherwise (or when the distance between the first and second candidate is  $\geq 0$  when more than two candidates competed in a race).

When defined this way the instrument fully captures the random variation of the electoral outcome. In other words  $Z_i$  is higher for firms that had more politicians which got lucky on election night and smaller for firms which had more politicians who were unlucky on election night. Luck here is defined as any outcome that might have tipped a close election one way or another (e.g. rain on election day that might have affected turnout). A firm could not foresee how many of its connected politicians would win or lose a close election, meaning that the IV is as good as exogenous over the expected outcome. This, however, does not prove the exclusion assumption, meaning that further validity checks of the identification strategy need to be done to justify the inclusion of this instrument.

#### **IV validity checks**

I perform two main validity checks, following the logic in Clots-Figueras (2011, 2012). The first examines if the instrument, the proportion of a firm's connected politicians who won a close election, is independent of any other firm-level characteristic. This is done by running OLS regressions with the instrument as the dependent variable and every firm-level characteristic in the sample as a right-hand side variable. Table 4.6 shows that none of the firm-level variables carry any effect on the proportion of a firm's connected politicians who won a close election for the 1% and 5% electoral margins. For the 3% margin there is a positive effect of asset size on the number of politicians who won a close election and a negative effect of

Tobin's Q. This suggests that within the sample looking at firms with winners under a 3% margin, larger firms and more overvalued firms had more close electoral winners. This could result in a biased estimated effect for this specific sample, however it could also be that for this specific subsample the p-values for the two variables are significant purely by chance.

Table 4.6: IV validity check: OLS regressions of instrument against the covariates

Covariate	Dependent variable: IV		
	$\pm 1\%$ margin	$\pm 3\%$ margin	$\pm 5\%$ margin
Log Assets	0.003 (0.233)	0.012** (0.018)	0.011 (0.129)
CAMELS risk rating	0.052 (0.196)	-0.022 (0.782)	-0.099 (0.455)
ROA	-0.056 (0.086)	0.004 (0.953)	0.152 (0.327)
Tobin's Q	-0.008 (0.232)	-0.044** (0.038)	-0.094 (0.067)
Earnings assets	-0.0119 (0.073)	-0.019 (0.434)	-0.0288 (0.340)
Leverage	0.0225 (0.13)	-0.011 (0.736)	-0.065 (0.284)
Deposits to assets	0.0074 (0.147)	0.014 (0.181)	0.023 (0.378)
Log salaries	-0.0005 (0.641)	-0.00006 (0.967)	0.0035 (0.113)
N of employees	$3.3x10^{-8}$ (0.259)	$1.6x10^{-7}$ (0.069)	$2.6x10^{-7}$ (0.076)
% foreclosures	-0.032 (0.152)	-0.188 (0.342)	-0.104 (0.690)
% subprime loans	0.004 (0.817)	0.064 (0.570)	0.154 (0.281)
Observations	580	580	580
R squared	0.0915	0.0596	0.0882

Notes: Table presents OLS regressions of the instrumental variable defined under 3 different close election margins against each of the covariates. p-values are reported in parenthesis. Standard errors are robust for heteroskedasticity. \*\* denotes significance at 5%.

An even better validity check is to compare firms where more politicians won than lost a close election to firms where more politicians lost than won a close election. This is carried out through a series of t-tests for each right-hand side variable across all three bandwidths and the results are presented in Table 4.7. The indicator variable for performing the t-tests puts all firms who had more politicians win than lose a close election in the treatment group, and all firms who had more politicians lose than win a close election in the control group. The expectation is that firms on both sides be similar to each other in all the given covariates, especially size and risk exposure.

The results suggest that the samples are not balanced between the treatment and control groups for the 5% and 3% margins, where the bigger and riskier firms have a larger number of politicians which won a close election (for the 3% subsample these are different variables than in the previous table, but at the 5% margin there is a clear bias in favour of bigger firms). However, this does not happen at 1% electoral margin. This is reassuring as these margins suggest very close elections where chance was more likely to play a role in the outcome. Looking into the dataset it is clear that a few large banks had more politicians winning at a 5% or 3% margin than losing, however for really close races the winners and losers across even the largest banks balanced out more evenly. This finding merely suggest that we should be careful when interpreting the estimated effect at the 5% or 3% margin as these results might still be biased due to firm size. For very close

electoral races, specifically those decided within a 1% electoral margin, this bias is reduced and our estimated effects should be more precise.

Table 4.7: IV balance checks: comparing firms where more politicians won than lost a close election to firms where more politicians lost than won a close election

Covariate		$\pm 1\%$	$\pm 3\%$	$\pm 5\%$
Log Assets	Difference in means	-0.544	-0.425	-0.656
	t-value	-1.59	-1.53	-2.86**
CAMELS risk rating	Difference in means	-0.0066	-0.03	-0.034
	t-value	-0.354	-2.04**	-2.70**
ROA	Difference in means	0.007	-0.004	-0.003
	t-value	1.12	-0.72	-0.604
Tobin's Q	Difference in means	-0.011	-0.019	-0.011
	t-value	-0.296	-0.66	-0.45
Earnings assets	Difference in means	0.153	0.061	0.171
	t-value	1.708	0.82	2.82**
Leverage	Difference in means	0.027	0.061	0.127
	t-value	0.356	1.00	2.49**
Deposits to assets	Difference in means	-0.018	0.003	0.074
	t-value	-0.263	0.055	1.55
Log salaries	Difference in means	-0.766	-1.41	-1.168
	t-value	-0.753	-1.74	-1.67
N of employees	Difference in means	-12154	-24711	-24705
	t-value	-0.922	-2.38**	-2.81**
% foreclosures	Difference in means	0.004	-0.003	-0.0008
	t-value	0.998	-0.78	-0.26
% subprime loans	Difference in means	-0.004	-0.007	-0.007
	t-value	-0.669	-1.36	-1.59

Notes: Table presents the results of t-tests performed for each covariate. The indicator variables for performing the t-tests puts all firms who had more politicians win than lose a close election in the treatment group, and all firms who had more politicians lose than win a close election in the control group. Three indicators are defined, each for one narrow margin of victory.

#### IV estimation and results

The performed validity checks present me with enough confidence to run the two-staged least squared (2SLS) estimation procedure using the instrument at hand. The reduced form equation is estimating the local average treatment effect (LATE) of the relative number of a firm's connected politicians who won a close election on the relative size of bailout funds it received.

$$\log B_i = \alpha_0 + \tau Z_i + \gamma \mathbf{C}_i + \epsilon_i \quad (4.7)$$

The dependent variables are, as before, *Log bailouts* and the *Share of bailouts in total assets*,  $\frac{B_i}{A_i}$  (not shown in Eq 4.7). I run the 2SLS estimation for each of the two main dependent variables and present both sets of results in Table 4.8.

The first stage for the sum of all connected politicians for firm  $i$  is:

$$\sum PolCon_i = \alpha_0 + \beta Z_i + \gamma \mathbf{C}_i + \epsilon_i \quad (4.8)$$

Table 4.8 reports the IV estimates for the average treatment effect of the number of politicians connected to the firm, instrumented by the proportion of the firm's connected politicians who won a close election, on relative bailouts the firm received. I report the results of all three 2SLS estimations each corresponding to a different margin of close electoral victory. I also report the OLS estimates of the basic relationship of interest in the final column, although these estimates are

Table 4.8: Firm-level instrumental variable results

Bandwidth	IV estimates			OLS
	$\pm 1$	$\pm 3$	$\pm 5$	
Effect of connected politicians on share of bailouts in total assets	0.005 (0.004)	0.003 (0.002)	0.004 (0.0026)	0.0006** (0.0002)
Size of effect	n.s.	n.s.	n.s.	1.5%
R2	0.0877	0.0764	0.108	0.2182
Effect of connected politicians on log bailouts	0.068** (0.034)	0.167** (0.082)	0.238** (0.112)	0.0195*** (0.006)
Size of effect	7.0%	18.2%	26.8%	1.9%
R2	0.2884	0.1734	0.1203	0.3403
First stage	418.2*** (72.57)	18.35 (10.7)	12.61 (7.7)	-
Controls	Yes	Yes	Yes	Yes
N	548	548	548	548

Reported are IV estimates for the average treatment effect of the number of politicians connected to the firm, instrumented by the proportion of the firm's connected politicians who won in 2008, conditional on having a close election. The bandwidth size indicates how close the election was; with a  $\pm 5\%$  margin, a  $\pm 3\%$  margin, or a  $\pm 1\%$  margin. The treatment variable is the number of connected politician for each firm, and the instrument is the percentage of connected politicians for each firm that won a closely contested election. Control variables are the same as in Table 4.2. Standard errors shown in parentheses and robust to heteroskedasticity. \*\*\* denotes significance at 1%, \*\* at 5%.

most likely biased given that larger firms had more politicians connected to them *and* received larger bailouts.

The effects from 2SLS estimation are only statistically significant when *Log bailouts* are the dependent variable, across all three electoral margins. They are no longer significant for *Bailouts to total assets*. The size of the effect is stronger the wider the electoral margin for politicians who won.

However, an interesting finding that sheds more light on the plausibility of the chosen instrument is that the first stage is not statistically significant for races won at a 5% and 3% margin. If there is no first stage effect the instrument does not satisfy one of the several key assumptions for IV estimation; that there should be a link from the instrument to the dependent variable. However the first stage assumption is satisfied only for the races won by a very small 1% margin. This is reassuring regarding the logic of the identification strategy in which only the very close races are those that are truly random, i.e. those where luck plays a crucial role. In other words the only viable results that can give some indication of a causal effect in Table 4.8 are the ones reported in column (1) for the very narrow races where the electoral outcomes were indeed as good as randomly assigned.

Furthermore, going back to Tables 4.6 and 4.7 it was concluded that estimation under the 5% and 3% margins produces an unbalanced sample where larger and riskier firms are more likely to have more connected politicians who won close elections at those margins. That means that the positive results for the 5% and

3% margins reported in Table 4.8 are most likely biased due to firm size. This however is not a problem when looking at the sample of winners at the 1% margin. The results of the IV estimation are therefore, unsurprisingly, the most accurate for the most narrow electoral winners (just as it was the case in the RD estimation), confirming the viability of the identification strategy. The size of this effect, the only one that should be of interest, is 7%, meaning that a bank that had one additional connected politician who won a close race at a 1% margin, got on average a 7% higher bailout package, holding other things equal.

#### **4.4.4 Discussion of results and different estimated effects**

The several different methods applied in this paper all suggest the same conclusion: among the recipients of TARP funds, political connections mattered as they increased the relative bailout package for the connected firms. However each method estimated a different relationship of interest which is why the effect size differs across the estimation methods. The OLS and matching estimators used firms as units of analysis and compared bailout packages between connected and unconnected firms. The explanatory variable is the binary indicator of political connections for each firm, which means that OLS and matching estimate the effect of a TARP recipient being politically connected on the size of its received bailout funds.

The RDD analysis used firm-politician as a unit of analysis and only focused

on the sub-sample of connected firms to generate the firm-politician unit. The running variable is the margin of victory of a connected politician who won a close election, which means that the interpretation of the effect is whether a close electoral victory of a connected politician increased the relative bailout allocation for that politician's connected firm. To be more precise it is the effect of an additional recipient candidate win holding all other recipient candidates' electoral performance constant.

Finally, the IV analysis again focused on firms as a unit of analysis, but its main explanatory variable is now the number of politicians connected to a firm (instead of board members connected to politics). The instrument is the proportion of these connected politicians who won a close election in 2008. The estimated effect is the impact of having one additional connected politician who won a close election on the relative size of bailout funds received.

In all three cases the dependent variables are the same, defined by equations 4.2 and 4.3, the sample selection is focused only on TARP recipients, excluding any firm that did not receive these funds, but the changes arise from units of analysis and the choices of the main explanatory variables. It would therefore be useful to present the main effects once more in Figures 4.5 and 4.6 so as to more easily differentiate between them.

The first obvious implication is a huge difference in effect size with the selection on observables approaches and the RDD and IV strategies (OLS and matching

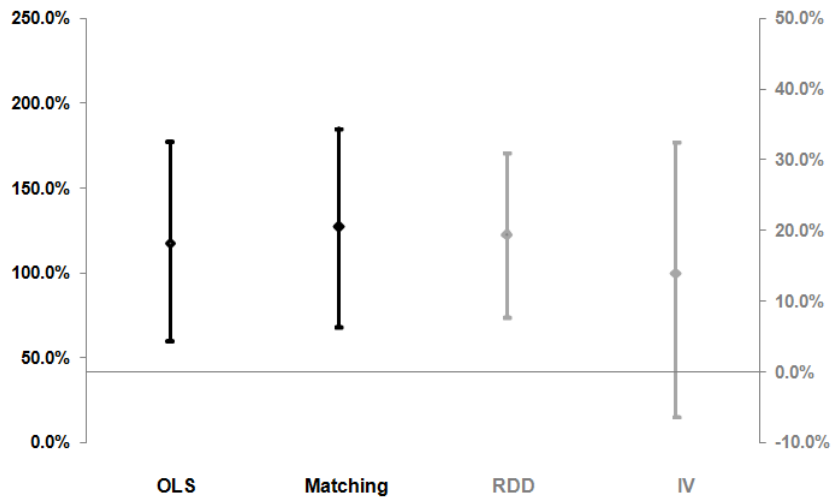


Figure 4.5: Estimate effects for different identification strategies for **Bailouts to total assets** as the dependent variable. OLS and matching effects shown on left vertical axis, RDD and IV effects shown on right vertical axis.

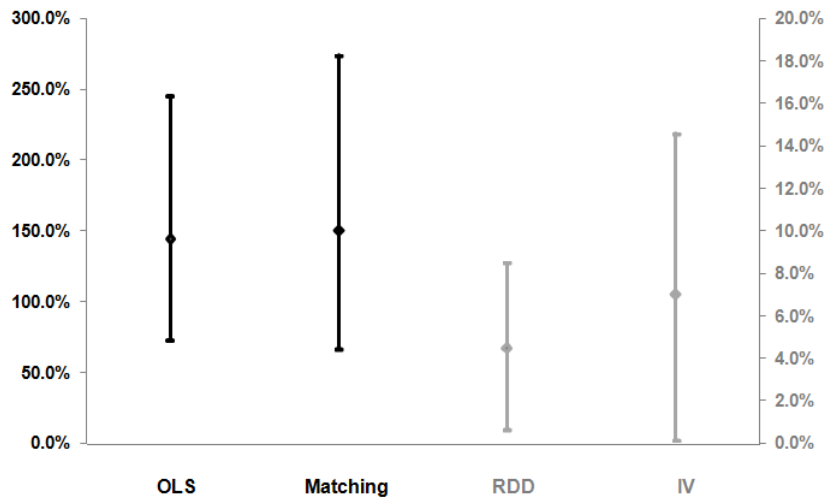


Figure 4.6: Estimate effects for different identification strategies for **Log bailouts** as the dependent variable. OLS and matching effects shown on left vertical axis, RDD and IV effects shown on right vertical axis.

effects shown on left axis, IV and RDD on right axis). This should not be surprising given that OLS and matching estimate an overall firm-level effect, while RDD and IV estimate the marginal effect of an additional connected politician. To make them remotely comparable one should multiply the individual marginal effect of a connected politician to the total number of connected politicians for each firm. For example in the IV sample the standard deviation of connected politicians who won a close election is 6.8. Moving up by one standard deviation, i.e. donating to 7 more politicians who would win a close election, would increase the firm's bailout funds by 47.6%, which is a much larger effect. It is easy to see how this effect grows stronger for firms which had more connected politicians, meaning that those at the upper extremes of the distribution will yield the most benefits from their political connections. The same is true for the RDD estimation. The OLS and Matching can only give us results for the mean values of the samples and are hence much less informative.

Altogether these results are highly suggestive that politics played an important role in the TARP allocation process. Individual congressmen obviously did not have so much discretionary power during the process, so it would be wrong to conclude that the entire allocation of bailout funds was skewed based on individual political connections, or that there was a deliberate mechanism that favoured some companies over others. However, in light of the evidence presented thus far, the inference of political connections on the allocation of bailouts cannot be ignored.

This does not suggest that the allocation included any illicit activities, but simply that the system worked in favour of those who were well-connected. Being a member of a congressman's social network obviously benefited the company.

## 4.5 Conclusion

This paper has shown, using different methodological approaches, that politically-connected TARP recipients secured a better bailout deal during the 2008-2009 US financial crisis than non-connected recipients. Using several different identification strategies it has provided evidence of a causal effect of political connections on the distribution of bailouts. The size of the effect varies with different strategies, but in each case it is strong and highly suggestive of the general conclusion that it pays off to be politically connected.

It would be wrong, however, to conclude that there was any deliberate mechanism connected politicians used to favour some firms over others. Individual congressmen or even high-ranked Treasury officials did not have enough discretionary decision-making power to choose which banks got better bailout deals, but being a member of their social network obviously did make a difference. The reason for this is most likely the very context of the crisis in which the decision-making process needed to be quick and effective to prevent further panic. It has been shown that in such stressful times decision-makers often look to their own social network for answers and guidance. However, such networks usually comprise of

the very same people they have to regulate or bail out. It is easy to see how their decisions might have been biased, even inadvertently, in favour of the members of their own personal networks.

This paper tried to show that the usual explanations derived from the economic theory of regulation are not sufficient to explain how exactly firms benefit when an event of this magnitude tends to occur. Financial crises do not happen very often, but stressful situations where quick decisions are required from the policy-makers are not that rare. Figuring out how policy-makers tend to make their decisions in such contexts and what are its consequential outcomes can be a good motivation for further research.

## Appendix

### Note on sample selection

Initial regression estimations include even the big banks that were “forced” by the government to participate in the first wave of the capital purchase program (CPP) on October 28th 2018<sup>12</sup>. The argument for their exclusion seems to suggest that they would have received the money anyway given their too-big-to-fail status (Duchin and Sosyura, 2012) and the fact that they could have been requested by the regulators to participate to send a signal to others (Solomon and Enrich, 2008). However there is strong reason to believe that it was the political power of these big banks in particular that triggered such a quick response from the policymakers. According to the interviews I held from September 2017 to October 2018 with various members of the SIGTARP, the TARP oversight committee, and former employees of the Treasury, they have all confirmed that the most significant influence over the TARP allocation process at the time came from the biggest and systemically most important institutions. They were involved in the process as their executives developed close relationships with policymakers and congressmen for years. In fact, Becker and Morgenson (2009) in the New York Times and Stewart (2009) in the New Yorker report a string of events, meetings, and phone calls that took place during the peaks of the panic in September and October

---

<sup>12</sup>This included the following eight big banks: Bank of America, JP Morgan, Citigroup, Goldman Sachs, Morgan Stanley, Wells Fargo, Bank of New York Mellon, and State Street.

of 2008, all of which suggested deep connections of the nine biggest banks' CEOs with the New York Fed Chairman Timothy Geithner and Treasury Secretary Henry Paulson<sup>13</sup>. Their efforts were focused on figuring out the best response strategy to the ongoing crisis by drafting the TARP deal, a process that heavily involved the biggest banks' top officials. The scope of the crisis necessitated this; however it would be wrong to exclude them given their crucial impact on the very bailout deal. The fact that they were "forced" to participate was by (their own) design. However in the subsequent RDD estimation I do exclude these big banks but for different reasons. The results and the corresponding intuition are unaffected.

### **Matching strategy**

I use three different matching algorithms: the default propensity score matching, kernel matching with a tricube kernel type, and a more superior matching method called entropy balancing (Hainmueller, 2012). I have also tried various versions of the nearest neighbour matching algorithm and the radius calliper matching algorithm, all of which balanced the sample similarly to the propensity score and kernel matching. Figure 4.7 reports how successful each of the three algorithms was in matching the covariates by showing the standardized biases. A standardized bias represents a t-test on the difference in mean values of each covariate in both the treatment and control units. A standardized bias close to zero implies a well-

---

<sup>13</sup>The two of them in particular were instrumental in landing a \$12.9bn deal for Goldman Sachs during the massive bailout of AIG (Business Insider, 2009) and were heavily criticised as being Wall Street insiders looking to strike a better deal for the banks. As regulators they were considered to be too close to those they regulated (Becker and Morgenson, 2009).

balanced sample. Visually (Figure 4.7) this is represented by the proximity of each dot (represented by different symbols for different methods) to the horizontal line. By examining the pre-matched means of every covariate (denoted by black squares) it is easy to see that the pre-treatment distributions of covariates could have biased the earlier results. As assumed, larger firms (by size of assets, employee salary, number of employees, etc.) were more likely to get bailouts, but interestingly firms with greater exposure to risk were less likely to get bailout funds (a higher Camels rating implies lower exposure to risk). Also firms with higher leverage ratios and greater shares of deposits to total assets were less likely to get bailout funds. This implies that without balancing the baseline OLS regressions could overestimate the effect of political connections given that size and risk played an important role in determining the allocation of bailouts.

Out of all the used matching algorithms entropy balancing was by far the most accurate as it completely eliminated the standardized bias between the treatment and control units across all the given covariates. Entropy balancing is a multivariate reweighting algorithm designed by Hainmueller (2012) that defines moment conditions of covariates in treated and control units and then matches them accordingly. By predefining the conditions for the balancing it prevents any loss of information and is therefore always superior to other conventional matching methods (Hainmueller and Xu, 2013). In other words entropy balancing will always almost perfectly balance the covariates and eliminate the standardized biases. It

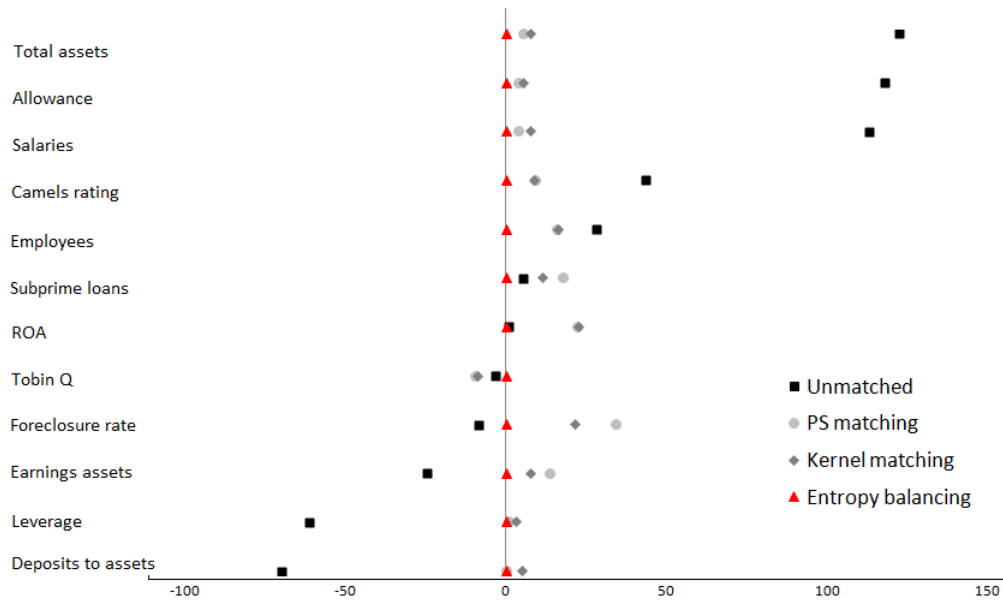


Figure 4.7: Covariate balancing before and after matching and entropy balancing. Standardized biases reported on the x-axis. The dots represent the standardized bias before matching (black squares) and after matching for three different matching algorithms (light grey circles for propensity score matching, grey diamonds for kernel matching, and red triangles for entropy balancing). The entropy balancing approach shows by far the best match as it all but eliminates the standardized bias between the treatment and control units for all the given covariates. Only values for the main set of covariates are reported in the case of entropy balancing, omitting the interactions and quadratic and cubic estimates, who were nevertheless included in the balancing estimations.

also balances for higher moments which includes interaction terms and squared and cubed values of the covariates (these were used for estimation, but are not shown in Figure 4.7). Despite being incredibly accurate in balancing the pre-treatment covariates, it's main disadvantage is that it represents a model-based approach which depends on the assumptions behind its weighting algorithm and the quality of its inputs — the observable covariates — and is therefore not a substitute for good research design. Just like any matching method it does not solve the omitted variable bias problem, but it does give us a very accurate, almost perfect match between observable treated and control units. When the treatment and control units are matched as in the case of entropy balancing it is very easy to estimate  $\tau_{ATE} = E[B_i(1) - B_i(0) | P_i = 1]$  as we can safely conclude that no other factor *that we control for* is driving the effect, and that the only difference between the treatment and control group is the amount of bailout funds they have received.

## Figures and tables

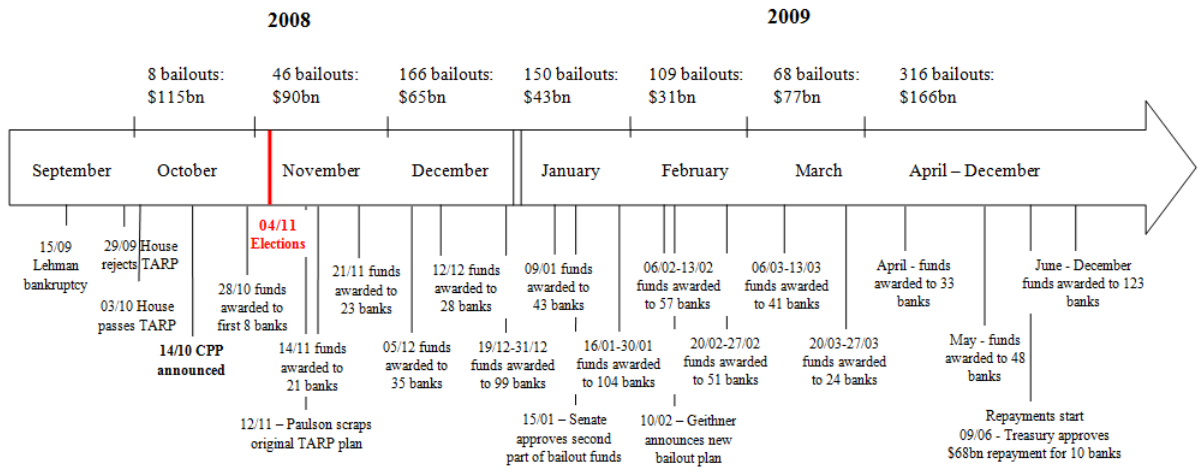


Figure 4.8: Timeline of the bailout allocation process. Source: Troubled Asset Relief Program Transaction Reports, March 2011, US Treasury (2013)

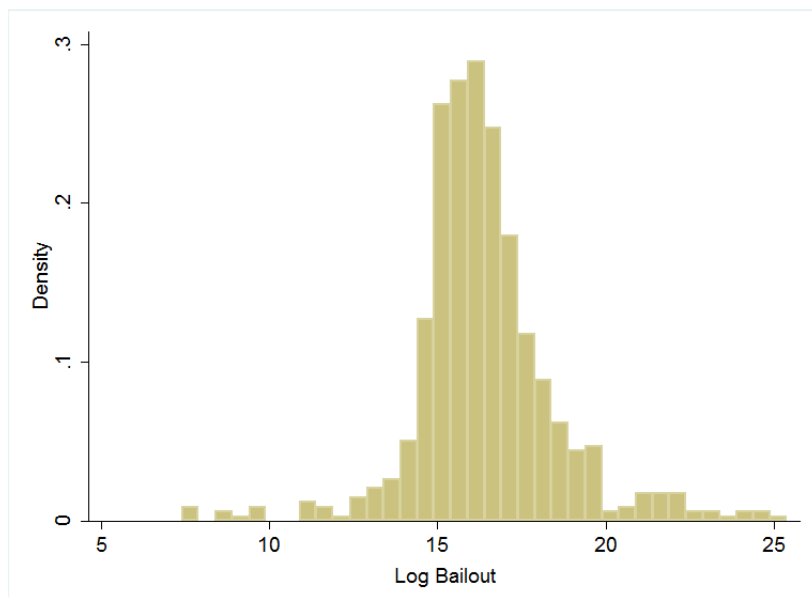


Figure 4.9: Histogram for the Log Bailouts outcome variable.

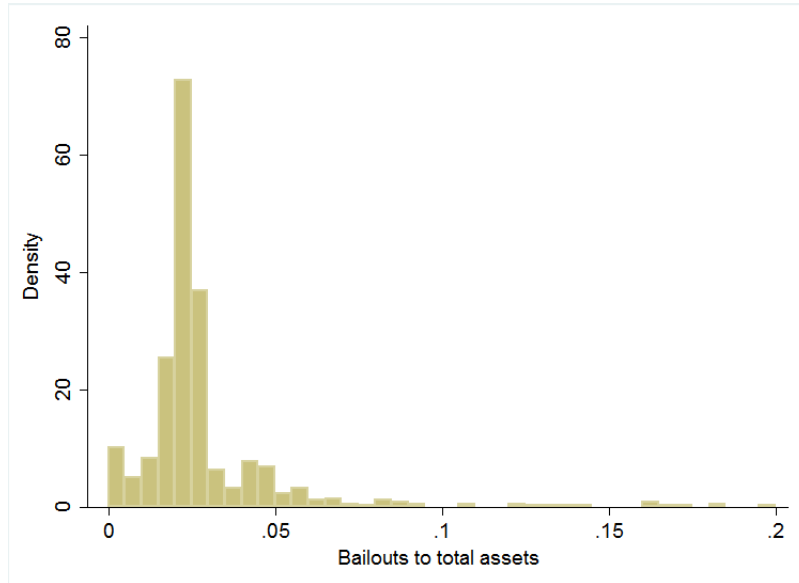


Figure 4.10: Histogram for the Bailouts to total assets outcome variable, excluding the outliers.

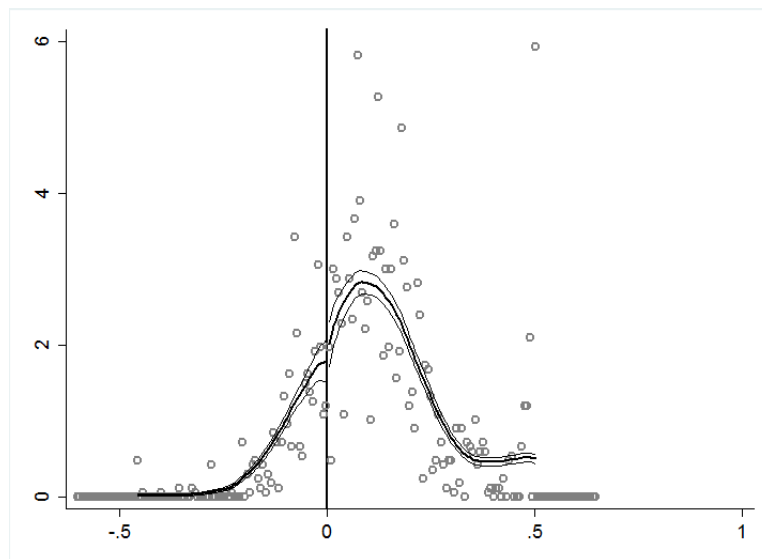


Figure 4.11: The McCrary (2008) density test shows no sign of sorting of the margin of victory variable around the zero threshold (the theta statistic is -0.086 with the standard error of 0.115 and is thus not significant).

Table 4.9: Summary statistics for the OLS and matching sample

Variable	Obs	Mean	Std.Dev.	Min	Max
Bailout	685	480,000,000	3,910,000,000	0	67,800,000,000
Log Bailout	679	16.408	2.119	7.406	24.940
Campaign donations	685	51,857	439,647	0	5,752,630
Log Campaign donations	153	8.908	2.408	5.338	15.565
Lobbying all years	685	1,119,167	7,766,865	0	110,000,000
Lobbying 2008-2009	685	243,354	1,725,575	0	23,300,000
Counterfactual lobbying	685	137,882	1,152,640	0	20,000,000
Abnormal lobbying	685	105,778	662,714	0	7,266,667
Total political spending	685	157,635	1,045,634	0	12,600,000
Log political spending	180	9.630	2.821	5.338	16.350
PolCon	685	0.385	0.487	0	1
Bailouts to total assets	685	0.040	0.082	0	0.961
Log Total assets	685	5.779	0.792	3.994	9.234
Log Deposits	684	5.651	0.757	2.778	8.878
Camels rating	685	0.068	0.034	-0.050	0.297
Camels Index	685	6.830	3.412	-4.990	29.730
Log Tier1 Capital	680	10.839	1.715	6.915	18.427
Log Risk-weighted assets	680	13.027	1.811	8.835	20.877
Risky assets ratio	681	0.785	0.131	0	1.318
Capital adequacy	683	0.118	0.066	0.010	1.251
Tobin's Q	683	0.900	0.044	0.125	0.999
Asset quality	682	0.005	0.106	-0.317	2.633
ROA	684	0.001	0.029	-0.144	0.652
Earnings to assets	677	0.880	0.098	0.069	0.994
Allowance	678	8.676	1.852	3.951	16.721
Leverage	680	0.899	1.342	0	35.727
Deposits to assets	681	0.805	1.057	0	28.192
Log Salaries	681	9.043	1.711	5.323	16.656
Employees	683	2153	14195	1	176003
Foreclosure rate	685	0.045	0.014	0.015	0.086
Subprime rate	685	0.114	0.020	0.045	0.177

Table 4.10: Summary statistics for the RDD sample

Variable	Obs	Mean	Std.Dev.	Min	Max
Log bailouts	2629	21.618	3.328	0	24.94
Bailouts to total assets	2491	0.048	0.058	0	0.902
PolCon	2629	0.839	0.366	0	1
Vote share	2208	0.618	0.159	0.045	1
Won dummy	2208	0.788	0.408	0	1
Margin of victory	2208	0.118	0.159	-0.455	0.5
Cut-off	2208	0.152	0.128	0.0005	0.5
Interaction	2208	0.135	0.137	0	0.5
Foreclosure rate	2629	0.011	0.017	0.00001	0.085
Log Assets	2491	7.906	1.342	3.994	9.234
Log Deposits	2491	7.485	1.418	0	8.877
Camels rating	2491	0.118	0.057	-0.049	0.297
Capital adequacy	2490	0.097	0.034	0	0.364
ROA	2491	0.0001	0.013	-0.144	0.243
Tobin's Q	2490	0.902	0.091	0	0.998
Earnings to assets	2491	0.583	0.297	0	0.994
Allowance	2489	11.43	5.238	0	16.72
Leverage	2491	0.594	0.238	0	0.96
Deposits to assets	2490	0.56	0.598	0	28.19
Log Salaries	2490	12.82	4.075	0	16.65
Employees	2491	68477	74704	0	176003
Foreclosures in state	2491	0.041	0.012	0.015	0.085
Subprime loans in state	2491	0.119	0.017	0.044	0.176

Table 4.11: Summary statistics for the IV sample

Variable	Obs	Mean	Std.Dev.	Min	Max
Log bailouts	590	16.158	2.75	0	24.94
Bailouts to total assets	590	0.039	0.081	0	0.961
Treatment	590	4.63	25.15	0	263
Instrument (1%)	590	0.0008	0.0073	0	0.125
Instrument (3%)	590	0.0073	0.050	0	1
Instrument (5%)	590	0.0137	0.072	0	1
Won	85	24.61	46.98	0	202
Close election	85	6.035	11.8	0	57
Log Assets	583	5.73	0.808	3.994	9.234
Camels rating	583	0.0675	0.034	-0.049	0.297
ROA	583	0.0002	0.018	-0.144	0.243
Tobin's Q	582	0.899	0.058	0	0.998
Earnings to assets	582	0.87	0.13	0	0.994
Leverage	582	0.845	0.120	0	0.96
Deposits to assets	582	0.813	1.14	0	28.19
Log Salaries	583	8.91	1.86	0	16.65
Employees	583	2442	15345	0	176003
Foreclosures in state	583	0.045	0.014	0.015	0.085
Subprime loans in state	583	0.114	0.019	0.044	0.176

## Bibliography

- [1] Acemoglu, D., Johnson, S., Kermani, A., Kwak, J., and Mitton, T. 2016. “The value of connections in turbulent times: Evidence from the United States.” *Journal of Financial Economics*, 121: 368-391.
- [2] Ansolabehere, S., de Figueiredo, J.M., and Snyder, J.M. 2003. “Why Is There so Little Money in U.S. Politics?” *Journal of Economic Perspectives*, 17(1): 105-130.
- [3] Baumgartner, F., Berry, J.M., Hojnacki, M., Kimball, D.C., and Leech, B.L. 2009. *Lobbying and Policy Change. Who Wins, Who Loses, and Why*. Chicago: University of Chicago Press.
- [4] Becker, J. and Morgenson, G. 2009. “Geithner, Member and Overseer of Finance Club.” *The New York Times*, April 26th, 2009.
- [5] Blanes i Vidal J., Draca M., Fons-Rosen C. 2012. “Revolving Door Lobbyists.” *American Economic Review*, 102(7): 3731-48.
- [6] Blau, B. 2017. “Lobbying, political connections and emergency lending by the Federal Reserve.” *Public Choice*, 172(3-4): 333-358.
- [7] Blau, B., Brough, T., and Thomas, D.W. 2013. “Corporate lobbying, political connections, and the bailout of banks.” *Journal of Banking & Finance*, 37(8): 3007-3017.

- [8] Boas, T.C., Hidalgo, F.D., and Richardson, N.P. 2014. "The Spoils of Victory: Campaign Donations and Government Contracts in Brazil." *Journal of Politics*, 76(2): 415-429.
- [9] Brollo, F., and Nannicini, T., 2012. "Tying Your Enemy's Hands in Close Races: The Politics of Federal Transfers in Brazil." *American Political Science Review*, 106(4): 742-761.
- [10] Business Insider, 2009. "The 25 Billion Dollar Secret: The NY Fed, Goldman & The AIG Cover-Up" *Business Insider*, November 6th, 2009.
- [11] Calomiris, C.W., and Khan, U. 2015. "An Assessment of TARP Assistance to Financial Institutions." *Journal of Economic Perspectives*, 29(2): 53-80.
- [12] Calonico, S., Cattaneo, M., and Titiunik, R. 2014. "Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs." *Econometrica*, 82(6): 2295-2326.
- [13] Caughey, D. and Sekhon, J.S. 2011. "Elections and the Regression Discontinuity Design: Lessons from Close U.S. House Races, 1942-2008." *Political Analysis*, 19: 385-408.
- [14] Center for Responsive Politics, 2017. Lobbying Database; PACs Database; Revolving Door Database; [online] Washington, DC. Available at: <https://www.opensecrets.org/influence/> [Accessed 29th April 2017]

- [15] Clots-Figueras, I., 2011. "Women in politics. Evidence from the Indian States." *Journal of Public Economics*, 95(2011): 664-690.
- [16] Clots-Figueras, I., 2012. "Are Female Leaders Good for Education? Evidence from India." *American Economic Journal: Applied Economics*, 4(1): 212-244.
- [17] Dal Bo, E., Dal Bo, P., and Snyder, J. 2009. "Political Dynasties." *Review of Economic Studies*, 76(1): 115-142.
- [18] Dash, E. 2008. "U.S. Said to Be Using Loose Rules in Bank Aid." *New York Times*, October 31st, 2008.
- [19] De Figueriedo, J.M., and Silverman, B.S. 2006. "Academic Earmarks and the Returns to Lobbying." *Journal of Law Economics*, 49(2): 597-625.
- [20] De Figueriedo, J.M., and Richter, B.K. 2014. "Advancing the Empirical Research on Lobbying." *17th Annual Review of Political Science* (2014): 163-185.
- [21] Desai, R.M., and Olofsgard, A., 2011. "The Costs of Political Influence: Firm-Level Evidence From Developing Countries." *Quarterly Journal of Political Science*, 6(2): 137-178.
- [22] Duchin, R., and Sosyura, D. 2012. "The politics of government investment." *Journal of Financial Economics*, 106 (1): 24-48.
- [23] Eggers, A., and Hainmueller, J. 2009. "MPs for Sale? Returns to Office in Postwar British Politics." *American Political Science Review*, 103(4): 1-21.

- [24] Emergency Economic Stabilization Act (EESA), 2008. Division A of Public Law 110-343, 122 Stat. 3765, Enacted October 3rd 2008, 110th US Congress, Washington DC.
- [25] Faccio, M. 2006. "Politically Connected Firms." *American Economic Review*, vol. 96(1): 369-386.
- [26] Faccio, M., Masulis, R., and McConnell, J. 2006. "Political Connections and Corporate Bailouts." *The Journal of Finance*, 61(6): 2597-2635.
- [27] Federal Deposit Insurance Corporation (FDIC), 2016. Quarterly Uniform Bank Performance Reports (Call reports). [online] Washington, DC. Available at: <https://www.treasury.gov/initiatives/financial-stability/reports/Pages/default.aspx>
- [28] Ferraz, C., and Finan, F. 2011. "Electoral Accountability and Corruption: Evidence from the Audits of Local Governments." *American Economic Review*, 101(4): 1274-1311.
- [29] Fisman, R. 2001. "Estimating the Value of Political Connections." *The American Economic Review*, 91(4): 1095-1102.
- [30] Gehlbach, S. 2006. "The Consequences of Collective Action: An Incomplete-Contracts Approach." *American Journal of Political Science*, 50(3): 802-823.
- [31] Gelman, A., and Imbens, G. 2018. "Why High-Order Polynomials Should

Not Be Used in Regression Discontinuity Designs.” *Journal of Business and Economic Statistics*, published online 14th May 2018.

- [32] Hainmueller, J. 2012. “Entropy Balancing for Causal Effects: A Multivariate Reweighting Method to Produce Balanced Samples in Observational Studies.” *Political Analysis*, 20(1): 25-46.
- [33] Hainmueller, J. and Xu, Y. 2013. “ebalance: A Stata Package for Entropy Balancing.” *Journal of Statistical Software*, 54(7), August 2013.
- [34] Helland, L. 2008. “Lobbying with Conflicting Interests: Norwegian Local-central Relations.” *European Journal of Political Research*, 47(2): 184-205.
- [35] Hochberg, Y.V., Sapienza, P., Vissing-Jorgensen, A. 2009. “A Lobbying Approach to Evaluating the Sarbanes-Oxley Act of 2002.” *Journal of Accounting Research*, 47(2): 519-583.
- [36] Hyytinen, A., Merilainen, J., Saarima, T., Toivainen, O., and Tukiainen, J. 2018. “Public Employees as Politicians: Evidence from Close Elections.” *American Political Science Review*, 112(1): 68-81.
- [37] Igan, D., Mishra, P., and Tressel, T. 2011. “A Fistful of Dollars: Lobbying and the Financial Crisis.” *NBER Working Paper* 17076, May 2011. Available at: <http://www.nber.org/chapters/c12416.pdf>
- [38] Imbens, G., and Kalayanaraman, K. 2012. “Optimal Bandwidth Choice for

- the Regression Discontinuity Estimator.” *Review of Economic Studies*, 79(3): 933-959.
- [39] Imbens, G., and Lemieux, T. 2008. “Regression discontinuity design: A guide to practice.” *Journal of Econometrics*, 142(2): 615-635.
- [40] Jayachandran, S. 2006. “The Jeffords Effect.” *Journal of Law Economics*, 49(2): 397-425.
- [41] Johnson, S., and Kwak, J. 2011. *13 Bankers: The Wall Street Takeover and the Next Financial Meltdown*. New York: Vintage Books.
- [42] Khwaja, A.I. and Mian, A. 2005. “Do Lenders Favor Politically Connected Firms? Rent Provision in an Emerging Financial Market.” *Quarterly Journal of Economics*, 120(4): 1371-1411.
- [43] Kwak, J. 2014. “Cultural Capture and the Financial Crisis.” in Carpenter and Moss (eds.) *Preventing Regulatory Capture. Special Interest Influence and How to Limit It*. Cambridge: Cambridge University Press.
- [44] Lee, D.S. 2008. “Randomized experiments from non-random selection in US House elections.” *Journal of Econometrics*, 142(2): 675-697.
- [45] Lee, D.S., Moretti, E., and Butler, M.J., 2004. “Do voters affect or elect policies? Evidence from the US House.” *Quarterly Journal of Economics*, 119(3): 807-859.

- [46] McCrary, J. 2008. "Manipulation of the Running Variable in the Regression Discontinuity Design: A Density Test." *Journal of Econometrics*, 142(2): 698-714.
- [47] Mian, A., Sufi, A., and Trebbi, F. 2010. "The Political Economy of the US Mortgage Default Crisis." *The American Economic Review*, 100(5): 1967-98.
- [48] Pana, E. and Wilson, L. 2012. "Political Influence and TARP Investments in Credit Unions." *Quarterly Journal of Finance*, 2(4): 1250017.
- [49] Querubin, P. and Snyder, J. 2013. "The Control of Politicians in Normal Times and Times of Crisis: Wealth Accumulation by U.S. Congressmen, 1850-1880." *Quarterly Journal of Political Science*, 8(4): 409-450.
- [50] Securities and Exchange Commission (SEC), 2017. EDGAR Database; Annual reports and financial statements for 52 publicly listed companies. [online] Washington, DC. Available at <https://www.sec.gov/edgar/searchedgar/webusers.htm> [Accessed 7th June 2017]
- [51] Snyder, J., Folke, O., and Hirano, S. 2015. "Partisan Imbalance in Regression Discontinuity Studies Based on Electoral Thresholds." *Political Science Research and Methods*, 3(2): 169-186.
- [52] Special Inspector General of the Troubled Asset Relief Program (SIGTARP), 2011. "Quarterly Report to Congress", [online] October 27th, 2011. Avail-

able at: [https://www.sig tarp.gov/Quarterly%20Reports/October2011\\_Quarterly\\_Report\\_to\\_Congress.pdf](https://www.sig tarp.gov/Quarterly%20Reports/October2011_Quarterly_Report_to_Congress.pdf)

- [53] Stewart, J.B. 2009. "Eight Days. The battle to save the American financial system." *The New Yorker*, September 21st, 2009.
- [54] US Department of Housing and Urban Development (HUD), 2010. Neighborhood Stabilization Program Data. [online] Washington, DC. Available at <https://www.huduser.gov/portal/datasets/NSP.html> [Accessed 20th June 2017]
- [55] US Department of Treasury, 2013. TARP Programs. TARP Investment Program Transaction Reports (latest update March 29th 2016). [online] Office of Financial Stability, Washington, DC. Available at <https://www.treasury.gov/initiatives/financial-stability/reports/Pages/default.aspx> [Accessed 15th April 2017]
- [56] Veronesi, P. and Zingales, L. 2010. "Paulson's gift." *Journal of Financial Economics*, 97(3): 339-368.
- [57] Wharton Research Data Service (WRDS), 2017. BoardEx database - North America. [online] The Wharton School, University of Pennsylvania, PA. Available at <https://wrds-web.wharton.upenn.edu/wrds/> [Accessed 30th April 2017]

## Chapter 5

# Elite networks and inequality

## Political connections and personal networks among top corporate incomes earners

### Abstract

The largest source of rising income inequality in the West, particularly over the past three decades, has been an increase in earnings of the top 1% and 0.1% of income earners. Among those, nearly two thirds are corporate executives. How important were political factors and personal connections in explaining this trend of rising executive earnings? This paper examines individual and firm-level data on top executive incomes and political connections from 2000 to 2015 for two countries with the highest share of top 1% earnings in total incomes in the West —

the United States and the United Kingdom. It provides a new way of measuring elite networks which are defined as informal social networks between politicians and senior corporate executives. It finds that better connected corporate executives — members of elite networks — were more likely to have higher earnings than non-connected executives outside elite networks. It also directly estimates the rent-seeking wage premium that executives receive if they are politically connected. The same mechanism is confirmed on a firm-level: firms that had more executives with strong political ties paid significantly higher salaries to those executives.

**Keywords:** Elite networks, inequality, top incomes, corporate executives, political connections, rent-seeking

## 5.1 Introduction

What explains the significant rise of income inequality over the past several decades? The conclusions of the overwhelming majority of the literature can be summarized as follows: rising income inequality since the 1980s is a consequence of (1) stagnating wages of low and middle-income earners (bottom 90% and even bottom 99%) and (2) a significant rise of wages and non-wage benefits among the top-income earners (top 1% and top 0.1%). Piketty and Saez (2003), Atkinson (2008), Atkinson, Piketty, and Saez (2011), Milanovic (2016) and Saez (2017) all show that the annual average real income growth for the bottom 99% of income earners in the

United States and the United Kingdom (two countries with the largest increase in Gini indices among the rich OECD economies in the past 40 years; see Atkinson et al, 2017) has been relatively flat from 1978 to 2015 (average annual growth of 0.6%). During the same period incomes for the top 1% of income earners grew at an annual 4 to 5% on average. The majority of this income growth for the top 1% of income earners did not come from salaries but from capital gains and business income (investments, dividends, business profits). As a consequence the top 1% of income earners in the US earned about 19% of all income by 2013, while in the UK they earned about 15% of all income by 2010. Even though these two countries represent the strongest increase in income disparities and also the largest share in total incomes for the top 1% of income earners, a similar trend of rising top decile earnings and rising Gini indices has been noticeable across the OECD economies (Atkinson, 2008; Atkinson et al, 2017). When decomposing the top income earners in the US, Bakija, Cole, and Heim (2012) have found that nearly two thirds of the top 0.1% are executives and managers (42% non-finance, and 18% in finance).

Many theories have been developed in attempting to explain this rapid growth in top incomes as well as the accompanying stagnation of low and middle incomes (for a summary see Deaton, 2013, or Atkinson, 2015). Explanations for the growth in top incomes have mostly focused on economic factors such as the skills-based technological progress hypothesis, the impact of globalization which increased demand for top talent and made executives of multinational companies

globally competitive, changes in the tax code, or the effect of innovation and the creation of new tech superstars. The stagnating low and middle incomes were explained through the impact of low-skilled immigration, outsourcing jobs, minimum wages not following inflation, declining bargaining power of labour unions, and even the gender wage gap which made a big difference as more women entered the workforce.

The inequality literature in most cases tends to overlook the roles of political connections and network effects in explaining the relative rise in top incomes. Recently, however, a few notable efforts have tried to unpack the mechanisms and consequences of elite interactions on inequality. Stiglitz (2012) set the tone by arguing that the majority of top 1% wealth came from rent-seeking activities rather than market innovations. This conclusion, emphasizing the importance of economic rents in explaining the rise of inequality, was further supported by a discussion paper from Furman and Orszag (2015), and a book by Lindsey and Teles (2017) who blame regulatory capture that stifles competition and redistributes wealth towards the elites. Another notable exception is Scheidel's (2017) detailed historical overview of inequality where he develops a hypothesis that only lethal pandemics and substantial acts of violence such as mass warfare or revolutions enabled the flattening of inequality over the past centuries. He suggests that inequality, since prehistoric times, was driven and amplified by political and/or military power. The "*original 1 percent*" according to Scheidel (2017) were the

ruling elites who captured political rents thanks to their position of power or close proximity to power: *“Political power was a critical source of income and wealth, ... and a powerful determinant of material inequality.”* (Scheidel, 2017, pp 63). Inequality is therefore deeply, almost evolutionary, rooted in politics. Finally, political science research has been more inclined to link certain political factors to inequality trends. Bonica et al (2013) suggested that one of the reasons behind the rise of inequality in the US is the tendency of top income earners to use lobbying, political connections and campaign contributions to skew legislative and regulatory decisions in their favour. Similarly, Hacker and Pierson (2011) attribute rising inequality to increasing power of interest groups representing wealthy elites (in particular big business) who used their political influence to promote beneficial tax and regulatory policies. The findings of Gilens (2012), Gilens and Page (2014), and Bartels (2016) all support this conclusion that the wealthy elites have been more successful in getting the policy decisions they wanted.

The crucial question is how does such behaviour of elite groups affect the distribution of top incomes? What is the role of personal social networks and what is the role of political connections in explaining the rise in top incomes? The problem that any social scientist has in trying to answer these questions is the lack of data on personal connections of top executives and therefore the inability to clearly define and measure an elite group. Furthermore, there are many indicators that suggest that rent-seeking in the US might be on the rise and that this could

be a potential culprit for inequality. For example Furman and Orszag (2015) cite an increasing concentration of high returns among the most profitable firms which all consequentially pay the highest salaries and drive the between-firm inequality in earnings (this empirical finding is supported by Song et al, 2016, and Autor et al, 2017). Most of these concentrated returns are not based on innovation, leading them to conclude that the returns could be a result of rent-seeking. Lindsey and Teles (2017) suggest that high levels of industry concentration, an increasing importance of intangible assets in corporate balance sheets, and growing barriers to entry in many industries all point towards increasing rent-seeking that is driving up inequality.

Inequality is most likely a product of many different interrelated factors. This paper focuses its attention on only one; a factor that has thus far mostly been overlooked in the discussions on inequality — the impact of elite networks. In this paper I provide a new measure of elite networks, and by directly estimating the rent-seeking wage premium that executives receive if they are politically connected. I estimate the impact of corporate executives joining and forming elite networks with politicians on their earnings. An elite network is formed between politicians in power (holding executive elected positions, cabinet positions, or key positions in parliamentary committees) and senior executives of the biggest publicly-listed companies in a country. The network is formed based on proximity to each other through a similar career path or membership in the same organization (clubs, so-

cieties, foundations, religious groups, etc.). Within an elite network favours are exchanged between the actors. These usually imply beneficial regulations or government contracts (*rents*) for the firm in exchange for political campaign donations or other form of support for the politician. The executive, who had successfully extracted rents for the firm, can demand a higher compensation for his or her efforts. An empirically observable consequence of this relationship would be that executives who are members of elite networks should have higher average annual earnings than executives who are not members of elite networks — they receive the rent-seeking wage premium. In order to approximate the effect of elite networks I use a unique individual-level and firm-level database from BoardEx across a 16-year time span, and identify which individuals had connections to politics either via their previous careers in the public sector (if they held any high-level positions in government) or if they were members of the same organizations as their connected politicians (this includes country clubs, foundations, charities, trusts, church or religious groups, professional organizations, etc.).

The paper finds that politically connected executives, i.e. members of elite networks, tend to be more valued by their companies and are indeed rewarded with higher compensations. Furthermore, elite network members tend to have a larger total number of people they are connected to, meaning that any member of an elite network is highly likely to be on average more (and hence better) connected than anyone outside an elite network. Both of these factors matter as an explanation of

higher executive earnings, however direct political connections matter more. I use US and UK individual and firm-level data over a time span of 16 years (from 2000 to 2015) to show that being politically connected results in higher annual earnings. Estimation is done using unit and time fixed effect regressions. The findings are further strengthened by using an instrumental variable estimation on a firm level with industry-average levels of connections as an instrument. Each case yields the same conclusion: being connected clearly matters in the business world, and it tends to get substantially rewarded.

The paper starts by presenting the theoretical argument behind elite network formation, its impact on firm behaviour and its wage structure. The next section describes the data sources and the coding of the main variables, while also presenting the trends in top income inequality derived from the database to see if they followed the same pattern as suggested by the literature. The empirical strategy section defines the identification of the estimation process while the results section presents the overall results separately for both countries, first using individual-level data and then firm-level data. The final section concludes.

## **5.2 Theory: elite networks and rent-seeking firms**

How are elite networks formed? Mostly they arise spontaneously leading to multiple different elite networks existing within a country. They are defined by either geography, economies of scale, or centering around political power. Given that elite

networks are social networks forming between persons of power they are characterized by high levels of clustering and positive assortativity<sup>1</sup> of well-connected agents (Jackson, 2008). Elite networks are therefore generated by high-degree nodes who tend to associate i.e. cluster with like-minded high-degree nodes with similar interests. This makes an elite network highly topocratic.

A topocratic system, according to Borondo et al (2014), is a system where average compensation depends on how connected an individual is, and what is his or her position within a network. In other words, the complete opposite of a meritocratic system — it matters more who you know than what you know. An economic system of a country is likely to be more topocratic and hence have higher economic inequality if there exists a large number of individuals who are poorly connected (say only to 150 other people, corresponding to the so-called Dunbar's number) and a very small number of individuals who are highly connected.

Elite networks represent the most central, highly-connected nodes within any topocratic system. Having a powerful politician (where power is determined by the politician's network centrality) linked to the network is a necessary condition for it to succeed, whereas having corporate executives linked to each other is a sufficient condition. Consider a group of corporate executives attempting to organize a cartel to achieve monopoly power and set high prices (i.e. engage in classic rent-seeking). They have an incentive to cooperate and form an elite network between

---

<sup>1</sup>The tendency of highly-connected individuals (high-degree nodes) to be connected with other highly-connected individuals.

them, however the success of their rent-seeking venture will depend on political and regulatory approval, or in other words if no regulation prevents them from colluding. The more powerful their political connection, the more likely that the venture will be successful. Special interests use lobbying and campaign donations to reach politicians, and this is always conditional on having a direct connection to the right politician.

The existence of highly-connected individuals, particularly in positions of political power, means they possess superior (or have privileged access to) information on opportunities that may be arising<sup>2</sup> and will only process this information among their own narrow social group. This increases informational asymmetry and undermines the market mechanism in the distribution of opportunities, making it more likely that the distribution of top incomes will also be captured within elite networks.

Is this theoretical implication rooted in empirical conclusions of the literature? Certainly. The empirical findings of Fisman (2001), Faccio (2006), Desai and Olofgard (2011) and many others suggest that politically connected individuals and by extension their firms incur excess benefits from their favourable position and proximity to power. On the other hand the literature also suggests that such benefits come at a cost for the connected firms — they shift their activities and resources from satisfying customers to lobbying for political favours, they are dis-

---

<sup>2</sup>E.g. A new law being passed, a regulatory opportunity, having privileged access to government tenders, etc.

couraged to invest into new technologies, and they have no incentive to acquire top talent to keep their business growing. Desai and Olofsgard (2011), using a sample of 8000 firms in 40 developing countries, measure such direct costs of political influence. They find that politically-favoured firms are less likely to invest and innovate (engage in R&D, restructure operations, open new plants or production lines), which as a consequence lowers their productivity and sales growth. Regardless of their privileged position which renders them substantial benefits (such as protection from expropriation, regulatory and tax benefits, or better access to finance) politically connected firms have disincentives to innovate and are therefore worse performers than non-connected firms. The empirical findings in Tanzi and Davoodi (1997), Faccio et al (2006) or Bellettini et al (2013) confirm these insights for a number of different countries.

As one potential extension to this mechanism proposed in the literature I classify the politically connected firms as *rent-seekers* (or *rent-extractors*), and non-connected firms as *customer-seekers*. The difference between them is their focus of interest: do they reward executives for their political connections or do they reward executives for market innovation. The customer-seeking firms have to constantly improve their product space in order to remain present on the market, whereas the rent-seekers have lesser incentives to innovate and compete for customers, as they can count on political rents. Rent-extraction implies either classical rent-seeking: protectionism, barriers to new entrants, or monopoly sta-

tus (Tullock, 1967, Krueger, 1974), or receiving fixed procurement contracts and subsidies (Bandiera, Prat and Valletti, 2009; Fazekas and Toth, 2016; Fazekas and Kocsis, 2017). In the case of monopoly power and barriers to new entrants this means that rent-extracting firms get to keep their large market share and therefore still realize revenues through gaining customers, but would not be able to do so without the help of political power. It should be noted that the definitions of innovation and investment here are not confined only to the implementation of new technologies. Innovation and investment imply regular competitive-based activities of firms: introducing better sales techniques, better positioning on the market, constant improvement of the quality of the product or the service based on customer feedback, and most importantly attracting and hiring top talent to ensure the company's long term success. Lack of such incentives to innovate and invest will shift the hiring decisions in rent-seeking firms towards executives who are members of elite networks which could benefit the firm. This mechanism can also work in reverse — the executive who has already self-selected into an elite network pushes the firm to become a rent-seeker in order to showcase his or her strength. The connected executive is generating benefits to the firm by securing favourable regulations or exclusive government contracts, and consequentially demanding higher compensation.

This extension is important in trying to address the reasons behind top executive income growth. The most basic interpretation of the conclusions of the

inequality literature is that the rise in top incomes is mostly a within-firm phenomenon. In other words the widening disparities in earnings between the executives and the workers were the main driver of inequality. However according to a recent paper by Song et al (2016) using extensive micro-level data for the United States the main driver of inequality seem to be differences *between* firms, rather than *within* firms (this finding is also supported by Card et al, 2013 for Germany, Hakanson et al, 2015 for Sweden, Mueller et al, 2015 for the United Kingdom, and Helpman et al, 2015 and Alvarez et al, 2015 for Brazil. Other papers that confirm the between-firm effect in the US are Davis and Haltiwagner, 1991, Barth et al, 2014, and Furman and Orszag, 2015). According to Song et al (2016) the entire increase of inequality for the bottom 99% has been driven by between-firm inequality, and almost all of the increase of inequality of the top 1% has also been driven by between-firm inequality (except for those in the top 0.02% of the income distribution). The authors suggest that a possible explanation could be the clustering of high-paid workers in more successful firms and low-paid in less successful ones (which would be similar to the conclusions of Autor et al, 2017 on the effect of “superstar firms” as the drivers of compensations at the top). One thing missing from the between-firm literature is the implication of political connections. Could between-firm inequality also be driven by rent-seeking motives of corporate executives where higher rent-seeking translates into higher earnings?

My hypothesis is that the decision to hire workers and thus enable the observed

clustering effects is contingent on how a firm survives on the market (i.e. how it realizes its revenues). If it survives by extracting rents on the political market it is indifferent in hiring workers to boost its productivity, but it will hire well-connected executives to extract rents. Therefore an obvious distinction between a rent-seeker and a customer-seeker is to see how well-connected their top executives are to politicians, and are they members of close networks with the politicians in power. This is how I define the indicator variable of political connections. A politically connected firm will have top executives with deep links to politics, i.e. CEOs and board members who used to work at top-level government jobs and who are direct members of the same social networks as the politicians in power.

### **5.3 Data and variables**

The main source of data are individual-level connections and earnings of top corporate executives in the US and the UK from 2000 to 2015, collected by the private company BoardEx, accessed via the Wharton Research Data Service (WRDS, 2017). The BoardEx database is assembled by looking at all publicly listed companies of a given country in addition to selected large private companies (not listed on the stock market but reaching considerable size in terms of both employees and revenues). The quality of the dataset is one of its main advantages. They use official sources to gather information on individual profiles for each senior executive in their sample of firms. When doing so they cross-reference whether the

data has been assigned correctly to each profile, and they make sure that each profile is completed, subject to data availability in the official registries. In the US they use Annual reports from the Securities and Exchange Commission (SEC), US stock exchange data (NYSE and NASDAQ), press releases and official data from corporate websites, and various other registries to track individual careers over time. For the UK they use official reports from Companies House, the London Stock Exchange, the Regulatory News Service, press releases and official reports from corporate websites, and various other registries of personal data.

They have data on 11,800 firms in the US and 3,500 firms in the UK (some of which got de-listed over time), while the total sample contains two thirds publicly listed companies and one third large private companies. In total they have data on over 750,000 individuals worldwide collected over a number of different periods. In each case they collect full information for each director and senior manager in the firm (Director, CEO, CFO, COO, President, Chairman, VP), including their wages and total earnings (containing bonuses and stock options), employment history, gender, age, education, total experience, performance, the size of their network, and membership in other activities outside their workplace. The size of the network represents the total number of people an individual executive is connected to through various business and personal interactions. For example, two executives that served on the same board in another company before, graduated on the same year from the same university degree, or are members of same organizations

(business clubs, professional organizations, non-profit associations, church groups, charities, school or university boards, etc.), are considered to be members of each other's networks. They might not be friends or interact frequently, but there is a connection that can enable one of them to easily get in touch with the other. The BoardEx dataset has been used in academic research thus far, mostly focusing on network effects, mergers and acquisitions, executive compensation, and corporate performance of CEOs (e.g. El-Khatib, Fogel, and Jandik, 2015; De Cesari and Ozkan, 2015; Kim, Kogut, and Yang, 2015; Schmidt, 2015; etc.).

From this extensive dataset I only focused on the period from 2000 to 2015 (since 2000 is the first year for which the data on network size and earnings was available). I extracted individual-level data on over 26,000 individuals working at 1300 firms in the US, in addition to over 21,000 individuals working at 2500 firms in the UK<sup>3</sup>. From the full dataset I created four panels, two individual-level and two firm-level. The initial datasets had to be further reduced by focusing only at senior top level executives and the company's highest earners (President, Chairman, Director, CEO, CFO, COO) thereby excluding all lower-level earners in the corporate hierarchy, many of which were included in the original dataset. The final individual-level datasets contain 46,299 observations for the US, and 31,422 observations for the UK. The firm-level datasets contain 12,829 observations for the US (around 800 firms), and 19,908 observations for the UK (around 1200 firms).

---

<sup>3</sup>The UK dataset included more private, unlisted firms than the US case.

### 5.3.1 Measuring networks and connections of individuals and firms

#### Network size

Measuring the size and scope of individual connections is the most valuable part of the BoardEx database. It allows a detailed insight into how important it is to be a part of a particular network. An individual executive's network is measured as the number of connections (links) he or she has with every other executive in the dataset. The larger the number of an individual's connections (i.e. the degree of a network), the more influential he or she tends to be. These connections are achieved either through previous jobs, memberships on boards in other companies, education (same generation, studying the same subject in the same university), or membership in various other organizations (which include professional organizations, clubs, charities, church groups, university or school boards, non-profits, etc.). An individual's network, or total number of connections, therefore includes everyone he or she was or currently is connected to throughout their career, education, or other activities.

Despite the effort the BoardEx team applies in making sure the data is entered correctly, there is still a possibility of measurement error. In particular, some individuals tend to have very large networks (over ten thousand people), while others seem to be connected to only a few people. This can be seen in the histograms of individual network size for both countries in the Appendix (Figures 5.9 and 5.10), both of which resemble a Poisson distribution. An executive at a high position with

such few connections is surely an underestimation, however given that BoardEx links executives only within its dataset it is entirely plausible for some to have such low levels of connections (less than 10), while others, on the other hand, are so central that they could in theory be connected to most or all executives in the sample. This means that the variable itself imperfectly captures an individual's total connections, but it does give an indication of how well connected an individual is within the sample of top executives in a country. This is therefore the first explanatory variable used in the paper, *network size*, attempting to estimate how the mere size of an executive's total personal and business connections influences his or her earnings. The hypothesis is that the larger the size of an individual executive's network, the greater his or her earnings, resting upon the assumption that a well-connected executive has a greater chance of securing a better deal for the firm.

### **Political connections**

Another unique advantage of the BoardEx database is the ability to examine career trajectories of executives and board members prior to their top positions in the firm, as well as their memberships in various organizations. These two facts enable me to match individuals to their previous positions in government and to each other in order to define the main variable of interest: *political connections*, defined as an indicator variable  $POLCON_{it} = \{0, 1\}$ . The main POLCON variable is

composed of two specific measures of connections: previous government experience, denoted  $POLCON_{Gov}$ , and membership in the same organization as the relevant politician<sup>4</sup>, denoted  $POLCON_{Org}$ . I first explain how I coded each of these to form my unique measure of political connections, and then I describe the dataset matching process through which I ensure that each individual is uniquely and correctly matched.

The first sub-variable,  $POLCON_{Gov}$ , defines an individual to be politically connected if that individual held a decision-making position in government at any time prior to the start of his current executive tenure in the private sector. Establishing this connection required additional cleaning of the database which implied cross-checking which type of position a senior executive held while working in the public sector. If this included medium or low-level bureaucratic positions (e.g. analyst, officer, civil servant, assistant, researcher, clerk, etc.) these individuals were coded as not being connected,  $POLCON_{Gov} = 0$ . However if they were members of various state and national committees, councils, or assemblies, not to mention if they held direct executive positions in government (manager, director, chief of staff, chairman, minister, Congressman, Senator), this does qualify as a political connection,  $POLCON_{Gov} = 1$ . This variable encapsulates the idea that after leaving their position in government, a senior executive continues his or her membership in the social network of existing political decision-makers.

---

<sup>4</sup>Relevance is determined either by geographical proximity (e.g. State representatives or city mayors), or by key positions in government or the legislature (ministers, members of key parliamentary committees, etc.)

The second sub-variable,  $POLCON_{Org}$ , looks at whether an executive was connected to a politician via membership in the same organization. What classifies an organization? The BoardEx (2017) Data Directory defines these through ‘other activities’ and provides a detailed list of all organizations every executive is or was an active member of. The list is large, and it includes a wide variety of organizations: professional organizations (excluding bar or accountant associations but including any type of industry lobby group), clubs (country clubs, Rotary clubs, various societies), university of school boards (and also active alumni organizations), NGOs, institutes and think tanks, trusts, foundations, charities, church groups and various other religious organizations, veteran or military organizations, various political advocacy groups, and even political parties.

Coding these two sub-variables required a lot of attention to detail. Establishing government experience was relatively straightforward: it required matching two separate datasets, one that tracked previous executive careers (manually excluding all lower-level bureaucratic positions first), and the main dataset that contained data on executive earnings and firm-level details. However coding  $POLCON_{Org}$  was more difficult. The first step was to recognize which entry in the sample is still a career politician (this included Congressmen, Senators, Members of Parliament, assembly legislators, mayors, governors, cabinet ministers). Then I divided the samples into (active) politicians and non-politicians (just executives) and matched the two samples by the organization ID. This enabled me to see which executive

and politician are members of the same group, and to how many politicians in total an individual executive is connected to. I've excluded all cases where the timing between the links does not fit to make sure that I correctly coded the connections.

The final  $POLCON_{it}$  variable was then created by simply adding the two sub-variables together, with the indicator taking the value of 1 if an individual worked at a decision-making position in the government at any time prior to his or her executive job, and/or if he is a member of the same organization as the local or national legislator (assemblymen, Congressmen, and Senators) or executive politician (mayor, governor, secretary of state or cabinet minister). In total for the US there were 4,561 cases (9.8%) of previous government experience (across 16 years), and 30,921 cases (66.7%) of same organization membership, however there were about 3,000 cases where an individual had both previous government experience and was member of the same organization as the politician. These were also coded as 1. In total for the US across 16 years there were 32,412 entries across 16 years coded as politically connected (70% of the sample). In the UK, there were 2,490 entries (7.9%) with previous government experience, and 12,063 cases (38%) of same organization membership. In total there were 12,850 entries across 16 years coded as politically connected (40.1% of the sample). This may seem like a lot, particularly in the US, but apparently executives from both countries realize the importance of being connected to politics. These connections need not however imply direct friendships. They just suggest that it is easier to reach a politician if

he or she is a member of your organization.

### **Firm-level political connections**

When aggregated on a firm level, I use two versions of the POLCON classification, each constructed by summing up individual executives by unique firm ID for each firm in the dataset. The first indicator of connections is constructed if a firm had at least one senior executive that was politically connected, denoted as  $POLCON_{mt}$  ( $POLCON Firm$  in the results section). However for this first case I had to be more restrictive and focus only on previous government experience. In other words I use only the  $POLCON_{Gov}$  variable to construct  $POLCON Firm$ . If I take into account same organization membership there would be virtually no variation in the US case as almost every firm had at least one person that was politically connected via same organization membership. In this case, when looking only at executives with previous government experience about 60% of US firms can be coded as politically connected. This allows enough variation to get comparable estimates as with the individual-level data. In the UK I get about 40% of firms that had a politically connected executive with previous government experience.

For this reason I also construct the second POLCON variable for firms, where I look at the total number of all politically connected senior executives in a firm, denoted as  $POLCON_{total}$ . The idea is to distinguish between firms with low and high numbers of connected executives in their boards. In reality all it takes is a

single well-connected executive to land an exclusive contract with the government. However firms where the entire board is filled with connected executives are clearly better examples of rent-seekers than those with only a single connected executive. Theoretically they should both be considered as politically connected, but this way I get a better distribution of connected individuals within them, and hence a more precise estimate of the marginal effect of political connections — the value of an additional connected board member. The distribution for  $POLCON_{total}$  is given by the corresponding histograms in Figure 5.11 in the Appendix. There is a clear difference between US and UK firms. In the US the distribution is wider (there are firms with up to 15 connected executives) and the majority of firms have between 1 and 5 connected executives. In the UK the distribution is more narrow, and politically connected firms have in most cases between 1 and 2 connected executives. This difference between the US and the UK is further examined in the results section.

### **Correlation between political connections and network size**

How do political connections correlate with network size? The first interesting descriptive finding of this paper is that a person who is politically connected has on average a much larger network. Table 5.1 shows t-tests for network size according to the indicator of political connections for the two countries. In both cases the network of politically connected executives is 2 to 3 times the size of politically

unconnected executives<sup>5</sup>. The firm level data for both countries indicates exactly the same conclusion — executives in politically connected firms have significantly larger networks than executives in non-connected firms. This adequately sums up the main explanatory variables of this paper — being part of elite networks also implies being better connected, and most likely more influential. This could imply that elite networks have a tendency to be topocratic, where it matters who you know in the decision-making hierarchy.

Table 5.1: Descriptive statistics and t-tests for individual network size with respect to political connections

	Mean	St.Dev.	Min	Max	N
United States network size					
Politically connected = 1	1539.7	1574.8	13	16338	31803
Politically connected = 0	673.7	814.1	10	6185	13621
United Kingdom network size					
Politically connected = 1	1473.2	1539.8	7	9792	12692
Politically connected = 0	399.6	701.1	4	7967	18300
T-test connected vs unconnected (US network size)			T-test connected vs unconnected (UK network size)		
866.1*** (60.8)			1073.6*** (82.7)		

The final two rows report the absolute difference in network size for connected and unconnected individuals, and the corresponding t-value for the t-tests, where \*\*\* denotes significance at 0.1%.

Table 5.10 in the Appendix disentangles the POLCON variable by its two

<sup>5</sup>The histograms for network size are provided in the Appendix in Figures 5.9 and 5.10. The relative size of each individual's network declines exponentially. In the US more than half have less than 1000 connections, and 90% have less than 4000 connections.

components, previous government experience and same organization membership. The conclusions are identical: no matter how we define political connections, those who are connected in each case have a much larger average network size.

### **5.3.2 Inequality in top executive incomes**

Another useful part of the BoardEx database is extensive data on annual individual earnings, including data on salaries, bonuses, stock options, pension contributions, and total direct compensation for each executive. Two outcome variables are created from this data: the logarithm of salary (*log salary*) for each individual, and the logarithm of total direct compensation (*log earnings*) as a measure of total annual pre-tax earnings for each executive which includes salary, bonuses received (if any), capital gains (if any), and pension contributions.

The reason I only focus on top executive earnings is based on a number of findings in the literature that top incomes (of the 1%, and 0.1% of income earners in the population) are the key driver of income inequalities over the past 30-40 years (see Atkinson, Piketty, and Saez, 2011; Song et al, 2016; Atkinson et al, 2017, etc.), while over 60% of those in the top 1 and 0.1% are corporate executives (Bakija, Cole, and Heim, 2012). The presumption is therefore that the majority of the increase in top incomes, and hence income inequality, can be explained by the increase of top executive incomes.

The first useful thing would be to plot this data to see if executive earnings

as reported in the database followed the same trends of reported increases in top incomes over the past years. Figures 5.12 and 5.13 in the Appendix show the distribution of individual executive earnings for both countries, and they suggest a similar finding to the majority of income inequality literature: even among top earners, the top 1% of executives have much higher total earnings than the rest. In these examples, the top 1% of highest-paid executives in the US had higher total annual earnings than 99% of all other executives combined (have in mind that the minimum total compensation in the sample was \$150,000) (Figure 5.12, right panel). For salaries the distribution is flatter, but it still suggests that the top 5% of highest-paid executives in the US had higher total annual earnings than 95% of all other executives combined (Figure 5.12, left panel). For the UK the distributions are almost identical, with the difference being that in the UK the top 1% of executives had slightly higher salaries than in the US, but slightly lower total earnings (most likely due to more lenient capital taxation rules in the US). Once again the top 1% of executives have higher total annual earnings than the other 99% of executives combined (Figure 5.13, right panel), while 5% of executives have higher annual salaries than the other 95% combined (Figure 5.13, left panel).

Next we look at the trends for log earnings over the observed 16-year period. Overall they suggest that executive incomes have continued their rise over the past decade and a half, and that even after a correction in the trend during the crisis and pre-crisis years the general trajectory hasn't changed. In the US there

is a noticeable difference in trends for salaries (Figure 5.1, left panel) which carry an almost uninterrupted upward trajectory during the entire period, and total earnings (Figure 5.1, right panel) which underwent a large decline that started two years before the crisis, only to recover after 2009 but growing somewhat slower than before the crisis. The trends for the UK are slightly different; both salaries and total earnings peaked just before the crisis in 2007, experienced a correction which lasted for two years, and then, ever since 2010, continued to grow at almost the same pace as before the crisis (Figure 5.2). By 2015 both salaries and total earnings in the UK were higher than their pre-crisis peak. For the US this was only the case for executive salaries, as total earnings still haven't bounced back to their pre-crisis levels.

#### Change in individual executive earnings, US

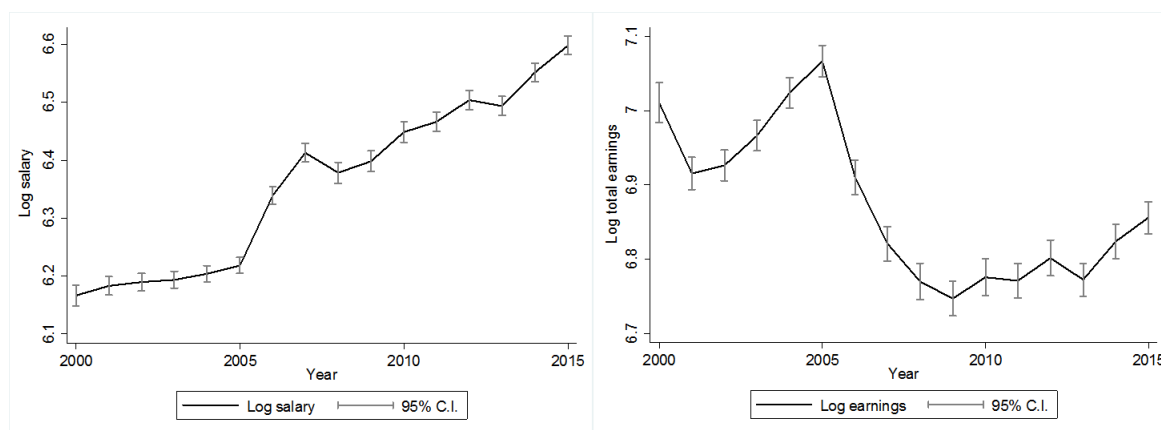


Figure 5.1: Annual changes in individual executive salaries (left panel) and total earnings (right panel), United States, 2000-2015. Logarithmic values of salaries and earnings are used.

Looking at the trends of top incomes on a firm level, similar conclusions can be

### Change in individual executive earnings, UK

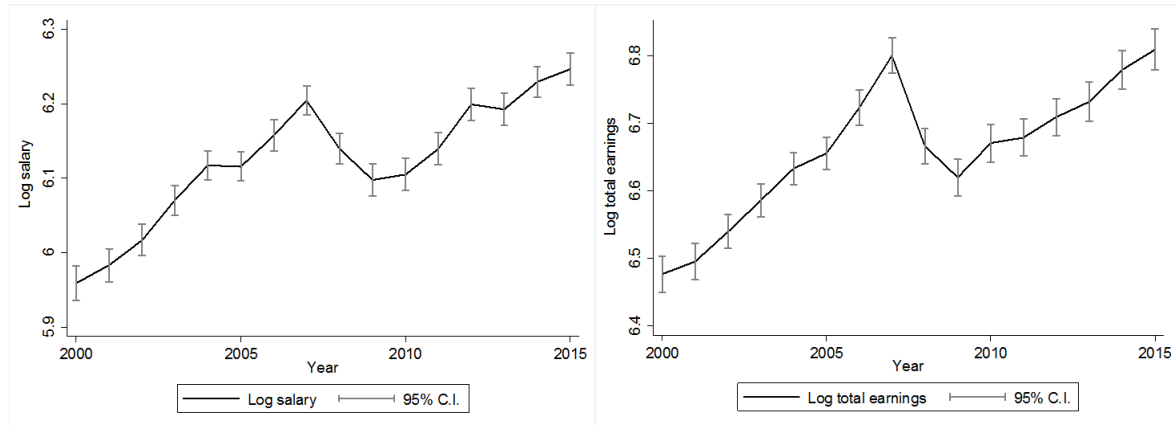


Figure 5.2: Annual changes in individual executive salaries (left panel) and total earnings (right panel), United Kingdom, 2000-2015. Logarithmic values of salaries and earnings are used.

inferred for the US: a steady rise of executive salaries (Figure 5.3, left panel), and a similar correction of executive total earnings (Figure 5.3, right panel) followed however by a stronger rebound after the crisis, reaching its pre-crisis peak in 2015. In the UK the firm-level data paints an entirely different picture (Figure 5.4). It suggests that both salaries and total earnings of executives have, at least on a firm level, been declining since 2000. However since 2009 the trend has picked up and is moving upwards again. The firm-level data in the UK suggests that average firm-level inequality has been gradually declining until after the crisis, in spite the fact that individual executive top incomes have been steadily rising in the same period. This fact is in alignment with the UK Gini coefficient and the UK top income share data (see Atkinson et al, 2017) which suggest stagnating income inequality during the observed 16 year period.

### Change in firm-level executive earnings, US

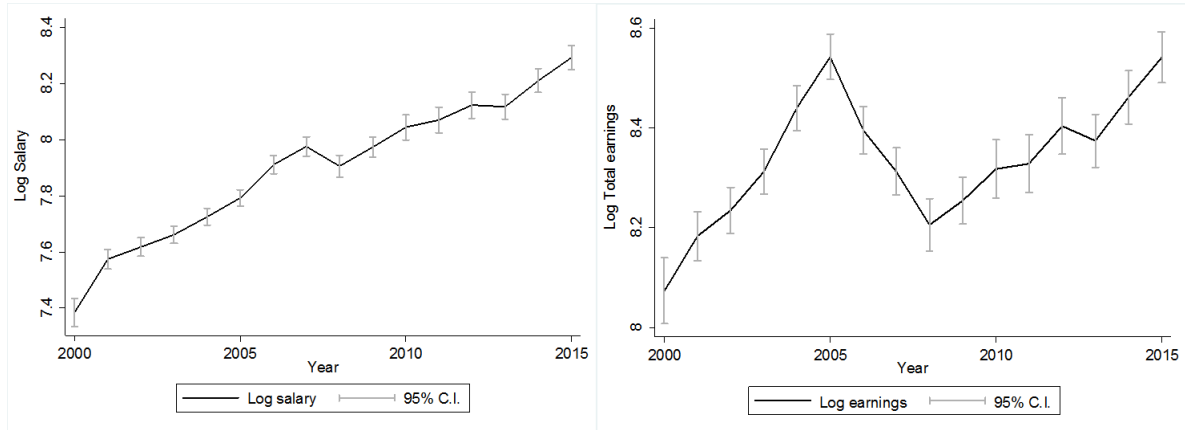


Figure 5.3: Annual changes in firm-level executive salaries (left panel) and total earnings (right panel), United States, 2000-2015. Logarithmic values of salaries and earnings are used.

### Change in firm-level executive earnings, UK

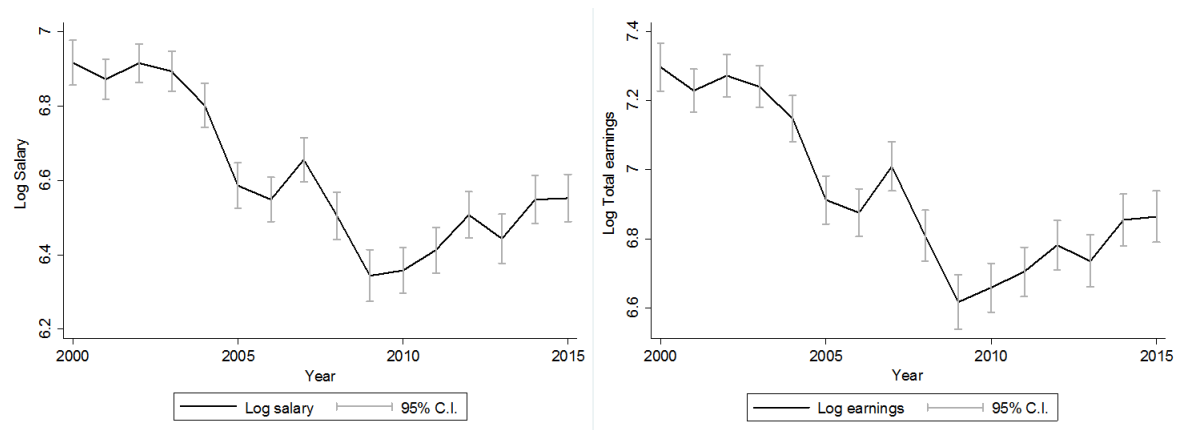


Figure 5.4: Annual changes in firm-level executive salaries (left panel) and total earnings (right panel), United Kingdom, 2000-2015. Logarithmic values of salaries and earnings are used.

Given that the focus of this research is to examine the impact of political connections on inequality, it would be useful to observe the same trends of increasing executive incomes with respect to individual and firm political connections, as defined in the previous section. Figures 5.5 and 5.6 decompose individual-level executive earnings by political connections (the main variable POLCON) for both countries, while figures 5.7 and 5.8 decompose firm-level executive earnings by political connections (again POLCON) for both countries. In all four cases the conclusion is identical: politically connected individuals and firms (solid black lines, denoted as 1) have had higher salaries and total earnings than unconnected individuals and firms (dashed grey lines, denoted 0) during the entire observed period. In some cases even the growth of earnings for connected executives was higher. For example, the trend of total executive earnings in the US (5.5, right panel) clearly shows that the growth of earnings for connected executives was faster than for non-connected executives pre-2005 but also since 2013. However according to only this evidence it is inconclusive whether political connections actually caused the increase in top incomes. It seems instead that the difference in top earnings by levels of political connections is a time-invariant characteristics of US and UK labour markets.

Other variations of this relationship with the POLCON variable unpacked by origin of connections (previous political experience, POLCON\_Gov or membership in the same organization, POLCON\_Org) are shown in the Appendix, and

### Individual executive earnings by political connections, US

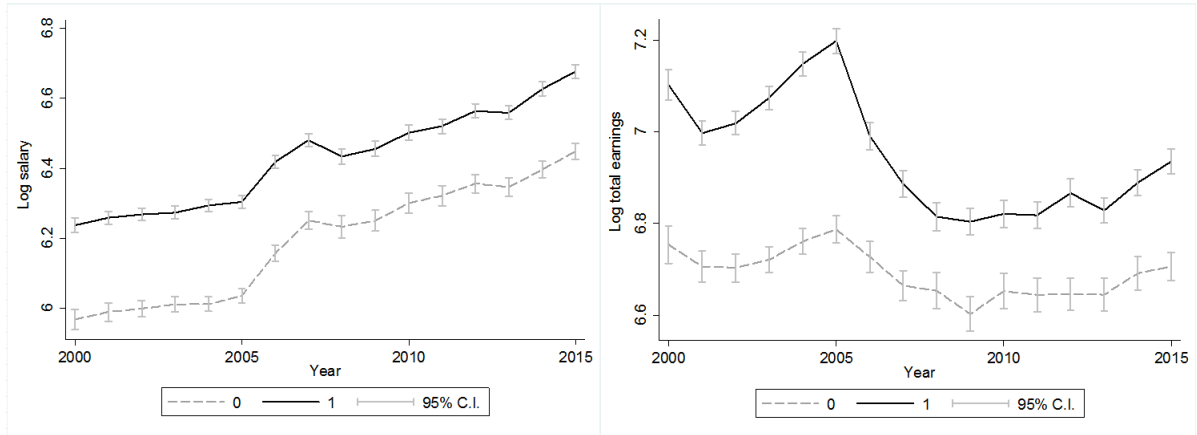


Figure 5.5: Annual changes in individual executive salaries (left panel) and total earnings (right panel), decomposed by political connections, United States, 2000-2015. Logarithmic values of salaries and earnings are used.

### Individual executive earnings by political connections, UK

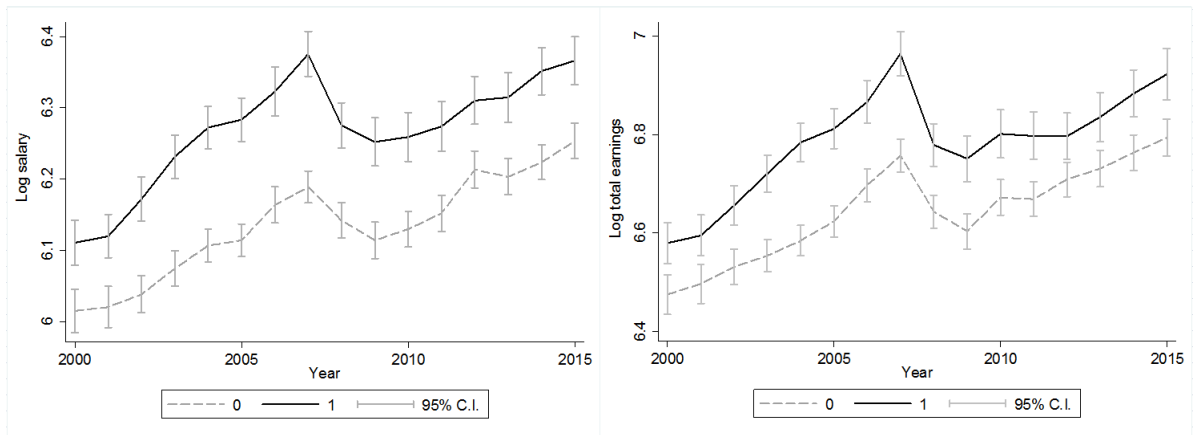


Figure 5.6: Annual changes in individual executive salaries (left panel) and total earnings (right panel), decomposed by political connections, United Kingdom, 2000-2015. Logarithmic values of salaries and earnings are used.

### Firm executive earnings by political connections, US

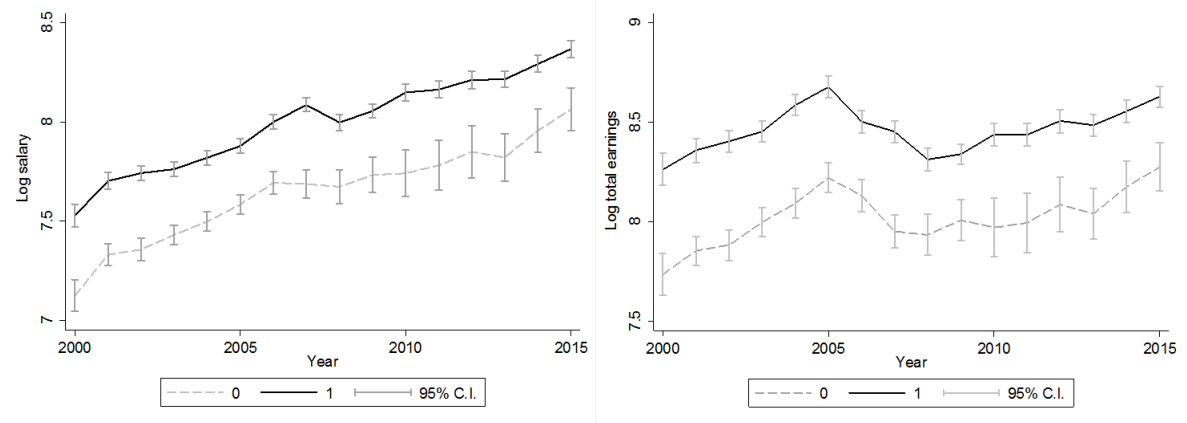


Figure 5.7: Annual changes in firm-level executive salaries (left panel) and total earnings (right panel), decomposed by political connections, United States, 2000-2015. Logarithmic values of salaries and earnings are used.

### Firm executive earnings by political connections, UK

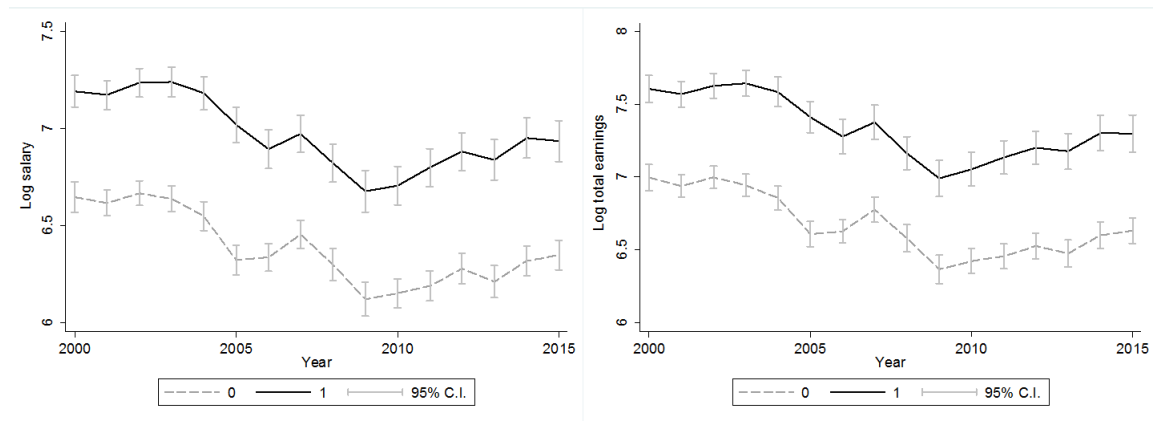


Figure 5.8: Annual changes in firm-level executive salaries (left panel) and total earnings (right panel), decomposed by political connections, United Kingdom, 2000-2015. Logarithmic values of salaries and earnings are used.

they all confirm the same story: no matter how we define political connections, being connected implies a higher *level* of executive salaries and total earnings. Figures 5.14 to 5.16 show the trends for salaries and earnings for the US, while Figures 5.17 to 5.19 show the same trends for the UK. Figures 5.14 and 5.17 look at the trends for absolute values of earnings and salaries for both countries, and they point to the same suggestion as their logarithmic versions in Figures 5.5 and 5.6. In the other 4 figures (5.15 and 5.16 for the US, and 5.18 and 5.19 for the UK) the left panel always shows same organization membership as the origin of a connection (the POLCON\_Org variable), while the right panel always shows previous government experience as the origin of a connection (the POLCON\_Org variable). These seem to suggest that in the US same organization membership is a more important driver of the earnings difference between connected and unconnected executives, while in the UK previous government experience drives a bigger wedge between connected and unconnected executive earnings.

## 5.4 Empirical strategy

It is difficult to impose a causal relationship between political connections, network size and executive earnings. First of all, a decision of a firm or individual to become connected and join an elite network is highly endogenous, meaning that connected individuals and firms self-select into the treatment group. Second, some firms are simply large in size and employ a lot of people in their local communities

which makes them more likely to capture the attention of politicians, regardless of whether they really benefit from any connections. And third, there could be a number of factors that affect both individual executive earnings and someone's incentive to join an elite network. For example individual ability or competence (or even likeability) makes someone more likely to earn a higher salary and at the same time more likely to engage with a larger network of other individuals. For firms, the decision on earnings and connections can be related to their relative stage in the life cycle. Firms in the later stages of the life cycle are less responsive to disruptive technologies which forces them to turn to political support and capturing rents in order to survive. In this case the position in the phase of the life cycle will determine the assignment of firms into connected and non-connected and will simultaneously also affect the earnings they pay to their executives.

Using fixed effects on a panel dataset for a period of 16 years helps resolve some of these difficulties. Fixed effects are useful in controlling for any time-invariant factors that cannot be observed (such as executive ability) or are difficult to measure precisely (firms that capture political attention; stage of the firm life cycle) but can be a source of endogeneity and between-unit differences. When observing the effect of changes in political connections or network size on the changes in earnings over time all such time-invariant differences get cancelled out allowing us to estimate a clear *within* effect. In this particular case I use firm-level fixed effects to estimate the effect of being politically connected vs being politically

unconnected within firms over time. I therefore answer the question of whether it pays off for an executive *within a firm* to be politically connected. The estimated effect is the marginal effect of an additional connected executive.

The logic behind including time fixed effects is similar — time fixed effects tend to capture all time-varying variables and trends that affect outcomes for all units in the same way. For example, in the figures above it was obvious that earnings growth was interrupted during the crisis. Including time fixed effects controls for this as the crisis was an event that had an impact on all units in the same way. The following regression is estimated for individual-level data for both countries.

$$\log W_{it} = \alpha_m + \gamma_t + \tau_1 POLCON_{it} + \tau_2 d(g)_{it} + \mathbf{X}\vartheta + \eta_{it} \quad (5.1)$$

The outcome variable  $\log W_{it}$  indicates either *log salary* or *log total earnings* for individual  $i$  in year  $t$ . The two main explanatory variables denote the indicator of political connections for an individual,  $POLCON_{it}$ , and the size of his or her network, or the degree of the network,  $d(g)_{it}$  where  $g$  represents the total network. These two estimates represent the rent-seeking wage premium.  $\alpha_m$  is the firm fixed effect and  $\gamma_t$  is the year fixed effect. Finally,  $\mathbf{X}$  represents a set of covariates which include education level, age, gender, total experience (time in current firm plus time in other firms), total number of boards the person sits on, bonus ratio (size of bonus relative to total earnings), and equity ratio (size of equity-based compensation relative to total earnings).

The same equation is estimated for firm-level data for both countries, using the same indicators and covariates but aggregated on a firm ( $m$ ) rather than an individual level. The only difference is that two versions of the POLCON variable are used, an indicator version  $POLCON_{mt}$ , and a continuous version  $POLCON_{total}$ .

$$\log W_{mt} = \alpha_m + \gamma_t + \pi_1 POLCON_{total,mt} + \pi_2 d(g)_{mt} + \mathbf{X}\vartheta + \eta_{mt} \quad (5.2)$$

The validity of the fixed effects approach rests upon the assumption that any unobservable factors related to the treatment are indeed fixed over time. The estimation tends to control for a number of factors that could affect executive earnings on both an individual and firm level, plus it tends to cancel out unobservable factors that remains fixed over time, but it can still be vulnerable to any unobservable factor that does vary across time and is not controlled out by the time fixed effect. Also with micro-level data there is always a potential problem with measurement error. If there is measurement error in the data the additive random errors of the main explanatory variables could bias the estimated coefficient towards zero.

In order to at least partially address these concerns on the firm level I use industry-level averages of political connections as firm-level instruments, a strategy suggested by Angrist and Krueger (2001) to resolve measurement error issues, and applied by Fisman and Svensson (2007) and Desai and Olofsgard (2011) to

control for firm-level unobservables. This strategy suggests taking average levels of connections (when defined as a continuous variable,  $POLCON_{total}$ ) across each industry and use it as an IV for firm-level connections within that industry. The assumption is that any variation across industries is not likely to be driven by unobserved firm-specific factors, but rather by industry-level characteristics that are likely to be exogenous to firm-level choices over its wage structure. In other words, all firm-level unobservables that are likely to be correlated with its executives' earnings (and that vary over time) are not affecting the outcome if we use industry-level connections. Given that the IV is an industry-level variable the variation only remains on an industry-level, rather than firm-level. This is the biggest disadvantage of the proposed IV approach.

The first stage is satisfied given that firm-level connections are correlated to industry-level connections, as any firm-level influence will depend on its industry-specifics (such as the regulatory status of that industry, its strategic importance to politics, sensitivity to foreign shocks, etc.). The 2SLS estimation procedure is used, where equation 5.3 estimates the first stage relationship while equation 5.4 estimates the second stage relationship:

$$POLCON_{total,mt} = \alpha + \rho IND_{total,mt}^{POLCON} + \mathbf{X}\vartheta + \varepsilon_{mt} \quad (5.3)$$

$$\log W_{mt} = \alpha + \rho \widehat{POLCON}_{total,mt} + \mathbf{X}\vartheta + \varepsilon_{mt} \quad (5.4)$$

Where  $IND_{total,mt}^{POLCON}$  is the industry-level average of political connections for each firm  $m$  in year  $t$ . All the other variables are the same as in equation 5.2.

The summary statistics for all four cases are given in the Appendix, in Tables 5.11 for US individual-level data, 5.12 for US firm-level data, 5.13 for UK individual-level data, and 5.14 for UK firm-level data.

## 5.5 Results

The results section reports the results separately first for the US, then for the UK. In each case I first present the individual-level effects estimated using fixed effects regressions, and then the firm-level effects estimated both using fixed effects regressions and the 2SLS estimator.

### 5.5.1 United States

Table 5.2 shows the results for the effect of political connections and network size on individual-level executive earnings in the US. The first two columns show results for log values of the outcome variables: log total earnings and log salary, while the final two columns show results for absolute levels of executive total earnings and salaries so as to get a better idea of the monetary value of the effect.

The results show a strong positive effect of both political connections and network size on individual executive total earnings and salaries. The interpretation of the main effect is focused on the difference between individuals within firms.

Table 5.2: Individual level executive earnings and political connections, United States

	(1)	(2)	(3)	(4)
	Log total earnings	Log salary	Total earnings	Salary
POLCON	0.119*** (0.00490)	0.121*** (0.00395)	153.9*** (15.99)	73.16*** (3.497)
Network size	0.0173*** (0.00154)	0.0163*** (0.00124)	30.51*** (5.027)	11.36*** (1.099)
Education	-0.00870*** (0.00210)	-0.00783*** (0.00169)	-9.934 (6.851)	-8.013*** (1.498)
Experience	0.00511*** (0.000224)	0.00564*** (0.000181)	7.030*** (0.732)	4.143*** (0.160)
All boards	0.0373*** (0.000581)	0.0308*** (0.000469)	69.14*** (1.897)	23.46*** (0.415)
Bonus ratio	1.514*** (0.0100)	-0.106*** (0.00810)	2907.1*** (32.75)	-39.37*** (7.162)
Equity ratio	0.116*** (0.00893)	0.145*** (0.00722)	51.89 (29.19)	74.20*** (6.383)
Gender	0.152*** (0.00780)	0.128*** (0.00630)	259.4*** (25.50)	88.89*** (5.576)
Age	0.0107*** (0.000331)	0.00952*** (0.000267)	21.60*** (1.081)	6.687*** (0.236)
Firm fixed effects	YES	YES	YES	YES
Time fixed effects	YES	YES	YES	YES
Observations	44071	44071	44071	44071
R squared	0.653	0.577	0.455	0.506

Standard errors shown in parentheses. \*\*\* denotes significance at 0.1%, \*\* at 1%, and \* at 5%.

Table 5.3: Individual level executive earnings and political connections by origin of connection, United States

	(1) Log total earnings	(2) Log total earnings	(3) Log total earnings	(4) Log salary	(5) Log salary	(6) Log salary
POLCON	0.119*** (0.00490)			0.121*** (0.00395)		
POLCON_Gov		0.0599*** (0.00665)			0.0546*** (0.00539)	
POLCON_Org			0.0963*** (0.00461)			0.101*** (0.00372)
Network size	0.0173*** (0.00154)	0.0231*** (0.00153)	0.0194*** (0.00153)	0.0163*** (0.00124)	0.0222*** (0.00124)	0.0181*** (0.00124)
Covariates	YES	YES	YES	YES	YES	YES
Firm fixed effects	YES	YES	YES	YES	YES	YES
Time fixed effects	YES	YES	YES	YES	YES	YES
Observations	44071	44071	44071	44071	44071	44071
R squared	0.653	0.648	0.651	0.577	0.569	0.575

Covariates same as in Table 5.2. Standard errors shown in parentheses. \*\*\* denotes significance at 0.1%, \*\* at 1%, and \* at 5%.

It helps us answer the question: does it pay off for an executive within a firm to be politically connected? Being a politically connected executive results in 12.6% total higher individual earnings (calculated as  $100 * [\exp(0.119) - 1]$  ), and about 12.8% higher salaries than non-connected executives. For an average executive compensation of around \$1.18 million per year, this corresponds to an annual pay rise of about \$150,000. The absolute value of the effect (estimated in columns 3 and 4) is similar. A switch from a non-connected to a connected individual within a firm increases total earnings by almost \$154,000, while it increases salaries by \$73,160. The substantial size of the estimated effect in each case suggests that it certainly does pay off to be politically connected within publicly listed firms in the US.

The network size effect also has to be taken into account, even though it is more modest. The variable network size was divided by 1000 for easier interpretation of the coefficients. The results from Table 5.2 suggest that for every increase in an individual's network size by 1000 people (which is two thirds of the standard deviation increase of 1435), salaries and total compensation go up by around 1.7%. In absolute terms this corresponds to \$30,500 higher total compensations and \$11,360 higher salaries. Given that it isn't likely for executives to expand their networks by 1000 people each year, the network size effect is even more modest. It is however suggestive that better connected individuals do carry a greater weight in firms' reward structures. It just happens that political connections carry an even

greater one. The main driver of the rent-seeking premium are therefore direct political connections. The size of this premium for individual executives is about \$70,000 in salaries, and about \$150,000 in total earnings.

The results reported in Table 5.2 only look at the total value of the POLCON variable. Table 5.3 unpacks the POLCON by the origin of an executive's connection — is it due to his or her previous experience in government, or is it due to membership in the same organization as the relevant politician? All three cases are reported, where columns (1) and (4) merely repeat the same findings as columns (1) and (2) in Table 5.2. In the US case it is clear that the *same organization membership effect* is stronger than the *previous experience in government effect*. In particular, a political connection of an executive resulting from previous government experience increases total compensation by 6.2%, and salaries by 5.6%. On the other hand a political connection of an executive resulting from membership in the same organization as a politician increases total compensation by 10.1%, and total salaries by 10.6%, which is closer to the overall POLCON effect. Clearly both of these forms of connections increase executive earnings, however, at least in the US case, being member of the same social group as a politician is more important than being connected to them through previous jobs.

The same disentangled effect is estimated in absolute terms in Table 5.15 in the Appendix, and even though it implies the same conclusion for salaries — \$60,000 higher salary for the organization effect and \$40,000 for the experience effect — it

suggests the opposite for total compensation. Total earnings increase by \$165,000 for an executive with previous government experience, and by only half as much for an executive which is a member of the same organization as a politician. The network effect in each case is similar to the one estimated in Table 5.2.

Table 5.4 presents the estimates of the same relationship but on a firm level where all the main variables and covariates are aggregated from the individual-level dataset. The table does not show results for absolute level earnings any more, but instead shows versions with and without time fixed effects. Many versions were estimated using different combinations of the covariates and interaction terms, and different versions of the outcome variables, but the results remain robust in each case. They yield an almost identical conclusion to the individual-level estimates: political connections on a firm level carry a strong, significant, and positive effect on top executive earnings.

The only difference is that the effect is more modest on a firm level, which is expected. Top earnings across all executives in the board should even out and produce a smaller effect than when looking only at individuals. A politically connected firm compared to a politically unconnected firm pays its executives between 2.5 and 2.7% more in total earnings, and between 1.8 and 2% more in salaries. Although this seems modest in absolute terms it translates to about \$30,000 higher annual earnings paid to executives in connected firms. The network effect is similar as before, between 2.3 and 2.7% higher total earnings, and around

Table 5.4: Firm-level executive earnings and political connections, United States

	(1)	(2)	(3)	(4)
	Log total earnings	Log total earnings	Log salary	Log salary
POLCON Firm	0.0269*** (3.87)	0.0248*** (3.68)	0.0197*** (3.55)	0.0182** (3.42)
Firm network size	0.027*** (15.97)	0.023*** (13.74)	0.022*** (16.82)	0.018*** (14.47)
Average education	-0.0036 (-0.13)	-0.0468 (-1.81)	-0.0125 (-0.55)	-0.0502* (-2.34)
Average experience	0.0301*** (12.19)	0.013*** (4.70)	0.0314*** (14.77)	0.0155*** (6.40)
All boards	0.011* (2.03)	0.0001 (0.03)	-0.004 (-0.90)	-0.0135** (-3.01)
Bonus ratio	2.642*** (37.88)	2.817*** (37.21)	0.121** (2.69)	0.3711*** (7.90)
Equity ratio	0.1051*** (3.24)	0.0155 (0.49)	0.149*** (5.68)	0.0782** (3.17)
Gender ratio	-0.0403 (-0.51)	0.0842 (1.11)	0.0018 (0.03)	0.0978 (1.64)
Average age	-0.0142*** (-6.19)	0.0159*** (5.21)	-0.029*** (-14.77)	-0.0004 (-0.16)
Firm fixed effects	YES	YES	YES	YES
Time fixed effects	NO	YES	NO	YES
Observations	12810	12810	12809	12809
R squared	0.462	0.5091	0.3512	0.4211

T-value shown in parentheses. \*\*\* denotes significance at 0.1%, \*\* at 1%, and \* at 5%.

2% higher salaries for every increase in network size by 1000 people.

Finally, the IV estimates on a firm level support the earlier conclusions. They also indicate that firm-level political connections and network size exhibit a large and positive effect on top executive earnings in the US. In Table 5.5 the first two columns report the estimated marginal effect of using political connections as a continuous variable, counting the total number of politically-connected board members for each firm. The final two columns use the indicator value of firm connections, as before.

The IV estimates, correcting for potential measurement errors by using industry-level connections as an IV, produce much higher effects than reported in Tables 5.2 and 5.4<sup>6</sup>. According to the first two columns, the more politically connected directors a firm had, the greater their earnings. In particular by adding only a single director that is politically connected to a firm's board (which is a bit smaller than a one standard deviation increase of 1.5) yields a 16.5% higher salary and 22% higher total executive earnings for that firm. The indicator value estimates of political connections of a firm are even larger. They suggest that by switching from an unconnected to a connected firm (by hiring a director with political connections), total executive earnings in that firm will go up by 132%, while salaries will go up by 87%. In both cases this is an enormous effect, much larger than the

---

<sup>6</sup>This shouldn't be surprising given the way I define the instrument with an emphasis on reducing the measurement error. Angrist and Krueger (2001) suggest that in cases where there the explanatory variable is likely to be biased due to measurement error, the estimated coefficients of standard OLS regressions would be biased towards zero. In this case this means that the non-IV estimates are likely to be lower than when correcting for potential measurement error using industry-level averages of the explanatory variable.

Table 5.5: IV estimates of firm-level executive earnings and political connections, United States

	(1)	(2)	(3)	(4)
	Log total earnings	Log salary	Log total earnings	Log salary
POLCON total	0.221*** (14.02)	0.165*** (12.54)		
POLCON Firm			0.842*** (13.67)	0.628*** (12.18)
Firm network size	0.018*** (13.77)	0.016*** (14.59)	0.025*** (25.46)	0.021*** (25.63)
First stage	2.655*** (30.72)	2.654*** (30.70)	0.6967*** (25.70)	0.6959*** (25.67)
Average education	-0.164*** (-8.61)	-0.124*** (-7.09)	-0.176*** (-8.66)	-0.132*** (-7.15)
Average experience	0.0221*** (18.95)	0.0223*** (22.36)	0.0169*** (12.96)	0.0184*** (16.72)
Total boards	0.0131*** (4.15)	-0.0003 (-0.13)	0.0101** (3.09)	-0.0026 (-0.89)
Bonus ratio	2.740*** (50.03)	0.0283 (0.74)	2.736*** (49.37)	0.0260 (0.66)
Equity ratio	0.411 (15.43)	0.441 (19.21)	0.383 (13.89)	0.420 (17.93)
Gender ratio	-0.467*** (-7.90)	-0.521*** (-10.39)	-0.428*** (-6.85)	-0.492*** (-9.30)
Average age	-0.0067*** (-3.39)	-0.014*** (-8.34)	-0.0047** (-2.43)	-0.0126*** (-7.68)
Observations	12810	12809	12810	12809
R squared	0.415	0.350	0.362	0.296

T-value shown in parentheses. \*\*\* denotes significance at 0.1%, \*\* at 1%, and \* at 5%.

very modest ones reported in Table 5.4. The network size effect remains roughly the same however, between 1.6% and 2.5% for salaries and earnings. As with the case of individuals, direct political connections seem to be the biggest contributor to the wage premium.

### 5.5.2 United Kingdom

For the UK we once again start with individual-level results shown in Table 5.6. As before the first two columns show results for log values of total earnings and salaries, while the final two columns show results for absolute values of the outcome variables.

Similar to the US individual-level results, the UK estimates also suggest a strong positive effect of political connections on individual executive earnings. The effects, however, are smaller than in the US. A politically-connected executive in the UK gets a 4.7% higher total compensation and a 5.5% higher salary than a non-connected executive within the same firm. In the UK sample the average executive total compensation was £965,000, meaning that being politically connected can result in a raise of almost £50,000 annually. The absolute numbers suggest an even larger effect, around £90,000 more in total earnings, and almost £70,000 in salaries.

What is surprising in the UK is that the network effect for individuals vanishes once political connections are involved, and even points to an opposite direction

Table 5.6: Individual level executive earnings and political connections, United Kingdom

	(1)	(2)	(3)	(4)
	Log total earnings	Log salary	Total earnings	Salary
POLCON	0.0466*** (0.00618)	0.0540*** (0.00552)	89.39** (34.47)	69.48* (29.70)
Network size	-0.00307 (0.00238)	-0.00236 (0.00213)	2.827 (13.28)	-10.45 (11.44)
Education	0.00582* (0.00248)	0.0127*** (0.00222)	-3.451 (13.82)	4.329 (11.91)
Experience	0.00628*** (0.000333)	0.00662*** (0.000297)	6.025** (1.856)	2.681 (1.599)
All boards	0.00135*** (0.000254)	0.00103*** (0.000227)	3.282* (1.415)	1.034 (1.220)
Bonus ratio	1.417*** (0.0129)	0.0416*** (0.0116)	1808.0*** (72.22)	30.91 (62.22)
Equity ratio	0.273*** (0.0109)	0.231*** (0.00973)	271.6*** (60.75)	126.6* (52.34)
Gender	0.131*** (0.0128)	0.137*** (0.0114)	145.6* (71.25)	71.36 (61.39)
Age	0.00468*** (0.000412)	0.00465*** (0.000369)	8.153*** (2.301)	3.422 (1.982)
Firm fixed effects	YES	YES	YES	YES
Time fixed effects	YES	YES	YES	YES
Observations	27236	27236	27236	27236
R squared	0.646	0.550	0.155	0.124

Standard errors shown in parentheses. \*\*\* denotes significance at 0.1%, \*\* at 1%, and \* at 5%.

Table 5.7: Individual level executive earnings and political connections by origin of connection, United Kingdom

	(1) Log total earnings	(2) Log total earnings	(3) Log total earnings	(4) Log salary	(5) Log salary	(6) Log salary
POLCON	0.0466*** (0.00618)			0.0540*** (0.00552)		
POLCON_Gov		0.0720*** (0.00907)			0.0793*** (0.00811)	
POLCON_Org			0.0338*** (0.00601)			0.0414*** (0.00538)
Network size	-0.00307 (0.00238)	0.00223 (0.00220)	-0.00109 (0.00237)	-0.00236 (0.00213)	0.00388* (0.00197)	-0.000396 (0.00212)
Covariates	YES	YES	YES	YES	YES	YES
Firm fixed effects	YES	YES	YES	YES	YES	YES
Time fixed effects	YES	YES	YES	YES	YES	YES
Observations	27236	27236	27236	27236	27236	27236
R squared	0.646	0.646	0.646	0.550	0.550	0.549

Covariates same as in Table 5.6. Standard errors shown in parentheses. \*\*\* denotes significance at 0.1%, \*\* at 1%, and \* at 5%.

(implying that having a larger network hurts an executive's earnings), however in neither case is it statistically significant. This surprising result needs to be examined more closely by again unpacking the origin of an executive's political connections. Is it due to one's strong social network built in clubs, societies, and NGOs, or is it due to one's previous government experience where all the important political connections were initially established? Table 5.7 seems to suggest that the latter is the case in the UK, exactly the opposite to the US. Previous government experience in the UK yields a stronger effect than the overall POLCON estimate — it increases total compensation by 7.5% and salaries by 8.25% (about £75,000 on average). The organization membership effect is smaller than the overall POLCON estimate — it increases total compensation by 3.4% and salaries by 4.2% (about £38,000 on average). Table 5.16 in the Appendix estimating the disentangled effect in absolute terms makes an even stronger case. The previous government experience effect increases total executive earnings by £230,000 annually, and salaries by £135,000 annually. The organization membership effect in absolute terms is not even statistically significant. The wage premium in the UK is therefore fully explained by political connections derived from previous government experience.

These findings could help explain the insignificance of the network effect in the UK. Clearly the predominant way political connections are formed in the UK is through direct government experience. The clubs, societies, and various organizations may attract a large membership but these are most likely clustered

around the same type of people. In other words, it is less likely in the UK that a club of corporate executives invites a politician to be a member, than it is in the US.

Jointly these results confirm the intuition from the US case that political connections are important determinants of executive earnings, and that political connections matter far more than the pure size of one's network. In the US, political connections formed through memberships in same organizations are the key drivers of higher earnings for executives, while in the UK previous government experience is more likely to increase an executive's earnings.

Table 5.8 presents the same relationship on a firm-level. As in the US case it does not report absolute earnings any more, but only different estimates with and without time fixed effects. All possible calculations and combinations produced similar effects yielding the same conclusion as in the US case and in line with the UK individual-level findings: political connections within a firm yield a positive and significant effect on top executive earnings. What is interesting is that in all four cases reported here (with and without time fixed effects for salaries and total earnings), the effect is the same: around 10%. This is significantly higher than the modest firm-level estimates in the US, but in line with the results for individual level estimates in the UK. Switching from a politically unconnected to a politically connected firm increases both total earnings and salaries of firm executives by an average 10%. In absolute terms this translates to almost £100,000 higher executive

Table 5.8: Firm-level executive earnings and political connections, United Kingdom

	(1)	(2)	(3)	(4)
	Log earnings	Log earnings	Log salary	Log salary
POLCON Firm	0.0982*** (8.46)	0.0994*** (8.61)	0.0981*** (8.70)	0.0988*** (8.76)
Firm network size	0.056*** (17.04)	0.055*** (16.32)	0.052*** (16.31)	0.051*** (15.76)
Average education	0.0033 (0.15)	-0.0008 (-0.03)	-0.009 (-0.41)	-0.0115 (-0.52)
Average experience	0.0257*** (9.04)	0.022*** (7.22)	0.0278*** (10.46)	0.0257*** (8.98)
Total boards	-0.0017 (-0.69)	-0.0023 (-0.92)	-0.004 (-1.61)	-0.004 (-1.75)
Bonus ratio	2.847*** (33.08)	2.864*** (33.41)	0.899*** (12.15)	0.909*** (12.36)
Equity ratio	0.0617 (1.21)	0.0751 (1.47)	0.0853 (1.61)	0.0932 (1.77)
Gender ratio	-0.0663 (-0.82)	-0.0322 (-0.40)	-0.0004 (-0.01)	0.0198 (0.25)
Average age	-0.0103*** (-4.28)	-0.0051 (-1.62)	-0.010*** (-4.25)	-0.0068* (-2.28)
Firm fixed effects	YES	YES	YES	YES
Time fixed effects	NO	YES	NO	YES
Observations	19835	19835	19835	19835
R squared	0.234	0.236	0.116	0.116

T-value shown in parentheses. \*\*\* denotes significance at 0.1%, \*\* at 1%, and \* at 5%.

annual earnings.

The network effect is now statistically significant and points to the right direction, however its effect is still modest compared to the political connection effect. Across all four estimates it stands around 5%, meaning that a 1000 people increase in network size increases executives' salaries and earnings around 5% annually on average. This may seem like a fairly decent effect size, however we must keep in mind that not many executives can expand their network by a 1000 people each year. This implies that the overall effect of adding individuals to one's network is almost negligible. Connections matter more on a firm level than an individual level in the UK, although the total size of the network effect is still not as impactful as the effect of political connections.

The IV estimates for UK firms are more modest than the corresponding estimates in the US case, although they also confirm all the earlier made conclusions. Results are presented in Table 5.9 where the first two columns report the effect when using political connections as a continuous variable, counting the total number of politically-connected board members for each firm, while the final two columns use the indicator value of firm connections.

Even though they are smaller than the corresponding US IV estimates, they do support the UK firm-level findings from Table 5.8. According to the first two columns, adding one politically connected director increases salaries by 16% and total earnings by 12.5%. The estimates using the indicator value of political

Table 5.9: IV estimates of firm-level executive earnings and political connections, United Kingdom

	(1)	(2)	(3)	(4)
	Log earnings	Log salary	Log earnings	Log salary
POLCON total	0.125** (3.26)	0.164*** (4.52)		
POLCON Firm			0.250*** (3.30)	0.329*** (4.58)
Firm network size	0.097*** (27.08)	0.084*** (24.99)	0.11*** (40.11)	0.089*** (38.13)
First stage	1.328*** (29.20)	1.328*** (29.20)	0.6638*** (24.35)	0.6638*** (24.35)
Average education	0.0287* (2.16)	0.0155 (1.22)	0.0218 (1.54)	0.00644 (0.47)
Average experience	0.0186*** (11.78)	0.0188*** (12.34)	0.0187*** (11.82)	0.0189*** (12.39)
Total boards	-0.012*** (-9.64)	-0.012*** (-10.77)	-0.012*** (-9.76)	-0.012*** (-10.92)
Bonus ratio	5.515*** (78.62)	3.334*** (49.88)	5.483*** (76.75)	3.292*** (48.43)
Equity ratio	0.924*** (17.65)	0.899*** (17.16)	0.917*** (17.62)	0.891*** (17.12)
Gender ratio	0.236*** (3.35)	0.291*** (4.31)	0.240*** (3.40)	0.295*** (4.38)
Average age	-0.0107*** (-6.10)	-0.0125*** (-7.38)	-0.0109*** (-6.11)	-0.0127*** (-7.42)
Observations	19835	19835	19835	19835
R squared	0.523	0.418	0.523	0.417

T-value shown in parentheses. \*\*\* denotes significance at 0.1%, \*\* at 1%, and \* at 5%.

connections are higher, however much more in line with all the earlier presented results. Switching from a non-connected to a connected firm increases total executive earnings in a firm by 28%, and executive salaries by an average 39%. This is a large effect indeed, but nowhere near the US levels of 87% and 132%. Taking all this into account, it could be that the US firm-level IV estimates for the indicator of political connections are slightly overestimated, and can thus be considered an outlier.

What explains such results overall? Executives who join firms coming from high-level government positions and who are members of elite networks tend to be more valued and hence better compensated by their firms. One of the obvious reasons for this, as shown already in Table 5.1, is that politically connected individuals have larger personal networks. In the business world where networking is an extremely important feature of securing new jobs and helping the company succeed, this is a valuable asset that tends to be rewarded. However the network effect is much more modest than the political connections effect, and for the UK it is not even statistically significant. It seems that in the UK social networks matter less than direct links to the government. They still do matter, particularly on a firm level, but their impact in the US is much stronger and hence more important.

## 5.6 Conclusion

In this paper I untangle the direct impact of political connections on income inequality. I observe how being a member of elite networks, representing the collusion of interests between in-office politicians and senior executives of publicly listed companies, affects the earnings of executives that are members of such networks. An elite network is measured directly, by examining the career trajectories of senior corporate executives and where and how they intertwine with politicians. I use two main definitions of connections; (1) if an executive had direct previous experience in senior government positions, and (2) if an executive is a member of the same social group as the politician (which includes various clubs, societies, professional organizations, church groups, trusts and foundations, charities, NGOs, political parties, etc.).

I use 16 years of individual-level and firm-level data for two countries with the highest share of top 1% earnings in total incomes — the United States and the United Kingdom — to confirm the hypothesis that senior executives within firms do get paid a wage premium if they are politically connected, compared to their non-connected colleagues. This wage premium is large and quite consistent over time. It is estimated to be around \$150,000 of total compensation in the US and around £90,000 of total compensation in the UK. Even though the network effect is an important determinant of the wage premium, it's main contributor are direct connections between executives and politicians. There is a difference between

the US and the UK — direct connections to politics via previous government experience matter much more in the UK than in the US. In the US connections via same organization membership matter more in driving the wage premium. This suggests that in order to extract rents in the UK in the forms of exclusive government contracts one should have some experience in government beforehand. In the US one is better off in joining a social group with the politician they intend to lobby.

The findings of this paper should add yet another important but thus far overlooked factor to the income inequality debate — the role of social networks formed on the highest level between executives and politicians. One of the biggest causes of increasing income inequality over the past 30 years have been rising earnings of the top 1% of income earners. Most of these are executives of publicly listed companies, and their earnings, at least ever since 2000, have been characterized by a wage differential resulting from direct political connections.

## Appendix

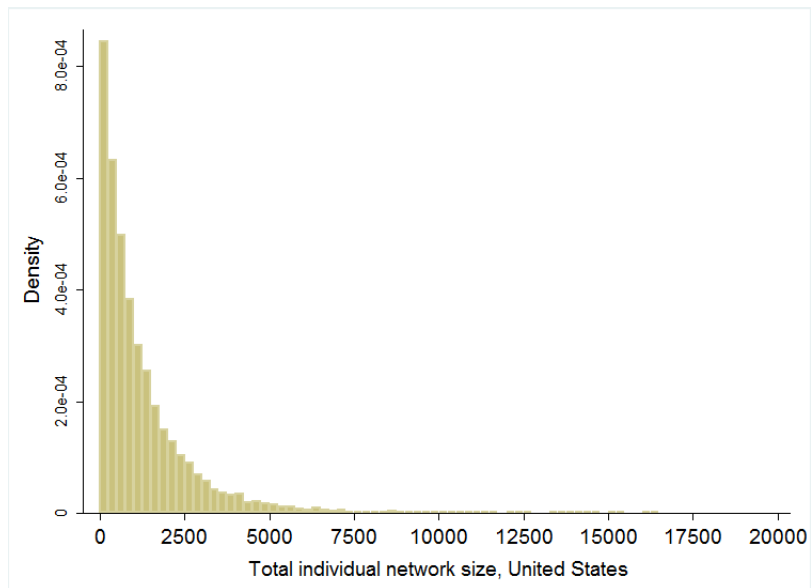


Figure 5.9: Distribution of total individual network size for the United States.

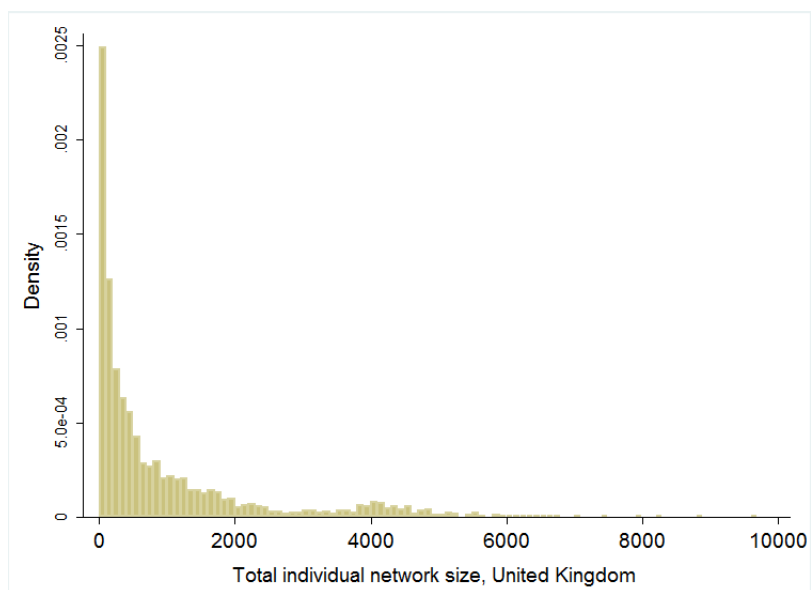


Figure 5.10: Distribution of total individual network size for the United Kingdom.

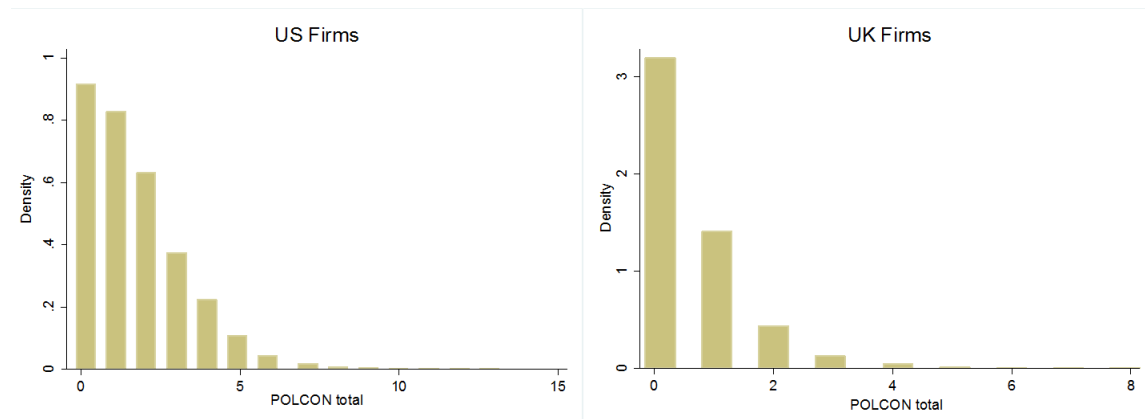


Figure 5.11: Distribution of all politically connected executives by firm for US and UK.

Table 5.10: Descriptive statistics for individual network size with respect to different origins of connections: previous government experience and membership in same organization as politician

	Mean	St.Dev.	Min	Max	N
United States network size					
Previous government experience = 1	1978.4	1883	19	15269	4491
Previous government experience = 0	1203.4	1368.9	10	16338	40933
Membership in same organizations = 1	1519.7	1554.2	13	16338	30335
Membership in same organizations = 0	798.1	1046.6	10	14021	15089
United Kingdom network size					
Previous government experience = 1	1524.3	1547.8	9	9669	2462
Previous government experience = 0	780.1	1192.5	4	9792	28530
Membership in same organizations = 1	1489.9	1539.4	7	9792	11912
Membership in same organizations = 0	433.0	769.7	4	9669	19080

### Distribution of executive earnings, US

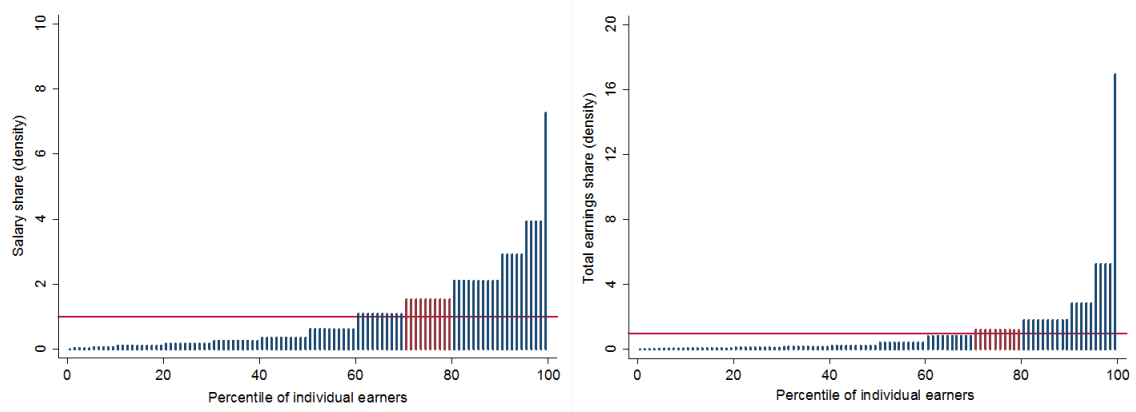


Figure 5.12: Percentile histogram for individual salaries (left panel) and individual total earnings (right panel), United States

### Distribution of executive earnings, UK

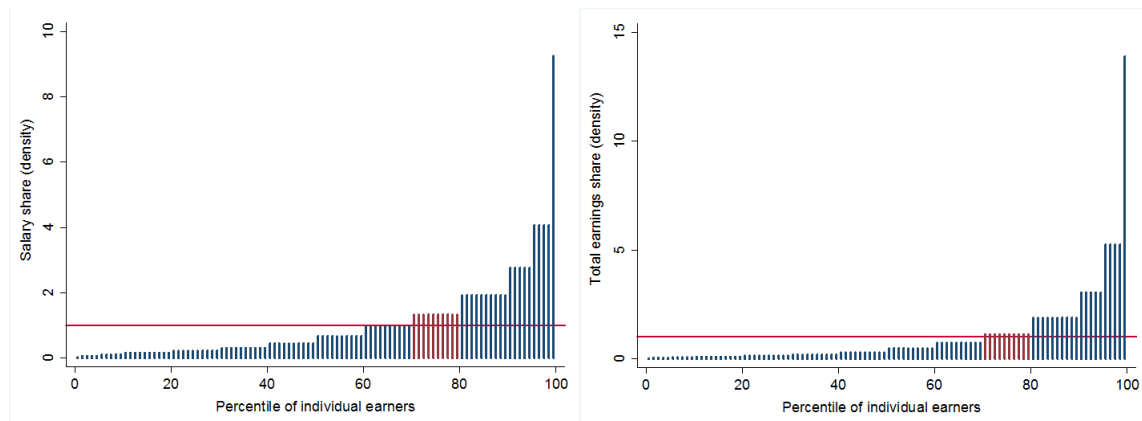


Figure 5.13: Percentile histogram for individual salaries (left panel) and individual total earnings (right panel), United Kingdom

### Absolute compensation and salary for individuals, US

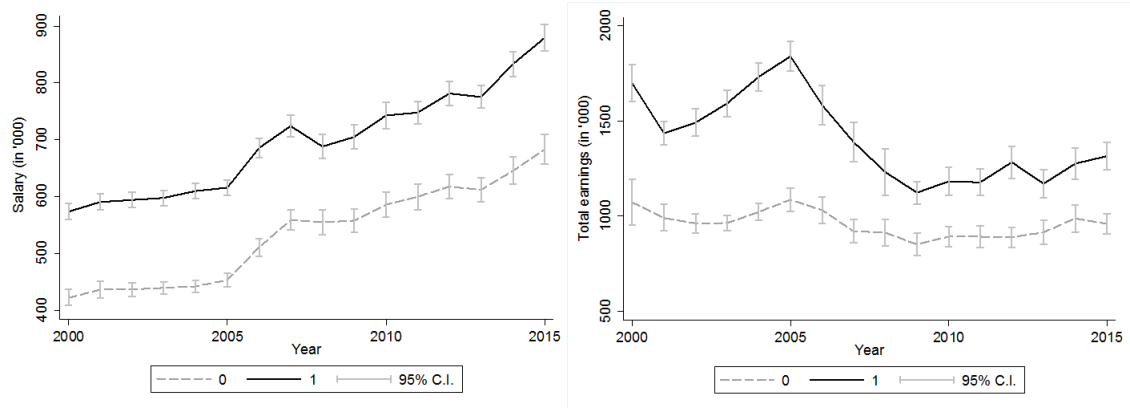


Figure 5.14: Total annual salary (left panel) and total annual earnings (right panel) by political connections, United States

### Log earnings by origin of political connections, US

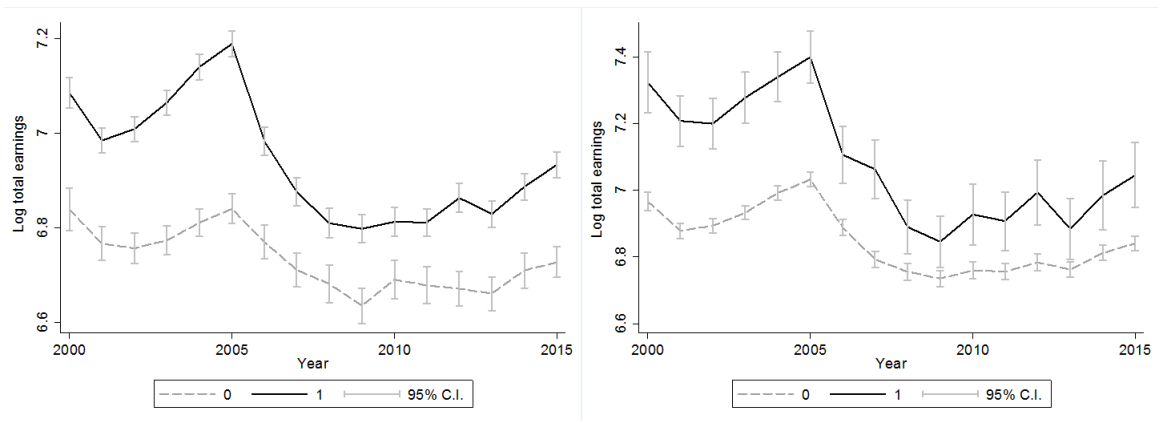


Figure 5.15: Log earnings when political connections are caused by same organization membership (left panel) and log earnings when political connections are caused by previous government experience (right panel), United States

### Log salary by origin of political connections, US

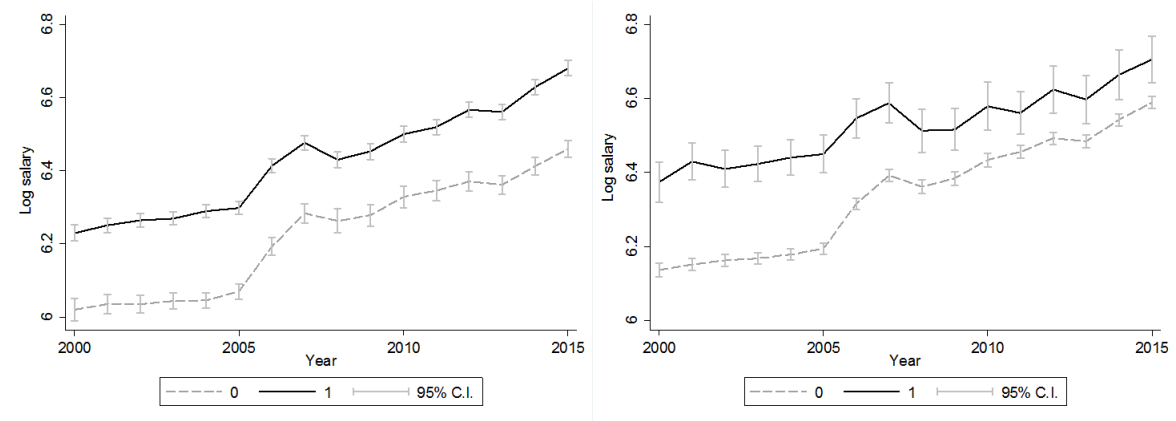


Figure 5.16: Log salary when political connections are caused by same organization membership (left panel) and log salary when political connections are caused by previous government experience (right panel), United States

### Absolute compensation and salary for individuals, UK

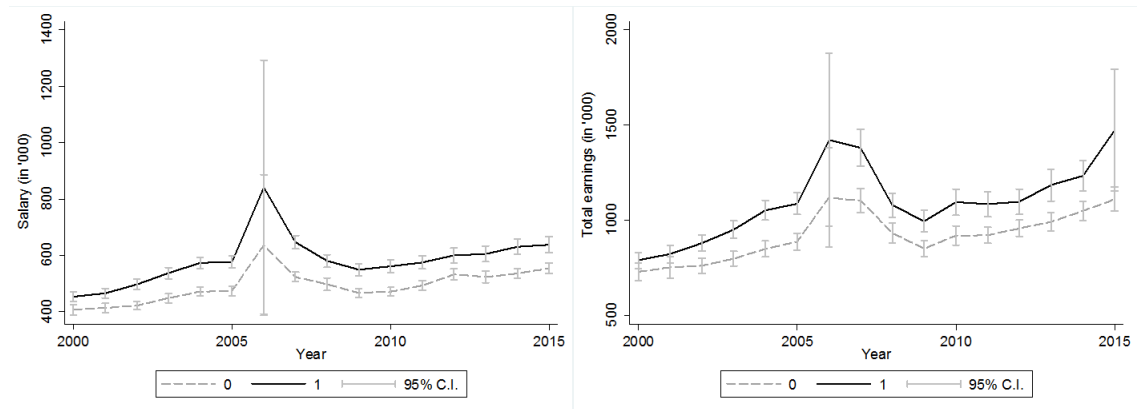


Figure 5.17: Total annual salary (left panel) and total annual earnings (right panel) by political connections, United Kingdom

### Log earnings by origin of political connections, UK

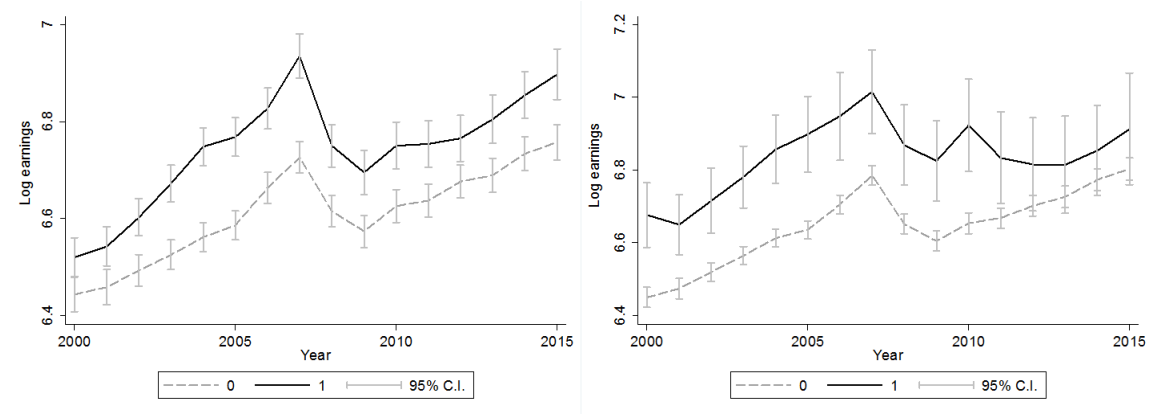


Figure 5.18: Log earnings when political connections are caused by same organization membership (left panel) and log earnings when political connections are caused by previous government experience (right panel), United Kingdom

### Log salary by origin of political connections, UK

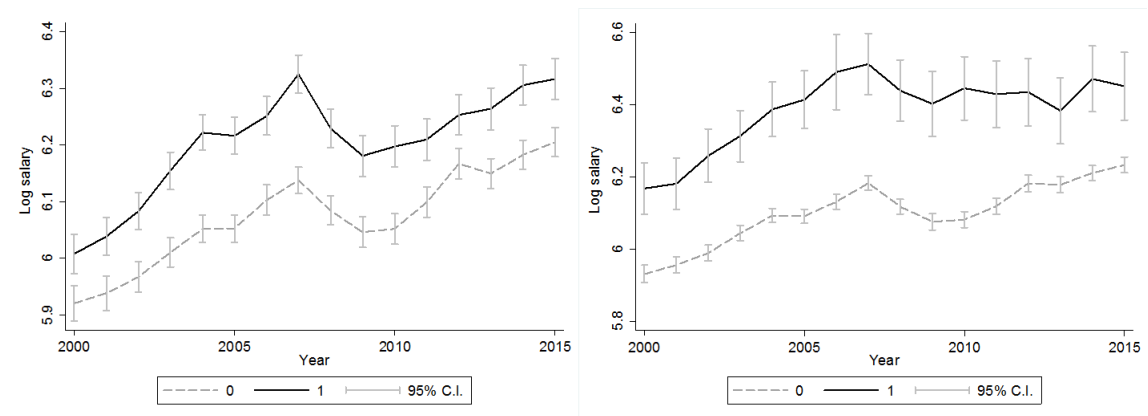


Figure 5.19: Log salary when political connections are caused by same organization membership (left panel) and log salary when political connections are caused by previous government experience (right panel), United Kingdom

Table 5.11: Summary statistics for individual-level data, United States

<b>Variable</b>	<b>Mean</b>	<b>Std. Dev.</b>	<b>N</b>
POLCON	0.700	0.458	46299
POLCON_Gov	0.099	0.298	46299
POLCON_Org	0.668	0.471	46299
Network size	1280.03	1446.618	45424
Network size (divided by 1000)	1.28	1.447	45424
Salary	634.244	379.052	46299
Total earnings	1307.377	1705.642	46299
Log salary	6.335	0.464	46299
Log earnings	6.893	0.637	46299
Education	1.953	1.016	46299
Experience	12.997	10.617	46299
All boards	2.88	3.852	46299
Bonus ratio	0.282	0.277	46299
Equity ratio	0.668	0.278	46299
Gender	0.936	0.245	45049
Age	63.68	7.719	44924

Table 5.12: Summary statistics for firm-level data, United States

<b>Variable</b>	<b>Mean</b>	<b>Std. Dev.</b>	<b>N</b>
POLCON total	1.635	1.609	12829
POLCON Firm	0.71	0.454	12829
Sum of network size	13990.22	9581.85	12829
Sum of salary	2994.855	1744.556	12829
Sum of total earnings	5621.929	6314.847	12829
Average salary	279.146	154.959	12829
Average earnings	534.579	737.485	12829
Log salary	7.848	0.597	12826
Log earnings	8.333	0.756	12829
Ind_POLCON	0.71	0.134	12829
Ind_network	14128.866	4400.641	12829
Ind_salary	282.188	68.738	12829
Ind_earnings	537.925	214.125	12829
Education	2.022	0.479	12829
Experience	12.256	5.124	12829
All boards	5.347	2.45	12829
Bonus ratio	0.12	0.132	12829
Equity ratio	0.553	0.245	12829
Gender ratio	0.893	0.098	12821
Average age	67.679	4.789	12810

Table 5.13: Summary statistics for individual-level data, United Kingdom

<b>Variable</b>	<b>Mean</b>	<b>Std. Dev.</b>	<b>N</b>
POLCON	0.409	0.492	31422
POLCON_Gov	0.079	0.27	31422
POLCON_Org	0.384	0.486	31422
Network size	839.268	1240.947	30992
Network size (divided by 1000)	0.839	1.241	30992
Salary	537.521	1720.487	31422
Total earnings	1012.635	2072.395	31422
Log salary	6.132	0.485	31422
Log earnings	6.673	0.617	31422
Education	1.587	1.163	31422
Experience	11.569	9.424	31422
All boards	6.564	11.843	31422
Bonus ratio	0.285	0.219	31422
Equity ratio	0.289	0.272	31422
Age	60.294	8.096	27566
Gender	0.96	0.196	27610

Table 5.14: Summary statistics for firm-level data, United Kingdom

<b>Variable</b>	<b>Mean</b>	<b>Std. Dev.</b>	<b>N</b>
POLCON total	0.575	0.896	19908
POLCON Firm	0.391	0.488	19908
Sum of network size	4193.071	4881.459	19894
Sum of salary	1226.377	3451.774	19908
Sum of total earnings	2043.828	4431.725	19908
Average salary	197.642	658.431	19908
Average earnings	316.736	743.277	19908
Log salary	6.583	1.149	19908
Log earnings	6.901	1.325	19908
Ind_POLCON	0.391	0.121	19908
Ind_network	4256.093	2090.345	19908
Ind_salary	196.242	67.648	19908
Ind_earnings	316.686	140.181	19908
Education	1.528	0.718	19908
Experience	9.343	5.277	19908
Total boards	8.523	7.035	19908
Bonus ratio	0.089	0.109	19908
Equity ratio	0.115	0.158	19908
Gender ratio	0.933	0.128	19849
Average age	63.036	5.966	19849

Table 5.15: Absolute individual level executive earnings and political connections by origin of connection, United States

	(1)	(2)	(3)	(4)	(5)	(6)
	Total earnings	Total earnings	Total earnings	Salary	Salary	Salary
POLCON	153.9*** (15.99)			73.16*** (3.497)		
POLCON_Gov		165.3*** (21.62)			40.73*** (4.745)	
POLCON_Org			88.89*** (15.03)			59.55*** (3.289)
Network size	30.51*** (5.027)	35.87*** (4.963)	34.99*** (5.003)	11.36*** (1.099)	14.79*** (1.089)	12.57*** (1.095)
Covariates	YES	YES	YES	YES	YES	YES
Firm fixed effects	YES	YES	YES	YES	YES	YES
Time fixed effects	YES	YES	YES	YES	YES	YES
Observations	44071	44071	44071	44071	44071	44071
R squared	0.455	0.454	0.454	0.506	0.501	0.504

Covariates same as in Table 5.2. Standard errors shown in parentheses. \*\*\* denotes significance at 0.1%, \*\* at 1%, and \* at 5%.

Table 5.16: Absolute individual level executive earnings and political connections by origin of connection, United Kingdom

	(1)	(2)	(3)	(4)	(5)	(6)
	Total earnings	Total earnings	Total earnings	Salary	Salary	Salary
POLCON	89.39** (34.47)			69.48* (29.70)		
POLCON_Gov		231.2*** (50.60)			135.6** (43.61)	
POLCON_Org			33.32 (33.55)			21.25 (28.90)
Network size	2.827 (13.28)	10.84 (12.29)	11.28 (13.20)	-10.45 (11.44)	-3.198 (10.59)	-3.188 (11.37)
Covariates	YES	YES	YES	YES	YES	YES
Firm fixed effects	YES	YES	YES	YES	YES	YES
Time fixed effects	YES	YES	YES	YES	YES	YES
Observations	27236	27236	27236	27236	27236	27236
R squared	0.155	0.155	0.155	0.124	0.124	0.124

Covariates same as in Table 5.6. Standard errors shown in parentheses. \*\*\* denotes significance at 0.1%, \*\* at 1%, and \* at 5%.

## Bibliography

- [1] Abowd, J., Kramarz, F., and Margolis, D., 1999. “High Wage Workers and High Wage Firms.” *Econometrica*, 67(2): 251-333.
- [2] Acemoglu, D., and Autor, D., 2011. “Skills, Tasks and Technologies: Implications for Employment and Earnings.” in Ashenfelter and Card (eds.) *Handbook of Labor Economics, Volume 4B*, Elsevier.
- [3] Alvarez, J., Engbom, N., and Moser, C., 2015. “Firms and the Decline of Earnings Inequality in Brazil.” Working Paper, Princeton University 2015.
- [4] Angrist, J. and Krueger, A., 2001. “Instrumental Variables and the Search for Identification: From Supply and Demand to Natural Experiments.” *Journal of Economic Perspectives*, 15(4): 69-85.
- [5] Atkinson, A.B., 2008. *The Changing Distribution of Earnings in OECD Countries*. Oxford: Oxford University Press.
- [6] Atkinson, A.B., 2015. *Inequality. What can be done?* Cambridge, MA: Harvard University Press.
- [7] Atkinson, A.B., Hasell, J., Morelli, S., and Roser, M. 2017. *The Chartbook of Economic Inequality*. Institute for New Economic Thinking, Oxford Martin School.

- [8] Atkinson, A.B., Piketty, T. and Saez, E. 2011. "Top Incomes in the Long Run of History." *Journal of Economic Literature*, 49(1): 3-71.
- [9] Autor, D., Dorn, D., Katz, L., Patterson, C., Van Reenen, J., 2017. "The Fall of the Labor Share and the Rise of Superstar Firms." *NBER Working Paper* No. 23396, National Bureau of Economic Research, May 2017.
- [10] Bagchi, S. and Svejnar, J., 2015. "Does wealth inequality matter for growth? The effect of billionaire wealth, income distribution, and poverty." *Journal of Comparative Economics*, 43: 505-530.
- [11] Bakija, J., Cole, A., and Heim, B.T., 2012. "Jobs and Income Growth of Top Earners and the Causes of Changing Income Inequality: Evidence from U.S. Tax Return Data." Unpublished manuscript, Williams College
- [12] Bandiera, O., Prat, A., and Valletti, T., 2009. "Active and Passive Waste in Government Spending: Evidence from a Policy Experiment." *American Economic Review*, 99(4): 1278-1308.
- [13] Bartels, L.M. 2016. *Unequal Democracy. The Political Economy of the New Gilded Age*. Second Edition. Princeton: Princeton University Press.
- [14] Barth, E., Bryson, A., Davis, J. and Freeman, R., 2014. "It's Where You Work: Increases in Earnings Dispersion across Establishments and Individuals in the U.S." *NBER Working Paper* 20447, National Bureau of Economic Research, September 2014.

- [15] Bellettini, G., Ceroni, C. B., and Prarolo, G. 2013. "Political persistence and economic growth." *European Journal of Political Economy*, 31: 165-179.
- [16] BoardEx, 2017. Data Dictionary. [online] BoardEx Ltd, New York, 2017. Available at <http://metalib.ie.edu/ayuda/Varios/BoardExWRDSDataDictionary.pdf> [Accessed November 12th 2017]
- [17] Bonica, A., McCarty, N., Poole, K.T., and Rosenthal, H. 2013. "Why Hasn't Democracy Slowed Rising Inequality?" *Journal of Economic Perspectives*, 27(3): 103-124.
- [18] Borondo, J., Borondo, F., Rodriguez-Sickert, C., and Hidalgo, C.A., 2014. "To Each According to its Degree: The Meritocracy and Topocracy of Embedded Markets." *Scientific Reports* 4, Article no. 3784.
- [19] Card, D., Heining, J., and Kline, P., 2013. "Workplace Heterogeneity and the Rise of West German Wage Inequality." *Quarterly Journal of Economics*, 128(3): 967-1015.
- [20] Davis, S. and Haltiwagner, J. 1991. "Wage Dispersion Between and Within US Manufacturing Plants, 1963-1986." *Brookings Paper on Economic Activity: Microeconomics*, 1991: 115-200.
- [21] De Cesari, A. and Ozkan, N., 2015. "Executive incentives and payout policy: Empirical evidence from Europe." *Journal of Banking & Finance*, 55: 70-91.

- [22] Deaton, A., 2013. *The Great Escape. Health, Wealth, and the Origins of Inequality*. Princeton, New Jersey: Princeton University Press.
- [23] Desai, R.M., and Olofsgard, A., 2011. “The Costs of Political Influence: Firm-Level Evidence From Developing Countries.” *Quarterly Journal of Political Science*, 6(2): 137-178.
- [24] El-Khatib, R., Fogel, K., and Jandik, T., 2015. “CEO network centrality and merger performance.” *Journal of Financial Economics*, 116(2): 349-382.
- [25] Faccio, M., 2006. “Politically Connected Firms.” *American Economic Review*, 96(1): 369-386.
- [26] Faccio, M., Masulis, R., and McConnell, J. 2006. “Political Connections and Corporate Bailouts.” *The Journal of Finance*, 61(6): 2597-2635.
- [27] Fazekas, M., and Toth, I.J., 2016. “From Corruption to State Capture: A New Analytical Framework with Empirical Applications from Hungary.” *Political Research Quarterly*, 69(2): 320-334.
- [28] Fazekas, M., and Kocsis, G., 2017. “Uncovering High-Level Corruption: Cross-National Objective Corruption Risk Indicators Using Public Procurement Data.” *British Journal of Political Science*, published online 24 August 2017.
- [29] Fisman, R. 2001. “Estimating the Value of Political Connections.” *The American Economic Review*, 91(4): 1095-1102.

- [30] Fisman, R. and Svensson, J., 2007. "Are corruption and taxation really harmful to growth? Firm level evidence." *Journal of Development Economics*, 83: 63-75.
- [31] Furman, J. and Orszag, P., 2015. "A Firm-Level Perspective on the Role of Rents in the Rise in Inequality." Presentation at "A Just Society" Centennial Event in Honor of Joseph Stiglitz, Columbia University, October 16, 2015.
- [32] Gilens, M. 2012. *Affluence and Influence. Economic Inequality and Political Power in America*. Princeton: Princeton University Press.
- [33] Gilens, M., and Page, B. 2014. "Testing Theories of American Politics: Elites, Interest Groups, and Average Citizens." *Perspectives on Politics*, 12(3): 564-581.
- [34] Hacker, J. and Pierson, P. 2011. *Winner-Take-All Politics. How Washington Made the Rich Richer - And Turned Its Back on the Middle Class*. New York: Simon & Schuster.
- [35] Hakanson, C., Lindqvist, E. and Vlachos, J., 2015. "Firms and Skills: The Evolution of Worker Sorting." *Working Paper* 2015:9, IFAU - Institute for Labour Market Policy Evaluation 2015.
- [36] Helpman, Elhanan, Oleg Itskhoki, Marc-Andreas Muendler, and Stephen Redding, 2015. "Trade and Inequality: From Theory to Estimation." Working Paper, Princeton University 2015.

- [37] Jackson, M.O., 2008. *Social and Economic Networks*. Princeton, New Jersey: Princeton University Press.
- [38] Kim, J.W., Kogut, B., Yang, J.S., 2015. "Executive Compensation, Fat Cats, and Best Athletes." *American Sociological Review*, 80(2): 299-328.
- [39] Krueger, A. 1974. "The Political Economy of the Rent Seeking Society." *American Economic Review*, 64: 291-303.
- [40] Lindsey, B., and Teles, S. 2017. *The Captured Economy. How the Powerful Enrich Themselves, Slow Down Growth, and Increase Inequality*. New York: Oxford University Press.
- [41] Milanovic, B. 2016. *Global inequality: a new approach for the age of globalization*. Cambridge, MA: Harvard University Press.
- [42] Mueller, H.M., Ouimet, P.P., and Simintzi, E. 2015. "Wage Inequality and Firm Growth." *NBER Working Paper 20876*, National Bureau of Economic Research, 2015.
- [43] Piketty, T., 2014. *Capital in the Twenty-First Century*. Cambridge, MA: Harvard University Press.
- [44] Piketty, T., and Saez, E., 2003. "Income Inequality in the United States, 1913-1998." *Quarterly Journal of Economics*, 118(1): 1-39.

- [45] Saez, E., 2017. "Income and Wealth Inequality: Evidence and Policy Implications." *Contemporary Economic Policy*, 35(1): 7-25.
- [46] Scheidel, W., 2017. *The Great Leveler. Violence and the History of Inequality from the Stone Age to the Twenty-First Century*. Princeton, New Jersey: Princeton University Press.
- [47] Schmidt, B., 2015. "Costs and benefits of friendly boards during mergers and acquisitions." *Journal of Financial Economics*, 117(2): 424-447.
- [48] Song, J., Price, D., Guvenen, F., Bloom, N., and Wachter, T., 2016. "Firming Up Inequality." *NBER Working Paper* No. 21199. National Bureau of Economic Research, May 2015, Revised version: June 2016.
- [49] Stiglitz, J., 2012. *The Price of Inequality. How Today's Divided Society Endangers Our Future*. New York: WW Norton.
- [50] Tanzi, V., and Davoodi, H., 1997. "Corruption, Public Investment and Growth." *IMF Working Paper* No.97/139, International Monetary Fund, Washington, DC.
- [51] Tullock, G., 1967. "The Welfare Costs of Tariffs, Monopolies and Theft." *Western Economic Journal*, 5(3): 224-232.
- [52] Wharton Research Data Service (WRDS), 2017. BoardEx database - North America. [online] The Wharton School, University of Pennsylvania, PA. Avail-

able at <https://wrds-web.wharton.upenn.edu/wrds/> [Accessed continuously between September 2017 and February 2018]

## Chapter 6

# Conclusions and implications

The goal of the thesis was to confirm the overall hypothesis that informal social networks between politicians and politically-connected firms — a type of collusion I define as elite networks — generate a positive effect on income inequality. The mechanism works in the following way: politicians and senior corporate executives of connected firms draw benefits from continuous mutual interactions. Politicians are given campaign donations and other forms of support which enable them to win and stay in office, while firms are rewarded with government procurement contracts, favourable regulation and legislation, monopoly status, or direct subsidies in times of need. The top-level corporate executives that are responsible for securing rents for their firms get rewarded with higher compensation. This is where the interactions of elite network members yield a direct negative effect on the distribution of income. Senior corporate executives are all in the top 1%

and even the top 0.1% (especially for publicly-listed firms) of income earners in a country. The inequality literature is consistent in showing that the growth of incomes of the highest earners (Piketty’s “supermanagers”) is the main cause of higher inequality in contemporary Western democracies. The conclusion of this thesis and its broader implication is that high income inequality that we observe today could also have been influenced by political connections of top income earners. Individual-level empirical findings in the thesis validate the many theoretical and case-study approaches which suggest that rent-seeking carries a large weight in explaining the causes of inequality.

The hypothesis was confirmed through three separate empirical papers, each supporting one part of the argument. The first paper estimated how politicians in Croatian local government who engage in corruption approximated through suspicious procurement contracts can increase their chances of re-election. It confirms that politicians benefit from engaging in elite network relationships with firms, at least on a local level. The second paper estimated the effect of political connections for banks that received TARP funds during the allocation of 2008 and 2009 bailouts in the US. It confirms that firms benefit from being connected to politics, this time on a national level. Finally, the third paper linked the benefits of elite network membership to its impact on income inequality. It shows that politically connected senior executives received higher earnings than non-connected executives during a 16-year period in the US and the UK.

In each case however the measurement of an elite network was a mere approximation, as interactions between its members are difficult to uncover, yet alone measure. The closest I have come in accurately defining an elite network in the thesis was in the third paper where I directly implicate top executives who associate with politicians on an informal basis (via memberships in the same clubs, societies, and associations) or who used to work together. A similar, although less restrictive approximation of an elite network was given in the second paper where the key was to implicate firms, looking at the benefits firms get if they donate money to political campaigns, lobby politicians, or have former executives who used to work in government. The most lenient approximation was given in the first paper where I link political survival to suspicious procurement contracts given to firms. Each of these approximations offers the best definition given the available data.

## **Discussion of external validity limitations and potential extensions**

To prove the main arguments in support of the hypothesis I applied several different quantitative approaches. The strength of any empirical conclusion is to which extent it can withstand the internal and external validity arguments. Throughout the thesis I am primarily concerned with internal validity, and I engage in lengthy discussions in each paper on the chosen empirical strategies. However I do not

touch upon the external validity arguments, and I only seldom discuss the limitations of each paper. Hence, this part of the concluding chapter will raise a few potential issues with the research findings.

First and foremost, the scope of this study is limited only to the countries that were examined. The impact of elite networks on inequality was shown for the US and the UK, the benefits for firms were shown only for the US, while the benefits for politicians were shown only for local government in Croatia. Second, each case provides an entirely different and very specific context, meaning that I could not show on a single dataset for a single country how both politicians and firms benefit, nor how their mutual benefits carry an indirect effect on inequality. The overall conclusion is drawn from three specific and separate cases, which raises a slight concern over the generalization of the hypothesis. Third, the estimated effects are in most cases local average treatment effects (constrained within a specific as-if random context), making it even more difficult to generalize the conclusion to all countries and all cases. In other words, even though all three papers satisfy various internal validity constraints, external validity has not been proven, nor has it been in the focus of the thesis.

To make the argument externally valid I would need to directly approximate elite networks across different societies in the same way as presented in this thesis, and observe how it differently affects inequality in each of them. If greater inequality is indeed rooted in politics, as this thesis aims to show, I should observe

cross-country differences in inequality based on the differences in how connected the private sector is to politics. In particular, I would need to show that in countries where top executives are not as closely connected to politics as they are in the UK or the US, earnings are distributed more equally. If low-inequality countries are indeed mostly deprived of these types of connections at the highest levels (i.e. if they have institutions that prevent elite networks from arising and abusing power), then we could make the conclusion that politics is an important determinant of higher inequality. If, however, this is not the case, the conclusion of the thesis would only suffice for a limited sample and perhaps even a limited time period.

Furthermore, the third paper is limited with how long it can follow the origins of an elite network. The data can only be traced back until the year 2000, meaning that I present a rather static image of the impact of connections on wages. In particular, better connected executives have had consistently higher salaries and higher total earnings throughout the observed 16-year period. There is unfortunately little data on people entering into elite networks in the sample (senior executives tend to be older and if they are entrenched, they tend to be entrenched for long periods of time, and certainly prior to my first point of examination, the year 2000). A potential extension would be to get the data which would allow tracing this process back in time until the 1950s, 1960s or at least 1970s, and see if the documented increase in income inequality since the end of the 1970s was

indeed preceded by the rise in power of elite networks. In other words, did US and UK senior executives started forming close ties to the government just before the observed rise in income inequality, or is this relationship merely a persistent peculiarity of these two countries (epitomized by WASPs in the US or Old Etonians in the UK) that is not necessarily tied to the rise in inequality?

Even in the case of obtaining a longer time-series of political connections, it would still be necessary to observe the same relationship cross-country to determine if it holds. The problem here, even if we assume that we can approximate for elite networks in other countries, is in the comparability of the data. In the US and UK samples, elite networks are proxied via a very specific indicator: whether executives used to work in politics or are connected to politics via personal relationships. If a similar proxy could be found in other countries it would have to measure the exact same thing in order to make the cross-country comparison possible.

Overall, despite the limitations in satisfying external validity of the argument, the thesis does provide a useful blueprint for further research on the topic. First and foremost it provides empirical backing of the argument that political factors could be an important determinant of inequality, even if this result is limited to a specific context and time period. Any further research on this topic, examining the political causes of inequality, should approach it from the same perspective: try to get an individual-level and firm-level dataset across different societies to measure the impact of connections on income dispersion. Further research can be aimed at

a more precise definition of elite networks, but simple enough for it to be easily measured in any society. This is a difficult task as elite network interactions are always hidden, however some interactions and particularly some outcomes based on informal networks can become visible and hence measurable.

Second, a very useful extension would be to capture the rise of power of elite networks over time. Are they indeed robust to political and economic systems, as is assumed in this thesis? How volatile is the accumulated power of elite networks across different societies? Do they vary in influence from one decade to another, or is their influence persistent? The assumption is that elite network membership is dynamic — all members are limited by the duration of their tenure, and ultimately by biology. What is the effect on society when these changes occur and when important members leave the group? Or is there no effect at all, given that the position of power of the person is what makes them a valuable member, not necessary the person himself. To continue in this direction one would have to combine the sociological and anthropological studies on elite groups with the political economy implications given in this thesis.

Third, the findings of the thesis could help provide empirical backing to Olson's (1982) argument that post-WWII stability increased the power of special interest groups which resulted in slower economic growth. Olson's lucid theory of interest group capture has never really been empirically justified. This thesis provides a good starting point of the type of analysis that would be necessary — to ob-

serve individual-level connections between politicians in power and the managers and financial backers of powerful interest groups (NGOs, corporate lobby groups, advocacy groups, religious groups, etc.).

Finally, a useful extension would be to examine whether elite networks carry a similar impact on social mobility, i.e. on the equality of opportunity. The mechanism is slightly different than the impact on inequality presented in the thesis. It materializes through hiring decisions on the labour market, where if an individual is not a member of the network, he or she is prevented from climbing the social ranks. In other words an individual can only reach the top of the corporate or political hierarchy if on that road he or she accepts the mutually-dependent favour exchanges with influential individuals. This would be almost impossible to measure directly, unless we could measure the negative effects on connected individuals when a person drops out of an elite network because of, for example, imprisonment.

Before any of these efforts are conducted, and before the theory can prove it is externally valid, the conclusions remain: politicians and firms in specific contexts do collude together and derive benefits from their interactions. These benefits to both groups hurt society as they make the distributions of incomes more unequal by pushing up the incomes of politically connected top earners. Politics is therefore an important factor that should not be overlooked when examining the causes of income inequality.

## Bibliography

- [1] Acemoglu, D., and Robinson, J. 2006. *Economic Origins of Dictatorship and Democracy*. Cambridge: Cambridge University Press.
- [2] Acemoglu, D., and Robinson, J. 2012. *Why Nations Fail: The Origins of Power, Prosperity, and Poverty*. London: Crown Publishers.
- [3] Acemoglu, D., Johnson, S., Kermani, A., Kwak, J., and Mitton, T. 2016. “The value of connections in turbulent times: Evidence from the United States.” *Journal of Financial Economics*, 121: 368-391.
- [4] Ackerman, K.D. 2005. *Boss Tweed: The Rise and Fall of the Corrupt Politician Who Conceived the Soul of Modern New York*. New York: Da Capo Press.
- [5] Ansell, B., and Samuels, D. 2014. *Inequality and Democratization. An Elite-Competition Approach*. Cambridge: Cambridge University Press.
- [6] Atkinson, A.B. 2008. *The Changing Distribution of Earnings in OECD Countries*. Oxford: Oxford University Press.
- [7] Atkinson, A.B. 2015. *Inequality. What can be done?* Cambridge, MA: Harvard University Press.
- [8] Atkinson, A.B., Hasell, J., Morelli, S., and Roser, M. 2017. *The Chartbook of*

*Economic Inequality*. Institute for New Economic Thinking, Oxford Martin School.

- [9] Atkinson, A.B., Piketty, T. and Saez, E. 2011. "Top Incomes in the Long Run of History." *Journal of Economic Literature*, 49(1): 3-71.
- [10] Bakija, J., Cole, A., and Heim, B.T., 2012. "Jobs and Income Growth of Top Earners and the Causes of Changing Income Inequality: Evidence from U.S. Tax Return Data." Unpublished manuscript, Williams College
- [11] Bartels, L.M. 2016. *Unequal Democracy. The Political Economy of the New Gilded Age*. Second Edition. Princeton: Princeton University Press.
- [12] Baumgartner, F., Berry, J.M., Hojnacki, M., Kimball, D.C., and Leech, B.L. 2009. *Lobbying and Policy Change. Who Wins, Who Loses, and Why*. Chicago: University of Chicago Press.
- [13] Beck, R., Hoskins, C., and Connolly, J.M. 1992. "Rent Extraction through Political Extortion: An Empirical Examination." *Journal of Legal Studies*, 21: 217-224.
- [14] Becker, G. 1968. "Crime and Punishment: An Economic Approach." *Journal of Political Economy*, 76(2): 169-217.
- [15] Becker, G. 1983. "A theory of competition among pressure groups for political influence." *Quarterly Journal of Economics*, 98: 371-400.

- [16] Bellettini, G., Ceroni, C. B., and Prarolo, G. 2013. "Political persistence and economic growth." *European Journal of Political Economy*, 31: 165-179.
- [17] Bermeo, N. 2009. "Does Electoral Democracy Boost Economic Equality?" *Journal of Democracy*, 20(4): 21-35.
- [18] Bertrand, M. and Mullainathan, S. 2001. "Are CEOs Rewarded for Luck? The Ones Without Principals Are." *Quarterly Journal of Economics*, 116(3): 371-400.
- [19] Blanes i Vidal J., Draca M., Fons-Rosen C. 2012. "Revolving Door Lobbyists." *American Economic Review*, 102(7): 3731-48.
- [20] Blau, B., Brough, T., and Thomas, D.W. 2013. "Corporate lobbying, political connections, and the bailout of banks." *Journal of Banking & Finance*, 37(8): 3007-17
- [21] Bonica, A., McCarty, N., Poole, K.T., and Rosenthal, H. 2013. "Why Hasn't Democracy Slowed Rising Inequality?" *Journal of Economic Perspectives*, 27(3): 103-124.
- [22] Borondo, J., Borondo, F., Rodriguez-Sickert, C., and Hidalgo, C.A., 2014. "To Each According to its Degree: The Meritocracy and Topocracy of Embedded Markets." *Scientific Reports* 4, Article no. 3784.

- [23] Bueno De Mesquita B., Smith, A., Siverson, R., and Morrow J., 2005. *The Logic of Political Survival*. Cambridge: MIT Press.
- [24] Bueno De Mesquita B., Smith, A., 2011. *The Dictator's Handbook. Why Bad Behavior Is Almost Always Good Politics*. New York: Public Affairs.
- [25] Calomiris, C.W. and Khan, U. 2015. "An Assessment of TARP Assistance to Financial Institutions." *Journal of Economic Perspectives*, 29(2): 53-80.
- [26] Center for Responsive Politics, 2016. Lobbying Spending Database: AARP. [online] Available at: <https://www.opensecrets.org/orgs/summary.php?id=D000023726>, Washington DC.
- [27] Clark, G. 2011. *A Farewell to Alms. A Brief Economic History of the World*. Princeton: Princeton University Press.
- [28] Clark, G. 2014. *The Son Also Rises. Surnames and the History of Social Mobility*. Princeton: Princeton University Press.
- [29] Dabla-Norris, E., and Wade, P., 2001. "Rent Seeking and Endogenous Income Inequality" *IMF Working Paper* 01/15: 1-29, International Monetary Fund, Washington, DC.
- [30] De Bondt, W.F.M. and Thaler, R. 1985. "Does the Stock Market Overreact?" *The Journal of Finance*, 40(3): 793-805.

- [31] de Figueiredo, J.M., and Silverman, B.S. 2006. "Academic Earmarks and the Returns to Lobbying." *Journal of Law Economics*, 49(2): 597-625
- [32] de Figueiredo, J.M., and Silverman, B.S. 2007. "How does the government (want to) fund science? Politics, lobbying, and academic earmarks." *NBER Working paper 13459*, National Bureau of Economic Research, October 2007.
- [33] de Figueiredo, J.M., and, Richter, B.K. 2014. "Advancing the Empirical Research on Lobbying." *17th Annual Review of Political Science* (2014): 163-185.
- [34] Deaton, A., 2013. *The Great Escape. Health, Wealth, and the Origins of Inequality*. Princeton: Princeton University Press.
- [35] Dinc, S. 2005. "Politicians and banks: Political influences on government-owned banks in emerging markets." *Journal of Financial Economics*, 77(2): 453-479.
- [36] Downs, A. 1957. *An Economic Theory of Democracy*. New York: Addison-Wesley.
- [37] Duchin, R. and Sosyura, D. 2012. "The politics of government investment." *Journal of Financial Economics*, 106(1): 24-48.
- [38] Dye, T. 1976. *Policy Analysis: What Governments Do, Why They Do It, and What Difference It Makes*. University of Alabama Press.

- [39] Earle, J.S. and Gehlbach, S. 2015. "The Productivity Consequences of Political Turnover: Firm-Level Evidence from Ukraine's Orange Revolution." *American Journal of Political Science*, 59(3): 708-723.
- [40] Eggers, A.C., and Hainmueller, J. 2009. "MPs for Sale? Returns to Office in Postwar British Politics." *American Political Science Review*, 103(4): 1-21.
- [41] Evans, D. 1996. "Before the Roll Call: Interest Group Lobbying and Public Policy Outcomes in House Committees." *Political Research Quarterly*, 49(2): 287-304.
- [42] Faccio M., 2006. "Politically Connected Firms." *American Economic Review*, 96(1): 369-386.
- [43] Faccio, M., Masulis, R., and McConnell, J. 2006. "Political Connections and Corporate Bailouts." *The Journal of Finance*, 61(6): 2597-2635.
- [44] Fisman, R. 2001. "Estimating the Value of Political Connections." *American Economic Review*, 91(4): 1095-1102.
- [45] Fortune Magazine, 2005. Fortune's "Power 25". The 25 Most Effective Interest Groups.
- [46] Furman, J. and Orszag, P., 2015. "A Firm-Level Perspective on the Role of Rents in the Rise in Inequality." Presentation at "A Just Society" Centennial Event in Honor of Joseph Stiglitz, Columbia University, October 16, 2015.

- [47] Gehlbach, S. 2006. "The Consequences of Collective Action: An Incomplete-Contracts Approach." *American Journal of Political Science*, 50(3): 802-823.
- [48] Gehlbach, S., Sonin, K., and Zhuravskaya, E. 2010. "Businessmen Candidates." *American Journal of Political Science*, 54(3): 718-736.
- [49] Gilens, M. 2012. *Affluence and Influence. Economic Inequality and Political Power in America*. Princeton: Princeton University Press.
- [50] Gilens, M., and Page, B. 2014. "Testing Theories of American Politics: Elites, Interest Groups, and Average Citizens." *Perspectives on Politics*, 12(3): 564-581.
- [51] Glaeser, E., Scheinkman, J. and Shleifer, A. 2003. "The injustice of inequality." *Journal of Monetary Economics*, 50(1): 199-222.
- [52] Goldman, E., Rocholl, J. and So, J. 2009. "Do politically connected boards affect firm value?" *Review of Financial Studies*, 22: 2331-60.
- [53] Griffiths, D., Lambert, P., Bihagen, E. 2014. "Measuring the Potential Power Elite in the UK and Sweden." *European Societies*, 16(5): 742-762.
- [54] Grossman, G.M., and Helpman, E. 2002. *Special Interest Politics*. Cambridge, MA: MIT Press.
- [55] Gupta, S., Davoodi, H. and Alonso-Terme, R. 2002. "Does corruption affect income inequality and poverty?" *Economics of Governance*, 3(1): 23-45.

- [56] Guttsman, W.L. 1965. *The British Political Elite*. London: MacGibbon & Kee.
- [57] Hacker, J. and Pierson, P. 2011. *Winner-Take-All Politics. How Washington Made the Rich Richer - And Turned Its Back on the Middle Class*. New York: Simon & Schuster.
- [58] Helland, L. 2008. "Lobbying with Conflicting Interests: Norwegian Local-central Relations." *European Journal of Political Research*, 47(2): 184-205.
- [59] Hochberg, Y.V., Sapienza, P., Vissing-Jorgensen, A. 2009. "A Lobbying Approach to Evaluating the Sarbanes-Oxley Act of 2002." *Journal of Accounting Research*, 47(2): 519-583.
- [60] Igan, D., Mishra, P., and Tressel, T. 2011. "A Fistful of Dollars: Lobbying and the Financial Crisis." *NBER Working Paper 17076*, National Bureau of Economic Research, May 2011.
- [61] Jackson, M.O. 2008. *Social and Economic Networks*. Princeton: Princeton University Press.
- [62] Jayachandran S. 2006. "The Jeffords Effect." *Journal of Law Economics*, 49(2): 397-425.
- [63] Kahneman, D. 2011. *Thinking, Fast and Slow*. New York: Farrar Straus Giroux.

- [64] Kelleher C.A., Yackee S.W. 2009. "A Political Consequence of Contracting: Organized Interests and State Agency Decision Making." *Journal of Public Administration Research Theory* 19(3):579-602.
- [65] Khwaja, A.I. and Mian, A. 2005. "Do Lenders Favor Politically Connected Firms? Rent Provision in an Emerging Financial Market." *Quarterly Journal of Economics*, 120(4): 1371-1411.
- [66] Krueger, A. 1974. "The Political Economy of the Rent Seeking Society." *American Economic Review*, 64(3): 291-303.
- [67] Kurer, O., 2001. "Why do voters support corrupt politicians?" in Jain, A.K. (ed.) *The Political Economy of Corruption*. London: Routledge.
- [68] Kwak, J. 2014. "Cultural Capture and the Financial Crisis." in Carpenter and Moss (eds.) *Preventing Regulatory Capture. Special Interest Influence and How to Limit It*. Cambridge: Cambridge University Press.
- [69] Lindsey, B., and Teles, S. 2017. *The Captured Economy. How the Powerful Enrich Themselves, Slow Down Growth, and Increase Inequality*. New York: Oxford University Press.
- [70] Maclean, M., Harvey, C., and Chia, R. 2010. "Dominant corporate agents and the power elite in France and Britain." *Organization Studies*, 31(3): 327-348.
- [71] Mauro, P., Romeu, R., Binder, A., and Zaman, A. 2013. "A Modern History

of Fiscal Prudence and Profligacy.” *IMF Working paper* 13/5, International Monetary Fund, Washington, DC.

[72] McChesney, F.S. 1997. *Money for Nothing. Politicians, Rent Extraction, and Political Extortion*. Cambridge: Harvard University Press.

[73] Meltzer, A., and Richard, S. 1981. “A Rational Theory of the Size of Government.” *Journal of Political Economy*, 89(5): 914-927.

[74] Mian, A., Sufi, A., and Trebbi, F. 2010. “The Political Economy of the US Mortgage Default Crisis.” *The American Economic Review*, 100(5): 1967-98.

[75] Michels, R. 1962 [1915]. *A Sociological Study of the Oligarchical Tendencies of Modern Democracy*. New York: Crowell-Collier.

[76] Miller, W.L., Grodeland, A.B., Koshechkina, T.Y., 2001. *A Culture of Corruption? Coping with Government in Post-Communist Europe*. Budapest: Central European University Press.

[77] Milanovic, B. 2016. *Global inequality: a new approach for the age of globalization*. Cambridge, MA: Harvard University Press.

[78] Milanovic, B. Lindert, P., and Williamson, J. 2011. “Pre-Industrial Inequality.” *The Economic Journal*, 121(551): 255-272.

[79] Mills, C.W. 1956. *The Power Elite*. Oxford: Oxford University Press.

[80] Mosca, G. 1939. *The Rulling Class*. New York: McGraw Hill.

- [81] Moss, A.L. 2008. *Selling Out America's Democracy. How Lobbyists, Special Interests, and Campaign Financing Undermine the Will of the People.* Westport, CT: Praeger Publishers.
- [82] North, D.C., Wallis, J.J., and Weingast, B.R. 2009. *Violence and Social Orders. A Conceptual Framework for Interpreting Recorded Human History.* Cambridge: Cambridge University Press.
- [83] Olson, M. 1971[1965]. *The Logic of Collective Action.* Cambridge, MA: Harvard University Press.
- [84] Olson, M, 1982. *The Rise and Decline of Nations.* New Haven: Yale University Press.
- [85] Olson, M. 2000. *Power and Prosperity. Outgrowing communist and capitalist dictatorships.* New York: Basic Books
- [86] Okun, A. 1975. *Equality and Efficiency. The Big Tradeoff.* Washington, DC: The Brookings Institution.
- [87] Pareto, V. 1935. *The mind and society.* New York: Harcourt, Brace and Company.
- [88] Peltzman, S. 1976. "Toward a more general theory of regulation." *Journal of Law and Economics*, 19: 211-240.

- [89] Persson, T. and Tabellini, G. 2000. *Political Economics. Explaining Economic Policy*. Cambridge, MA: MIT Press.
- [90] Piketty, T. 2014. *Capital in the 21st Century*. Cambridge, MA: Harvard University Press.
- [91] Richter, B.K., Samphantharak K., and Timmons, J.F. 2009. "Lobbying and Taxes." *American Journal of Political Science*, 53(4): 893-909.
- [92] Riordan, W.L. 1995. *Plunkitt of Tammany Hall: A Series of Very Plain Talks on Very Practical Politics*. New York: Signet Classics.
- [93] Ruostetsaari, I. 2006. "Elites and Democracy: Are they Compatible?", in Engelstad, F. and Gulbrandsen, T. (eds.) *Comparative Studies of Social and Political Elites*, Emerald Group Publishing Limited, Comparative Social Research, Volume 23: 265-274.
- [94] Rundquist, B.S., Strom, G.S. and Peters, J.G., 1977. "Corrupt Politicians and Their Electoral Support: Some Experimental Observations." *The American Political Science Review*, 71(3): 954-963.
- [95] Sapienza, P. 2004. "The effects of government ownership on bank lending." *Journal of Financial Economics*, 72(2): 357-384.
- [96] Scheidel, W. 2017. *The Great Leveler. Violence and the History of Inequal-*

*ity from the Stone Age to the Twenty-First Century*. Princeton: Princeton University Press.

- [97] Schone, K., Koch, E., and Baumont C. 2013. “Modelling Local Growth Control Decisions in a Multi-city Case: Do Spatial Interactions and Lobbying Efforts Matter?” *Public Choice*, 154(1-2): 95-117.
- [98] Schoenman, R. 2014. *Networks and Institutions in Europe’s Emerging Markets*. Cambridge: Cambridge University Press.
- [99] Shleifer, A. and Vishny, R. 1994. “Politicians and Firms.” *Quarterly Journal of Economics*, 109: 995-1025.
- [100] Shleifer, A., and Vishny, R. 1998. *The Grabbing Hand*. Cambridge, MA: Harvard University Press.
- [101] Smith, A., 1904 [1776]. *An Inquiry into the Nature and Causes of the Wealth of Nations*. 5th ed. London: Methuen & Co.
- [102] Stigler, G. 1971. “The theory of economic regulation.” *Bell Journal of Economics and Management Science*, 2 (spring): 3-21.
- [103] Stiglitz, J. 2012. *The Price of Inequality. How Today’s Divided Society Endangers Our Future*. New York: WW Norton.
- [104] Stokes, S.C., Dunning, T., Nazareno, M., and Brusco V. 2013. *Brokers, Vot-*

- ers, and Clientelism. The Puzzle of Distributive Politics.* Cambridge: Cambridge University Press.
- [105] Svejnar, J. and Bagchi, S. 2015. "Does wealth inequality matter for growth? The effect of billionaire wealth, income distribution, and poverty." *Journal of Comparative Economics*, 43(3): 505-530.
- [106] Taleb, N.N. 2008. *The Black Swan. The Impact of the Highly Improbable.* New York: Penguin Press.
- [107] Tanzi, V. and Schuknecht, L. 2000. *Public Spending in the 20th Century. A Global Perspective.* Cambridge: Cambridge University Press.
- [108] Transparency International, 2016. Corruption by country. [online] Available at: [www.transparency.org/country](http://www.transparency.org/country) [Accessed 2nd February 2017]
- [109] Tullock, G., 1967. "The Welfare Costs of Tariffs, Monopolies and Theft." *Western Economic Journal*, 5(3): 224-232.
- [110] US Census Bureau, 2016. Income and Poverty in the United States: 2015. Report Number: P60-256, September 13, 2016, Washington, DC.
- [111] Useem, M. 1984. *The Inner Circle: Large Corporations and the Rise of Business Political Activity in the U.S. and U.K.* Oxford: Oxford University Press.
- [112] Weber, M. 1978 [1922]. *Economy and Society.* Oakland, CA: University of California Press.

- [113] Winters, M.S., and Weitz-Shapiro, R. 2013. "Lacking Information or Condoning Corruption: When Do Voters Support Corrupt Politicians?" *Comparative Politics*, 45(4): 418-436.
- [114] You J.S., and Khagram, S. 2005. "A Comparative Study of Inequality and Corruption." *American Sociological Review*, 70(1): 136-157.