Essays on the Political Economy of Public Finance

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Preface

> *"If once they [the people] become inattentive to the public affairs, you and I, and Congress, and Assemblies, judges and governors shall all become wolves. It seems to be the law of our general nature, in spite of individual exceptions."*
>
> – Thomas Jefferson (1787)

In democracies, many rules and institutions are in place to prevent individuals or small groups from gaining too much power and taking decisions that are not in the interest of public welfare. But even with these rules in place, political decision-makers find ways to manipulate policy-making for self-serving purposes such as private financial gains. Fortunately, in democratic countries, electoral accountability ensures that misbehavior is punished and bad leaders cannot stay in office endlessly.

However, as Jefferson (1787) writes in a letter to Edward Carrington, as long as the voters do not know about the performance and actions of the government, public authorities can behave as they please. Indeed, the literature on political budget cycles points out that the better informed the voters are, the less representatives can manipulate them (see, e.g., Shi and Svensson 2006). Still, in order to punish bad behavior of politicians in the sense of voting the incumbent government out of office, citizens need to be patient until the next election, which can take several years. To control the actions of the government to some extent during the term, many countries have put in place the possibility to veto decisions via direct democracy. Besides vetoing decisions of the government, citizens can also propose new policies via voter initiatives.

This thesis comprises of three chapters, which are based on independent essays. Each chapter has a focus on another player in the political process. The

first two chapters examine public representatives and how they potentially find room to reap some personal benefits from their work. The third chapter focuses on the behavior of citizens and how they make use of their political power in the form of direct democracy. More precisely, the first chapter analyzes national governments and their efforts to stay in office. The second chapter investigates bureaucrats at the European Investment Bank (EIB) – the bank of the European Union – who theoretically should not have a political agenda such as reelection concerns. I test whether they approve more loans from their home regions irrespective of project quality criteria. Finally, the third chapter asks whether the direct democratic activity in one municipality has spillover effects to the neighboring municipalities.

All chapters are empirical and the econometric methods used rely on different techniques with the aim to estimate causal effects. In the first chapter, I make use of panel fixed effects regressions. Being interested in the dynamics before elections, it is not possible to show flat pre-trends before the intervention (here, the election) to demonstrate causal effects of the intervention. Hence, I use different fixed effects, control variables, and robustness checks to reduce endogeneity concerns to a minimum. The second chapter employs difference-in-differences estimations and a distributed lag model. Thereby, a large set of fixed effects is again included to minimize omitted variable bias. Moreover, with the distributed lag model, I am able to show parallel pre-trends for the control and treatment group before the intervention. The third chapter employs a spatial lag model. To deal with the inherent endogeneity, I rely on an instrumental variable approach to estimate causal effects. Therefore, all analytical designs include strategies to provide internal validity and I can thus draw credible policy recommendations from the results of the chapters.

The content of the chapters is the following: Chapter 1, which is single-authored, analyzes whether we can observe election cycles in public procurement in European Union Member States.[1] Political budget cycles describe a phenomenon widely studied in the literature where fiscal variables follow an election cycle. This observation is explained by the fact that incumbent governments wish

[1]This chapter also circulates as Havlik (2020).

to stay in office and therefore try to convince the electorate to vote for them by increasing expenditure, decreasing taxes or other policies, from which citizens might benefit. I study whether this phenomenon also exists in the use of public procurement. Public procurement is a major spending tool of the public sector being the process where public authorities buy products, services, and works from companies. Additionally, the availability of project-level data allows me to study different points in time of the procurement process as well as different product and visibility categories. Therefore, this paper is a contribution to the literature, as existing studies mostly rely on yearly aggregate public budget data and do not focus on the events of publishing calls for tenders and awarding public procurement contracts. Moreover, I am able to differentiate between different degrees of project visibility according to different visibility definitions, which is another novelty in the literature. The results of the paper show an increase of calls for tenders and project awards in the national parliamentary election year. There is no evidence for an increase in project completions prior to elections. Furthermore, project awards in visible categories are increased in the election year; in particular in categories defined as visible by the literature and categories with a higher labor intensity.

Chapter 2 is joint work with Zareh Asatryan and studies the EIB.[2] We ask the question, whether regions that have a representative at the decisive body for loan approval at the EIB, the Board of Directors, have a higher chance of receiving a loan. We compile a dataset of hand-collected information on the career paths of all Board members and project-level data on loans, both since the foundation of the EIB in 1958, aggregated to sub-national regions. We find evidence for the hypothesis and show that this effect diminishes right after the representative leaves the Board. Moreover, the effect is especially pronounced for very large infrastructure projects. We conduct several tests to analyze whether the observed home bias can be explained with an information advantage of the Board members or with favoritism. Among others, we provide evidence that Board members might already send money to regions they move to after their mandate at the EIB, which is a piece of evidence for favoritism, as they cannot have an informa-

[2]This chapter is based on Asatryan and Havlik (2020).

tion advantage about a region they have not been to yet. The other tests point in a similar direction that the source of the home bias is favoritism, but this case of resource misallocation cannot be conclusively demonstrated. The chapter is a contribution to the literature on the political economy of international organizations by expanding it to the EIB, the world's largest multilateral lending institution. Moreover, the EIB being a European institution, the chapter contributes to the debate on the need for reforms of the institutional architecture of the EU, as the current framework suffers from weak political accountability (for a discussion, see, Hahm et al. 2020).

Chapter 3, which is coauthored with Zareh Asatryan and Frank Streif, investigates interactions in direct democracy.[3] In particular, we study spillover effects in voter initiatives among German municipalities. The literature has extensively studied how interactions between governments influence their policy choices. This paper adds to this literature by studying interactions among citizens via direct democracy. Like the studies on government interactions, we assume that there are cross-border information externalities on the use of direct democratic instruments. The difference to government interactions is that now the policy is decided on by the citizens instead of the government as the initiative is binding. Using a sample of 3,300 initiatives in around 13,000 municipalities from 2002 to 2014, the study finds a positive spillover effect from a municipality's neighbors' initiatives to its own probability to launch a voter initiative. This effect is largest for a neighborhood defined within the radius of 20 km and fades for increasing distances. Successful initiatives and initiatives in a similar policy area are the main driver of this result and the effect is larger in neighborhoods with higher information flows.

In light of the deep current economic crisis, due to which the European Union and its Member States have adopted economic stimulus packages in historical dimensions, this thesis contains some important recommendations. First, politicians might use public procurement projects for reelection purposes. It remains an open question whether these projects are economically efficient. Given the current situation, where many citizens are unhappy with the handling of the cri-

[3]This chapter is based on Asatryan et al. (2017).

sis by their respective government, politicians might choose projects to present themselves in a positive way without much consideration on project quality. Second, the EIB, as the largest multilateral lending institution in the world, takes on important roles during economic crises, like also currently during the coronavirus crisis by installing a dedicated guarantee fund (EIB 2020). Given the results of this thesis, more transparency of the decision-making process within the EIB is needed. This is necessary to dissipate doubt that the allocation of loans happens free of favoritism and hence in an economically efficient manner. Finally, citizens can use their voice when they feel that public projects are not handled properly by launching binding initiatives. In this regard, the thesis shows that spillovers in direct democracy can raise awareness in other jurisdictions to exhibit more control over politicians.

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Contents

1

Political budget cycles in European public procurement

1.1 Introduction

How do elected politicians use public procurement to ensure to stay in office? This paper studies pre-election behavior of incumbent governments and asks whether there are political budget cycles (PBCs) in public procurement. PBCs describe the phenomenon of fiscal variables following an election cycle. Nordhaus (1975) was the first to formalize a theory in which governments manipulate macroeconomic variables in order to gain votes with a backward-looking electorate. Over time, studies relaxed the assumption of a backward-looking electorate[1] and also studied electoral cycles in fiscal variables like public expenditure, taxes or deficit (for reviews, see, e.g., De Haan and Klomp 2013; Philips 2016).

Public procurement is the procedure where public authorities purchase work, products, and services from firms and accounts for around 14% of GDP in the European Union (European Commission 2020a). Therefore, it is a major spending category of the public sector and a useful tool for politicians to deliver public goods to specific voter groups. Thereby, a project can be of a very local nature, e.g., a school or a park, and thus serve a limited group of voters in the sense of

[1] See, for example, Rogoff and Sibert (1988) who assume temporary information asymmetries between the government and the electorate or Shi and Svensson (2006) who find that PBCs are present as long as some voters are uninformed about the manipulation.

pork barrel politics. It can also be of large-scale, e.g., a highway or broadband expansion, so that many voters can benefit from the project. Moreover, delivering a public procurement project involves many steps over a long period of time, from first budgetary decisions over public tenders up to the completion of the project. This lengthy process offers several possibilities for manipulating the timing of certain steps that may serve the incumbent government.

I use project-level public procurement data from Tenders Electronic Daily (TED) for all Member States of the European Union (EU) to test for PBCs of different project steps as well as for different spending and visibility categories. I hypothesize that public procurement contracts increase in size and number prior to each election. Moreover, I test whether more "visible" projects are awarded close to elections. Thereby, visible projects are defined in three different ways: First, I use the definition of visible projects already employed in the literature. These categories are, for example, transportation, electricity or recreational buildings. Second, I ask whether labor-intensive projects are undertaken, as the workers of a firm receiving a public procurement contract are also potential voters. Finally, I test in particular whether or not larger projects are typically initiated in the run up to elections.

In the estimation, I employ panel fixed effects regressions and event study analyses with a Poisson pseudo-maximum-likelihood (PPML) estimator. I find evidence that the amount and aggregate value of public procurement contract notices (calls for tenders) and awards is higher prior to elections and interpret this result as a "credible election promise", as the money is not spent yet and only committed. Furthermore, the project is not yet delivered at the time of the awarding of the contract, hence citizens might have to wait until construction work is finished to benefit from the potential public good. Additionally, there is evidence, which suggests that more contracts for visible projects are awarded prior to elections. This is true for the amount and aggregate value of projects defined as visible in the literature and for the amount of labor-intensive projects. However, I do not find that projects are bigger prior to elections.

This paper adds two contributions to the literature. First, it is possible to study various key dates of public procurement, namely the call for tenders, the

award of a contract, and the completion of a project. Second, an analysis of various specific spending and visibility categories is also possible. In particular, whether or not bigger projects are present prior to elections, more labor-intensive projects or projects in specific categories, which are identified as visible in the literature.

To my knowledge, there are only two other papers that study political budget cycles in public procurement. Chong et al. (2014) find that public work contracts in French municipalities are more likely to end prior to legislative elections in case the mayor runs for reelection compared to municipalities where the mayor does not run for another term. Marx (2018) studies development projects funded by the World Bank in Sub-Saharan Africa and finds that national incumbent governments are rewarded for the completion of visible projects prior to the election. Both papers concentrate on the completion of public procurement contracts. While Chong et al. (2014) only study this point in time, Marx (2018) finds a dominance of the completion over the initiation of new projects. In my analysis, I find stronger effects for the initiation of contracts, i.e., publishing a contract notice and awarding a contract, than for their completion. In my view, this is the more logical result, as it is easier for the incumbent to control the beginning of a public procurement project than the end, as construction projects often take unforeseeable delays.[2] Moreover, both papers study very specific projects, public works projects and projects funded by the World Bank respectively, while I use the universe of public procurement areas on the national level in the European Union.

The "classical" literature on PBCs mostly relies on aggregate figures of fiscal variables that are only suggestive of how the manipulation works. A meta-study on the more recent PBC literature by Philips (2016) finds a small but statistically significant increase in public expenditure and debt around elections, controlling for fiscal variables, countries, data, methods, and other features. However, this result only provides little evidence on the mechanisms of how politicians try to win the support of voters.

Some cross-country studies on PBCs employ disaggregated data for differ-

[2]One very extreme example would be the new Berlin Airport BER that has a nine year delay (Lopez 2019).

ent spending categories to shed more light on the mechanisms. Enkelmann and Leibrecht (2013) conclude that PBCs predominantly exist for new Eastern European democracies and particularly in the spending categories administration, environment, as well as economic and social expenditure. Bove et al. (2017) observe that OECD countries have higher social expenditures around elections at the expense of military spending. Vergne (2009) finds no effect for infrastructure spending in developing countries but rather an increase in wages and subsidies, while Schuknecht (2000) detects the existence of public investment cycles. These papers are just a small selection of existing papers analyzing different spending categories. De Haan and Klomp (2013) provide an overview and explain differences in findings with heterogeneous level of development, institutional quality, level of democracy, and constitutional rules. Besides these cross-country studies, more recent papers often analyze local public goods in single countries.[3] Many of these studies analyze very specific spending categories and therefore manage to pin down the underlying mechanism. However, the caveat is that their results are not generalizable. They lack external validity, as they often exploit reforms or an institutional environment specific to the country in question.

Even though the studies mentioned have some indication on the specific manipulation, there are two distinct differences to this paper. First, most of them refer to annual public budget data, whereas project-level public procurement data is used in this paper. Second, these papers study a different point in time compared to the time frame taken into consideration in this paper. While the main focus in this paper is on publishing a call for tenders and the awarding of a contract, so-called election promises, the other papers study the point in time when money is spent, which is not yet the case for the two events analyzed here.[4] While I also study different categories of public procurement, I place a particular focus on categories that are perceived as visible such as large projects and projects with a higher labor force.

I also contribute to the literature on favoritism in public procurement. These papers study the effect of connections between firms and politicians and how these

[3]See Foremny et al. (2018) for an overview of sub-national studies on PBCs.

[4]I also study the point in time of the completion a project, when most of the money has been spent. But as there are only a few observations that indicate the completion date, I focus on the other two events.

connections influence public procurement outcomes. Connections are defined as donations to political parties or politicians (Titl and Geys 2019; Boas et al. 2014; Ruiz 2020), CEOs and politicians sharing the same educational background (Do et al. 2019), CEOs being (former) members or sympathizers of a political party (Straub 2014; Goldman et al. 2013) or being part of the same social network (Schoenherr 2019). The general finding of the papers is that connected firms often receive more (valuable) public procurement contracts in the event that their political connection has won the election. Moreover, Mironov and Zhuravskaya (2016) observe for Russia that firms bribe local politicians around elections for public procurement contracts and that in corrupt jurisdictions unproductive firms receive public procurement contracts. While this strand of the literature rather analyzes the effects on public procurement after elections, I study the mechanisms of public procurement before elections. Moreover, increased public spending or a higher number of public procurement contracts before elections is perfectly legal, while favoritism as studied in the previously mentioned strand of the literature may or may not be legal, in particular with regard to corruption.

1.2 Hypotheses, data, and empirical model

1.2.1 Theoretical considerations

I test several hypotheses concerning the existence of PBCs in European public procurement, i.e., I analyze whether there are election cycles in public procurement. With public procurement projects, the incumbent is able to target very specific voter groups in order to gain their votes, also known as pork barrel politics. At the same time, the incumbent can also implement huge projects, e.g., highways or broadband expansion, which many citizens can benefit from and therefore enables the incumbent to foster her chances of reelection.

Public procurement is a lengthy process with many steps involved until the project is finally delivered.[5] There are different ways of initiating a project; either the government or administration itself intend on implementing a project or the

[5]Of course, the specific steps might vary across different countries, but the rough steps are similar everywhere.

project can be initiated by citizens via different forms of direct democracy. In both cases, a proposal has to be made to the responsible representative body who then decides whether the budget is approved. If this is the case, the call for tenders (also called contract notice) for a public procurement project is subsequently published and firms can submit their bids. The length of the period, in which the firms can apply, is defined in the call for tenders. After the submission deadline, the public procurement authority chooses the winning offer according to the criteria that were also defined in the contract notice[6] and the contract is awarded to one or several firms. Afterwards, the project phase starts, which can be very short, e.g., if the project is the purchase of new pencils for a public authority, or very long, e.g., the construction of a new highway. Finally, once the project is completed there may be and opening ceremony where politicians cut ribbons and declare the project finished. While the payment for supplies projects happens more or less simultaneously with project delivery, there will be several payments throughout the duration of the project for works or service projects that have a longer duration.

In the data, which is used for the analysis, I observe the following three project steps: the contract notice, the project award, and the completion of the project. An increase in the call for tenders can be seen as a signal that the government is ready to invest, while the award (that is followed by the call) directly obliges the government to undertake the public expenditure.[7] Moreover, the firm winning the contract is made public at the time of the contract being awarded. Employees of the winning firm are another potential group of voters. As no immediate public expenditure is undertaken with the contract award, especially for projects with a long time duration, I define the contract award as a "credible election promise". Finally, the project completion is arguably the most visible part of the process and also the one where the incumbent can signal the most competence. However, manipulating the timing of the end of the project in a way that it happens not too long before and not after the election might be more difficult

[6]The types of procedures in TED are: award without prior publication of a contract notice, competitive dialogue, negotiated without a call for competition, negotiated with a call for competition, open, and restricted.

[7]Of course, the budget was already approved at this point in time, and this is also a signal, but I do not observe this event in my data.

than manipulating the timing of the contract notice and the contract award. Especially with construction projects, there are often unforeseen delays which can defer the project completion. Timing the contract notice and award in a way that fits to the election schedule in order to signal competence to voters is much easier. Therefore, I expect a bigger political budget cycle effect for contract notices and awards.

Hence, the first set of hypotheses is:

H1a: *More (valuable) public procurement calls for tenders are published prior to an election.*
H1b: *More (valuable) public procurement contracts are awarded prior to an election.*
H1c: *More (valuable) public procurement projects are completed prior to an election.*

I thereby also test whether there are heterogeneous effects according to the type of contract, i.e., a services, works or supplies contract. As mentioned before, a public procurement contract on supplies has a shorter project period than services or works contracts on average, which makes it more likely for incumbents to deliver a project within a quicker time scale and with less chance of delay. Chong et al. (2014) find a PBC effect for the completion of works contracts in French municipalities. So-called "ribbon-cutting" in front of new buildings or other infrastructure projects is often a very prominent event, which attracts a lot of media attention. Hence, works projects might be a popular tool to gain votes for elected politicians despite the high levels of uncertainty with regard to timing.

Moreover, I analyze heterogeneous effects according to the object of the contract, i.e., the sector classified in the International Standard Industrial Classification (ISIC). As previously discussed in the introduction, there is a substantial amount of literature studying PBCs in different spending categories. There is no universal result which expenditure categories dominate prior to elections. De Haan and Klomp (2013) provide an overview of papers studying different spending categories and discuss different determining factors for the heterogeneous findings.

The literature agrees, however, that only spending more will not lead to re-

election for incumbents. The spending has to be visible for citizens. Without the electorate being aware of the project, the incumbent cannot gain any votes on the grounds of this project being initiated. Therefore, it is necessary that projects are made visible to the general public. A large bulk of public procurement might not be visible and also not interesting to voters, such as the purchase of office supplies for public servants. Hence, I distinguish between visible and non-visible projects according to three different visibility categories. First, I classify projects as visible as already done in the literature. Chong et al. (2014) classify visible projects in the context of France as streets and public buildings such as sports, recreational, social buildings and schools. Marx (2018) studies the effect of project completion on electoral success in Africa and classifies visible projects as being in the transportation, electricity, water, education, and health sector. Second, I classify projects according to the labor intensity of the sector they correspond to. Higher labor intensity means that the project needs a higher labor force on average and this labor force consists of potential voters. Finally, I test whether larger projects are present before elections assuming that bigger projects have a higher visibility. I identify large projects as projects with a high value in monetary terms.

Therefore, the second set of hypotheses is:

H2a: *More (valuable) projects are published / awarded / completed prior to an election that are classified as visible in the literature.*
H2b: *More (valuable) labor-intensive projects are published / awarded / completed prior to an election.*
H2c: *More (valuable) big projects are published / awarded / completed prior to an election.*

1.2.2 Data

Public procurement data: The data on public procurement contracts are taken from Tenders Electronic Daily (TED), a platform provided by the European Commission containing all public procurement notices and awards whose values exceed a certain threshold outlined in the EU Public Procurement Directives 2014/23/EC and 2014/24/EC. The lowest threshold for certain types of services is

139,000 euros.[8] Moreover, countries also have the possibility to publish contracts below the thresholds on the platform on a voluntary basis.

The TED data contains information on the contracting authority including the address, details on the procured goods like the main activity and the final price, the number of bidders, as well as information on the winning bidder. In my analysis, I examine the time frame 2008 to 2018.

While the platform tries to harmonize public procurement information across Europe, there is a large heterogeneity across countries, as some countries only publish the information required, while other countries publish contracts with values below the thresholds voluntarily. In addition, it is well possible that politicians increase the number of contracts below the thresholds or even break down big contracts into several small ones to be able to speed up the procedure without compliance of rules for contracts above the thresholds as found in Castellani et al. (2018) for Italy. While Germany publishes much less than 10% of the total public procurement volume, Latvia publishes more than 50% according to an estimation by Skuhrovec (2017).

As I study national elections, I only include public procurement contracts from national authorities; a demand also made by Potrafke (2020) who finds different effects of government ideology on the budget composition comparing central and general government data. Table 1.1 shows the different authority types present in TED. Unfortunately, there are some categories which are not clearly identifiable as national or non-national. These are called "Body governed by public law", "Other", and "Not specified" and make up for a substantial share of the database. I focus my analysis on the categories "Ministry or any other national or federal authority" and "National or federal agency / office", which are clearly identified as being governed by the national government.

Figure 1.1 plots the number and aggregate value of national contracts both by country and by year. In each sub-figure, the values are shown for contract notices, contract awards, and contract completions. The number of contracts by year (Sub-figure a) is quite stable over time, with slightly higher levels in 2017 and 2018. The number of contract completions is only a small fraction of

[8] More information on the thresholds can be taken from European Commission (2020b) or from the mentioned Directives.

Table 1.1: Authorities in TED

	Notices		Awards		Completions	
Authority	Freq.	Percent	Freq.	Percent	Freq.	Percent
National or federal ministry / authority	191,993	10.09	190,563	10.87	29,208	9.49
Regional or local authority	583,227	30.66	517,039	29.49	110,772	35.98
Water, energy, transport and telecommunications	183,000	9.62	156,455	8.92	27,482	8.93
European Union institution / agency	10,380	0.55	10,533	0.6	175	0.06
Other international organization	879	0.05	602	0.03	87	0.03
Body governed by public law	408,100	21.45	390,517	22.27	54,482	17.7
Other	401,024	21.08	345,539	19.71	67,609	21.96
National or federal agency / office	31,862	1.67	30,766	1.75	4,620	1.5
Regional or local agency / office	52,940	2.78	47,997	2.74	10,091	3.28
Not specified	38,941	2.05	63,309	3.61	3,343	1.09
Total	1,902,346	100	1,753,320	100	307,869	100

Source: own calculations from TED data.

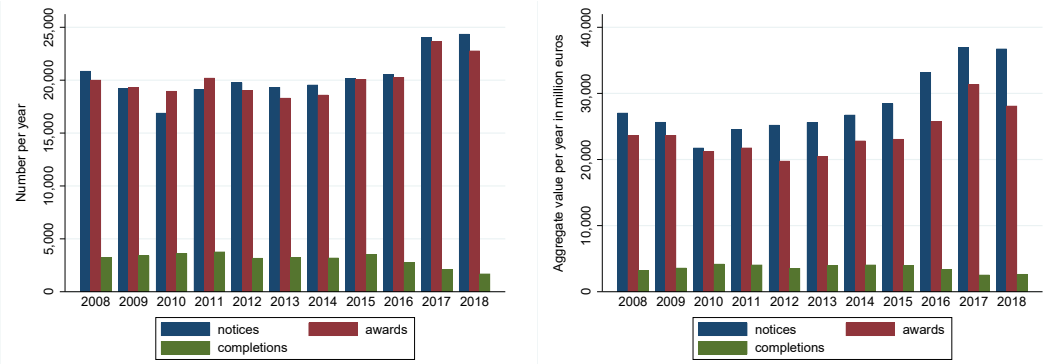
the notices and awards, as very few observations in the data have the contract completion date indicated. The amounts of contract notices and awards are very close together. Reasons for deviations between these two numbers might be: contract notices that are canceled before the award, the notice and the award not taking place in the same year, notices being split up into several awards, several notices being combined into one award, awards without prior notices. For the aggregate value of contracts (Sub-figure b), we see a slight increase between 2010 and 2017. The notices constantly have a higher value than the awards.

Turning to the number of contracts by country (Sub-figure c), the highest number of contract notices and awards is from France, followed by the Czech Republic, Germany, and Poland. Germany has the highest number of contract completions available. The picture changes when looking at the aggregate value of contracts (Sub-figure d). Here, the United Kingdom has the highest value of contract notices, but with a huge gap to its value of contract awards. France's value of contract awards is still higher, with Spain ranking in third, followed by the Czech Republic, Germany, Italy, Poland, and Romania.

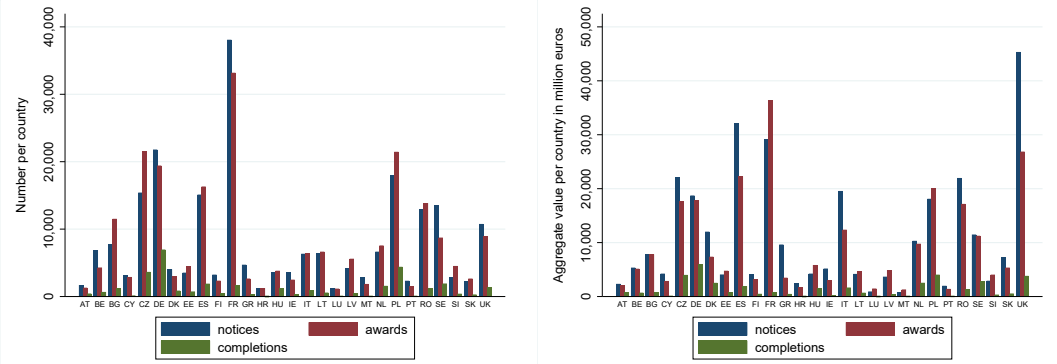
The database classifies the contracts into three broad categories: services, supplies, and works. Table 1.2 shows the number of contract awards per category for the national authorities named above. The category services has the highest aggregate value and also the highest number, followed by the supplies category.

Figure 1.1: Summary statistics for public procurement contracts

(a) *Number of contracts by year* (b) *Aggregate value of contracts by year*



(c) *Number of contracts by country* (d) *Aggr. value of contracts by country*



Notes: The figures plot the amount and aggregate value of contracts by year and country for contract notices, awards, and completions. Only national contracts from the categories "Ministry or any other national or federal authority" and "National or federal agency / office" are included. Source: own calculations from TED data.

Table 1.2: Number and value of contract awards by type for national authorities

Contract	Aggregate value in bio. euros	Number
Services	124.78	112,884
Supplies	93.88	84,224
Works	71.53	16,828

Source: own calculations from TED data.

Table 1.A.1 in the Appendix lists the number and aggregate values of contract awards by sector according to the section in the International Standard Industrial Classification of All Economic Activities (ISIC) Revision 4. The matching between public procurement contracts and ISIC sections was done manually via the Common Procurement Vocabulary (CPV) 2008 version indicated for each

public procurement contract in TED.[9] The first two digits of the CPV correspond to the product division that can be easily matched to the ISIC section. Table 1.A.9 in the Appendix presents the matching of CPV and ISIC codes. Some ISIC sections correspond to several CPV divisions, especially for Manufacturing, while a few CPV divisions correspond to several ISIC sections. Table 1.A.1 shows that Manufacturing is the largest sector in number and value of contracts. The construction sector nearly has the same aggregate value but much less contracts. The second largest sector in the number of awards is "Professional, scientific and technical advice".

Election data: The data on national elections is taken from the Voter Turnout Database by the International Institute for Democracy and Electoral Assistance (International IDEA) and contains information on the election year, whether it was a parliamentary or presidential election and the turnout. The election months were collected by hand. The sample from 2008 to 2018 contains 81 parliamentary elections and 30 presidential elections in the European Union.

Control variables: I include economic and demographic variables from Eurostat. These are GDP growth rate, government expenditure as share of GDP, unemployment rate, population size, and the share of population being younger than 15 and older than 64 years of age. Public procurement can serve as a tool for anti-cyclical spending, hence we might expect a negative effect of the GDP growth rate on public procurement. Moreover, I include a variable capturing the ideology of the government, as this is often correlated with public expenditure. I choose the seat share of social democratic and left parties in parliament from the "Comparative Political Data Set" (Armingeon et al. 2020).[10] General summary statistics of the variables employed are provided in Table 1.A.2 in the Appendix.

[9]Some observations in the TED data still contain the CPV 2003 version. This was updated manually.

[10]The exact definition of this variable is: "Government composition: relative power position of social democratic and other left parties in government based on their seat share in parliament, measured in percentage of the total parliamentary seat share of all governing parties. Weighted by the number of days in office in a given year." (Armingeon et al. 2020)

1.2.3 Empirical model

The project-level data allows me to conduct an analysis on a very detailed level. I aggregate the public procurement data to the monthly level by country. The main analysis uses a Poisson pseudo-maximum-likelihood (PPML) model by Santos Silva and Tenreyro (2006). They show that with heteroskedastic data, log-linearized estimation equations, and count data, the PPML estimator is less biased than OLS. The authors also show that their estimator is a good way to deal with zeros in the dependent variable. The method is frequently used for trade data (Santos Silva and Tenreyro 2006), but also has applications in estimating effects on merger and acquisition deals (Todtenhaupt et al. 2020). Public procurement data can be seen as count data. Moreover, when analyzing specific product categories, the dependent variable will contain a non-negligible amount of zeros, hence the PPML estimator is the appropriate method to use within this study.

I estimate the following model:

$$\begin{aligned} Y_{imt} &= exp(x'_{imt}\beta) \text{ with} \\ x'_{imt}\beta &= \alpha_1 + \gamma \cdot \text{election-year}_{imt} + \delta \cdot \mathbf{X_{it}} + \psi_i + \mu_{mt} + \epsilon_{imt} \end{aligned} \tag{1.1}$$

Y_{imt} is the outcome variable of country i in month m in year t. The outcome is the total value or the number of contract notices, awards or completions.

The variable election-year$_{imt}$ is defined as the twelve months leading up to an election with the last month being the election month. This "election year" definition is different from the one employed in many papers concerning PBC literature, where the election year is the calendar year when an election is happening. There is no alternative available for these papers if they only have yearly data for their outcome variable. My definition has the advantage that it is more homogeneous than the calendar year definition, as elections happen in different months in different countries. The calendar definition lumps elections in January together with elections in December, which might distort the pre-election effect.

I further include demographic and economic controls $\mathbf{X_{it}}$ as described above. ψ_i are country fixed effects and μ_{mt} are month $\times$ year fixed effects to extract

seasonal effects that affect all countries homogeneously. The error term ϵ_{imt} is clustered at the country level.

In order to understand the dynamics of public procurement around elections better, I also use an event study approach to estimate effects for each month. I again use a PPML model.

Following Fuest et al. (2018), the equation for the event study reads as follows:

$$Y_{imt} = exp(\alpha_1 + \sum_{k=-24}^{+12} (\gamma_k \cdot election_{i(mt+k)}) + \delta \cdot \mathbf{X_{it}} + \psi_i + \mu_{mt} + \epsilon_{imt}) \quad (1.2)$$

The outcome variable is the same as before. γ_k is the coefficient of interest, i.e., the effect of an election. I include 24 leads and 12 lags to capture the evolution of two years before and one year after the election. The event dummies are binned up at the window ends -24 and +12 like in Fuest et al. (2018), accounting for all elections outside the window. Different to standard practice, I exclude and normalize to zero L1 (the month after the election) instead of F1 (the month before the election), because I am primarily interested in the dynamics before the election. Moreover, country fixed effects, month × year fixed effects, and control variables are also included. The error term ϵ_{imt} is again clustered at the country-level.

A possible endogeneity problem arising from both regression models is that the timing of the election might be endogenous. Due to political scandals or very bad performance, the incumbent might decide to resign and to call an early election. Bad performance can include bad performance of the economy and therefore also impact public procurement. The control variables can explain part of this endogeneity, when the timing is not only shifted by a few months within one year but when an election is moved up into previous years. In robustness checks, I will include country × year fixed effects instead of the control variables to reduce potential omitted variable bias. Additionally, I will also exclude elections outside the normal schedule in robustness tests.

1.3 Results

1.3.1 Baseline results

Before turning to the results of the empirical analysis, I present some descriptive graphs. Figure 1.2 shows the average of contract notices (Sub-figures a and b), contract awards (Sub-figures c and d), and contract completions (Sub-figures e and f) per month around parliamentary elections for both the number and the aggregate value. For the number of contract notices and both contract award graphs, there is a lower level of contracts in the beginning of the observed period, i.e., from 24 to around 16 months prior to the election. For both contract notice graphs and the number of awards, we see a large drop after the election with a quick recovery. These two pieces of evidence could hint to a political budget cycle. We cannot observe any evidence of a political budget cycle for project completions from these graphs.

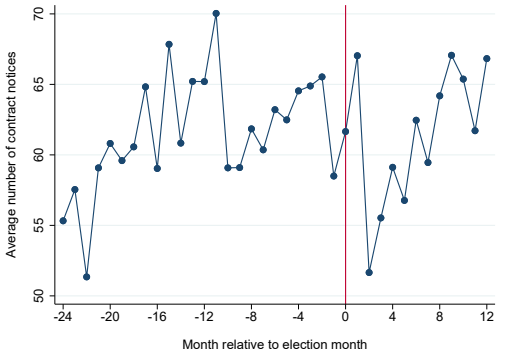
Table 1.3 shows the baseline results for Equation 1.1, i.e., the estimation for Hypotheses 1a, 1b, and 1c. I analyze the three different points in time – publishing a public procurement project (columns 1-4), awarding the contract (columns 5-8), and completing the project (columns 9-12) – for the number of public procurement projects and their total value.

For the contract notices, there are statistically significant effects on the 1% level for both the number of contracts and the aggregate value in the parliamentary election year. This means that in the election year there are on average 8% more contracts and the aggregate value is around 13% higher.[11] Also for contract awards around parliamentary elections, the effects for both number and aggregate value are highly statistically significant. For both variables, the effect is around 13%. For both contract notices and awards, the effects in the presidential election year are not statistically significant. For project completions, the only slightly statistically significant effect can be observed for the aggregate value in the parliamentary election year. The effect has a size of around 9%, but is only significant on the 10% level.

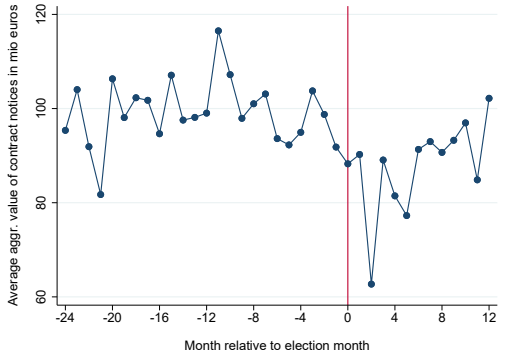
[11] To interpret a coefficient of a PPML as a semi-elasticity, one has to apply the transformation (exp(coefficient)-1)*100.

Figure 1.2: Descriptive evidence

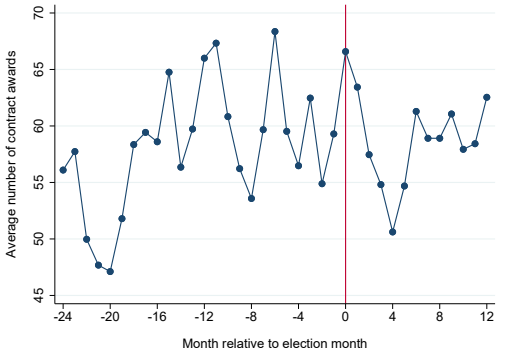
(a) *Number of contract notices*



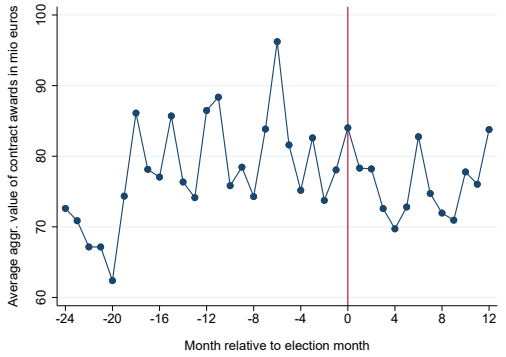
(b) *Aggregate value of contract notices*



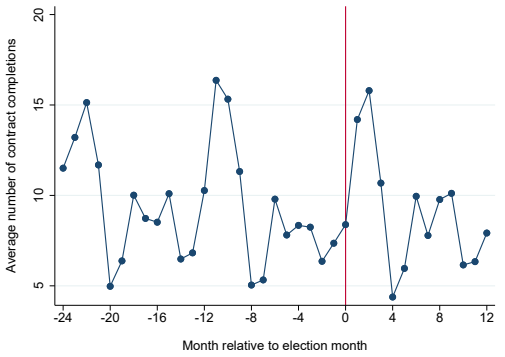
(c) *Number of contract awards*



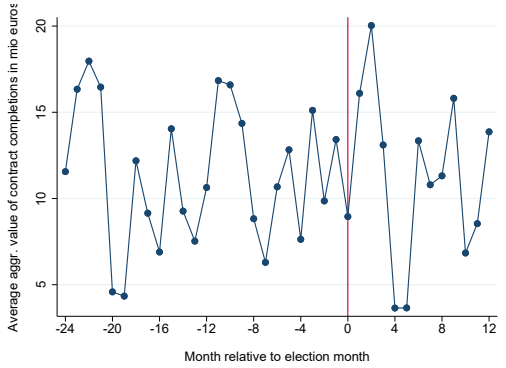
(d) *Aggregate value of contract awards*



(e) *Number of contract completions*



(f) *Aggr. value of contract completions*



Notes: The figures plot monthly averages of the number and the value of contract notices, contract awards, and contract completion around parliamentary elections.

Hence, we can observe a political budget cycle for contract notices and awards in parliamentary election years. There is only small evidence for PBCs in contract completions, which could also be explained by the small number of observations for which we know the project completion date. However, the finalization of a project is difficult to manipulate for the incumbent, as many players are involved

in projects, especially in large projects. Therefore, the result is only logical that the manipulation happens at the stage of a contract notice and award. Both steps do not involve any expenditures yet. The contract notice can be seen as a signal that the government is ready to invest in the near future and the contract award is a clear commitment to allocate money to a specific contract winner. With the contract awarded, a date for the project to commence is also set and this might be before or after the election. In any case, the contract award is a "credible election promise" to both the contract winner but also the citizens that will potentially benefit from the project.

Turning to the control variables, the GDP growth rate has several statistically significant and positive effects on contract notices and awards, hence it does not seem that public procurement serves as a anti-cyclical policy tool but rather that better performing countries are also more active in public procurement. However, the effect is quite small, i.e., at most 2%. The unemployment rate mostly has negative effects, which is particularly evident through the negative correlation with the completion of a contract. Here, the coefficients are statistically significant on the 1% level and have a size of more than 7% for a decrease in the unemployment rate by one percentage point. For the ideological variable, we see statistically but not economically significant and positive effects on contract notices.

I cluster the standard errors at the country level. As there are only 28 countries in the sample, the number of clusters might be too small. This might be problematic because too few clusters might lead to an over-rejection (Cameron et al. 2008). Therefore, I reestimate Table 1.3 using the "score bootstrap" method by Kline and Santos (2012), which is an adaptation of the wild cluster bootstrap that is appropriate for non-linear models (Roodman et al. 2019). The results are collected in Table 1.A.3 in the Appendix. The effects for the parliamentary election year keep their statistical significance like before, so too few clusters does not seem to be an issue.

Table 1.3: Baseline regression results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Category	Contract notices				Contract awards				Contract completions			
VARIABLES	Number		Aggregate value		Number		Aggregate value		Number		Aggregate value	
Parliamentary election year	0.0806***		0.1259***		0.1235***		0.1258***		0.0153		0.0867*	
	(0.0290)		(0.0440)		(0.0314)		(0.0343)		(0.0203)		(0.0510)	
Presidential election year		0.0676		0.0909		0.1038		0.0869		0.0283		-0.0367
		(0.0459)		(0.0874)		(0.0732)		(0.0933)		(0.0364)		(0.0814)
GDP growth rate	0.0042	0.0049*	0.0123***	0.0129***	0.0070**	0.0081***	0.0192***	0.0200***	-0.0081	-0.0079	-0.0039	-0.0040
	(0.0026)	(0.0027)	(0.0027)	(0.0024)	(0.0032)	(0.0030)	(0.0033)	(0.0030)	(0.0049)	(0.0050)	(0.0075)	(0.0077)
Unemployment rate	-0.0199**	-0.0198***	0.0012	0.0012	-0.0107	-0.0097	-0.0293***	-0.0287***	-0.0603***	-0.0598***	-0.0770***	-0.0767***
	(0.0077)	(0.0077)	(0.0105)	(0.0104)	(0.0119)	(0.0118)	(0.0093)	(0.0095)	(0.0174)	(0.0173)	(0.0220)	(0.0223)
Ln population	0.3601	0.3906	4.7379**	4.8213**	0.2960	0.2727	4.2051***	4.2369***	9.0779**	9.0413**	13.7412***	13.8705***
	(1.2928)	(1.3272)	(2.1898)	(2.2656)	(1.4672)	(1.5208)	(1.3632)	(1.4365)	(3.6324)	(3.5836)	(3.7827)	(3.8247)
Gov. expenditure / GDP	0.0078	0.0095	0.0072	0.0098	-0.0129	-0.0102	0.0177	0.0204	0.0223	0.0226	0.0538***	0.0545***
	(0.0067)	(0.0066)	(0.0236)	(0.0244)	(0.0141)	(0.0136)	(0.0167)	(0.0177)	(0.0149)	(0.0149)	(0.0176)	(0.0178)
Share population under 15	0.0472	0.0502	0.0533	0.0586	0.2223**	0.2241**	-0.0445	-0.0406	0.2173	0.2164	-0.0184	-0.0119
	(0.0675)	(0.0681)	(0.0964)	(0.1014)	(0.1000)	(0.1008)	(0.0734)	(0.0769)	(0.1332)	(0.1319)	(0.1409)	(0.1418)
Share population over 64	0.0224	0.0182	-0.0179	-0.0262	0.0858	0.0774	0.0168	0.0084	-0.0020	-0.0034	0.0608	0.0535
	(0.0703)	(0.0698)	(0.0939)	(0.0924)	(0.0942)	(0.0944)	(0.0538)	(0.0530)	(0.0895)	(0.0893)	(0.0773)	(0.0757)
Left seat share in parliament	0.0015**	0.0014**	0.0020***	0.0020***	0.0005	0.0004	0.0011	0.0011	0.0002	0.0002	-0.0007	-0.0007
	(0.0007)	(0.0007)	(0.0004)	(0.0005)	(0.0013)	(0.0013)	(0.0010)	(0.0011)	(0.0016)	(0.0016)	(0.0017)	(0.0017)
Constant	-4.8429	-5.3784	-59.3805*	-60.7532	-6.7520	-6.3902	-50.9125**	-51.4489**	969.6820**	985.0164**	1,120.8895	1,170.5916
	(21.2545)	(21.8307)	(35.8633)	(37.1629)	(24.3203)	(25.2597)	(22.0698)	(23.2447)	(475.8082)	(474.5168)	(1,452.8870)	(1,453.7274)
Observations	3,696	3,696	3,696	3,696	3,696	3,696	3,696	3,696	3,696	3,696	3,696	3,696
Country FEs	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Month x Year FEs	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Mean of dependent variable	60.44	60.44	96410332	96410332	57.81	57.81	77165223	77165223	9.33	9.33	12166313	12166313
Pseudo LL	-21929	-22013	-6.560e+10	-6.600e+10	-22596	-22786	-3.780e+10	-3.810e+10	-11895	-11895	-2.140e+10	-2.140e+10

*** p<0.01, ** p<0.05, * p<0.1. Notes: The table presents results for the estimation of the model in Equation 1.1. Standard errors are in parentheses and clustered at the level of countries.

As the effects for presidential elections are not statistically significant, I will focus on parliamentary elections throughout the analysis from now on. Table 1.A.4 presents results for different years in the parliamentary election cycle, i.e., from two years before the election until one year after the election. For contract notices (Panel A), the only other statistically significant effect besides the election year is a negative effect on the aggregate value of notices for the post-election year, which is about the same size in absolute value like the positive effect in the election year. For contract awards (Panel B), all election years are statistically significant, at least on the 10% level. The pre-election year has a positive effect on the number and aggregate value of contract awards, but they are less than half the size of the effects in the election year. The post-election year also has a negative effect here, which is, however, not as high as for contract notices. Again, for the number and value, the effect sizes are half the absolute value of the election year effects in the post-election year. Moreover, there is a lower number and value of contract awards two years before the election, but the coefficient sizes are the smallest of all years in absolute values. Finally, for the project completions (Panel C), the only other statistically significant effect besides the election year for completion values is the pre-election year in the aggregate value regression, which is significant on the 10% level. The effect is negative and bigger in absolute value than the positive effect in the election year.

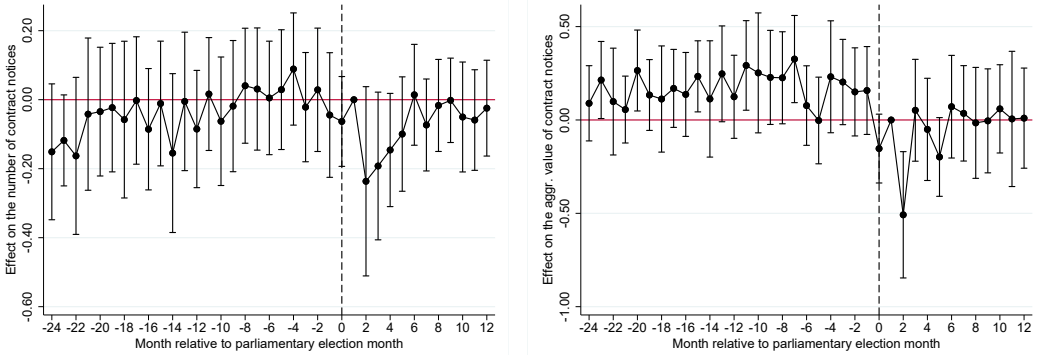
Because of the few observations of contract completions and the not very significant results, I will not continue to analyze contract completions and will instead focus on contract notices and awards in the following. In the baseline sample, where I do not differentiate between different spending or visibility categories, there is not a large number of zeros in the variables of contract notices and contract awards. Therefore, I do another robustness check and estimate Equation 1.1 as a linear model with the natural logarithm of the dependent variable.[12] The results are collected in Table 1.A.5 columns 1-4 of the Appendix. The effects for the number of contract notices and awards have a lower statistical power in the linear model, but are still significant at a level of 5%. The coefficient sizes are also smaller, but still economically meaningful with 7% and 10% more contract

[12]To be precise, the dependent variable is: ln(variable +1).

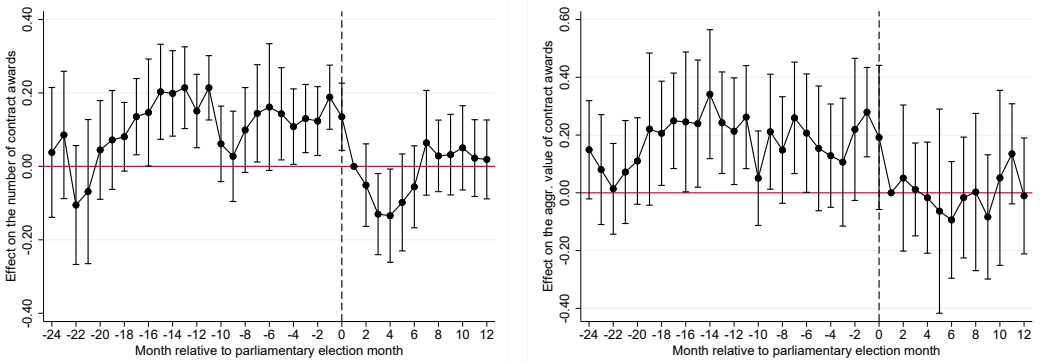
notices and awards in the election year, respectively. However, the effects on the aggregate values for both notices and awards lose all statistical power.

Figure 1.3: Event study analysis

(a) *Number of contract notices* (b) *Aggregate value of contract notices*



(c) *Number of contract awards* (d) *Aggregate value of contract awards*



Notes: The figure presents results for the estimation of the event study model in Equation 1.2 as PPML model. Point estimates are plotted with 95% confidence intervals. The results are also collected in Table 1.A.6 columns 1-4. Standard errors are in parentheses and clustered at the level of countries.

Next, I turn to the event study analysis from Equation 1.2 for contract notices and awards in parliamentary elections. Again, I first estimate a PPML model and then a linear model with the dependent variable in log. Figure 1.3 shows the results of the PPML model and plots the coefficients for the monthly dummies with the 95% confidence intervals. Table 1.A.6 in the Appendix collects all corresponding results from the event study. For the number of contract notices (Sub-figure a), all coefficients show no statistical significance. For the aggregate value of contract notices (Sub-figure b), there a few statistically significant and positive effects in the pre-election period, but overall the effects are statistically

not very precise. The level of point estimates is higher in the pre-election period than in the post-election period. The picture is much clearer for contract awards. For the number of awards (Sub-figure c), there are clear statistical significant and positive effects between 17 and 11 months before the elections, which then start again seven months before the election up until the election month. After the election, the dummies drop in size and even become negative and increase to a zero effect again seven months after the election. This observation is consistent with the findings of the analysis for different years of the election cycle in Table 1.A.4. For the aggregate value of contract awards (Sub-figure d), the statistical power is somewhat lower than for the number of awards, but still presents a similar picture. Turning to the results from the linear regression in Figure 1.A.1 in the Appendix, all effects are estimated with less precision, but the overall trend is still the same.

Before I turn to the estimations by category, I exclude elections that were held early, so-called snap elections. There are 26 parliamentary snap elections in my dataset; Table 1.A.7 in the Appendix lists these elections. Table 1.A.5 columns 5-8 collect the results for the estimation without snap elections. The effects are very robust to this exclusion; they are even bigger than the baseline results in Table 1.3 in two out of four cases.

As a last robustness check, I include country $\times$ year fixed effects instead of control variables. The results are collected in Table 1.A.5 columns 9-12 and are robust to the baseline results.

Until now, I presented robust evidence that there are political budget cycles in public procurement. They are present in both contract notices and awards, for both the number and the aggregate value. I interpret this finding as a "credible election promise". Although funds for public procurement projects have been approved before posting the call for tenders, awarding the contract makes the project credible, at least for the firm winning the contract. Moreover, I only call it an election promise as the money has not yet been paid out at the time I measure the contract award, which is the date of the winner announcement. The effects on the contract awards are the most robust throughout the regressions. Therefore, I only use contract awards for the following analyses.

1.3.2 Different categories

In this section, I analyze election cycles in contract awards according to different categories. First, I split up the contracts into the three main types – services, supplies, and works – to see whether the effect observed before is driven by one specific category or whether it is equal across the categories. Second, I split up the contracts according to the sector following the ISIC classification.

Table 1.4: Election effects by type of contract award

	(1)	(2)	(3)	(4)	(5)	(6)
	Number of contract awards			Aggregate value of contract awards		
CATEGORY	Services	Supplies	Works	Services	Supplies	Works
Parliamentary election year	0.1403***	0.1047***	0.1014*	0.1552***	0.0807*	0.2205***
	(0.0368)	(0.0290)	(0.0573)	(0.0396)	(0.0476)	(0.0693)
GDP growth rate	0.0037	0.0117***	0.0023	0.0128***	0.0258***	0.0096
	(0.0032)	(0.0037)	(0.0061)	(0.0046)	(0.0045)	(0.0096)
Unemployment rate	-0.0119	-0.0044	-0.0464**	-0.0104	-0.0217	-0.0865***
	(0.0133)	(0.0148)	(0.0207)	(0.0137)	(0.0143)	(0.0332)
Ln population	0.6557	-1.0620	3.9449**	4.9479***	-0.2530	5.6105**
	(1.2217)	(2.1199)	(1.7590)	(1.8482)	(1.6724)	(2.3027)
Government expenditure / GDP	-0.0120	-0.0207	0.0292	-0.0076	0.0095	0.0593***
	(0.0094)	(0.0230)	(0.0211)	(0.0181)	(0.0132)	(0.0228)
Share population under 15	0.1286	0.3082**	0.0031	-0.1535**	-0.0261	-0.1446
	(0.0893)	(0.1278)	(0.0991)	(0.0779)	(0.1005)	(0.1365)
Share population over 64	-0.0126	0.2240*	0.0345	-0.1034	0.2113**	-0.0440
	(0.0928)	(0.1159)	(0.0931)	(0.0717)	(0.0844)	(0.0810)
Left seat share in parliament	0.0012	-0.0008	0.0013	0.0016**	0.0010	0.0021
	(0.0011)	(0.0015)	(0.0020)	(0.0007)	(0.0009)	(0.0018)
Constant	-9.9576	10.4594	-65.2736**	-59.0245**	15.9223	-75.0382**
	(20.1130)	(34.9505)	(28.6641)	(29.7968)	(26.6301)	(38.2184)
Observations	3,696	3,696	3,696	3,696	3,696	3,696
Country FEs	yes	yes	yes	yes	yes	yes
Month × year FEs	yes	yes	yes	yes	yes	yes
Mean dep. variable	30.59	22.79	4.63	30373251	22386944	29737493
Pseudo LL	-16109	-16457	-7993	-2.140e+10	-1.840e+10	-5.820e+10

*** p<0.01, ** p<0.05, * p<0.1. Notes: The table presents results for the estimation of the model in Equation 1.1. The dependent variable in each regression only includes contract awards of one category – services, supplies or works, respectively. Standard errors are in parentheses and clustered at the level of countries.

Table 1.4 collects the results for the three types of contracts. For the number of contract awards, the strongest effect is exhibited by the services category with a point estimate of around 15%. This effect is also statistically significant on the 1% level. The other two categories both have effects of a size of around 11%,

while the supplies category coefficient is estimated with much more precision than the one in the works category. For the aggregate value of contract awards, the category with the strongest effect is the works category. The aggregate value of works contracts is on average around 25% higher in parliamentary election years than in other years. For the services category, this effect is around 17%. Both effects are statistically significant on the 1% level. The effect for the supplies category is weaker and also estimated with less precision. In summary, there is no clear dominance of one category, although there might be a tendency that incumbents prefer to award services and works contracts in election years.

Table 1.5: Election effects by sector of contract award

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
VARIABLES	Number of contract awards in a specific sector									
Election year	0.0759	-0.0390	0.1213***	0.1112**	0.1015*	0.1394*	0.2284***	0.0875**	0.2713**	-0.0009
	(0.0653)	(0.0404)	(0.0300)	(0.0451)	(0.0586)	(0.0822)	(0.0784)	(0.0376)	(0.1333)	(0.1341)
ISIC	A	B	C	D/E	F	H	I	J	K	L
Observations	3,696	3,696	3,696	3,696	3,696	3,696	3,696	3,696	3,696	3,200
Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Country FEs	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
M × Y FEs	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Mean dep. var.	.73	1.64	19.56	3	4.58	1.44	.77	6.69	0.69	.16
Pseudo LL	-3473	-4935	-15736	-6206	-7875	-5234	-3274	-8959	-3321	-1132

	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)
VARIABLES	Number of contract awards in a specific sector								
Election year	0.0991**	0.0708	0.2904***	0.1400	0.1916*	0.3304**	0.1473***	0.0024	0.1240*
	(0.0433)	(0.0955)	(0.0907)	(0.1245)	(0.1153)	(0.1297)	(0.0430)	(0.0000)	(0.0648)
ISIC	M	O	P	Q	R	S/U	V	W	X
Observations	3,696	3,696	3,696	3,564	3,696	3,696	3,696	2,832	3,696
Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes
Country FEs	yes	yes	yes	yes	yes	yes	yes	yes	yes
M × Y FEs	yes	yes	yes	yes	yes	yes	yes	yes	yes
Mean dep. var.	10.32	.44	1.43	1.42	.28	.43	3.32	.11	.97
Pseudo LL	-10633	-2181	-4947	-4709	-1957	-2683	-6841	-777.6	-3708

*** p<0.01, ** p<0.05, * p<0.1. Notes: The table presents results for the estimation of the model in Equation 1.1. The dependent variable in each regression only includes contract awards of one sector as defined in Table 1.A.1. Standard errors are in parentheses and clustered at the level of countries.

Table 1.5 presents the results for the election effect on the number of contract

awards by sector.[13] Many sectors exhibit statistically significant results. The biggest point estimate, which is also statistically significant on the 5% level, is the one for the sector "Other service activities and activities of extraterritorial organizations and bodies" (S/U) with an effect of 39%. This category includes services like car park management, port management, accommodation management, janitorial services, and many more. According to Table 1.A.1, it is a quite small category. The second biggest effect is the one in the education sector (P), with 34% more contract awards in parliamentary election years. The categories that follow are "Financial and insurance activities" (K), "Accommodation and food service activities" (I), and "Arts, entertainment and recreation" (R) with the last one having an effect of 21%. Table 1.A.8 collects the results for the value of contract awards by sector. There are less categories with statistically significant effects.[14] The education sector has by far the biggest effect, followed by the agricultural (A) and construction (F) sector. As the construction sector has a very high aggregate value, but not such a high number of contract awards, it is logical that the effect is only present for the value of contract awards but not for the amount. Ultimately, the baseline effects seem to be driven by a few sectors, although these are not same when looking at the number of contract awards and their aggregate value, except education that exhibits statistically significant and big effects in both regressions.

1.3.3 Visible projects

If incumbents aim to signal competence and to get attention with projects they plan to implement, they should put more emphasis on visible projects. In this section, I use three different definitions of visible projects. First, projects are classified into visible and non-visible projects according to the existing literature. As a second step, I analyze projects according to their labor intensity. Firms might need to hire more workers in order to undertake the project or they might just be able to secure jobs due to winning the project. More workers means more potential voters for the incumbent. Finally, I look at different size categories

[13] see Table 1.A.1 for sector names

[14] The categories W and X did not reach convergence in the estimation.

according to the contract value, as bigger projects should be more visible on average.

For the first visibility category, I create a dummy variable that takes the value 1 for visible projects as classified by Chong et al. (2014) and Marx (2018) (see Section 1.2.1) according to the two-digit CPV division in TED. The categories classified as visible are indicated with an asterisk in Table 1.A.9. The results in Table 1.6 clearly show that the effects for visible contracts awards (columns 1 and 2) are larger than the effects for non-visible contract awards (columns 5 and 6) for both the number and the aggregate value. However, the effects for non-visible awards are also statistically significant, hence, the incumbent governments do not only rely on visible projects. As a robustness check, I exclude the education category from the visible projects, which had a very strong effect in the analysis in Section 1.3.2. The effects reported in columns 3 and 4 of Table 1.6 decreased a little in comparison with columns 1 and 2, but are still bigger than the effects for non-visible projects.

To get an approximation of the labor intensity of each sector for the second visibility definition, I use OECD data on the gross value added (GVA) and employee compensation and calculate the share of employee compensation in GVA (OECD 2020). The two variables are broken down into sectors according to ISIC rev. 4, therefore I am again able to match the CPVs of the TED data to the ISIC codes. For each country, I only use the most recent available year, hence, the labor intensity variable does not vary over time in my dataset.[15] The matching of CPV to ISIC codes is not the same as for the analysis in Tables 1.5 and 1.A.8, as the sectors in the OECD data are broken down to lower levels of the ISIC classification for some countries. The structure of sectors is also different for each country.

For the analysis, I classify the CPV divisions of the public procurement projects into four different quartiles according to their labor intensity with the fourth quartile having the highest labor intensity. I also do the analysis for CPVs having a labor intensity below or above the median labor intensity. Looking at the results in Table 1.7 on the quartiles, all effects are statistically significant

[15]For most countries, the latest year is 2018. For the UK it is 2015 and for Bulgaria, Croatia, Greece, and the Netherlands it is 2017.

Table 1.6: Election effects by visibility of contract award

	(1)	(2)	(3)	(4)	(5)	(6)
CATEGORY	Visible awards				Non-visible awards	
VARIABLES	Number	Aggr. value	Number	Aggr. value	Number	Aggr. value
Election year	0.1415***	0.1675***	0.1232***	0.1450***	0.1127***	0.0773*
	(0.0385)	(0.0423)	(0.0400)	(0.0431)	(0.0307)	(0.0429)
GDP growth rate	0.0081**	0.0160***	0.0101**	0.0169***	0.0057*	0.0192***
	(0.0041)	(0.0043)	(0.0044)	(0.0044)	(0.0029)	(0.0038)
Unemployment rate	-0.0167	-0.0383***	-0.0207	-0.0428***	-0.0081	-0.0228
	(0.0145)	(0.0115)	(0.0140)	(0.0117)	(0.0122)	(0.0144)
Ln population	1.7246	6.1620***	1.7788	6.3924***	-0.3016	2.9502**
	(1.2003)	(1.6305)	(1.2278)	(1.7598)	(1.6778)	(1.3433)
Government expenditure / GDP	0.0029	0.0438**	0.0081	0.0485**	-0.0203	-0.0036
	(0.0096)	(0.0211)	(0.0095)	(0.0206)	(0.0175)	(0.0142)
Share population under 15	0.1646*	-0.0558	0.1566	-0.0511	0.2475**	-0.0514
	(0.0996)	(0.0782)	(0.0967)	(0.0795)	(0.1042)	(0.0813)
Share population over 64	-0.0002	-0.0439	0.0076	-0.0413	0.1440	0.0882
	(0.0917)	(0.0547)	(0.0899)	(0.0549)	(0.0960)	(0.0625)
Left seat share in parliament	0.0013	0.0013	0.0012	0.0014	-0.0001	0.0008
	(0.0013)	(0.0013)	(0.0013)	(0.0014)	(0.0013)	(0.0007)
Constant	-28.8653	-83.4149***	-30.3568	-87.4727***	1.2156	-31.4184
	(19.9638)	(26.3793)	(20.5101)	(28.5112)	(27.7026)	(21.8356)
Sample	full	full	no education		full	full
Observations	3,696	3,696	3,696	3,696	3,696	3,696
Country FEs	yes	yes	yes	yes	yes	yes
Month x Year FEs	yes	yes	yes	yes	yes	yes
Mean dep. Variable	20.14	34206373	18.72	33165655.9	37.84	43404436
Pseudo LL	-14358	-3.440e+10	-14011	-3.460e+10	-18933	-2.600e+10

*** p<0.01, ** p<0.05, * p<0.1. Notes: The table presents results for the estimation of the model in Equation 1.1. The dependent variable in each regression only includes contract awards of one category - visible or non-visible, respectively. The election year variable only includes parliamentary elections. Standard errors are in parentheses and clustered at the level of countries.

on conventional levels. The highest effects are observed for the number and aggregate value of contract awards with a labor intensity in the second quartile, but the effects for the fourth quartile are only slightly smaller. As the quartile analysis does therefore not give a clear picture, we turn to the regressions below and above the median. For the number of awards, the effect is higher above the median, with 14% more contract awards in the parliamentary election year on average. However, for the aggregate value of contract awards, the effect below the median is bigger than the one above. Here, the effects translates into a higher aggregate value of contract awards by 14.6% in the election year. Therefore, the evidence on whether incumbent governments choose more labor-intensive public

Table 1.7: Election effects by labor intensity of contract award

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Number of contract awards					
CATEGORY	Quartile 1	Quartile 2	Quartile 3	Quartile 4	Below median	Above median
Election year	0.0719*	0.1596***	0.1062***	0.1521***	0.1158***	0.1306***
	(0.0392)	(0.0441)	(0.0318)	(0.0389)	(0.0340)	(0.0327)
Observations	3,564	3,696	3,696	3,696	3,696	3,696
Control variables	yes	yes	yes	yes	yes	yes
Country FEs	yes	yes	yes	yes	yes	yes
Month × Year FEs	yes	yes	yes	yes	yes	yes
Mean dependent variable	12.82	11.34	14.7	19.34	23.7	34.04
Pseudo LL	-11345	-10721	-12185	-15279	-14733	-18376

	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Aggregate value of contract awards					
CATEGORY	Quartile 1	Quartile 2	Quartile 3	Quartile 4	Below median	Above median
Election year	0.1294**	0.1368**	0.0900**	0.1334***	0.1359***	0.1157***
	(0.0522)	(0.0564)	(0.0444)	(0.0286)	(0.0494)	(0.0294)
Observations	3,564	3,696	3,696	3,696	3,696	3,696
Control variables	yes	yes	yes	yes	yes	yes
Country FEs	yes	yes	yes	yes	yes	yes
Month × Year FEs	yes	yes	yes	yes	yes	yes
Mean dependent variable	20183942	17973666	17031564	22656435	37407638	39690139
Pseudo LL	-2.400e+10	-2.150e+10	-2.040e+10	-2.340e+10	-3.030e+10	-2.860e+10

*** p<0.01, ** p<0.05, * p<0.1. Notes: The table presents results for the estimation of the model in Equation 1.1. The dependent variable in each regression only includes contract awards of one category, i.e., projects with a labor intensity in the respective category. The election year variable only includes parliamentary elections. Standard errors are in parentheses and clustered at the level of countries.

procurement projects to gain more votes in the election is mixed.

Finally, I classify the projects according to their size in terms of their contract award value. As for the labor intensity analysis, I split up the projects into quartiles according to their project size and into below and above the median project value before aggregating them to the country-year-month level. The results in Table 1.8 show a decreasing effect the higher the project size. What this means, is that the highest effect is observed for the number and value of contract awards in the first quartile and the lowest in the fourth quartile. Likewise, the effect below the median project size is bigger than the effect above the project size for both number and aggregate value of contract awards. Hence, incumbent governments

do not choose to award bigger public procurement contracts as visible projects before elections to signal their competence.

Table 1.8: Election effects by project size of contract award

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Number of contract awards					
CATEGORY	Quartile 1	Quartile 2	Quartile 3	Quartile 4	Below median	Above median
Election year	0.1956***	0.1368***	0.1352***	0.1157***	0.1675***	0.1255***
	(0.0390)	(0.0385)	(0.0362)	(0.0304)	(0.0329)	(0.0318)
Observations	3,696	3,696	3,696	3,696	3,696	3,696
Control variables	yes	yes	yes	yes	yes	yes
Country FEs	yes	yes	yes	yes	yes	yes
Month × Year FEs	yes	yes	yes	yes	yes	yes
Mean dependent variable	12.75	12.64	12.61	12.69	25.39	25.3
Pseudo LL	-14538	-11619	-11256	-11518	-17605	-14917

	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Aggregate value of contract awards					
CATEGORY	Quartile 1	Quartile 2	Quartile 3	Quartile 4	Below median	Above median
Election year	0.1448***	0.1308***	0.1311***	0.1238***	0.1335***	0.1248***
	(0.0268)	(0.0395)	(0.0359)	(0.0363)	(0.0353)	(0.0349)
Observations	3,696	3,696	3,696	3,696	3,696	3,696
Control variables	yes	yes	yes	yes	yes	yes
Country FEs	yes	yes	yes	yes	yes	yes
Month × Year FEs	yes	yes	yes	yes	yes	yes
Mean dependent variable	823224	2645528	7214237	66496608	3468694	73710363
Pseudo LL	-4.920e+08	-1.090e+09	-2.950e+09	-4.080e+10	-1.230e+09	-3.880e+10

*** p<0.01, ** p<0.05, * p<0.1. Notes: The table presents results for the estimation of the model in Equation 1.1. The dependent variable in each regression only includes contract awards of one category, i.e., projects with a project size in the respective category. The election year variable only includes parliamentary elections. Standard errors are in parentheses and clustered at the level of countries.

To summarize this section, incumbents make use of visible projects defined in the literature, e.g., projects in education, health, construction, recreational services before elections to try to convince citizens to give them their vote. There is also some evidence that they increase the number of contract awards of labor-intensive projects. Incumbents do not use bigger projects as a type of visible projects in general to signal competence to voters.

1.4 Conclusion

This paper studies whether political budget cycles exist in public procurement across the European Union. Therefore, I analyze different steps of the public procurement process. The results show significant increases in the posting of calls for tenders and the awarding of public procurement contracts prior to national parliamentary elections. There is no evidence of more contract completions before elections. I interpret this finding as a "credible election promise" as at the time of the contract notice and award the budget is only committed, but no expenditure has been undertaken yet. Additionally, citizens cannot profit from the potential public good immediately, as the project most likely takes some time until completion.

The effect is not driven by a specific type of contract, i.e., neither services, supplies, nor works contracts dominate prior to elections. Instead, we observe stronger effects for certain sectors, especially the education sector. Moreover, a higher number and aggregate value of public procurement contracts is awarded for visible projects in parliamentary election years such as public utilities, education, health, construction, and cultural services. Furthermore, more labor-intensive contracts are awarded in parliamentary election years.

The paper is a contribution to the literature on political budget cycles, as it studies a novel aspect by analyzing different steps of the public procurement process and specific project categories that provides a better understanding of the mechanisms behind political budget cycles.

Public procurement data is a very good tool to analyze these election cycles and should be exploited more in future research, e.g., by investigating product categories on a more detailed level. Additionally, political budget cycles in public procurement should be analyzed for regional and local elections, as many expenditure categories are decentralized, especially in federal countries.

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1.A Appendix

Table 1.A.1: Contract awards by ISIC section for national authorities

Section	Aggr. value[1]	Number	ISIC description
A	2270.52	3084	Agriculture. forestry and fishing
B	14215.3	6848	Mining and quarrying
C	87145.64	84189	Manufacturing
D/E	11982.38	12714	Electricity, gas, steam and air conditioning supply, Water supply; sewerage, waste management and remediation activities
F	82578.81	19659	Construction
H	9074.6	6056	Transportation and storage
I	3619.97	3103	Accommodation and food service activities
J	35990.99	27435	Information and communication
K	3564.8	2858	Financial and insurance activities
L	761.1	579	Real estate activities
M	36966.87	43743	Professional, scientific and technical activities
O	3199.34	1762	Public administration and defence; compulsory social security
P	5120.02	5911	Education
Q	6213.02	5349	Human health and social work activities
R	654.42	1136	Arts, entertainment and recreation
S/U	2040.46	1868	Other service activities, Activities of extraterritorial organizations and bodies
V	16914.39	13637	Repair and maintenance services
W	344.33	299	Installation services (except software)
X	5998.33	3924	Postal and telecommunication services

[1] Aggregate value of contract awards in million euros.

Source: own calculations from TED data.

Table 1.A.2: Summary statistics

Variable	Obs	Mean	Std.Dev.	Min	Max	Source
Number of contract notices	3,696	60.44	66.87	0	621	TED
Aggregate value of contract notices	3,696	9.640e+07	1.300e+08	0	1.010e+09	TED
Number of contract awards	3,696	57.80763	66.08512	0	896	TED
Aggregate value of contract awards	3,696	7.72e+07	9.60e+07	0	1.18e+09	TED
Number of contract completions	3,696	9.332522	19.65615	0	278	TED
Aggr. value of contract completions	3,696	1.22E+07	2.95e+07	0	4.53e+08	TED
Parliamentary election year	3,696	0.247	0.431	0	1	International IDEA
Presidential election year	3,696	0.0920	0.289	0	1	International IDEA
GDP growth rate	3,696	2.913	5.644	-22.91	34.91	Eurostat
Unemployment rate	3,696	9.151	4.594	2.200	27.50	Eurostat
Ln population	3,696	15.87	1.408	12.92	18.23	Eurostat
Government expenditure / GDP	3,696	45.60	6.536	25.30	65.10	Eurostat
Share population under 15	3,696	15.76	1.710	13.10	21.90	Eurostat
Share population over 64	3,696	17.45	2.378	10.80	22.60	Eurostat
Left seat share in parliament	3,696	33.54	35.66	0	100	CPDS

Table 1.A.3: Score bootstrap of baseline table

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Category	Contract notices				Contract awards				Contract completions			
VARIABLES	Number		Aggregate value		Number		Aggregate value		Number		Aggregate value	
Parliamentary election year	0.081		0.126		0.124		0.126		0.015		0.087	
	[2.086]		[2.753]		[2.778]		[2.598]		[0.402]		[2.090]	
	(0.009)		(0.008)		(0.000)		(0.001)		(0.717)		(0.043)	
Presidential election year		0.068		0.091		0.104		0.087		0.028		-0.037
		[0.925]		[0.820]		[1.017]		[0.669]		[0.475]		[-0.328]
		(0.514)		(0.514)		(0.413)		(0.870)		(0.656)		(0.787)
GDP growth rate	0.004	0.005	0.012	0.013	0.007	0.008	0.019	0.020	-0.008	-0.008	-0.004	-0.004
	[0.892]	[1.186]	[1.657]	[2.028]	[1.576]	[1.901]	[2.079]	[2.243]	[-0.708]	[-0.690]	[-0.104]	[-0.041]
	(0.412)	(0.268)	(0.117)	(0.041)	(0.137)	(0.064)	(0.038)	(0.013)	(0.598)	(0.595)	(0.954)	(0.984)
Unemployment rate	-0.020	-0.020	0.001	0.001	-0.011	-0.010	-0.029	-0.029	-0.060	-0.060	-0.077	-0.077
	[-1.190]	[-1.145]	[0.244]	[0.288]	[-0.794]	[-0.742]	[-1.150]	[-1.093]	[-1.593]	[-1.590]	[-2.058]	[-2.029]
	(0.299)	(0.310)	(0.808)	(0.768)	(0.424)	(0.518)	(0.277)	(0.325)	(0.129)	(0.115)	(0.057)	(0.062)
Ln population	0.360	0.391	4.738	4.821	0.296	0.273	4.205	4.237	9.078	9.041	13.741	13.870
	[0.696]	[0.616]	[1.781]	[1.689]	[0.600]	[0.528]	[2.201]	[2.047]	[1.934]	[1.960]	[2.152]	[2.195]
	(0.498)	(0.569)	(0.095)	(0.116)	(0.610)	(0.629)	(0.041)	(0.047)	(0.034)	(0.029)	(0.033)	(0.033)
Government expenditure / GDP	0.008	0.009	0.007	0.010	-0.013	-0.010	0.018	0.020	0.022	0.023	0.054	0.055
	[1.585]	[1.644]	[0.847]	[1.025]	[0.228]	[0.494]	[2.496]	[2.366]	[-0.125]	[-0.091]	[1.090]	[1.099]
	(0.123)	(0.102)	(0.466)	(0.350)	(0.830)	(0.692)	(0.017)	(0.010)	(0.904)	(0.939)	(0.324)	(0.299)
Population under 15	0.047	0.050	0.053	0.059	0.222	0.224	-0.045	-0.041	0.217	0.216	-0.018	-0.012
	[0.966]	[0.973]	[1.892]	[1.847]	[1.168]	[1.171]	[0.526]	[0.540]	[1.026]	[1.030]	[0.606]	[0.664]
	(0.396)	(0.389)	(0.076)	(0.092)	(0.287)	(0.294)	(0.621)	(0.621)	(0.351)	(0.345)	(0.555)	(0.530)
Population over 64	0.022	0.018	-0.018	-0.026	0.086	0.077	0.017	0.008	-0.002	-0.003	0.061	0.053
	[0.247]	[0.218]	[-0.078]	[-0.112]	[0.787]	[0.759]	[0.283]	[0.246]	[0.936]	[0.930]	[1.502]	[1.506]
	(0.726)	(0.780)	(0.942)	(0.909)	(0.648)	(0.670)	(0.794)	(0.809)	(0.373)	(0.349)	(0.172)	(0.136)
Left seat share in parliament	0.001	0.001	0.002	0.002	0.000	0.000	0.001	0.001	0.000	0.000	-0.001	-0.001
	[2.459]	[2.393]	[1.391]	[1.399]	[0.329]	[0.318]	[0.590]	[0.587]	[0.425]	[0.420]	[-0.419]	[-0.408]
	(0.017)	(0.031)	(0.181)	(0.201)	(0.781)	(0.789)	(0.613)	(0.645)	(0.707)	(0.697)	(0.722)	(0.715)
N	3696	3696	3696	3696	3696	3696	3696	3696	3696	3696	3696	3696

Notes: The table presents results for the estimation of the model in Equation 1.1, i.e., it repeats Table 1.3 and estimates standard errors according to the score bootstrap method. Wald test z-statistics are reported in brackets and p-values in parentheses. Standard errors are clustered at the level of countries. The results were estimated using the boottest command in Stata by Roodman et al. (2019) with 999 replications and Rademacher weights.

Table 1.A.4: Effects of different years in the election cycle

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A				Contract notices				
			Number				Aggregate value	
Pre-pre-election year	-0.0478				-0.0053			
	(0.0304)				(0.0348)			
Pre-election year		0.0090				0.0661		
		(0.0323)				(0.0484)		
Election year			0.0806***				0.1259***	
			(0.0290)				(0.0440)	
Post-election year				-0.0247				-0.1273***
				(0.0248)				(0.0368)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel B				Contract awards				
			Number				Aggregate value	
Pre-pre-election year	-0.0381*				-0.0443**			
	(0.0228)				(0.0225)			
Pre-election year		0.0423**				0.0596*		
		(0.0175)				(0.0306)		
Election year			0.1235***				0.1258***	
			(0.0314)				(0.0343)	
Post-election year				-0.0556***				-0.0553*
				(0.0192)				(0.0310)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel C				Contract completions				
			Number				Aggregate value	
Pre-pre-election year	-0.0461				0.0335			
	(0.0477)				(0.0662)			
Pre-election year		-0.0068				-0.1231*		
		(0.0421)				(0.0639)		
Election year			0.0153				0.0867*	
			(0.0203)				(0.0510)	
Post-election year				0.0076				-0.0092
				(0.0256)				(0.0454)
Observations	3,696	3,696	3,696	3,696	3,696	3,696	3,696	3,696
Control variables	yes	yes	yes	yes	yes	yes	yes	yes
Country FEs	yes	yes	yes	yes	yes	yes	yes	yes
Month × Year FEs	yes	yes	yes	yes	yes	yes	yes	yes

*** p<0.01, ** p<0.05, * p<0.1. Notes: The table presents results for the estimation of the model in Equation 1.1, with the difference that the election year variable represents a different year in each regression. Standard errors are in parentheses and clustered at the level of countries.

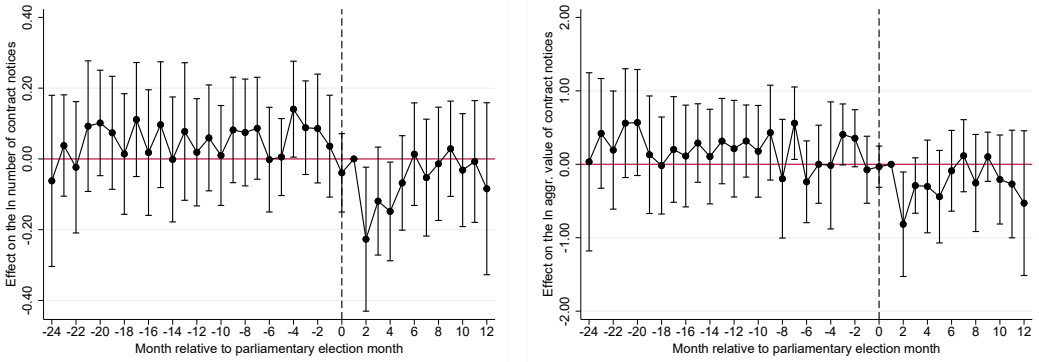
Table 1.A.5: Robustness: Linear model, exclusion of snap elections, and country×year FEs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Contract notices		Contract awards		Contract notices		Contract awards		Contract notices		Contract awards	
	Number	Aggr. value	Number	Aggr. value	Number	Aggr. value	Number	Aggr. value	Number	Aggr. value	Number	Aggr. value
Parliamentary election year	0.0681**	0.1084	0.0992**	0.2797	0.0782**	0.1436***	0.1424***	0.1224***	0.0558*	0.1314***	0.1003***	0.0822**
	(0.0302)	(0.1462)	(0.0374)	(0.1689)	(0.0327)	(0.0510)	(0.0375)	(0.0357)	(0.0327)	(0.0488)	(0.0256)	(0.0391)
GDP growth rate	0.0099**	0.0322	0.0096*	0.0347*	0.0038	0.0109***	0.0062*	0.0183***				
	(0.0046)	(0.0193)	(0.0051)	(0.0200)	(0.0027)	(0.0028)	(0.0033)	(0.0030)				
Unemployment rate	-0.0031	0.0896	0.0013	0.0419	-0.0199**	0.0003	-0.0107	-0.0297***				
	(0.0182)	(0.0926)	(0.0212)	(0.0941)	(0.0078)	(0.0104)	(0.0122)	(0.0096)				
Ln population	-1.0773	-12.7775	-1.3112	-10.0486	0.3934	4.8667**	0.2818	4.2893***				
	(2.7769)	(15.7103)	(2.9755)	(15.4667)	(1.2973)	(2.1744)	(1.4814)	(1.3764)				
Gov. expenditure / GDP	0.0111	0.0184	0.0084	0.0280	0.0078	0.0067	-0.0125	0.0178				
	(0.0071)	(0.0590)	(0.0098)	(0.0495)	(0.0067)	(0.0235)	(0.0136)	(0.0169)				
Share population under 15	-0.1144	-0.7492	-0.0705	-0.8979	0.0498	0.0601	0.2247**	-0.0380				
	(0.1438)	(0.7857)	(0.1604)	(0.7878)	(0.0676)	(0.0990)	(0.0988)	(0.0730)				
Share population over 64	-0.0374	-0.7711	0.0020	-0.6544	0.0222	-0.0221	0.0854	0.0133				
	(0.1119)	(0.5946)	(0.1211)	(0.5748)	(0.0706)	(0.0945)	(0.0938)	(0.0544)				
Left seat share in parliament	0.0027***	0.0045	0.0014	0.0039	0.0015**	0.0019***	0.0005	0.0011				
	(0.0009)	(0.0034)	(0.0010)	(0.0036)	(0.0007)	(0.0004)	(0.0013)	(0.0010)				
Constant	22.2518	241.7756	24.6830	199.1270	-5.4006	-61.4036*	-6.5569	-52.2676**	4.5050***	18.8710***	4.4844***	18.6152***
	(47.0276)	(266.5309)	(50.5097)	(263.7331)	(21.3129)	(35.6027)	(24.5001)	(22.2740)	(0.0083)	(0.0132)	(0.0068)	(0.0104)
Method	OLS	OLS	OLS	OLS	PPML	PPML	PPML	PPML	PPML	PPML	PPML	PPML
Election sample	all	all	all	all	no snap	no snap	no snap	no snap	all	all	all	all
Observations	3,696	3,696	3,696	3,696	3,696	3,696	3,696	3,696	3,660	3,660	3,660	3,660
Country FEs	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Country × Year FEs	no	no	no	no	no	no	no	no	yes	yes	yes	yes
Month × Year FEs	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Mean dependent variable	3.63	17.06	3.54	17.12	60.44	96410332	57.81	77165223	61.03	97358630	58.38	77924225
Pseudo LL					-21954	-6.560e+10	-22566	-3.790e+10	-18194	-5.000e+10	-16849	-2.860e+10

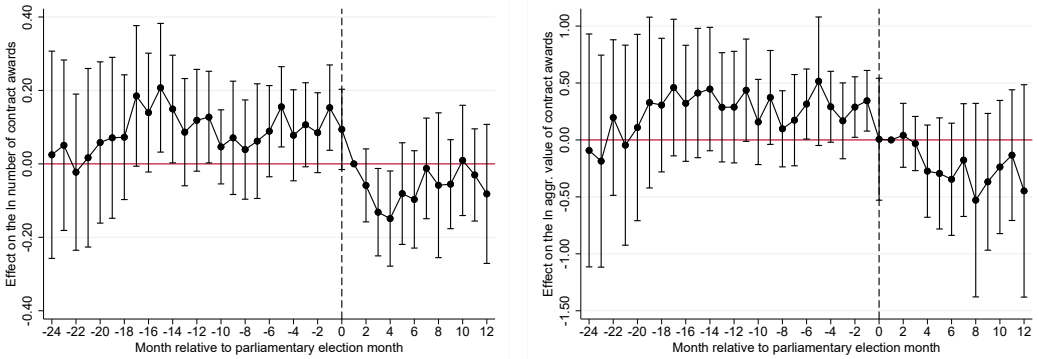
*** p<0.01, ** p<0.05, * p<0.1. Notes: The table presents results for the estimation of the model in Equation 1.1. Columns 1-4 estimate a linear model with the dependent variable defined as the natural logarithm, that is ln(variable+1). Columns 5-8 estimate the PPML model but exclude snap elections. Columns 9-12 include country × year fixed effects instead of control variables. Standard errors are in parentheses and clustered at the level of countries.

Figure 1.A.1: Event study analysis as linear model

(a) *Number of contract notices* (b) *Aggregate value of contract notices*



(c) *Number of contract awards* (d) *Aggregate value of contract awards*



Notes: The figure presents results for the estimation of the event study model in Equation 1.2 in the linear version. The dependent variable is defined as the natural logarithm, that is ln(variable+1). Point estimates are plotted with 95% confidence interval. The results are also collected in Table 1.A.6 columns 5-8. Standard errors are in parentheses and clustered at the level of countries.

Table 1.A.6: Event study regressions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Contract notices		Contract awards		Contract notices		Contract awards	
	Number	Aggr. value	Number	Aggr. value	Number	Aggr. value	Number	Aggr. value
Election in *t+24*	-0.1511	0.0892	0.0380	0.1490*	-0.0620	0.0333	0.0247	-0.0922
	(0.1005)	(0.1028)	(0.0902)	(0.0868)	(0.1179)	(0.5911)	(0.1375)	(0.4984)
Election in *t+23*	-0.1180*	0.2140**	0.0856	0.0804	0.0379	0.4208	0.0507	-0.1862
	(0.0673)	(0.1053)	(0.0885)	(0.0970)	(0.0698)	(0.3640)	(0.1131)	(0.4537)
Election in *t+22*	-0.1629	0.0986	-0.1053	0.0138	-0.0237	0.1933	-0.0226	0.1965
	(0.1161)	(0.1459)	(0.0826)	(0.0801)	(0.0904)	(0.3921)	(0.1036)	(0.3330)
Election in *t+21*	-0.0419	0.0558	-0.0687	0.0719	0.0926	0.5601	0.0166	-0.0460
	(0.1126)	(0.0914)	(0.1001)	(0.0909)	(0.0900)	(0.3612)	(0.1185)	(0.4281)
Election in *t+20*	-0.0346	0.2648**	0.0448	0.1101	0.1017	0.5686	0.0582	0.1092
	(0.0952)	(0.1106)	(0.0685)	(0.0764)	(0.0726)	(0.3516)	(0.1070)	(0.3988)
Election in *t+19*	-0.0230	0.1337	0.0720	0.2206	0.0739	0.1299	0.0710	0.3283
	(0.0950)	(0.0967)	(0.0687)	(0.1344)	(0.0778)	(0.3901)	(0.1068)	(0.3655)
Election in *t+18*	-0.0577	0.1122	0.0807*	0.2060**	0.0139	-0.0169	0.0726	0.3060
	(0.1159)	(0.1451)	(0.0477)	(0.0920)	(0.0831)	(0.3220)	(0.0828)	(0.2859)
Election in *t+17*	-0.0023	0.1691	0.1354**	0.2493***	0.1114	0.2019	0.1850*	0.4600
	(0.0942)	(0.1066)	(0.0530)	(0.0843)	(0.0785)	(0.3503)	(0.0933)	(0.2923)
Election in *t+16*	-0.0856	0.1368	0.1466**	0.2453**	0.0180	0.1125	0.1396*	0.3218
	(0.0897)	(0.1147)	(0.0744)	(0.1235)	(0.0865)	(0.3376)	(0.0789)	(0.2483)
Election in *t+15*	-0.0111	0.2336**	0.2031***	0.2395**	0.0968	0.2895	0.2073**	0.4120
	(0.0922)	(0.0971)	(0.0660)	(0.1123)	(0.0866)	(0.2605)	(0.0854)	(0.2768)
Election in *t+14*	-0.1548	0.1126	0.1987***	0.3414***	-0.0014	0.1049	0.1494**	0.4468
	(0.1175)	(0.1589)	(0.0593)	(0.1138)	(0.0861)	(0.3140)	(0.0714)	(0.2640)
Election in *t+13*	-0.0050	0.2471*	0.2142***	0.2427***	0.0775	0.3156	0.0863	0.2864
	(0.1024)	(0.1309)	(0.0568)	(0.0897)	(0.0947)	(0.2832)	(0.0712)	(0.2337)
Election in *t+12*	-0.0851	0.1242	0.1507***	0.2132**	0.0188	0.2125	0.1187*	0.2882
	(0.0867)	(0.1135)	(0.0510)	(0.0942)	(0.0738)	(0.3207)	(0.0676)	(0.2387)
Election in *t+11*	0.0164	0.2920**	0.2140***	0.2620***	0.0595	0.3164	0.1273**	0.4367*
	(0.0835)	(0.1224)	(0.0447)	(0.0909)	(0.0729)	(0.2393)	(0.0608)	(0.2189)
Election in *t+10*	-0.0625	0.2521	0.0613	0.0506	0.0098	0.1769	0.0463	0.1577
	(0.0950)	(0.1639)	(0.0524)	(0.0835)	(0.0688)	(0.3044)	(0.0492)	(0.1823)
Election in *t+9*	-0.0186	0.2280*	0.0273	0.2115**	0.0819	0.4312	0.0709	0.3733*
	(0.0970)	(0.1285)	(0.0627)	(0.1015)	(0.0726)	(0.3148)	(0.0752)	(0.2009)
Election in *t+8*	0.0405	0.2259*	0.0990*	0.1481	0.0748	-0.1961	0.0389	0.0977
	(0.0850)	(0.1258)	(0.0589)	(0.0941)	(0.0736)	(0.3940)	(0.0659)	(0.1629)
Election in *t+7*	0.0309	0.3262***	0.1445**	0.2594***	0.0867	0.5595**	0.0619	0.1735
	(0.0904)	(0.1191)	(0.0676)	(0.0984)	(0.0702)	(0.2403)	(0.0761)	(0.1952)
Election in *t+6*	0.0051	0.0771	0.1615*	0.2067**	-0.0021	-0.2384	0.0890	0.3153**
	(0.0839)	(0.1087)	(0.0880)	(0.1045)	(0.0721)	(0.2713)	(0.0605)	(0.1499)
Election in *t+5*	0.0291	-0.0027	0.1432**	0.1538	0.0053	0.0007	0.1554***	0.5156*
	(0.0886)	(0.1184)	(0.0641)	(0.1101)	(0.0530)	(0.2591)	(0.0533)	(0.2750)
Election in *t+4*	0.0889	0.2313	0.1082**	0.1286	0.1406**	-0.0154	0.0778	0.2911*
	(0.0830)	(0.1530)	(0.0525)	(0.0912)	(0.0661)	(0.4215)	(0.0604)	(0.1518)
Election in *t+3*	-0.0216	0.2033*	0.1302***	0.1061	0.0883	0.4064*	0.1064*	0.1678
	(0.0808)	(0.1165)	(0.0474)	(0.1129)	(0.0645)	(0.2023)	(0.0558)	(0.1623)
Election in *t+2*	0.0286	0.1504	0.1234***	0.2197*	0.0859	0.3547*	0.0849	0.2887**
	(0.0912)	(0.1204)	(0.0478)	(0.1256)	(0.0749)	(0.1894)	(0.0530)	(0.1295)
Election in *t+1*	-0.0444	0.1578	0.1883***	0.2793***	0.0361	-0.0746	0.1531**	0.3439**
	(0.0921)	(0.1200)	(0.0445)	(0.0789)	(0.0702)	(0.2224)	(0.0567)	(0.1298)
Election in *t*	-0.0631	-0.1532	0.1351***	0.1917	-0.0394	-0.0329	0.0939*	0.0060
	(0.0664)	(0.0940)	(0.0466)	(0.1273)	(0.0540)	(0.1368)	(0.0533)	(0.2609)
Election in *t-2*	-0.2365*	-0.5079***	-0.0511	0.0511	-0.2268**	-0.8162**	-0.0586	0.0406
	(0.1399)	(0.1726)	(0.0574)	(0.1290)	(0.0992)	(0.3468)	(0.0484)	(0.1369)
Election in *t-3*	-0.1922*	0.0519	-0.1300**	0.0117	-0.1192	-0.2891	-0.1316**	-0.0315
	(0.1092)	(0.1392)	(0.0564)	(0.0820)	(0.0743)	(0.1837)	(0.0580)	(0.1160)
Election in *t-4*	-0.1458*	-0.0504	-0.1343**	-0.0168	-0.1484**	-0.3018	-0.1490**	-0.2740
	(0.0837)	(0.1397)	(0.0648)	(0.0981)	(0.0679)	(0.3076)	(0.0631)	(0.1974)
Election in *t-5*	-0.0996	-0.1981*	-0.0982	-0.0635	-0.0679	-0.4412	-0.0810	-0.2943
	(0.0846)	(0.1076)	(0.0673)	(0.1803)	(0.0651)	(0.3070)	(0.0674)	(0.2378)
Election in *t-6*	0.0142	0.0711	-0.0556	-0.0937	0.0135	-0.0883	-0.0968	-0.3454
	(0.0746)	(0.1402)	(0.0569)	(0.1030)	(0.0706)	(0.2680)	(0.0646)	(0.2401)
Election in *t-7*	-0.0733	0.0355	0.0642	-0.0163	-0.0528	0.1167	-0.0123	-0.1767
	(0.0680)	(0.1303)	(0.0728)	(0.1067)	(0.0805)	(0.2392)	(0.0668)	(0.2413)
Election in *t-8*	-0.0167	-0.0155	0.0286	0.0027	-0.0137	-0.2545	-0.0582	-0.5287
	(0.0681)	(0.1516)	(0.0497)	(0.1389)	(0.0780)	(0.3221)	(0.0961)	(0.4142)
Election in *t-9*	-0.0018	-0.0048	0.0319	-0.0835	0.0288	0.1026	-0.0553	-0.3676
	(0.0624)	(0.1421)	(0.0560)	(0.1097)	(0.0656)	(0.1633)	(0.0590)	(0.2926)
Election in *t-10*	-0.0502	0.0595	0.0506	0.0518	-0.0316	-0.2075	0.0094	-0.2378
	(0.0813)	(0.1206)	(0.0585)	(0.1545)	(0.0778)	(0.2952)	(0.0732)	(0.2850)
Election in *t-11*	-0.0590	0.0056	0.0225	0.1350	-0.0073	-0.2685	-0.0302	-0.1335
	(0.0744)	(0.1848)	(0.0534)	(0.0884)	(0.0838)	(0.3572)	(0.0612)	(0.2797)
Election in *t-12*	-0.0245	0.0098	0.0190	-0.0107	-0.0842	-0.5291	-0.0818	-0.4475
	(0.0709)	(0.1368)	(0.0548)	(0.1023)	(0.1184)	(0.4802)	(0.0923)	(0.4545)
Method	PPML	PPML	PPML	PPML	OLS	OLS	OLS	OLS
Observations	3,024	3,024	3,024	3,024	3,024	3,024	3,024	3,024
Control variables	yes	yes	yes	yes	yes	yes	yes	yes
Month × Year FEs	yes	yes	yes	yes	yes	yes	yes	yes
Country FEs	yes	yes	yes	yes	yes	yes	yes	yes
Pseudo LL	-17607	-4.840e+10	-16819	-2.870e+10				

*** p<0.01, ** p<0.05, * p<0.1. Notes: The table presents results for the estimation of the event study model in Equation 1.2. Columns 1-4 estimate the PPML model. Columns 5-8 estimate a linear model with the dependent variable defined as the natural logarithm, that is ln(variable+1). Standard errors are in parentheses and clustered at the level of countries.

Table 1.A.7: List of parliamentary snap elections with sources

Country	Election date	Source
Austria	Sep 2008	BBC, September 29, 2008
Austria	Oct 2017	The Guardian, December 16, 2017
Belgium	Jun 2010	Deutsche Welle, June 13, 2010
Bulgaria	May 2013	novinite.com, May 12, 2013
Bulgaria	Oct 2014	BBC, October 4, 2014
Bulgaria	Mar 2017	BalkanInsight, December 20, 2016
Croatia	Sep 2016	Politico, July 16, 2016
Czech Republic	Oct 2013	Deutsche Welle, August 23, 2013
Greece	Oct 2009	Reuters, September 2, 2009
Greece	May 2012	The New York Times, April 11, 2012
Greece	Jun 2012	BBC, May 16, 2012
Greece	Jan 2015	AlJazeera, December 29, 2014
Greece	Sep 2015	BBC, August 20, 2015
Italy	Apr 2008	BBC, February 6, 2008
Latvia	Sep 2011	September 18, 2011
Luxembourg	Oct 2013	VOA News, October 21, 2013
Malta	Jun 2017	BBC, May 1, 2017
Netherlands	Jun 2010	Financial Times, February 23, 2010
Netherlands	Sep 2012	Deutsche Welle, September 13, 2012
Portugal	Jun 2011	The Wall Street Journal, April 1, 2011
Slovakia	Mar 2012	The New York Times, March 11, 2012
Slovenia	Dec 2011	The Slovenia Times, February 7, 2012
Slovenia	Jul 2014	Deutsche Welle, July 13, 2014
Spain	Nov 2011	The New York Times, July 29, 2011
Spain	Jun 2016	The Washington Post, June 22, 2016
United Kingdom	Jun 2017	Independent, April 19, 2017

Notes: This table presents parliamentary snap elections excluded for the analysis in Table 1.A.5.

Table 1.A.8: Election effects by sector on value of contract award

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Aggr. value of contract awards in a specific sector								
Election year	0.2623**	0.0535	0.1245***	0.0973	0.2503***	-0.0684	0.0979	0.1034	0.0406
	(0.1167)	(0.0874)	(0.0422)	(0.0761)	(0.0709)	(0.0912)	(0.1327)	(0.0788)	(0.1390)
ISIC	A	B	C	D/E	F	H	I	J	K
Observations	3,696	3,696	3,696	3,696	3,696	3,696	3,696	3,696	3,696
Control variables	yes	yes	yes	yes	yes	yes	yes	yes	yes
Country FEs	yes	yes	yes	yes	yes	yes	yes	yes	yes
Month × Year FEs	yes	yes	yes	yes	yes	yes	yes	yes	yes
Mean dep. var.	426065	5430587	17878068	2272321	28380407	2685311	818574	7977616	1128705
Pseudo LL	-1.870e+09	-2.350e+10	-1.610e+10	-5.320e+09	-5.590e+10	-1.410e+10	-4.360e+09	-1.220e+10	-5.140e+09

VARIABLES	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)
	Aggr. value of contract awards in a specific sector							
Election year	-0.8129***	0.0605	-0.2927*	0.4711***	0.2090	0.2468	0.5260	-0.0575
	(0.2012)	(0.0472)	(0.1698)	(0.1197)	(0.1492)	(0.2167)	(0.3608)	(0.0887)
ISIC	L	M	O	P	Q	R	S/U	V
Observations	3,100	3,696	3,696	3,696	3,564	3,668	3,696	3,696
Control variables	yes	yes	yes	yes	yes	yes	yes	yes
Country FEs	yes	yes	yes	yes	yes	yes	yes	yes
Month × Year FEs	yes	yes	yes	yes	yes	yes	yes	yes
Mean dep. var.	440189	7406146	1411562	1038905	1536982	120973	454347	3696886
Pseudo LL	-1.880e+09	-9.710e+09	-4.000e+09	-3.590e+09	-4.860e+09	-6.700e+08	-2.760e+09	-9.110e+09

*** p<0.01, ** p<0.05, * p<0.1. Notes: The table presents results for the estimation of the model in Equation 1.1. The dependent variable in each regression only includes contract awards of one sector as defined in Table 1.A.1. The election year variable only includes parliamentary elections. Standard errors are in parentheses and clustered at the level of countries.

Table 1.A.9: Correspondence CPV and ISIC codes

CPV label	CPV	ISIC	ISIC rev. 4 label
Agricultural, farming, fishing, forestry and related products	3	A	Agriculture, forestry and fishing
Agricultural, forestry, horticultural, aquacultural and apicultural services	77	A	Agriculture, forestry and fishing
Petroleum products, fuel, electricity and other sources of energy	9	B	Mining and quarrying
Mining, basic metals and related products	14	B	Mining and quarrying
Services related to the oil and gas industry	76	B	Mining and quarrying
Food, beverages, tobacco and related products	15	C	Manufacturing
Agricultural machinery	16	C	Manufacturing
Clothing, footwear, luggage articles and accessories	18	C	Manufacturing
Leather and textile fabrics, plastic and rubber materials	19	C	Manufacturing
Printed matter and related products	22	C	Manufacturing
Chemical products	24	C	Manufacturing
Office and computing machinery, equipment and supplies except furniture and software packages	30	C	Manufacturing
Electrical machinery, apparatus, equipment and consumables; lighting	31	C	Manufacturing
Medical equipments, pharmaceuticals and personal care products	33	C	Manufacturing
Transport equipment and auxiliary products to transportation	34	C	Manufacturing
Security, fire-fighting, police and defence equipment	35	C	Manufacturing
Musical instruments, sport goods, games, toys, handicraft, art materials and accessories	37*	C	Manufacturing
Laboratory, optical and precision equipments (excl. glasses)	38	C	Manufacturing
Furniture (incl. office furniture), furnishings, domestic appliances (excl. lighting) and cleaning products	39	C	Manufacturing
Industrial machinery	42	C	Manufacturing
Machinery for mining, quarrying, construction equipment	43	C	Manufacturing
Construction structures and materials; auxiliary products to construction (except electric apparatus)	44	C	Manufacturing
Collected and purified water	41*	D/E	Electricity, gas, steam and air conditioning supply, Water
Public utilities	65*	D/E	supply; sewerage, waste management and remediation
Sewage, refuse, cleaning and environmental services	90*	D/E	activities
Construction work	45*	F	Construction
Transport services (excl. Waste transport)	60	H	Transportation and storage
Supporting and auxiliary transport services; travel agencies services	63	H	Transportation and storage
Hotel, restaurant and retail trade services	55	I	Accommodation and food service activities
Radio, television, communication, telecommunication and related equipment	32	J	Information and communication
Software package and information systems	48	J	Information and communication
IT services: consulting, software development, Internet and support	72	J	Information and communication
Financial and insurance services	66	K	Financial and insurance activities
Real estate services	70	L	Real estate activities
Architectural, construction, engineering and inspection services	71*	M	Professional, scientific and technical activities
Research and development services and related consultancy services	73	M	Professional, scientific and technical activities
Business services: law, marketing, consulting, recruitment, printing and security	79	M	Professional, scientific and technical activities
Administration, defence and social security services	75	O	Public administration and defence; compulsory social security
Education and training services	80*	P	Education
Health and social work services	85*	Q	Human health and social work activities
Recreational, cultural and sporting services	92*	R	Arts, entertainment and recreation
Other community, social and personal services	98*	S/U	Other service activities, Activities of extraterritorial organizations and bodies
Repair and maintenance services	50*	V	Repair and maintenance services
Installation services (except software)	51*	W	Installation services (except software)
Postal and telecommunications services	64*	X	Postal and telecommunication services

Notes: This table presents the matched CPV division codes in the TED data with the ISIC rev. 4 section codes.

2

The political economy of multilateral lending to European regions

2.1 Introduction

We study the political economy of loan allocation decisions of the European Investment Bank (EIB) or "the bank of the European Union". We ask whether these lending decisions made by the powerful Board of Directors of the EIB, which consists of representatives of European Union (EU) Member States, purely follow the economic and EU integration related goals set by the bank, or if they can be explained instead by political or personal motives of the Member State representatives running the bank. Beyond the direct importance of understanding what determines the lending of the world's largest multilateral lending (and borrowing) institution, this work may provide additional insights into the question of how the interplay between the incentives of non-elected officials and national interests shapes the policies of international organizations.

With the emergence of the European project, the EIB has gained importance with near exponential rates of growth in lending over the last few decades (see, Figure 2.1-a). In 2017, new commitments by the EIB in Europe summed up to

around 76 billion euros (EIB 2017b).[1] The EIB together with the European Fund for Strategic Investments also serves as an important investment instrument in Europe, with current debates on the economic governance of the Eurozone often highlighting an even larger future role for the EIB (EIB 2015b, 2018).[2]

There are important reasons to believe that state investment banks like the EIB can serve the general interest of society. Traditional arguments stress the market failure fixing roles of these banks (Stiglitz 1994), while more recent views celebrate the capacity of these banks to invest in large and risky innovative projects, which may potentially have positive spillovers across the whole economy (Mazzucato and Penna 2016). The hopes of European policy makers to secure funding for large public investment projects, which the private markets fail to provide, are often tied to the EIB since the small EU budget typically cannot afford to finance these (Clifton et al. 2018). However, a potential trade-off is whether this type of government intervention is prone to other forms of governance failure such as rent-seeking by the technocrats running the bank. Since the EIB is not under democratic scrutiny directly, another hope is that this financing instrument would be largely free of the constraints of distributive politics such as pork barrel type of spending. This goes in contrast to the EU budget, which has too often served as a tool to please political and national appetites.[3] On the other hand, the absence of electoral incentives and related constraints of political accountability may open other opportunities for decision-makers at the EIB to discriminate in lending decisions such as based on their personal gains or preferences.

Our specific focus is the Board of Directors, which is the decisive body for the approval of loans at the EIB. This body includes a Director nominated by each

[1] To put this number into perspective, note that the EU's annual budget, which does not include the EIB, is around twice this much (European Commission 2018).

[2] For example, the EIB took a leading role in the European economic policy package to fight the effects of the Covid-19 crisis. Early in the crisis it was announced on April 9, 2020 that the EIB group would establish a European Guarantee Fund that shall mobilize 200 billion euros of finance for companies, which corresponds to 37% of the overall package (Eurogroup 2020). The global financial crisis presents another example of EIB's role in anti-cyclical economic policy in Europe. See, e.g., Corsetti et al. (2020) for a discussion of how the EU utilized official lending to respond to the crisis.

[3] See, e.g., Gehring and Schneider (2018) and Aksoy (2010) who show that, respectively, the EU Commissioner for Agriculture and the EU President are able to influence the budget allocation in favor of their home countries.

EU Member State and one from the European Commission. We hand-collect data on the careers of the population of all 470 Directors with the aim of measuring the connections of Directors at the level of European sub-national regions as far as these can be captured by the Directors' work experience.[4] We then match the Director-level data to administrative project-level data of EIB loans granted since the foundation of the Bank in 1959[5] again aggregated at the level of European regions. We describe the data, provide access to it, and explain the programs we use in Appendix 2.B.

Using difference-in-differences and distributed lag models, we provide evidence supporting the hypothesis that lending is more likely to flow to the home regions of Directors compared to other regions upon appointment at the Board. This phenomenon, that we label "home bias" effect, amounts to an average 17 percentage points (or 40% of the sample mean) increase in the likelihood of receiving a loan. Interestingly, the home bias effect is entirely driven by a relatively small sub-set of very large infrastructural mega-projects.

One crucial question regarding the home bias effect that we document is whether these discriminatory lending practices facilitate economic efficiency or whether they result in inefficient misallocation of resources. There are several potential explanations behind the home bias effect, all of which predict a larger flow of transactions into the home regions,[6] but with divergent predictions on the economic value of these transactions. On the one hand, Directors may have a personal gain in transferring resources to their home regions. The EIB's rules of "Code of Conduct for the Members of the Board of Directors" (EIB 2012) reveal the potential existence of issues of this nature by preventing former Board members to "lobby with members of the EIB governing bodies and Bank staff for their business, client or employer" within six months after leaving the Board. We label this mechanism as favoritism. The EIB is different from democratic contexts where politicians have electoral motives, however, in addition to personal

[4]In additional tests we also study the Directors' region of education.

[5]We focus on direct project loans by the EIB. Projects that cost at least 25 million euros qualify for this type of loans.

[6]One exception in the literature documenting positive effects of home bias in various outcomes is Fisman et al. (2020) who shows that bureaucrats sharing a hometown or college connection with an incumbent member of the Chinese Politburo are actually penalized in their probability to be promoted to the Politburo.

gains due to favoritism, the directors may also simply have social preferences towards their home regions. For example, Transparency International EU (2016) points out that senior managers of the EIB have a lot of freedom to favor their home countries without citing the reasons to do so. Either way, this favoritism or preference-based discrimination in lending practices will likely lead to resource misallocation. On the other hand, Directors may be able to reduce information asymmetries between the EIB and the borrowers in their region of work, thereby creating more value for both parties. For example, an informed Director may be able to reduce search costs or relax the costly needs of enforcement effort by identifying the set of projects most worthy of investments.

Five pieces of evidence speak against the information hypothesis. First, we study a sub-sample of Directors who change their work region during or after their service at the EIB. While sending money to their pre-EIB regions can be a mix of the information and favoritism channels, we show some evidence that the resources are sent to the post-EIB regions during the end of the Board membership, which is likely to be due to favoritism assuming that the Directors cannot have a priori information about the new region. Second, as laid out by Rajan (1992), Persson and Zhuravskaya (2016), and Fisman et al. (2017), the degree of information a Director has about a region (measured by her length of experience at the region) should be positively correlated with the amount of home bias lending. Third, following Cornell and Welch (1996) and Fisman et al. (2017), we hypothesize that more informed lending practices should increase the variance of loan sizes because with more precise signals the Director's prior beliefs of borrower quality have a wider distribution. Our evidence does not support either of these hypotheses. Fourth, our evidence that the home bias phenomenon is entirely driven by infrastructural mega-projects may be more consistent with favoritism rather than the informational channels since it is likely that there is already much common information about such project as compared to smaller and more sophisticated projects. This finding is also in line with Do et al. (2017) and Persson and Zhuravskaya (2016) who show that favoritism in Vietnam and China, respectively, operates through expenditures on construction infrastructure rather than social expenditures such as education. Finally, following Persson and Zhu-

ravskaya (2016), we study the timing of formation of the home bias and show that the additional lending is flowing to regions of Directors' workplace rather than their education regions, which may speak against the hypothesis that favoritism is driven by social preferences rather than by personal gain.

Overall, our evidence is consistent with the view that the regional home bias at the EIB is driven by the favoritist practices of its Directors thus leading to resource misallocation and economic inefficiency. However, we ultimately fail to reject that other efficiency-enhancing factors can be responsible for the home bias effect.[7] We also note that our findings are based on observable connections that are self-reported on CVs of Directors. Unobservable connections may play an important role, but obviously we cannot analyze these. The institutional setup and a number of tests such as showing the absence of pre-trends support the view that the region of work of a Director is plausibly exogenous to her nomination decision. However, we cannot rule out potential region-specific time-variant unobservables that are correlated both with the probability to lend and the nomination decision. One major candidate is regional demand for loans, which may in principle respond to the nomination decision.

This paper contributes to several strands of literature. First, it is related to work on the political economy of international organizations, which focuses on the United Nations (UN), the International Monetary Fund (IMF) and the World Bank (WB), among others. This literature typically finds that political economy factors are major determinants behind important decisions at these institutions (for a review, see, Dreher and Lang 2019). For example, a number of studies show that the probability of receiving IMF and WB loans (as well as the leniency

[7]Several papers try to isolate the favoritism and information channels in different contexts. For example, Khwaja and Mian (2005) find that politically connected firms in Pakistan borrow 45% more than their non-connected peers. By studying the outcomes of these loans they show that the connected firms have 50% higher default rates. Haselmann et al. (2018) show that German firms whose CEOs are in elite social networks with bank representatives receive more lending. Although the immediate terms of loans such as interest rates or defaults that connected firms receive are not different from those of control firms, the paper shows that the ex-post loan performance as measured by return on loans is substantially lower for connected firms. Unfortunately, data unavailability on the performance of EIB loans prohibits us from performing such an exercise. Another strategy is used by Fuchs and Gehring (2017). The paper first documents that rating agencies more favorably rate the sovereign bonds of their home countries. However, a test that compares countries where information is likely to be abundant to countries where information is not as easily accessible suggests that home biased ratings are probably not driven by the informational advantages of agencies towards their homes.

of the attached conditions) is positively correlated with the recipient countries' voting behavior at the international arena, such as whether they vote in line with the US or G7 countries at the UN Security Council (see, among others, Stone 2004; Barro and Lee 2005; Sturm and de Haan 2005; Dreher et al. 2009; Kilby 2009; Kaja and Werker 2010; Moser and Sturm 2011; Dreher and Sturm 2012). This literature on international organizations has paid little attention to the EIB, which is surprising given the size of the Bank. Robinson (2009), Clifton et al. (2018), and Mertens and Thiemann (2019) are the few papers on EIB that we are aware of. These papers describe the Bank, its functions, and evolution using a mix of qualitative and quantitative methods. Our contribution is to bridge this gap.

Second, the paper adds to the related literature studying the politics behind different financing instruments of the EU (for general reviews, see, Alesina et al. 2005; Baldwin and Wyplosz 2012; Dür et al. 2020). In particular, studies find that political factors, such as voting and proposal powers of the Member States in the EU, but also other international organizations, systematically affect the allocation of the EU Budget (see, among others, Bachtler and Mendez 2007; Aksoy 2010, 2012; Bodenstein and Kemmerling 2012; Schneider 2013; Mikulaschek 2018; Gehring and Schneider 2018).[8] Our contribution to this literature is to document the existence of home bias at the regional level in addition to the previously found biases at the national level. This is important since regional home bias may have very different implications. In addition, the failure to account for regional home bias of EU level politicians might have led the previous papers to wrongly attribute these type of bias to national bias since the home regions of politicians are often situated within their home countries.[9]

Third, the paper contributes to a recent strand of mostly development-related research on regional favoritism. The literature shows that political leaders systematically give favors to their ethno-linguistic groups (Kudamatsu 2009; Miquel 2007; Franck and Rainer 2012; Kramon and Posner 2016, 2013; Dickens 2018) and

[8]Nevertheless, there is evidence that the EU budget has positive albeit very small economic effects (Becker et al. 2010), which are only concentrated in areas with high levels of human capital and quality of government (Becker et al. 2013).

[9]For a comparison between home bias in intranational versus home bias in international trade, see Wolf (2000).

their regions of origin (Do et al. 2017; Dreher et al. 2019) in terms of higher federal transfers and public goods, or as observed in higher intensity night light data more generally (Hodler and Raschky 2014). Golden and Min (2013) presents an overview of this literature. Following several recent extensions of these results to democracies (see, e.g., Carozzi and Repetto 2016; Fiva and Halse 2016; Baskaran and Lopes da Fonseca 2018; Fabre and Sangnier 2017; Dahan and Yakir 2019, for evidence on Germany, Italy, Israel, France and Norway, respectively), we show that favoritism also takes place in institutionally mature environments.

Finally, this paper is related to a field in financial economics studying whether political considerations influence credit allocations of government-owned banks. This literature finds that, unlike private banks, lending by government-controlled banks is likely to follow political business cycles and to flow to electorally important districts both in advanced (e.g., Chavaz and Rose 2019; Englmaier and Stowasser 2017, with evidence on US and Germany, respectively) and in less developed countries (among others, see Dinc 2005; Cole 2009; Carvalho 2014, for evidence on Brazil, India and a set of 36 countries, respectively).[10]

2.2 Institutional setting and data

The European Investment Bank: The EIB was founded in 1958 following the Treaty of Rome. One of the important aims of the Bank from the very start was to support the EU in reaching its goals of integration. The annual sum of signed loans has risen substantially over time from 34 million euros in 1959 to around 77 billion euros in 2015, this positive trend kicking off especially from the 1980s.[11] The EIB mainly lends to EU Member States (90% of signed loans in 2017, EIB 2017b), but also to other countries all over the world. Today, the bank is the largest multilateral lending (and borrowing) institution in the world. It is the main EU funding source for some policy areas like transport, and, for some countries, EIB funds are larger than resources flowing from EU regional policies (Robinson 2009). A significant portion of the funds goes to poorer regions. The

[10] A related field studies whether political connections of firms influence their opportunities to attract lending (among others, see Sapienza 2004; Khwaja and Mian 2005; Faccio et al. 2006; Haselmann et al. 2018).

[11] Own calculations based on EIB (2020c)

EIB's target is to lend to projects related to cohesion spending at the amount equivalent to 30% of its annual new operations in the European Union, Pre-Accession and EFTA countries (EIB 2020b).

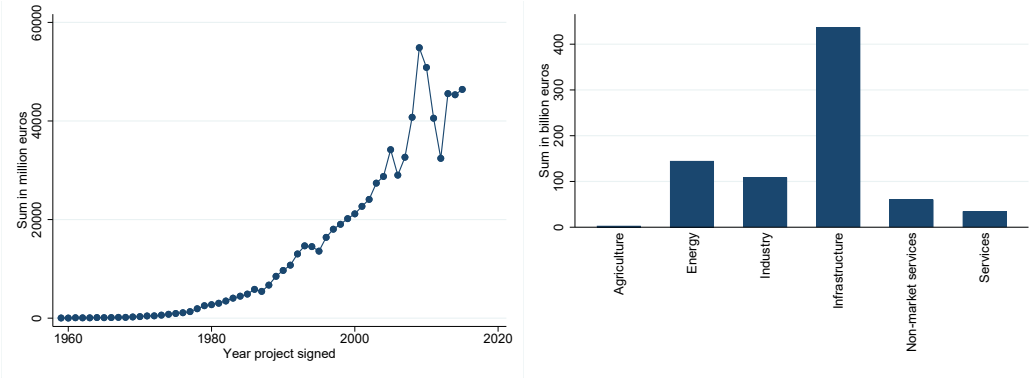
Applicants for a loan can be from all levels of government, as well as private and public firms. Projects that cost less than 25 million euros are disbursed via intermediate banks. As the Bank of the EU, the EIB generally finances projects that are in line with the economic policy objectives of the EU. Currently, some of the main priorities of the Bank include support to innovation activities, small and medium sized enterprises, infrastructure projects, and projects enhancing sustainable environment (EIB 2020a).[12]

Figures 2.1 and 2.2 (a) present the size and targets of the EIB project loans. Sub-figure 2.1(a) shows that the loans have been growing substantially in size over time. Sub-figure (b) shows the distribution of loans across sectors. Infrastructure is the largest sector. When looking at the distribution of the total amount of loans over countries in Sub-figure (c), the major shareholders of the Bank seem to receive the largest shares of the EIB loans. Sub-figure (d) shows the average annual share of EIB loans over GDP from 1999 to 2014 for EU Member States. On average over this period, the largest recipient is Estonia, receiving funding amounting to more than 1% of its GDP, while the Netherlands gets the least with about 0.1% of GDP. Finally, Figure 2.2 (a) shows the geographical distribution of loans on the regional level. This map demonstrates substantial heterogeneity across regions with the distribution being skewed to the poorer Southern regions of the EU.

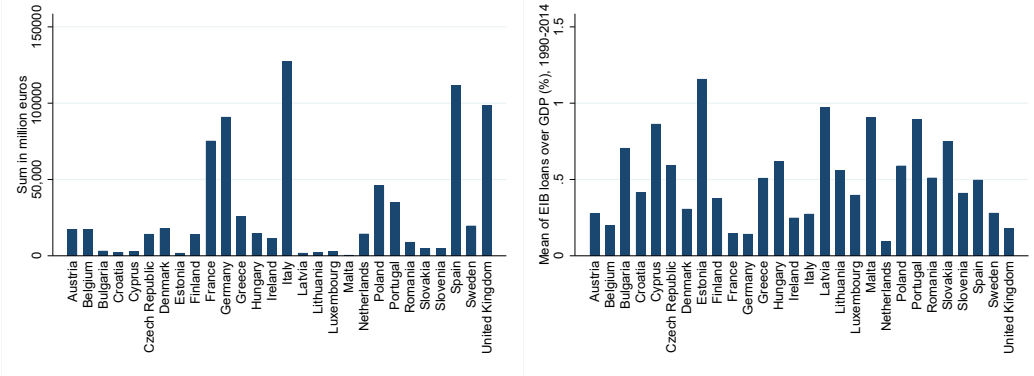
[12]In the 2017 Activity Report, the EIB describes some show-case examples: A project on innovation developed an in-organic substitute for coconut shells as supercapacitors. A microfinance firm in Luxembourg has been part of the support for SMEs. The expansion of electric cars in Paris was part of the infrastructure dimension and environmental projects included the rewilding of a region in Bulgaria (EIB 2017a).

Figure 2.1: Descriptives on the size, evolution, and distribution of EIB loans

(a) *Over time* (b) *By sector*



(c) *By country* (d) *Average share in GDP*



Notes: The figures are constructed from EIB data taken from EIB (2020c). Data on GDP is in 2005 prices and is taken from the European Regional Database.

Board of Directors and the approval of loans: Each EU Member State appoints one representative Director while an additional Director is nominated by the Commission. Moreover, today, there are 19 Alternate Directors. Most of the Directors are leading bureaucrats in their respective country, e.g., in the Ministry of Finance. Being a Director at the EIB is not a full-time job, they still follow their main occupation and only travel occasionally to the EIB in Luxembourg. The Directors are appointed for a period of five years and meet at least six times a year to decide on loan allocations. The Alternate Directors are also present at the meetings and support the Full Directors. The four big countries Germany, France, Italy, and the United Kingdom have two Alternates each, while all other countries share one Alternate in groups of two to eight.[13]

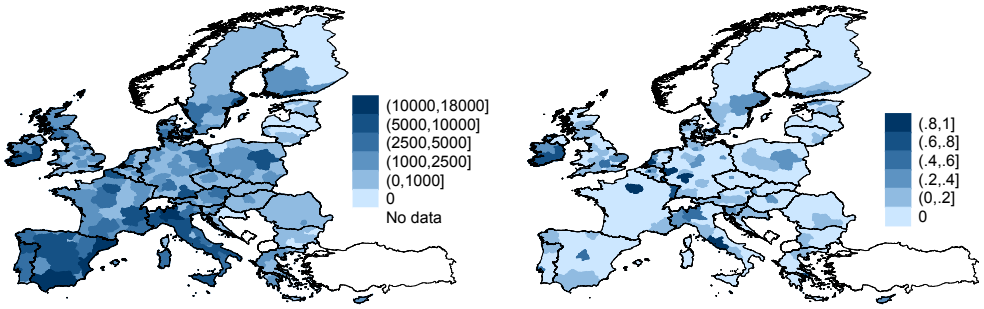
A loan is approved when at least one third of the Board members is in favor

[13]With the departure from the European Union, the United Kingdom is not involved in the EIB anymore.

of the project and when these members represent at least 50% of the subscribed capital. The shareholders are the Member States. Each country's share corresponds to the relative size of the country's GDP in the EU at the time of joining the EU. Germany, France, Italy, and the United Kingdom each hold 16.1% of the total shares (EIB 2015a). With such significant weight, these four countries together can veto decisions.[14]

Figure 2.2: Distribution of EIB loans across European regions

(a) *Distribution of loans* (b) *Distribution of Directors*



Notes: Map (a) plots the total sum of EIB loans in million euros that the respective region received within the period 1959-2015. Data source: (EIB 2020c). Map (b) plots the share of years a region had (at least one) representative at the EIB Board of Directors within the period 1959-2015.

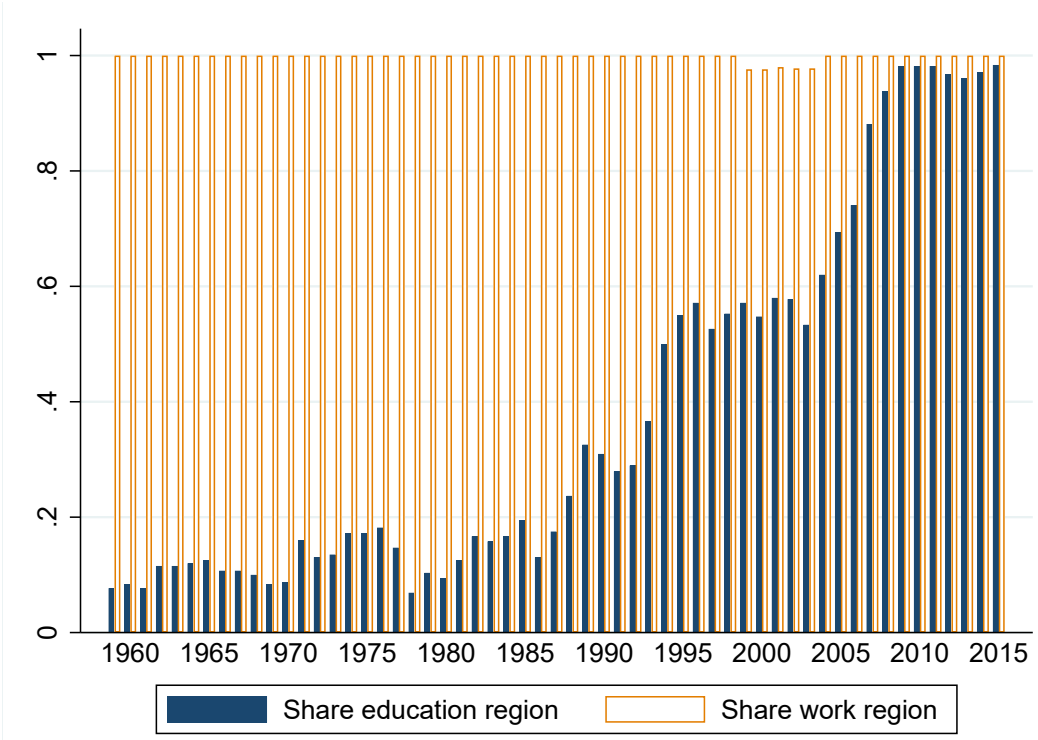
Data on Directors' careers: The treated regions are defined based on a coding of the CVs of the EIB's Board of Directors. Our sample includes 470 Board members from 1959 to 2015, including 254 full and 216 alternate members.[15] Sub-figure (a) of 2.4 shows the length of the mandates of the Board members. The large bulk of Directors stayed two or three years in the Board. Sub-figure (b) represents a time line starting in 1959 showing the amount of Directors appointed to (positive values) and leaving (negative values) the Board per year. Finally, in Figure 2.2 (b), we show the distribution of our treatment variable over space by plotting the share of years each region had one or more representative at the EIB Board. 84 regions are treated at least once. Comparing Figures 2.2 (a) and (b), we can see a slight correlation of "darker" areas, i.e., between regions that

[14]With the departure of the United Kingdom from the EU, Germany, France, and Italy now each hold 18.8% of the total shares (EIB 2020d).

[15]To get an idea how the CVs look like, the official EIB website provides with the CVs of the current Board of Directors (EIB 2020e).

have been treated more intensely and regions receiving more loans. Overall, the treated regions receive a share of 24% of the total project volume. 435 out of the 470 Directors come from regions including the capital city. 109 Directors work in lagging regions as defined by the regions receiving money from the European Cohesion Fund.[16]

Figure 2.3: Availability of treatment variable



Notes: The bright and dark bars depict the share of Directors per year for whom we know the work and education region, respectively.

We code the CVs of Board members in terms of the region they have worked when joining the EIB Board, and separately for the region where they obtained their highest degree of education. It would have been useful to also study the birth regions of Board members, however, such data is not available for privacy reasons. In the end, our preferred treatment variable is the work region dummy

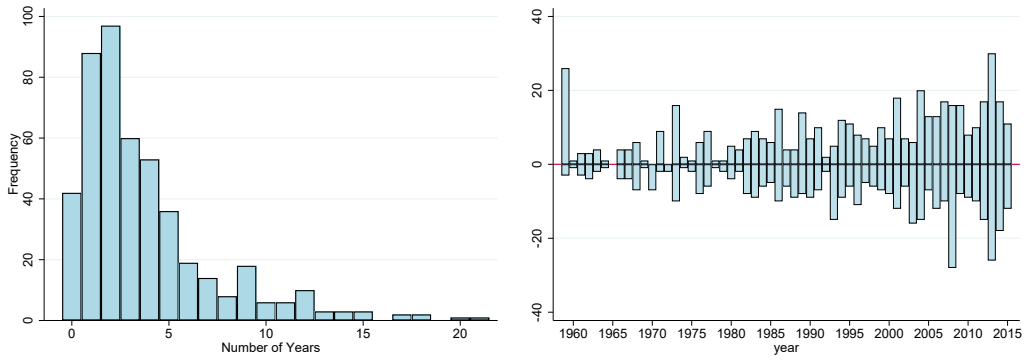
[16]Countries are eligible for the Cohesion Fund in case their gross national income per inhabitant is lower than 90% of EU average. We use data from the European Commission (European Commission 2020) to look which regions actually received money from the Cohesion Fund.

since we have complete information on this measure.[17] For this variable, we have 36 regions that have been treated at some point in time. 19% of the overall loan volume are received by these 36 work regions. The education region, on the other hand, is missing for a substantial share of members that may introduce a downward bias in our estimates (since missing information is coded as 0, thus inflating the control group upwards, assuming that there is a home bias in EIB lending). Figure 2.3 shows the share of Board members for whom we know the respective work and education regions. We use the education definition in Section 2.5 to study whether the timing when preferences towards home are realized matter for the interpretation of our results.

Throughout the paper, we exclude the Brussels region, as it is quite a special region in the European context. being the home of many European institutions.

Figure 2.4: EIB members' length of mandate over time

(a) *EIB members' length of mandate* (b) *Joining and leaving the EIB Board*



Notes: Sub-figure (a) is a histogram on the number of years the Directors stay in office. Sub-figure (b) plots the amount of Directors joining and leaving the Board per year, depicted on the positive and negative scale of the y-axis, respectively.

Data on EIB lending: The data on EIB loans goes back to 1959 and is publicly available on the website of the EIB (EIB 2020c). The website provides information

[17]The EIB provided us with 157 CVs. In these CVs, the Directors voluntarily list the prior workplaces and other information they wish to provide to the public. We then coded the regions manually from this information. For the remaining Directors, we took information on their work region from the EIB annual reports available in the Historical Archives of the European Union (European University Institute 2020). We also tried to complement the education region manually via a Google search, which increased the number of available education regions to 262.

on the size of the loan, the country, the sector, and the exact date when the contract was signed. Information on the region (either NUTS 1, NUTS 2 or NUTS 3) was provided to us by the EIB directly. This enables us to conduct a detailed analysis on a sub-national level.

Table 2.1 shows the availability of the loan data. Full information is available on the country level. Here, the total size of loans amounts to nearly 787 billion euros. The number and total size of projects decrease the more we zoom into countries, i.e., as our focus becomes confined to smaller administrative units. One reason for less observations in the smaller administrative units is that some projects are allocated on higher administrative units and we do not know whether and how the money is distributed among sub-regions. The difference between number of projects and number of items comes from the fact that some loans within one project flow to several regions.

Table 2.1: EIB loans aggregated to different region definitions

Level	Sum in billion EUR	No. of items	No. of projects
Country	787.40	15,932	6,495
NUTS 1	578.71	14,010	5,366
NUTS 2	476.10	12,709	4,830
NUTS 3	285.25	7,917	3,443

Other data: Regional data on control variables is taken from the European Regional Database (ERD) by Cambridge Econometrics. The dataset starts in 1980, however, Central and Eastern European countries[18] as well as Malta and Cyprus are only available from 1990 onward. Our control variables are GDP, population size, hours worked, compensation of employees and gross fixed capital formation. Summary statistics of these and all other variables are collected in Table 2.A1. For most countries we rely on the NUTS 2 region. For Estonia, Latvia, and Lithuania, we use the NUTS 3 region to get some sub-national variation, as their NUTS 2 regions correspond to the whole countries. For Cyprus and Luxembourg, even the NUTS 3 region corresponds to the entire country, thus leaving no variation for us to explore given our country-by-time fixed effects.

[18]Bulgaria, Czech Republic, Estonia, Latvia, Lithuania, Hungary, Poland, Romania, Slovenia, Slovakia

2.3 Methodology

2.3.1 Difference-in-differences

We estimate the following difference-in-differences (diff-in-diff) model:

$$EIB\_loans_{ijt} = \alpha_1 + \beta_1 \cdot Home_{ijt} + \gamma_1 \cdot \mathbf{X_{ijt}} + \psi_{ij} + \mu_{tj} + \epsilon_{ijt} \tag{2.1}$$

The index i stands for the region in country j, and year t. EIB_loans_{ijt}, our outcome variable, is either a dummy to measure the extensive margin of receiving an EIB loan, or the natural logarithm of the amount of EIB loans. To combine extensive and intensive margin analyses, we use a Poisson pseudo-maximum-likelihood (PPML) model with two different dependent variables: the loans-GDP-ratio and the share of loans a region received in total amount of loans in a given year.

$Home_{ijt}$ is our main variable of interest. This variable measures whether a region had at least one representative at the EIB's Board of Directors at a given point in time. Region-year observations are coded as treated ($Home_{ijt}$) whenever a person currently part of the Board either studied or is currently working in the given region (as reported in the Board of Directors' CVs). With 28 Full Directors representing one EU Member State each, one Full Director from the Commission, and 19 Alternate Directors, who are elected for a term of five years, we have a good degree of both cross-regional and cross-time variation in the treatment variable (see Figure 2.2).

$\mathbf{X_{ijt}}$ is a vector of control variables.[19] We also include region fixed effects (ψ_{ij}) and country-by-year fixed effects (μ_{tj}). These two-way fixed effects help us capture several potential endogeneity issues in the allocation of loans. Region fixed effects allow us to control for time-invariant region-specific factors. Importantly, our design with regional variation allows the inclusion of country-by-year fixed effects, which account for time-variant macroeconomic shocks such as national fiscal and monetary policy changes that affect countries differently but regions within a country similarly.

[19] See Section 2.2 for details.

2.3.2 Distributed lag model

We use a distributed lag model to study the timing of the effect of having a representative at the EIB Board on lending. In so doing, we include pre-trends of joining and lags of leaving the Board and, following Fabre and Sangnier (2017), separate treatment dummies for the first three years the Board member is in office and a fourth dummy for the remainder of the time in office. The equation is as follows:

$$\begin{aligned} EIB\_loans_{ijt} = \alpha_1 + \sum_{w=-4}^{-1} \beta_w 1st\_year\_Board_{ijt}^{w} + \sum_{w=1}^{3} \gamma_w in\_office\_year_{ijt}^{w} \\ + \gamma_4 \mathbb{1}_{X \geqslant 4} + \sum_{w=4}^{1} \delta_w L_w last\_year\_Board_{ijt} + \kappa_1 \cdot \mathbf{X_{ijt}} + \psi_{ij} + \mu_{tj} + \epsilon_{ijt} \\ \text{where } \mathbb{1}_{X \geqslant 4} \begin{cases} 1 & \text{if } in\_office\_year_{ijt} \geqslant 4 \\ 0 & \text{otherwise} \end{cases} \end{aligned} \tag{2.2}$$

The expression $\sum_{w=-4}^{-1} \beta_w 1st\_year\_Board_{ijt}^{w}$ defines the four pre-trends of the entry of each Board member. $\sum_{w=1}^{3} \gamma_w in\_office\_year_{ijt}^{w}$ are dummies for the first, second and third year in office. $\mathbb{1}_{X \geqslant 4}$ is a dummy for being in office the fourth and any further year. Finally, $\sum_{w=4}^{1} \delta_w L_w last\_year\_Board_{ijt}$ stands for four lags of the exit of the Board. The rest of the variables are the same as in Equation 2.1.

2.4 Results

2.4.1 Baseline results

In this section, we present our baseline analysis on whether regional favoritism affects the distribution of EIB lending to European regions. We start by discussing the estimation results of the difference-in-differences model as shown in Table 2.2, then proceed to discussing the results of the distributed lag model as plotted in Figure 2.5. In both cases the treatment variable captures whether a region has a "representative" in the EIB's decisive body, its Board of Directors. This variable takes a dummy equal to one if at least one Board member has worked in a given

NUTS 2 region, and is 0 otherwise.

Table 2.2: Baseline: Regional favoritism in the allocation of EIB loans

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLE	EIB Loan Dummy					Ln Loans	Loans / GDP	Loans / Tot. Loans
Work region dummy	0.1457***	0.1768***	0.1712***	0.2064***	0.1776***	-0.0256	0.9834***	0.6050**
	(0.0487)	(0.0462)	(0.0521)	(0.0762)	(0.0513)	(0.2533)	(0.3124)	(0.2786)
Ln population			0.4608*	-0.3558	0.6064**	1.4334	-0.2290	-0.5164
			(0.2381)	(0.6956)	(0.2351)	(1.2371)	(1.0254)	(0.8516)
Ln GDP			0.1299	-0.4264***		2.1337***		2.7845***
			(0.1337)	(0.1635)		(0.7535)		(0.5775)
Ln GDP p.c. $t-6$ to $t-1$					0.1204			
					(0.1228)			
Hours worked per employee			0.4395*	-0.2512	0.4527*	0.1459	-0.0700	-2.1312*
			(0.2427)	(0.3430)	(0.2437)	(1.8445)	(1.0652)	(1.1477)
Compensation per employee			0.0009	0.0061	0.0016	0.0205	-0.0523***	-0.0305*
			(0.0045)	(0.0048)	(0.0045)	(0.0171)	(0.0192)	(0.0168)
Ln gross fixed capital formation			-0.0794	0.1253	-0.0755	0.1262	1.4807***	0.7588***
			(0.0511)	(0.0822)	(0.0532)	(0.3782)	(0.2297)	(0.2746)
Sample start year	1959	1990	1990	1990	1990	1990	1990	1990
Method	OLS	OLS	OLS	OLS	OLS	OLS	PPML	PPML
Region FEs	×	×	×	×	×	×	×	×
Country-year FEs	×	×	×	×	×	×		
Year FEs							×	×
Region-specific time trend				×				
Observations	16,530	7,540	6,642	6,642	6,581	2,782	6,442	6,442
R-squared	0.3844	0.2557	0.2540	0.3268	0.2526	0.2992	0.1910	0.4429
Number of regions	290	290	266	266	266	258	258	258
Mean of dep. variable	.25	.39	.42	.42	.42	4.18	.0017	.0039

*** p<0.01, ** p<0.05, * p<0.1. Notes: The table presents results for the estimation of the model in Equation 2.1. Standard errors are in parentheses and clustered on the level of NUTS 2 regions. Given the dependent variables, columns 1 to 5 analyze the extensive margin, column 6 the intensive margin, and columns 7 and 8 combine the intensive and extensive margin.

Columns 1-5 of Table 2.2 show the extensive margin results for the work region, where the dependent variable is a dummy indicating whether a region received at least one EIB loan in a given year or not. The evidence supports the hypothesis that the treated regions receive more EIB loans compared to regions that do not have a representative at the EIB Board. This extensive margin effect

is robust across the specifications 1 to 5 of Table 2.2.[20] The size of the effect is an increase of 15 to 21 percentage points in the likelihood of receiving a loan. The average underlying probability that a region receives any lending is between 25% and 42%. Therefore, the home bias effect amounts to a large 40-60% increase in the probability of lending compared to the sample mean.

In column 6 of Table 2.2, we study the intensive margin, that is we ask whether treated regions receive larger EIB loans given that they received at least one loan. For the dependent variable, we take the (log) size of all lending aggregated to the region-year level. As a result regions with no EIB loans in a certain year are dropped from the sample. The estimated intensive margin result is small and not distinguishable from zero.

Columns 7 and 8 of Table 2.2 combine the extensive and intensive margins and use a Poisson pseudo-maximum likelihood (PPML) estimator.[21] As outcomes variables, we use the loans-over-GDP ratio as well as study the share of loans in total loans. The home bias effect is positive and statistically significant in both cases. The sizes of the coefficients are interpreted as a 167% increase in the loans-over-GDP ratio and a 83% percent increase in the share of loans in total loans, respectively.[22] This effect is much larger than the one for the extensive margin, however, the sample means in these specifications are very small, 0.17% and 0.39%, respectively.

We now proceed to the estimation results of the distributed lag model as specified in Equation 2.2. Given the findings of Table 2.2, we focus on the extensive margin response. In particular, we are interested in the timing of this effect. Figure 2.5 shows that the treatment effect becomes positive and remains so during

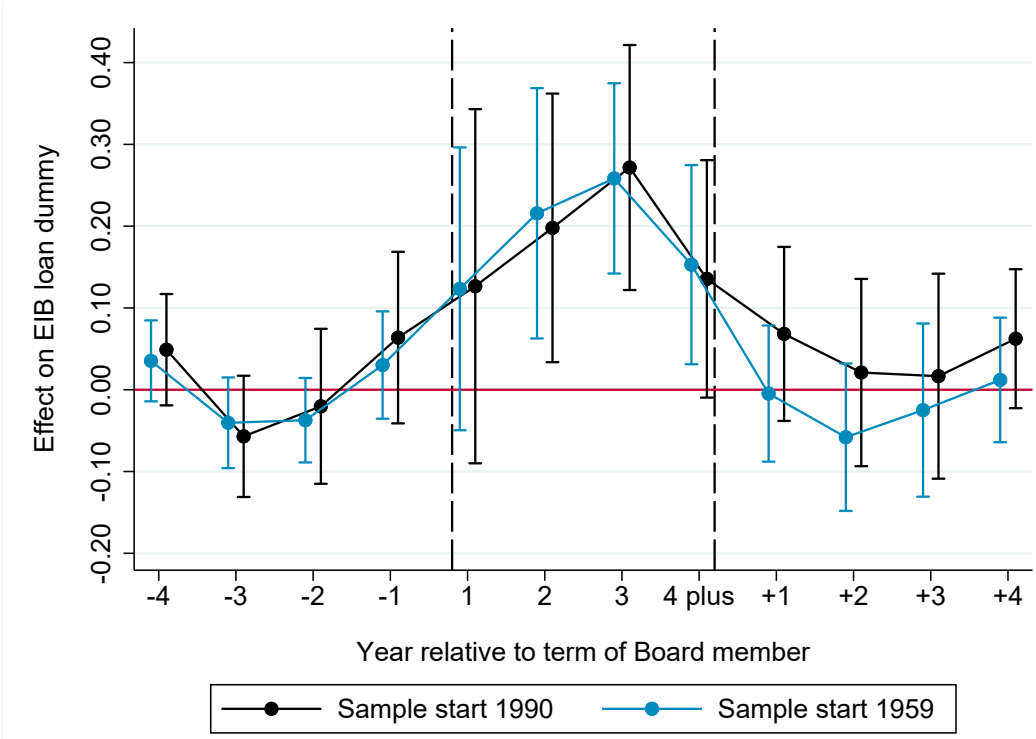
[20]The first column estimates our specification on the full sample starting in 1959. The sample for the rest of the table is restricted to the post-1990 period. This is the period when control variables are available for all regions. All regressions in Table 2.2 include region fixed effects. Columns 1 to 6 control for country-by-year effects, while the non-linear models of columns 7 and 8 instead control for year fixed effects. Column 4 additionally includes a region-specific linear time trend to account for the possibility that regions develop differently over time. In addition to controlling for contemporaneous GDP as part of our standard set of control variable, column 5 additionally includes the moving 5-year average of GDP per capita in the 5 years proceeding the treatment year. This is done to make sure it is not the cofinancing of EU Structural and Investment Funds that drives our results.

[21]Note that in these regressions, the country-by-year fixed effects are left out due to computational reasons, and we instead introduce year fixed effects.

[22]To interpret a PPML coefficient as a semi-elasticity, one has to apply the transformation (exp(coefficient)-1)*100.

the whole tenure. The point estimates are large[23] and statistically significant in all but one year of tenure. Once the Director has left the Board, the point estimates drop almost immediately and are not statistically significant anymore indicating that the home bias effect is only present during the Director's tenure. The immediate effects is plausible because our outcome variable measures lending commitments rather than their actual disbursement. The trends prior to the treatment are not significantly different from zero in any of the four lags that we estimate.[24] The full regression results corresponding to Figure 2.5 are reported in Table 2.A2 in columns 1 and 2.

Figure 2.5: Probability of receiving a loan around the time of joining and leaving the EIB Board



Notes: This graph presents the estimation of Equation 2.2. The corresponding regression table can be found in Table 2.A2 columns 1 and 2.

[23]The treatment effects in Figure 2.5 are larger than the baseline estimates of Table 2.2. One reason is that the figure shows the treatment on the individual Board member level and not on the region level as in the baseline regressions. For example, if a region is treated by two Board Directors who follow each others term, the dummy for the first year of treatment switches on for both of the Directors.

[24]The point estimate in $t-1$ is positive and larger than the one in $t-2$, which might indicate a pre-trend, but it has a small size compared to the point estimates during tenure and is in any case statistically indistinguishable from zero.

Figure 2.5 replicates the estimation for both the full sample since 1959 without control variables and the post-1990 sample where the control variables are available. The results look very similar, which is not surprising given that the EIB started to lend actively starting from the 1980s. Therefore, we rely on the post-1990 sample and benefit from the availability of control variables for all of our analyses that follows.

2.4.2 Robustness tests

NUTS definition and clustering: To check the robustness of our results, we redo the analysis for the extensive margin using the treatment dummy by clustering on the NUTS 1 region instead of the NUTS 2 region, and by defining the regions as NUTS 3 or NUTS 1 regions. The results are collected in columns 1-3 of Table 2.3, and are robust to our baseline results.

Quarterly analysis: To further test the robustness of our results, we disaggregate the data to the quarterly level. This data enables us to include country-by-year-by-quarter fixed effects that further increase the validity of our results by controlling for the business-cycle on the national level. Column 4 of Table 2.3 shows the results for the extensive margin. The effect is smaller in size but still of substantial magnitude and statistically different from zero.

Joining the EU: Another concern with our analysis has to do with the fact that the EU has expanded in several waves within our sample period. Even though we control for region and country-year interacted fixed effects, this expansion may still be an issue if the treatment regions of the small countries that join the EU are concentrated in the capital and if these regions simultaneously benefit disproportionally more from joining the EU. Thus, in Table 2.3 column 5, we include an interaction term between capital cities and a post-EU dummy. The result is nearly identical with the baseline result in Table 2.2 column 3.

Capital cities: As can be seen from Figure 2.2, the capital cities are more likely to be treated than other parts of countries while being economic centers they are also more likely to be receiving loans from the EIB. Although this potential

confounding effect should be fully accounted for by the region fixed effects, we do two additional robustness tests by excluding all regions with capital cities from the sample, or by only including these regions in the sample. The results are collected in Table 2.3 columns 6 and 7. In both instances, we observe a positive and significant treatment effect.

EU Structural and Investment Funds: A further concern relates to the fact that a large share of EIB loans co-finance the EU Structural and Investment Funds (ESI Funds). While we controlled for the potential eligibility for such funds in Table 2.2 column 5, we now conduct a more direct test. We want to analyze whether our baseline results still hold when we control for the fact that regions received some of the ESI Funds. We rely on annual disbursement data of the following four ESI Funds: the European Regional Development Fund, the Cohesion Fund, the European Social Fund, and the European Agricultural Fund for Rural Development. We opt for a simultaneous disbursement of ESI Funds and EIB treatment effects, well aware that lagged specifications might be appropriate as well. The results in column 8 of Table 2.3 show no distortion of the main treatment effects.

Spillover effects: Furthermore, we test whether neighbor regions of treated regions also have an increased probability of receiving loans. For that purpose, we create a spatial lag where we weight the home region dummy of the other regions by their inverse distance. The results collected in Table 2.3 column 9 do not show evidence of regional spillover effects.

Excluding countries and time periods: We analyze whether specific countries or time periods drive the baseline results. Table 2.A3 shows the baseline estimates by dropping the 28 EU Member States one-by-one. The estimated treatment effect is fairly stable in size (the point estimate varies from to 0.1543 to 0.1926) and is always significantly different from zero at the 1% level. Thus, it seems that no specific Member State is solely responsible for the baseline result.

Similarly, Table 2.A4 drops decades or five-year periods one-by-one. When the period from 1999 to 2008 is excluded, the treatment effect reduces by about

twice in size and becomes statistically indistinguishable from zero. However, when we exclude the first and second five years of this decade separately, both effects are positive and significantly different from zero. Therefore, we conclude that although it seems that much of the favoritism that we document may be coming from the post-1999 period, we cannot say that the result is solely driven by this period.

Model choice: We have so far used simple linear probability models when studying the extensive margin response, and Poisson pseudo-maximum likelihood models in specifications that combine the extensive and intensive margin responses (see Table 2.2). Since our dependent variable in the extensive margin specifications is a dummy variable, we can interpret the expected value of the estimate as a probability. Non-linear models provide potential benefits when the underlying model is highly non-linear. At the same time they lead to considerable complications including the incidental parameter problem as well as computational difficulties. These problems especially aggravate in our case, which models a large set of fixed effects, in particular due to the inclusion of country-by-year fixed effects. Moreover, the main disadvantages of using a linear model, that are conceptual arguments against linearity or the argument that predictions might lie outside of the theoretically possible range of (0,1), do not materialize in our case. We, therefore, estimate one computationally plausible non-linear model as a robustness test, but keep the linear models as our baseline estimator. In particular, in Table 2.3 column 10, we specify a logit regression without country-by-time fixed effects on quarterly level data. The quarterly data has a longer time-series than the annual data which somewhat downplays the incidental parameter problem. The marginal effect of the treatment effect is slightly larger than that of the baseline model but consistent in direction and significance.

Table 2.3: Robustness checks

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	EIB Loan Dummy									
Work region dummy	0.1712***	0.1891***	0.1758***	0.0896***	0.1705**	0.1871**	0.4960***	0.1747***	0.1836***	0.3672***
	(0.0546)	(0.0527)	(0.0441)	(0.0272)	(0.0789)	(0.0909)	(0.1362)	(0.0518)	(0.0546)	(0.0840)
Capital city * joining EU					0.0011					
					(0.1021)					
Ln ESI funds								0.0057		
								(0.0041)		
Spatial lag									0.6903	
									(0.7894)	
Sample definition	full	full	full	full	full	no capitals	only capitals	full	full	full
Sample start year	1990	1990	1990	1990	1990	1990	1990	1990	1990	1990
Method	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	Logit
Time unit	year	year	year	quarter	year	year	year	year	year	quarter
NUTS level	2	3	1	2	2	2	2	2	2	2
Cluster level	1	3	1	2	2	2	2	2	2	2
Control variables included	×		×		×	×	×	×	×	
Region FEs	×	×	×	×	×	×	×	×	×	×
Country-year FEs	×	×	×		×	×	×	×	×	
Year-quarter FEs										×
Country-year-quarter-FEs				×						
Observations	6,642	34,164	2,269	29,848	6,642	6,018	624	6,642	6,642	27,872
R-squared	0.2540	0.1007	0.4502	0.4050	0.2540	0.2584	0.9119	0.2542	0.2541	
Number of regions	266	1,314	91	287	266	241	25	266	266	269
Mean of dependent variable	.42	.1	.67	.17	.42	.41	.48	.42	.42	.18

*** p<0.01, ** p<0.05, * p<0.1. Notes: The table presents results for the estimation of the model in Equation 2.1. Standard errors are in parentheses and they clustered on the level as indicated in the table.

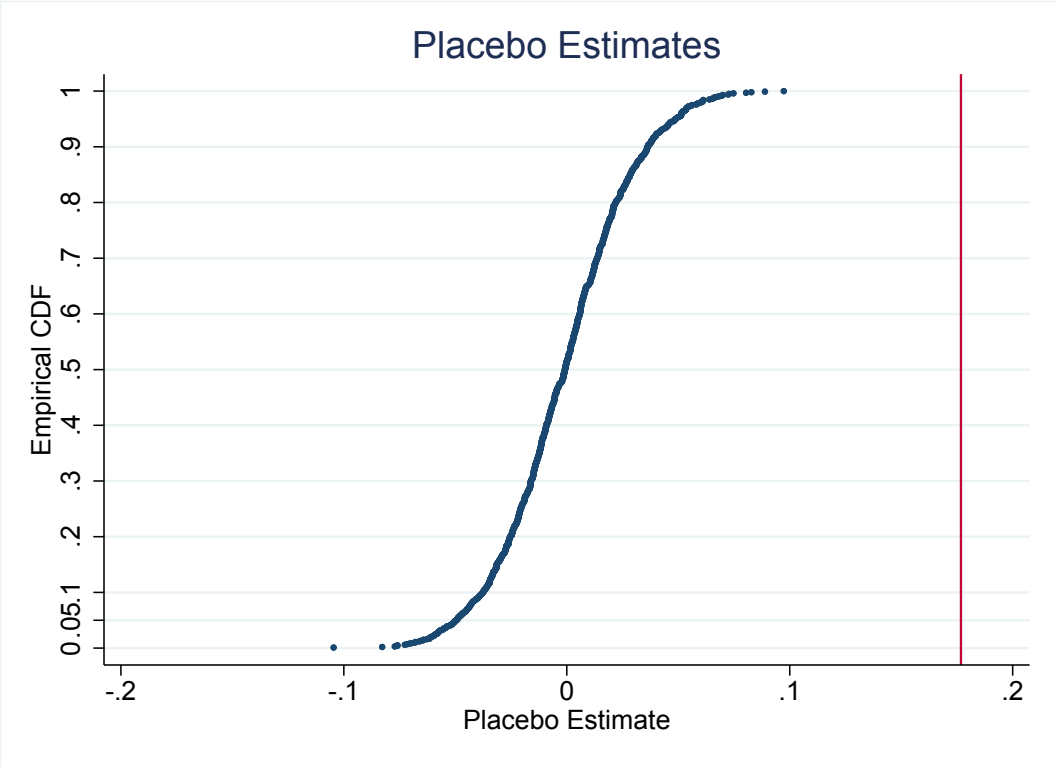
Few treated clusters: One problem with our setting could be that the number of treated clusters is small. With few treated clusters, inference problems can arise because the large-sample approximations for inference are no longer applicable (Conley and Taber 2011). As the share of treated region-years is 7% for the post-1990 sample, we conduct randomization inference by randomizing the treatment. In the original dataset, the treated observations are distributed across 36 of 291 regions. Therefore, we conduct a two-step randomization of treatment where in the first step 36 regions are randomly assigned to be treated regions. Among them, we again choose the same number as treated observations as in the "real" sample before running the specification. This randomization inference mechanism is conducted for 1000 replications, which gives us 1000 placebo treatment effects. We compute the cumulative distribution function of these placebo effects and compare it to our treatment effect. The resulting graph is depicted in Figure 2.6. The graph shows that our result is rare and that the few treated clusters are unlikely to give rise to issues in the sense of Conley and Taber (2011) in our analysis.

2.4.3 Result heterogeneity

Governing bodies: The EIB Board has 29 Full and 19 Alternate Directors. As the name suggests, Alternate Directors mostly assist the work of the Full Directors. Also, Full Directors represent an individual Member State while most Alternate Directors represent a group of countries. It is, therefore, our expectation that the home bias of Alternate Directors is smaller than that of the Full Directors. In Sub-figures 2.7 (a) and (b), we replicate the analysis for Full and Alternate Board members separately. As expected, we see large and statistically significant point estimates of the treatment effect for the Full Directors but not for the Alternate Directors. The positive lags for the Alternate Directors after exiting the EIB can be explained by the fact that some Alternates become a Full member after their term as Alternate.

The Management Committee is the executive body of the EIB, and currently consists of one President and eight Vice-Presidents. Since the foundation of the EIB, there have been 54 members of this Committee in total including seven

Figure 2.6: Randomization inference

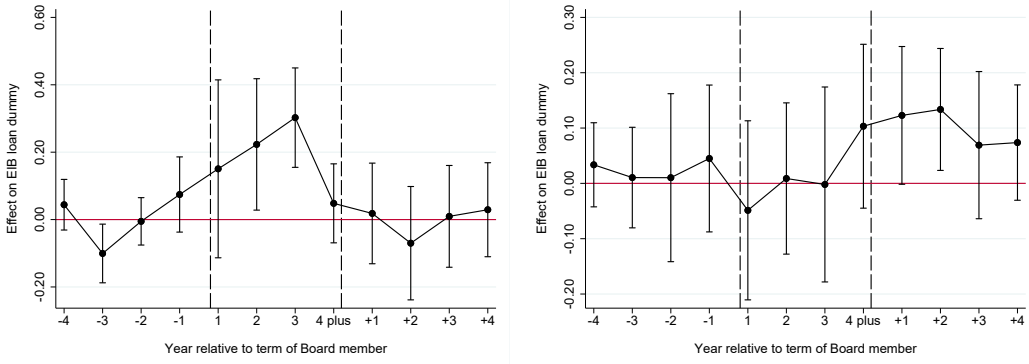


Notes: The graph plots the cumulative distribution function on the y-axis and the placebo treatment effects on the x-axis. The vertical line indicates our treatment effect reported in Table 2.2 column 3.

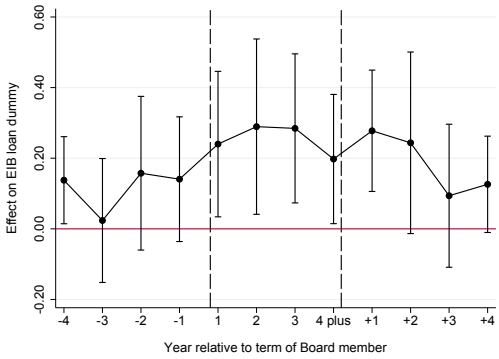
Presidents. We have data on the work region prior to joining the EIB of 49 Committee members. Even though the Management Committee of the EIB has no direct influence on the approval of loans, anecdotal evidence suggests that these are generally influential positions within the EIB. For example, Counter Balance (2016) reports, for instance, that Philippe de Fontaine Vive, a Vice-President at the EIB from 2003 to 2015, joined CMA-CGM, a container shipping conglomerate, which received a public-private partnership contract from EIB. He is now on the board of BMCE Bank, which has a long association with EIB. Counter Balance (2016) also reports about Gillian Day who held senior positions at the EIB at the same time when EIB has awarded numerous loans to the Royal Bank of Scotland where she served as Managing Director until February 2015. Moreover, this body together with the staff is also responsible for preparing the documents for the loan approval by the Board of Directors. Figure 2.7 (c) plots the analysis for the Management Committee. We observe positive and

Figure 2.7: Full and Alternate Directors and Management Committee

(a) *Full Directors* (b) *Alternate Directors*



(c) *Management Committee*



Notes: These graphs present three estimations of Equation 2.2 where the the treatment effect is the work region of: a) Full Directors, b) Alternate Directors, and c) Members of the Management Committee. The corresponding regressions can be found in Table 2.A2 columns 3-5.

statistically significant effects in the treatment period, which would lend support to the hypothesis that the Management Committee not only has influence on loan approvals but also engages in home bias lending. However, we also note that the confidence intervals in this exercise are quite large owing to the small number of Management Committee members.

2.5 Potential mechanisms

After having documented the body of evidence on the existence of the home bias effect in lending, the crucial question is whether these discriminatory lending practices facilitate economic efficiency or whether they result in inefficient misallocation of resources. As discussed in Section 2.1, the two main mechanism that

predict a larger flow of transactions into the home regions are that this lending is either driven by favoritism or by the information advantage of Directors. Both of these potential mechanisms are consistent with our evidence, however, they have divergent predictions on the economic value of these transactions. While favoritism-driven lending will arguably lead to resource misallocation, lending due to a potential information advantage of Directors regarding their home regions may enhance efficiency. The net welfare effects of home bias can therefore be both positive or negative depending on the relative strength of either of these two channels. In this section, we design a number of indirect tests to try to isolate the two motives behind lending decisions.

Sector and project size: First, we study potential treatment heterogeneity along the size distribution of loans as well as along six broad sectors that our data captures.[25] This exercise allows us to identify on a more granular level the types of projects that drive the finding of home bias. This is an interesting exercise by itself, and may additionally be informative about the mechanisms in play.

For the latter case of sectors, we estimate Equation 2.1 with the dependent variable being a dummy of having at least one loan in a certain sector. The results are collected in Table 2.4. Column 1 shows the treatment effects by sector for the extensive margin independent of the project size. The home bias is only present for infrastructure projects. The effect of 13 percentage points translates to an increase of 46% compared to the sample mean, which is in line with our baseline result. We then split the approved loans into quartiles according to their size within each sector. To control for differences between poor and rich countries, and size effects over time, we classify the loans into quartiles depending on the country and decade. Columns 2 to 5 of Table 2.4 show the treatment effects per quartile and sector. For the infrastructure sector, the treatment effects are positive and statistically significant for projects above the median.[26]

[25]These sectors are: Agriculture, Industry, Energy, Infrastructure, Non-market services, and Services.

[26]In this matrix of 24 treatment estimates, we see one (three) more point estimates that are different from zero at the 5 (10%) level, which however do not show a meaningful pattern of effects on size within other industries.

Table 2.4: Heterogeneous effects according to sectors and loan sizes

VARIABLES	no.[1]	mean[1]	(1)	(2)	(3)	(4)	(5)	(6)	Obs	(7)	Obs.
					Loan dummy						
			all	1st q.tile	2nd q.tile	3rd q.tile	4th q.tile	no. loans		ln loans	
Total	8,772	48.21	0.1712***	0.0179	0.0602*	0.0575	0.1274**	0.7976***	6,442	-0.0256	2,782
			(0.0521)	(0.0421)	(0.0331)	(0.0558)	(0.0625)	(0.2688)		(0.2533)	
Agriculture	28	35.62	0.0019	0.0010	0.0011	0.0012	0.0023	32.5068*	108	omitted	17
			(0.0020)	(0.0009)	(0.0011)	(0.0011)	(0.0020)	(16.7606)			
Energy	1,812	39.71	-0.0246	-0.0064	-0.0286**	-0.0204	-0.0283	0.6201***	4,667	-0.4318	895
			(0.0346)	(0.0138)	(0.0138)	(0.0222)	(0.0295)	(0.2064)		(0.5369)	
Industry	1,609	49.48	0.0091	-0.0275	-0.0429*	-0.0299	0.0431	-0.1088	4,917	0.2310	908
			(0.0431)	(0.0279)	(0.0242)	(0.0251)	(0.0318)	(0.2687)		(0.5254)	
Infrastructure	4558	49.61	0.1332**	-0.0017	0.0276	0.0814**	0.1528***	1.2456***	5,968	0.7775***	1,859
			(0.0522)	(0.0288)	(0.0303)	(0.0397)	(0.0539)	(0.3308)		(0.2884)	
Non-Market Services	472	70.87	0.0729	0.0027	0.0165	0.0529*	0.0018	0.4562	2,598	-2.2379	341
			(0.0465)	(0.0052)	(0.0125)	(0.0285)	(0.0221)	(0.4374)		(0.6550)	
Services	293	36.65	0.0046	-0.0104*	-0.0007	0.0102	0.0157	0.1158	2,494	-0.0313	217
			(0.0329)	(0.0060)	(0.0186)	(0.0095)	(0.0162)	(0.5886)		(0.7934)	
Sample start year			1990	1990	1990	1990	1990	1990		1990	
Region FEs			×	×	×	×	×	×		×	
Country-year FEs			×	×	×	×	×	×		×	
Controls			×	×	×	×	×	×		×	
Method			OLS	OLS	OLS	OLS	OLS	PPML		OLS	
Observations			6642	6,642	6,642	6,642	6,642	-		-	
Number of regions			266	266	266	266	266	-		-	

[1]no. corresponds to the number of projects per sector and mean to the mean project size in million euros after 1990.

*** p<0.01, ** p<0.05, * p<0.1. Notes: The table presents results for the estimation of the model in Equation 2.1. Only the coefficients of the $Home_{ijt}$ variable are shown. Standard errors are in parentheses and clustered on the level of NUTS 2 regions.

Column 6 shows the treatment effect on the number of loans and column 7 analyzes the intensive margin by looking at the log loan size, for which we could not find a statistically significant result in the baseline estimation in Table 2.2. Infrastructure is the only sector where we find significant treatment effects in both columns.[27] The magnitudes are large and can be interpreted as an increase of 248% in the number of loans (or 1.9 more loans compared to the mean of 0.75 loans) or a 116% increase in the size of infrastructure loans.

Although not a direct test, this evidence that the home bias phenomenon is driven by infrastructural mega-projects may hint towards favoritism rather than an informational channel, as the need for these large projects is rather common knowledge than the need for smaller and more sophisticated projects. Papers by Do et al. (2017) and Persson and Zhuravskaya (2016) find similar evidence for construction infrastructure in Vietnam and China, respectively.

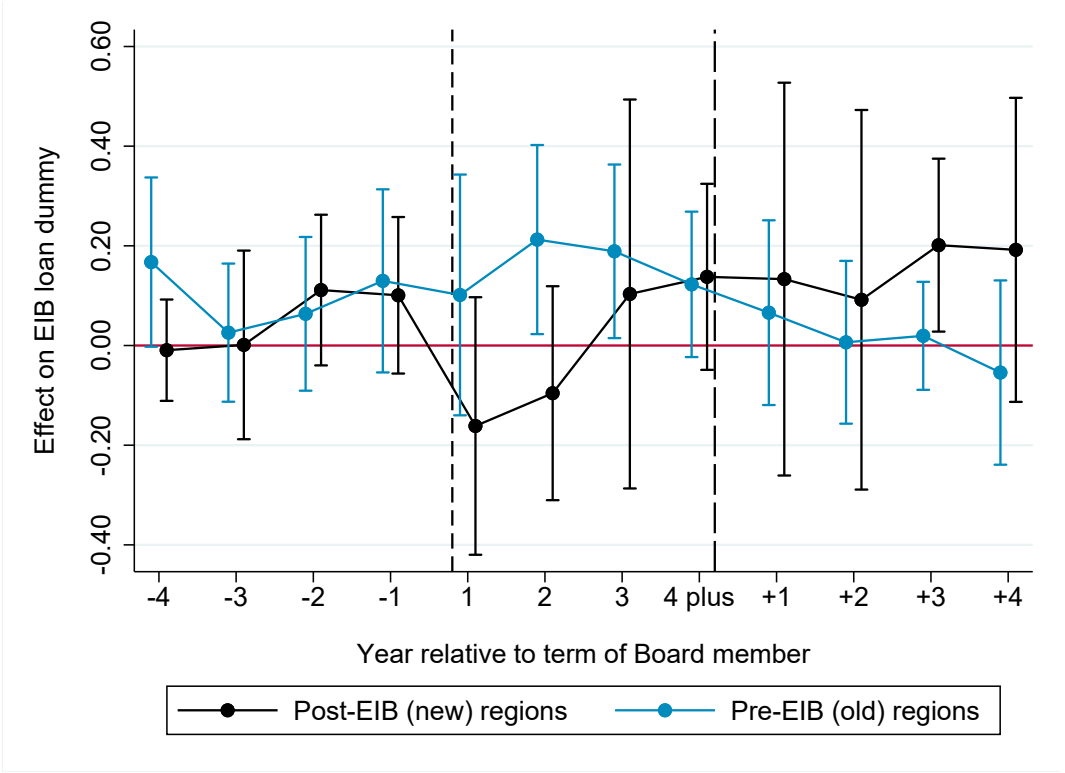
Job switchers: As a second test of mechanism, we collect additional data on the work regions of Directors after their service at the EIB,[28] and limit the analysis to a sub-sample of Directors who switch jobs. We then separately study the probability of switchers to lend money to either their pre-EIB regions or the new post-EIB regions while in office at the Board. The assumption behind this test is that a Director who has not yet worked in a certain region does not have an information advantage about that region, while sending money to a pre-EIB region can be a mix of both the favoritism and information channels.

Figure 2.8 is estimated with Equation 2.2 and identifies the timing of effects of job-switchers serving at the EIB Board on lending to either their pre-EIB ("old") regions or their post-EIB ("new") regions. The results for the old regions are positive and statistically significant in the treatment, but not in the pre-

[27]The energy sector has a positive significant point estimate in column 6 which is not robust to the evidence in column 7. The agricultural sector has an implausible large significant effect in column 6 which is an artifact of having too few observations left after splitting the sample by sector. As a result the estimation for agriculture in column 7 is not identified.

[28]The CVs we have of the Board members do not cover the period after working at the EIB. Hence, we collected information on the workplace after the time at the EIB via internet search. In so doing, we find information on 132 Board members working at the EIB after 1990, who make around 40% of the sample of Directors over the same period. For the rest of the Directors, including those who served at the EIB before 1990, we fail to find reliable career information. Out of the 132 Director for whom we have data, 68 stayed in the same region and 64 switched to new regions, of which 51 are within the EU.

Figure 2.8: Tests for mechanisms: Job switchers



Notes: This graph presents the estimation of Equation 2.2. The corresponding regression can be found in Table 2.A2 columns 6 and 7.

treatment periods, which confirms our baseline result on this sub-sample limited to job switchers. If an information advantage was the main mechanism at play, we would expect the entire home bias effects to be driven by the transfers to old regions and nearly zero transfers flowing to the new regions. In contrast, Figure 2.8 does not find a precise zero effect on transfers to these regions. The point estimates of lending to new post-EIB regions are increasing with tenure at the Board and they reach near-statistical significance at end of the period. This evidence is weak possibly due to the few number of switchers in our sample. However, at the very least, the evidence does not reject the null hypothesis that Directors are unable to send transfers to regions over which they are unlikely to have any observable informational advantages.

Degree of information: Third, we follow Rajan (1992), Persson and Zhuravskaya (2016), and Fisman et al. (2017) and hypothesize that the degree of information a Director has about a region should be positively correlated with

the amount of home bias lending. Similar to this work, we measure the degree of information by Directors' length of experience in the region measured in years.[29] Column 1 of Table 2.5 adds a variable capturing the number of years in work regions to our baseline specification. Next, we limit the sample to treated observation only, and in columns 2 and 3 of Table 2.5 test whether treatment effects are heterogeneous in this sub-sample according to, respectively, the number of years of experience as a continuous variable or an indicator function specifying several intervals of the experience variable. None of these tests confirms the hypothesis that more experience, a likely correlate of information, drives more lending.

Loan size dispersion: Fourth, we follow Cornell and Welch (1996) and Fisman et al. (2017) and hypothesize that informed Directors should have higher precision signals about the creditworthiness of borrowers originating from their home regions which would increase the variance of the distribution of priors across these borrowers. This hypothesis of higher variance of priors leads to the testable prediction that the dispersion of loan sizes flowing to home regions are higher than those of loans going to other regions. We adopt our baseline specification and instead of the dependent variable in columns 4 and 5 of Table 2.5 take as measures of loan dispersion the (log of) inter-quartile range and the standard deviation of loan sizes per region and year, respectively. The sample is limited to region-year observations having two and more loans. This evidence again fails to find any support that the information mechanism is the main driver of the home bias lending.

Social preferences: Ruling out the information channel as the main mechanism at play tells us that this type of lending is not likely to be welfare-enhancing. However, even if we could entirely rule out the information mechanism, the favoritism explanation would not be the only remaining explanation. One competing explanation, as advanced for example by Do et al. (2017) in a different context, is that Directors may simply have social preferences towards their home

[29] As explained in Section 2.2, a large share of the work region variable was collected via annual reports of the EIB. This source does not provide with the years the Directors have already spent in one region. Therefore, we tried to complement the information of the CVs by manual Google search. In the end, we know the experience in one region for 202 Directors.

Table 2.5: Tests for mechanisms

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Loan dummy			Ln IQR	Ln Std. Dev.		Loan dummy		
Work region dummy in t	0.1419**			0.4911	0.4392		0.1564***	0.1090**	0.1251**
	(0.0585)			(0.4908)	(0.3920)		(0.0585)	(0.0545)	(0.0496)
Education region dummy in t						0.0534	0.0340	0.0283	
						(0.0364)	(0.0390)	(0.0392)	
Experience in work region	0.0069	0.0061							
	(0.0054)	(0.0045)							
region experience - reference: 0 years									
1-3 years			-0.1150						
			(0.2361)						
4-6 years			-0.5602						
			(0.6629)						
7-9 years			0.0160						
			(0.2227)						
10-12 years			0.2301						
			(0.4267)						
12+ years			0.1995						
			(0.1242)						
Sample start year	1990	1990	1990	1990	1990	1990	1990	1990	1990
Method	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
Sample	full	treatment = 1		without zero loans		full	full	both regions known	
Control variables included	×	×	×	×	×	×	×	×	×
Region FEs	×	×	×	×	×	×	×	×	×
Country-year FEs	×	×	×	×	×	×	×	×	×
Observations	6,642	470	470	1,600	1,600	6,642	6,642	6,642	6,642
R-squared	0.2547	0.9042	0.9081	0.4003	0.3934	0.2528	0.2542	0.2537	0.2535
Number of regions	266	29	29	221	221	266	266	266	266
Dependent variable mean	.42	.68	.68	17.03	16.79	.42	.42	.42	.42

*** p<0.01, ** p<0.05, * p<0.1. Notes: The table presents results for the estimation of the model in Equation 2.1. Standard errors are in parentheses and clustered on the level of NUTS 2 regions.

regions. There may be various alternative explanations as well, but to the degree that we are aware of the literature this and other mechanisms are all likely to lead to misallocation of resources.

Nevertheless, in a final step, we follow Persson and Zhuravskaya (2016) and exploit the timing of formation of the home bias assuming that social preferences towards a region are likely to take shape before the last region of work and perhaps much closer to years of early adulthood. For a sub-sample of 263 Directors, we are able to code the regions of education. Columns 6 to 9 of Table 2.5 then asks whether the additional lending is flowing to regions of Directors' workplace rather

than their education regions.[30] Our evidence seems to be driven by regions of workplace rather that of education, which may speak against the hypothesis that favoritism is driven by social preferences rather than personal gain.

2.6 Conclusion

In this paper, we study political economy aspects of lending decisions within the European Investment Bank. This is an important extension of the literature on the political economy of international organizations for at least two reasons. First, the EIB is the largest multilateral lending institution in the world, and thus an important case study by itself. Second, the EIB is closely nested within the larger framework of the European Union institutions, thus this paper may have wider relevance for policy reform in the EU.

We document that Member State-nominated technocrats governing the Bank favor their home regions by allocating more EIB lending towards these regions. Our evidence does not provide a definitive answer to the crucial question of whether this bias is economically inefficient. However, we think that the question of whether favoritism plays a role in resource allocation decisions at the EU and particularly at the EIB deserves a further debate.

In particular, the EIB may benefit by increasing the level of transparency in its decision-making processes. This is in line with ongoing calls to reform EIB institutions, such as by introducing stronger rules for the disclosure of conflict of interest by the EIB Board of Directors and other senior staff. This evidence also stresses the important role to be given to debates on institutional reforms for the EU's various financing instruments before any further and more complex arrangements are established. The well-known accountability problems in the EU cannot be solved by simply delegating authority to technocrats who are often perceived to be rather independent of political constraints given the absence of electoral incentives.

[30] We do several tests to make sure that the positive effect for the work region and the absence for the education region is not driven by the poor availability of the education variable. In column 7, we first include both treatment effects together. In columns 8-9, we only include the Directors for which both the education and work region are available. The effects do not change.

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2.A Appendix A: Additional tables

Table 2.A1: Summary statistics of variables

Variable	Obs	Mean	Std.Dev.	Min	Max	Start	Source
EIB loan dummy	16,530	0.251	0.434	0	1	1959	EIB
EIB loans in million EUR	16,530	28.70	99.26	0	1874	1959	EIB
EIB loans over sum of EIB loans in year t	16,530	0.00345	0.0148	0	0.471	1959	EIB
EIB loans over GDP	9,251	0.00139	0.00397	0	0.0733	1980[1]	EIB
EIB loans interquartile range	4,148	2.550e+07	5.860e+07	0	9.850e+08	1959	EIB
EIB loans standard deviation	2,418	3.220e+07	4.890e+07	0	6.970e+08	1959	EIB
Ln loans without zeros	4,148	3.797	1.546	-2.717	7.536	1959	EIB
Capital city * joining EU	16,530	0.0457	0.209	0	1	1959	
GDP in billion EUR	9,251	35.57	45.27	0.271	565.0	1980[1]	ERD
GDP p.c. $t-6$ to $t-1$	8,960	18822	10118	1708	96309	1980[1]	ERD
Population in thousand	9,249	1694	1477	22.76	12070	1980[1]	ERD
Thousand hours worked per employee	8,686	1310	1150	19.43	9572	1980[1]	ERD
Compensation per employee in thousand EUR	8,726	18236	21533	219.6	246062	1980[1]	ERD
Gross fixed capital formation in million EUR	8,736	7763	9279	46.97	124611	1980[1]	ERD
Home region dummy work	16,530	0.0492	0.216	0	1	1959	EIB, EU Archives
Home region dummy education	16,530	0.0454	0.208	0	1	1959	EIB, EU Archives
Spatial lag Home region dummy work	16,473	0.0525	0.0298	0.0132	0.198		EIB, Eurostat
Ln sum EU funds	16,530	6.474	8.479	0	21.60	1994	EU Archives

[1]For Eastern European countries, the variables are only available since 1990.

Notes: ERD stands for European Regional Database.

Table 2.A2: Results from the distributed lag model

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	EIB Loan Dummy						
First year in office in $t-4$	0.0353	0.0490	0.0441	0.0336	0.1377**	0.1674*	-0.0094
	(0.0251)	(0.0346)	(0.0382)	(0.0386)	(0.0626)	(0.0863)	(0.0516)
First year in office in $t-3$	-0.0404	-0.0570	-0.1006**	0.0105	0.0236	0.0258	0.0012
	(0.0282)	(0.0376)	(0.0442)	(0.0462)	(0.0891)	(0.0704)	(0.0961)
First year in office in $t-2$	-0.0373	-0.0203	-0.0053	0.0103	0.1575	0.0636	0.1113
	(0.0262)	(0.0481)	(0.0357)	(0.0771)	(0.1105)	(0.0783)	(0.0768)
First year in office in $t-1$	0.0302	0.0638	0.0744	0.0450	0.1406	0.1297	0.1008
	(0.0334)	(0.0533)	(0.0567)	(0.0674)	(0.0897)	(0.0933)	(0.0798)
First year in office	0.1234	0.1266	0,1506	-0.0487	0.2400**	0.1015	-0.1615
	(0.0879)	(0.1100)	(0.1341)	(0.0822)	(0.1046)	(0.1227)	(0.1313)
Second year in office	0.2157**	0.1979**	0.2231**	0.0088	0.2894**	0.2126**	-0.0957
	(0.0778)	(0.0834)	(0.0990)	(0.0694)	(0.1261)	(0.0964)	(0.1090)
Third year in office	0.2584***	0.2717***	0.3025***	-0.0020	0.2845***	0.1890**	0.1034
	(0.0591)	(0.0761)	(0.0749)	(0.0895)	(0.1072)	(0.0884)	(0.1982)
Fourth year or more in office	0.1529**	0.1355*	0.0482	0.1033	0.1977**	0.1227*	0.1378
	(0.0619)	(0.0737)	(0.0595)	(0.0752)	(0.0929)	(0.0741)	(0.0948)
Last year in office in $t+1$	-0.0047	0.0683	0.0182	0.1229*	0.2777***	0.0659	0.1333
	(0.0423)	(0.0540)	(0.0758)	(0.0632)	(0.0873)	(0.0941)	(0.2002)
Last year in office in $t+2$	-0.0581	0.0210	-0.0701	0.1336**	0.2436*	0.0065	0.0918
	(0.0458)	(0.0581)	(0.0854)	(0.0560)	(0.1306)	(0.0830)	(0.1934)
Last year in office in $t+3$	-0.0249	0.0166	0.0095	0.0691	0.0937	0.0194	0.2014**
	(0.0538)	(0.0637)	(0.0767)	(0.0676)	(0.1029)	(0.0550)	(0.0881)
Last year in office in $t+4$	0.0120	0.0624	0.0292	0.0737	0.1261*	-0.0543	0.1919
	(0.0387)	(0.0431)	(0.0708)	(0.0529)	(0.0693)	(0.0939)	(0.1549)
Ln population		0.4522*	0.4467*	0.4211	0.3732	0.4699	0.4097
		(0.2647)	(0.2649)	(0.2645)	(0.2633)	(0.3175)	(0.3188)
Ln GDP		0.1365	0.1554	0.2021	0.1782	0.2620	0.3019*
		(0.1350)	(0.1364)	(0.1423)	(0.1356)	(0.1613)	(0.1625)
Hours worked per employee		0.4153*	0.4121	0.3997	0.3476	0.4102*	0.3894
		(0.2472)	(0.2499)	(0.2487)	(0.2408)	(0.2433)	(0.2409)
Compensation per employee		0.0003	-0.0002	-0.0001	-0.0005	-0.0004	-0.0025
		(0.0051)	(0.0052)	(0.0052)	(0.0051)	(0.0056)	(0.0051)
Ln gross fixed capital formation		-0.0764	-0.0742	-0.0827	-0.0652	0.0195	0.0206
		(0.0590)	(0.0603)	(0.0625)	(0.0595)	(0.0699)	(0.0734)
Sample start year	1959	1990	1990	1990	1990	1990	1990
Sample	Full and	Alternate	Full	Alternate	MC	old regions	post regions
Region FEs	×	×	×	×	×	×	×
Country-year FEs	×	×	×	×	×	×	×
Observations	14,210	5,844	5,844	5,844	5,844	4,788	4,788
R-squared	0.3632	0.2563	0.2562	0.2535	0.2559	0.2200	0.2197
Number of regions	290	266	266	266	266	266	266
Mean of the dependent variable	.25	.41	.41	.41	.41	.42	.42

*** p<0.01, ** p<0.05, * p<0.1. Notes: The table presents results for the estimation of the model in Equation 2.2. Standard errors are in parentheses and clustered on the level of NUTS 2 regions.

Table 2.A3: Drop countries one-by-one

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
	EIB Loan Dummy													
Work region dummy	0.1641***	0.1708***	0.1518***	0.1712***	0.1909***	0.1543***	0.1709***	0.1712***	0.1845***	0.1746***	0.1679***	0.1672***	0.1712***	0.1669***
	(0.0545)	(0.0521)	(0.0524)	(0.0520)	(0.0517)	(0.0563)	(0.0520)	(0.0521)	(0.0490)	(0.0528)	(0.0537)	(0.0520)	(0.0521)	(0.0546)
Without country	AT	BE	BG	CY	CZ	DE	DK	EE	EL	ES	FI	FR	HR	HU
Observations	6,417	6,392	6,492	6,617	6,442	5,700	6,517	6,642	6,317	6,167	6,517	5,992	6,642	6,467
R-squared	0.2501	0.2505	0.2525	0.2509	0.2445	0.2815	0.2483	0.2540	0.2615	0.2577	0.2467	0.2769	0.2540	0.2505
Number of regions	257	256	260	265	258	228	261	266	253	247	261	240	266	259
Mean of dependent variable	.42	.42	.43	.42	.42	.43	.42	.42	.42	.39	.42	.41	.42	.43
Work region dummy	0.1712***	0.1806***	0.1712***	0.1712***	0.1712***	0.1712***	0.1710***	0.1596***	0.1713***	0.1658***	0.1606***	0.1898***	0.1926***	0.1629***
	(0.0521)	(0.0532)	(0.0521)	(0.0520)	(0.0521)	(0.0520)	(0.0570)	(0.0545)	(0.0543)	(0.0551)	(0.0541)	(0.0519)	(0.0524)	(0.0558)
Without country	IE	IT	LT	LU	LV	MT	NL	PL	PT	RO	SE	SI	SK	UK
Observations	6,592	6,117	6,642	6,617	6,642	6,617	6,342	6,242	6,467	6,442	6,442	6,592	6,542	5,717
R-squared	0.2501	0.2603	0.2540	0.2527	0.2540	0.2517	0.2592	0.2381	0.2542	0.2503	0.2478	0.2518	0.2526	0.2640
Number of region	264	245	266	265	266	265	254	250	259	258	258	264	262	229
Mean of dependent variable	.42	.39	.42	.42	.42	.42	.43	.43	.41	.43	.42	.42	.42	.42
Sample start year	1990	1990	1990	1990	1990	1990	1990	1990	1990	1990	1990	1990	1990	1990
Region FEs	×	×	×	×	×	×	×	×	×	×	×	×	×	×
Country-year FEs	×	×	×	×	×	×	×	×	×	×	×	×	×	×
Controls	×	×	×	×	×	×	×	×	×	×	×	×	×	×

*** p<0.01, ** p<0.05, * p<0.1. Notes: The table presents results for the estimation of the model in Equation 2.1. Each regression excludes one country as indicated in the table. Standard errors are in parentheses and clustered on the level of NUTS 2 regions.

Table 2.A4: Drop ten and five year periods one-by-one

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	EIB Loan Dummy							
Work region dummy	0.1621***	0.1506***	0.1539***	0.1558***	0.1453***	0.1544***	0.1614***	0.1513***
	(0.0461)	(0.0470)	(0.0474)	(0.0474)	(0.0473)	(0.0492)	(0.0458)	(0.0487)
Without years	59-68	59-63	64-68	69-78	69-73	74-78	79-88	79-83
Observations	13,630	15,080	15,080	13,630	15,080	15,080	13,630	15,080
R-squared	0.3383	0.3599	0.3713	0.3790	0.3786	0.3873	0.3945	0.3898
Mean of dependent variable	.3	.27	.27	.28	.27	.26	.25	.26
Work region dummy	0.1551***	0.1613***	0.1536***	0.1496***	0.0871	0.1385***	0.1057*	0.1316*
	(0.0456)	(0.0406)	(0.0438)	(0.0458)	(0.0624)	(0.0496)	(0.0599)	(0.0698)
Without years	84-88	89-98	89-93	94-98	99-08	99-03	04-08	09-15
Observations	15,080	13,630	15,080	15,080	13,630	15,080	15,080	14,500
R-squared	0.3896	0.3785	0.3789	0.3868	0.4138	0.3953	0.4002	0.3779
Mean of dependent variable	.25	.23	.24	.24	.23	.24	.24	.22
Number of regions	290	290	290	290	290	290	290	290
Region FEs	×	×	×	×	×	×	×	×
Country-year FEs	×	×	×	×	×	×	×	×
Controls								

*** p<0.01, ** p<0.05, * p<0.1. Notes: The table presents results for the estimation of the model in Equation 2.1. Each regression excludes a set of years as indicated in the table. Standard errors are in parentheses and clustered on the level of NUTS 2 regions.

2.B Appendix B: Data

The dataset and the do-files to replicate the analysis of the chapter can be found online: https://link.springer.com/article/10.1007/s11558-020-09385-y

- We use three sets of data:
  - Project-level data on EIB loans (N=15,932)[31]: includes the size of loans, time of commitment, region (i.e., NUTS) identifier, name of project, sector, etc.[32]
  - Person-level data on EIB Board of Directors (N=470): regions of workplace for all 470 Directors[33] and regions of education of highest degree for 262 Directors.[34]
  - Person-level data on EIB Management Committee (N=70): regions of workplace for 49 Directors[35]
  - Region-level data on socio-economic characteristics: includes GDP, population, compensation and hours worked per employee, and gross fixed capital formation;[36] as well as data on disbursement of European Structural and Investment Funds (European Commission 2020) and data on distances between regions (Eurostat 2020).
- We aggregate our data to the level of European regions:
  - We use the NUTS 2 level of aggregation (classification as of 2010) for our main analysis.[37]

[31] This is the total number of loans in the data, flowing to all different levels of regions. See also Table 2.1 for details.

[32] The source of this data is the EIB.

[33] The work region was hand-collected from 157 CVs provided by the EIB, and for the remaining 313 Directors it was complemented with data collected from EIB annual reports available in the Historical Archives of the European Union (European University Institute 2020). We have full coverage for this variable.

[34] The education region was likewise hand-collected via the CVs and augmented via a manual Google search. Here, we have a partial coverage of only 262 Directors.

[35] The work region was equally hand-collected CVs provided by the EIB.

[36] This database is called the European Regional Database and it was purchased from Cambridge Economics.

[37] The countries Estonia, Latvia, and Lithuania are exceptions where we use the NUTS 3 aggregation.

  - This gives us 291 regions in total.[38]
  - The sample starts in 1959 and ends in 2015.
  - In total, we have a balanced sample of 16,530 observations.
  - For summary statistics see Table 2.B1 below.
  - For robustness tests we also use data aggregated to NUTS 1, NUTS 3 and NUTS 2-quarterly level (see do-files for further details).

- Do-files:
  - The do-file Asatryan_Havlik_dataprep.do, first, aggregates the raw project- and person-level data into region-level data, and merges all necessary variables.
  - The do-file Asatryan_Havlik_descriptives.do plots all the descriptive figures and tables of the paper.
  - The do-file Asatryan_Havlik_regressions.do estimates all the regressions of the paper.

[38]The Brussels region will we dropped from the analysis (NUTS code BE10) as explained in Section 2.2.

Table 2.B1: Summary statistics of variables in public data

Variable	Label	Obs	Mean	Std.Dev.	Min	Max
year	year	16,530	1987	16.45	1959	2015
nuts_code	NUTS code, string					
nuts_code_panel	NUTS code for panel ID	16,530	145.5	83.72	1	290
country_code	Country code, string					
country_code_panel	Country code for panel ID	16,530	14.64	8.408	1	28
analysis	Region-time level used for analysis					
first_year_work_full	First year at EIB, work region, Full member	16,530	0.0126	0.111	0	1
last_year_work_full	Last year at EIB, work region, Full member	16,530	0.0128	0.113	0	1
max_office_work_full	Number of years at EIB, work region, Full member, maximum for several people	730	5.421	3.978	1	22
first_year_work_alt	First year at EIB, work region, Alternate member	16,530	0.00938	0.0964	0	1
last_year_work_alt	Last year at EIB, work region, Alternate member	16,530	0.00980	0.0985	0	1
max_office_work_alt	Number of years at EIB, work region, Alt. member, maximum for several people	468	4.737	3.621	1	21
first_year_educ_full	First year at EIB, educ. region, Full member	16,530	0.00901	0.0945	0	1
last_year_educ_full	Last year at EIB, educ. region, Full member	16,530	0.00907	0.0948	0	1
max_office_educ_full	Number of years at EIB, educ. region, Full member, maximum for several people	597	4.363	3.351	1	19
first_year_educ_alt	First year at EIB, educ. region, Alternate member	16,530	0.00581	0.0760	0	1
last_year_educ_alt	Last year at EIB, educ. region, Alternate member	16,530	0.00551	0.0740	0	1
max_office_educ_alt	Number of years at EIB, educ. region, Alt. member, maximum for several people	276	2.866	1.932	1	13
max_office_work	Number of years at EIB, work region, maximum for several people	813	5.748	4.070	1	22
max_office_educ	Number of years at EIB, educ. region, maximum for several people	751	4.185	3.163	1	19
first_year_work	First year at EIB, work region	16,530	0.0180	0.133	0	1
last_year_work	Last year at EIB, work region	16,530	0.0178	0.132	0	1
first_year_educ	First year at EIB, education region	16,530	0.0137	0.116	0	1
last_year_educ	Last year at EIB, education region	16,530	0.0130	0.113	0	1
max_reg_start_work	Number of years in region when starting at EIB, max. for several people	16,530	0.145	1.458	0	33.36
treated_work	Work region dummy	16,530	0.0492	0.216	0	1
treated_educ	Education region dummy	16,530	0.0454	0.208	0	1
cat_exp	Number of years in region when starting at EIB, in 3-year intervals	16,530	0.0474	0.394	0	5

Table 2.B1: Summary statistics of variables in public data (continued)

Variable	Label	Obs	Mean	Std.Dev.	Min	Max
treated_work_reduced	Work region dummy with both regions known	16,530	0.0319	0.176	0	1
treated_educ_reduced	Education region dummy with both regions known	16,530	0.0454	0.208	0	1
first_year_old	First year at EIB, pre-EIB region	16,530	0.00278	0.0527	0	1
last_year_old	Last year at EIB, pre-EIB region	16,530	0.00272	0.0521	0	1
max_office_old	Number of years at EIB, pre-EIB region, maximum for several people	152	3.303	1.993	1	10
first_year_post	First year at EIB, post-EIB region	16,530	0.00200	0.0446	0	1
last_year_post	Last year at EIB, post-EIB region	16,530	0.00206	0.0453	0	1
max_office_post	Number of years at EIB, post-EIB region, maximum for several people	115	3.504	2.075	1	10
MC_first_year_work	First year at EIB, Management Committee, work region	16,530	0.00248	0.0497	0	1
MC_last_year_work	Last year at EIB, Management Committee, work region	16,530	0.00194	0.0440	0	1
max_mc_office_work	Number of years at EIB, Management Committee, maximum for several people	235	5.591	4.681	1	24
MC_first_year_educ	First year at EIB, Management Committee, education region	16,530	0.00212	0.0460	0	1
MC_last_year_educ	Last year at EIB, Management Committee, education region	16,530	0.00157	0.0396	0	1
max_mc_office_educ	Number of years at EIB, Management Committee, maximum for several people	186	4.726	3.740	1	19
sum_loans	EIB loans in million euros	16,530	28.70	99.26	0	1874
iqr_loans	Interquartile range of loans per region-year	4,148	2.550e+07	5.860e+07	0	9.850e+08
sd_loans	Standard deviation of loans per region-year	2,418	3.220e+07	4.890e+07	0	6.970e+08
gdp	Billions of euros, 2005 constant prices, ERD	9,251	35.57	45.27	0.271	565.0
population	Thousands of people, ERD	9,249	1694	1477	22.76	12070
gva	Gross value added, millions of euros in 2005 constant prices, ERD	9,251	31915	40913	99.52	511771

Table 2.B1: Summary statistics of variables in public data (continued)

Variable	Label	Obs	Mean	Std.Dev.	Min	Max
active_population	Employed+unemployed pop. minus students+pensioners, living place measure, ERD	8,723	842.7	710.6	8.169	6008
employment	Thousands of people, workplace measure, ERD	9,251	734.5	666.2	11.03	6115
hours_worked	Total annual hours worked, in million, ERD	8,686	1310	1150	19.43	9572
compensation_employees	Total renumeration of employees, million euros in 2005 prices, ERD	8,726	18236	21533	219.6	246062
gross_fixed_capital_formation	Million euros in 2005 prices, ERD	8,736	7763	9279	46.97	124611
ln_population	Ln population	9,249	13.98	0.927	10.03	16.31
compensation_employees_1000pc	Compensation per employee, in 2005 constant 1000 euros	8,726	22.59	10.64	1.162	105.7
hours1000_worked_pc	1000 hours worked per employee	8,686	1.718	0.204	1.293	2.456
ln_gfcf	Ln gross fixed capital formation	8,736	22.26	1.102	17.66	25.55
ln_gdp	Ln GDP	9,251	23.67	1.251	19.42	27.06
gdp_mio	GDP, millions of euros, 2005 constant prices	9,251	35569	45274	271	564982
gdp_pc	GDP per capita, 2005 constant prices	9,249	19435	10442	1585	98787
gdp_pc_mov_avg_5yr	GDP p.c. from t-6 to t-1	8,960	18822	10118	1708	96309
ln_gdp_pc_movavg_5yr	Ln GDP p.c. from t-6 to t-1	8,960	9.652	0.698	7.443	11.48
ln_sum_eu_payments	Ln sum of European Structural and Investment Funds	16,530	6.474	8.479	0	21.60
capital_city	capital	16,530	0.100	0.300	0	1
year_joining_EU	year_joining_EU	1,653	1989	17.81	1959	2013
capital_joinEU	Value=1 if nuts region is capital city and after joining EU	16,530	0.0457	0.209	0	1
loan_dummy	EIB loan dummy	16,530	0.251	0.434	0	1
share_loans	Share of regional loans in total loans in given year	16,530	0.00345	0.0148	0	0.471
ln_loans_nozeros	Ln EIB loans without zeros	4,148	3.797	1.546	-2.717	7.536
share_loans_gdp	Share of loans in GDP	9,251	0.00139	0.00397	0	0.0733
spl_treated_work	Spatial lag (dist) EIB directors	16,473	0.0525	0.0298	0.0132	0.198

3

Vetoing and inaugurating policy like others do: evidence on spatial interactions in voter initiatives

3.1 Introduction

A sizeable literature in economics and political science studies the question of how strategic interactions among political jurisdictions affect their choice of public policies. Such interactions may occur horizontally or vertically and between or within countries, in general, because of learning, coercion, fiscal and yardstick competition. The fields of public finance and public economics have put forward several mechanisms that underlie such spatial relationships in the governments' spending and taxing decisions (Revelli 2005) and in other public sector policies (Brueckner 2003).

According to the *externality* mechanism, a government may find it optimal to internalize a policy set by another government, say in the field of education or health care, when making its own decision to build more or less schools and hospitals (Case et al. 1993). A particular economic constraint may be due to the *competition* for attracting mobile resources such as labor and capital through fiscal competition (Tiebout 1956; Wilson 1999). In a principal-agent framework

with incomplete information, a decision-maker is additionally subject to *yardstick competition*. Such competition arises if the principals form certain *expectations* in regard to their jurisdiction's (not perfectly observable) performance, for example in the quality of public service provision, by relying on other jurisdictions' (again not perfectly observable, but comparable) performance as a yardstick (Besley and Case 1995). Cross-border information externalities may also grant representatives opportunities to *learn* from neighbor's policies (see, e.g., Gilardi 2010, for learning between OECD countries). Based on theoretical models of policy choice, Volden et al. (2008) formalize such learning-based *policy-diffusion* mechanisms and Mukand and Rodrik (2005) conceptualize the related idea of *policy experimentation*.

The policy outcomes of these different mechanisms can be similar, however often with quite different implications. For example, whereas yardstick competition assumes that politicians have electoral incentives, the learning channel is silent on the political-economy incentives of politicians when replicating neighbor's policy. From a public choice perspective then, it is not ex ante clear why politicians would engage in learning. Given such potentially different implications, previous literature has been interested in disentangling these often competing mechanisms (Shipan and Volden 2008), particularly with an empirical strategy of comparing sub-national jurisdictions within countries (Brueckner 2003). However, what this literature has in common is that it almost exclusively focuses on economic systems based on a pure representative form of government.

Our paper adds to this literature by studying political systems where decisions can be made also directly by voters through initiatives or other direct democratic instruments (for a review of the direct democracy literature, see Matsusaka 2004, 2018). Similar to all the channels discussed above, we also assume that cross-border information externalities exist. However, unlike these channels where policy is ultimately implemented by the governing politician, here, voters are entrusted with discretion to implement a preferred policy directly by a binding initiative. With direct democratic institutions in place, the argument is then that a new decision-making institution exists which may be legitimately mimicked across jurisdictions. The main contribution of this paper, more specifically, is to

test empirically whether initiatives in one jurisdiction have spillover effects on the direct democratic actions of voters in neighboring jurisdictions.

Our design focuses on the population of German municipalities from 2002 to 2014, where since the mid-1990s citizens have the power to veto (some of the) local governments' decisions and propose certain new policies by launching initiatives (in total around 3,300 for the 13,000 municipalities in the study period). We match the data on direct democratic activity to panel data on the towns' sociodemographic and fiscal characteristics. The whole dataset is available in our online appendix (see also Appendix 3.B.). We apply spatial reaction functions, and exploit a plausibly exogenous instrument based on the differences in direct democratic laws to identify interactions between municipalities.[1] Following Asatryan (2016), which studies the effect of direct democracy on local government size,[2] our main instrument for the number of initiatives in the neighboring municipalities is the amount of signatures required for the initiative to be successful. We use municipality fixed effects to control for unobserved constant spatial correlation across municipalities. Our findings suggest that the probability of observing an initiative in a municipality is positively driven by its neighbors' activity in direct democracy.

This paper is organized as follows. Section 3.2 briefly introduces the German institutions of direct democracy and discusses the earlier work. Section 3.3 describes our data and identification strategy, Section 3.4 presents the results, and Section 3.5 concludes.

[1] In doing so, we follow recent papers that try to isolate possible common shocks or spatially correlated (unobservable) effects from real spatial patterns in jurisdictions' policies of interest (Gibbons and Overman 2012). Lyytikäinen (2012), Isen (2014), and Baskaran (2014, 2019), among others, rely on arguably more credible identification techniques by utilizing sources of exogenous variation and find that some of the previously documented strong effects could be due to spurious correlations. On the other hand, Eugster and Parchet (2019) find evidence for local tax competition which is, however, confined to a fairly small spatial scale.

[2] See also Asatryan, Baskaran, Grigoriadis, and Heinemann (2017) and Asatryan, Baskaran, and Heinemann (2017) for evidence on the effect of direct democracy on local spending and taxes, respectively.

3.2 Institutional setting and previous literature

3.2.1 Institutions

Most German states (in German: Länder) introduced local-level direct democratic institutions in the 1990s after the German reunification. Baden-Württemberg is an exception with institutions of direct democracy on the local level already established in 1956. Berlin is the last state which introduced laws of local direct democracy in 2005.

These institutions enable citizens to launch so-called *citizen initiatives* ("Bürgerbegehren") which are divided into *new initiatives* ("Initiativbegehren") and *corrective initiatives* ("Korrekturbegehren"). The latter are used to veto policies proposed by the city council, while the former allow to launch new policies. For an initiative to be successfully implemented, the initiators face several constraints. First, a town-specific amount of signatures has to be collected (*minimum signature requirement*) within a predefined time. If this is achieved, the city council will decide if it wants to realize the issue at hand or not. In case of a negative decision, the next step of the procedure is reached, i.e., citizens vote on the respective issue.

Looking into the data, we observe a higher activity of direct democracy in states with less strict institutions. For example, in Bayern, where there are comparatively liberal institutions, around 2,700 initiatives have been launched until 2015. On the contrary, in Baden-Württemberg, where very rigorous regulations are in place, only around 800 initiatives have been launched until 2015.[3] The geographical distribution of the number of initiatives is illustrated in a heat-map in Figure 3.A.1. Table 3.A.1 summarizes the local-level institutions of direct democracy per state.

With these direct democratic institutions in place, the argument is that there is an additional mechanism at the hands of voters which may be used to (ban)

[3]Arnold and Freier (2015) and Asatryan, Baskaran, Grigoriadis, and Heinemann (2017) show that the signature requirements affect the probability of holding initiatives.

mimic (non-)preferred policies.[4] Anecdotal evidence from the widely discussed construction of a new railway station in the city of Stuttgart[5] and a debate in two nearby towns in the state of Bayern on whether to build a new city hall or to renovate the old one[6] serve as examples of direct democratic activity being contagious across jurisdictions. Besides learning about the *possibility* of conducting initiatives, the second case illustrates an additional notion of learning, namely learning about specific *policies*.

3.2.2 Previous literature

This paper is related to the literature studying inter-governmental interactions in public policy in general, and from the representative versus direct democracy angle in particular. For a review of studies on inter-jurisdictional spatial interactions see Brueckner (2003), and for meta-regression evidence on interactions in fiscal policy see Costa-Font et al. (2014, 2015).

In the theoretical framework of Hugh-Jones (2009), interactions may exist either for policy experimentation (citizens themselves observe the effects of policy),

[4]In practice, the possible interactions can not only occur by mimicking of initiatives but also by less formal means, for example by demonstrations or informal initiatives. In a sense, our results therefore constitute a lower bound estimate for spillover effects in direct democratic activity of citizens.

[5]The so-called Stuttgart 21 project calls for deconstructing two wings of a century-old train station, and replacing above-ground tracks with a tunnel system which is supposed to speed up travel times. However, there have been several initiatives which all aimed at stopping the project, and in 2010, protests against this long-term project accumulated in large demonstrations (see, e.g., October 6, 2010 issue of the New York Times: http://www.nytimes.com/2010/10/06/world/europe/06germany.html?_r=0). This direct democratic engagement by the population appears to have had spillover effects on the citizens of other municipalities, for example, in the close-by town of Leonberg where citizens launched an initiative in the same year against the demolition of a public indoor swimming pool and the related plans of building a new swimming pool. One of the initiators of the initiative explicitly stated that their activities have been inspired by the Stuttgart 21 opposition (see, e.g., regional newspaper "Stuttgarter Zeitung", which serves subscribers in both municipalities Stuttgart and Leonberg: http://www.stuttgarter-zeitung.de/inhalt.buerger-begehren-die-sanierung-des-sportzentrums.4c32408a-5936-44dd-93b8-5bef9a6a138b.html).

[6]In Denklingen, state of Bayern, there was a long-standing discussion whether to build a new city hall, or to renovate and extend the old one. This led to an initiative against building a new city hall which was accompanied by many newspaper articles about the topic (see, e.g., reports from two regional newspapers which cover both municipalities: http://www.augsburger-allgemeine.de/landsberg/754-Unterschriften-fuer-Rathaus-Stopp-id28639427.html or http://www.kreisbote.de/lokales/landsberg/buergerentscheid-ueber-rathaus-stopp-3354717.html). Shortly after, a very similar discussion arose in close-by municipality of Seefeld which then also led to an initiative.

which is possible only in a direct democratic system, or for yardstick competition in representative systems. The theoretical paper by Boehmke (1999) argues that interactions can be more intensive between jurisdictions that have direct democratic systems compared to representative democracies, but this is explained primarily by informational advantages of the former system.

On the empirical side, Schaltegger and Küttel (2002), somewhat in contrast to the latter, argue that direct democracy (and fiscal autonomy) significantly increases the level of political competition and, therefore, reduces the scope of policy-mimicking. The authors, however, do not analyze the channel that we propose here – that is the potential scope for spillovers through direct democratic institutions.[7] Perhaps most related is Hawley and Rork (2015), which studies the spatial determinants of the property tax limit overrides in Massachusetts and demonstrates that a town's likelihood of holding an initial vote increases by 10-15% if a neighboring town has already held a vote at some point in the past. This evidence combined with our findings reinforce the result of strong spatial interactions in direct democratic instruments in two different settings. In contrast to our paper, however, the focus of Hawley and Rork (2015) is again on referendums called by the government, which only allows studying government-level interactions. Furthermore, the analyzed referendums are about a specific topic (i.e., the property tax limits), whereas our study covers a broader range of policy issues.

The new channel that we propose is related to the mechanisms described by the literature on social interactions analysis (Manski 2000), which in our context can be thought of (groups of) voters as collective decision-makers interacting with each other similar to individuals. Of course, voters do have a role to play in a representative democracy, where, for example, they can "vote with their feet" affecting competition and the implied interactions between governments.

Interactions between voters can also influence politics by spillovers in the decision to turn out to vote (Sinclair et al. 2012), as well as outside of elections such as through popular mobilization. In fact, a large literature in political

[7] Also, the focus is on referendums in Switzerland, thus, only on the veto-power of direct democracy, while the agenda-setting function of initiatives, which by definition cannot limit but may actually enhance the policy space (Matsusaka 2018), is neglected.

science and sociology argues that such instances of collective action do not take place in isolation, but are often the result of significant spillovers across time and jurisdictions.[8] Proximal models stipulate that actors mimic strategies of other people or groups which are spatially or culturally important to them (e.g. Soule 1995, and Soule 1997, in the context of student movements). In these settings, diffusion is promoted by *direct* and *indirect* channels. Direct channels refer to the existence of frequent contacts between the actors or even their overlapping engagement in more than one movement. These direct channels might also occur in the case of direct democracy when, for example, special interest groups spread to close-by municipalities. However, it is also possible that diffusion takes place by more indirect channels like media coverage (and the consequent informational flows between voters) as described by Snow et al. (2004, p. 295).[9]

3.3 Research design

3.3.1 Data

Our data consists of an unbalanced panel of over 13,000 German municipalities across all German states for the years from 2002 to 2014 except of the city states Berlin and Hamburg.[10] Table 3.A.1 summarizes the data on: municipality-level institutions of direct democracy per state (collected from the respective municipal codes), municipality-level data on the frequency of observed initiatives as our dependent variable, and a number of control variables[11].

[8] See Snow et al. (2004) for a discussion of social movements in general, and chapter 13 for a summary of the diffusion research in this field in particular.

[9] In the context of race riots in the United States, for example, Myers (2000) finds that wider media coverage increases the penetration of riots in neighboring areas. Relatedly, Revelli (2008) shows that voters compare their jurisdiction's performance with jurisdictions they share the same local media with. Aidt et al. (2020) study the role of information in the spread of Captain Swing riots of 1830-31.

[10] We exclude these special "city states" since initiatives there are either implemented on the level of the state (same as city) or district, both being different than municipalities. However, we keep the state Bremen as it consists of the municipalities Bremen and Bremerhaven. The panel is unbalanced because of amalgamations of municipalities.

[11] Unemployment rate, population, the share of population above 65 years old, and the sum of the vote shares for the Green Party (Bündnis90/Die Grünen), the Social Democratic Party (SPD) and the Left Party (Die Linke) in the federal elections (denoted by "left share" hereafter)

3.3.2 Spatial lag model

The paper tests for spillovers in direct democratic activity by specifying a reaction function (spatial lag model) similar to the approach employed by the literature on tax competition and public budget spillovers (see, e.g., Devereux et al. 2008; Redoano 2014; Foucault et al. 2008). We specify the following linear probability model in order to test whether citizens mimic their direct democratic activities across jurisdictions:

$$d\_p_{it} = \delta \sum_{j \neq i}^{N} w_j p_{jt} + X_{it}\beta_2 + \alpha_{2i} + \mu_{2t} + \varepsilon_{2it} \tag{3.1}$$

where the dependent variable d_p_{it} is a dummy which is one if there was at least one initiative launched by citizens of municipality i in year t. On the right-hand side, X_{it} is a set control variables[12]; α_{2i} is a municipality fixed effect, μ_{2t} a year dummy, and ε_{2it} an unobserved error term clustered at the county level. The spatial lag ($\sum_{j=1}^{N} w_j p_{jt}$) constitutes the variable of interest, that is the average number of initiatives in the neighbor municipalities of i. The same weight w_j is attached to each neighbor municipality j of municipality i. We normalize the sum of these equal weights to one, thus the spatial lag is simply the average number of initiatives in the neighbor municipalities. In the main specification, municipalities within a 50 km radius of municipality i qualify as neighbors. This reflects the idea that spillovers in direct democratic actions are likely to be a rather regional phenomenon and that municipalities beyond 50 km may be on average too far away for having an effect on municipality i, for example due to limited information flows across regions.[13] Consistently, we also exclude

[12] In the baseline specification the control variables include (log) population, unemployment rate, share of population above 65, and vote share of left-wing parties in federal elections.

[13] With respect to *direct* exchange between individuals, Mok and Wellman (2007) show that distance matters for interpersonal contact. Also, regional newspapers make up almost 75% of the total sales of daily newspapers in Germany in 2014 (Bundesverband Deutscher Zeitungsverleger e.V. 2015, p. 5). These regional newspapers put a strong emphasis on regional news. Similarly, the fiscal spillover literature also assumes geographically close jurisdictions to have a greater effect on each other than more remote jurisdictions (for example, Foucault et al. (2008) in the context of spending interactions between French municipalities and Redoano (2014) with respect to tax competition among European countries.)

municipalities which are close to a country border of Germany.[14] For robustness, we vary the definition of neighbor municipalities and look at radii from 5 to 70 km.

3.3.3 Two-stage spatial lag model

Reaction functions like specified in Equation 3.1 may be subject to a major endogeneity concern: It is explicitly assumed that the likelihood of having an initiative in municipality i depends on the average number of initiatives in municipality j and vice versa – this makes the spatial lag endogenous by definition. The problem can be mitigated by applying appropriate instruments to the spatial lag. The literature cited above on budget and tax-setting spillovers uses the (weighted) averaged demographic and political control variables of the neighbor municipalities to instrument the spatial lag. However, as argued recently by Baskaran (2014, 2019) this is no golden way out since this approach is not robust to possible common shocks or spatially correlated (unobservable) effects.

Following Asatryan (2016), we address this problem by relying on a plausibly exogenous variable as our main instrument, namely the minimum requirement for the number of signatures which have to be collected within a predefined time. The first-stage specification therefore regresses the spatial lag on the averaged control variables of the neighbor municipalities (including the *signature requirement*) and all of the previous regressors and takes the following form:

$$\sum_{j \neq i}^{N} w_j p_{jt} = \beta \sum_{j \neq i}^{N} w_j X_{jt} + X_{it}\beta_1 + \alpha_{1i} + \mu_{1t} + \varepsilon_{1it} \tag{3.2}$$

Clearly, our main instrument, the signature requirement, is *relevant* for the frequency of initiatives in a municipality (non-zero covariance between $\sum_{j \neq i}^{N} w_j p_{jt}$ and $\sum_{j \neq i}^{N} w_j X_{jt}$). Furthermore, the exogeneity condition is fulfilled since the signature requirement in municipality j has no direct effect on the number of initiatives in municipality i (signature requirement in j is uncorrelated with the error term of the second stage ε_{2it}). This can be safely concluded since the

[14] We define closeness analogous to the respective neighborhood definition in the different specifications. Border municipalities are not excluded in the spatial lag of municipalities not close to country borders.

signature requirements are determined by state-laws and not by the municipalities themselves and are, hence, exogenous to the single municipalities. Although set by the states, the instrument does not only vary across states but also across municipalities within states depending on population thresholds, as well as over time within states due to policy reforms.[15] The instrument is measured in percent of a municipality's population and varies between 0.43% in a municipality in Sachsen-Anhalt and up to 15% in some municipalities in Rheinland-Pfalz, Sachsen and Sachsen-Anhalt (see Table 3.A.1).

To estimate our (second stage) specification, we choose a linear probability model (LPM) which allows for a binary dependent variable. The advantages of LPM prevail in our setting, similar to Angrist and Pischke (2008) and Angrist (2001) who argue in favor of the LPM instead of alternative models for which the conditions are likely not to be fulfilled. A probit model would ensure that the fitted values fall between zero and one, which is not always the case with LPM; however, curve-fitting grounds and predictions are not decisive in our context (Angrist and Pischke 2008, p.80). Furthermore, including fixed effects would make probit estimates inconsistent (Fernández-Val 2009). However, in our model it is crucial to include municipality and time-fixed effects in order to control for unobserved time-invariant municipality factors and common dynamics in direct democratic activity over time. Moreover, employing an instrumental variable approach within the framework of non-linear models would lead to severe additional complexity.[16]

3.4 Results

3.4.1 Main results

Our baseline second stage results are collected in Table 3.1. The main explanatory variable of interest is the spatial lag, i.e., the average number of initiatives in

[15] Several state-level reforms have changed the signature requirements in our sample, such as the reforms of Niedersachsen in 2004, Thüringen in 2009, Rheinland-Pfalz in 2010, Hessen in 2011, Sachsen in 2013, Schleswig-Holstein in 2013, and Sachsen-Anhalt in 2014.

[16] Angrist and Pischke (2008, p. 80) also put this point forward when arguing in favor of LPM. Beck (2011) discusses the trade-off between LPM and non-linear models.

neighbor municipalities within a 50 km radius. The first stage results are reported in Table 3.A.2. Our main instrument, the signature requirement, is statistically significant and negative. This evidence of a negative effect of stricter signature requirements on direct democratic activity is consistent with the existing literature (Arnold and Freier 2015; Asatryan, Baskaran, Grigoriadis, and Heinemann 2017).

Table 3.1: Baseline results: spillover effects of neighbors' initiatives

	(1)	(2)	(3)	(4)	(5)	(6)
CITIZEN INITIATIVES	Dummy					Amount
Spatial lag (neighbor initiatives) in t	0.8714***	0.6446*				1.0520***
	(0.2691)	(0.3599)				(0.4009)
Spatial lag (neighbor initiatives) in $t-1$			0.8902**			
			(0.4188)			
Spatial lag (neighbor initiatives) in $t-2$				0.9525**		
				(0.4016)		
Spatial lag (neighbor initiatives) in $t-3$					0.0438	
					(0.3263)	
Signature requirement		-0.1335	-0.1248	-0.1727*	-0.2292**	-0.1121
		(0.1067)	(0.1089)	(0.1000)	(0.0911)	(0.1089)
Ln population		0.0075	0.0072	0.0092	0.0064	0.0051
		(0.0064)	(0.0077)	(0.0106)	(0.0153)	(0.0073)
Unemployment rate		0.0435	0.0655**	0.0635*	0.0132	0.0560*
		(0.0272)	(0.0296)	(0.0350)	(0.0389)	(0.0312)
Share of population over 65		0.0217	0.0122	0.0147	0.0004	0.0340
		(0.0292)	(0.0309)	(0.0323)	(0.0352)	(0.0327)
Left share		-0.0033	-0.0091	-0.0208*	-0.0256*	-0.0059
		(0.0124)	(0.0130)	(0.0119)	(0.0136)	(0.0130)
Observations	101,673	100,481	90,767	80,994	71,167	100,481
Number of municipalities	9,944	9,939	9,716	9,432	8,897	9,939
Hansen-J p-value	0.570	0.747	0.486	0.192	0.234	0.907
Kleibergen-Paap F	18.19	10.54	13.66	18.76	21.17	10.54
Kleibergen-Paap LM	49.42	35.97	45.47	59.55	74.80	35.97

*** p<0.01, ** p<0.05, * p<0.1. Notes: The table presents results for the second stage estimation of the model specified in Equation 3.1. All regressions include time and municipal fixed effects. Standard errors are clustered at the county level. The dependent variable in columns (1) to (5) is a dummy which takes the value 1 in case the municipality has at least one initiative in a given year and in column (6) it is the amount of initiatives in a municipality in a given year.

In Table 3.1, we report several tests for the validity of our specification and the strength of our instruments. The Hansen-J overidentification test checks if the instruments are uncorrelated with the error term. All specifications show large p-values which imply that our instruments are valid. The Kleibergen-Paap LM

underidentification statistic tests whether the employed instruments are relevant; the null hypothesis being that they are irrelevant. In addition, the Kleibergen-Paap F statistic tests for weak identification; the null hypothesis being that instruments are weak.[17] The results of the Kleibergen-Paap statistics show that the instruments are sufficiently strong.

In the main results presented in Table 3.1, we obtain a significant and positive estimate for the spatial lag of initiatives which is robust to a number of specifications. In columns (1) to (5), the dependent variable is the dummy as we define it in Section 3.3. In the last column, we use an alternative dependent variable, i.e., the amount of initiatives. After controlling for municipal characteristics and fixed effects, the probability of having an initiative in period t increases significantly when the neighbors host more initiatives in t (column 2).

In columns 3-5 of Table 3.1, we substitute the contemporaneous spatial lag by the first, second and third lagged values in order to study whether the spillover effects are persistent over time. Indeed, we observe that the effects hold over time. For the second lag, the estimated coefficient is 0.95, which indicates that the probability of having an initiative increases by 95 percentage points if – on average – all neighbor municipalities have one initiative more. Given that the average number of neighbor municipalities is 460, this implies an unrealistic increase in the number of neighbor initiatives by 460. To aid in interpretation, we compute results that show the effect of a one standard deviation increase in the number of neighbor initiatives. A one standard deviation increase in neighbor initiatives increases the probability of having an initiative by 2.13 percentage points.

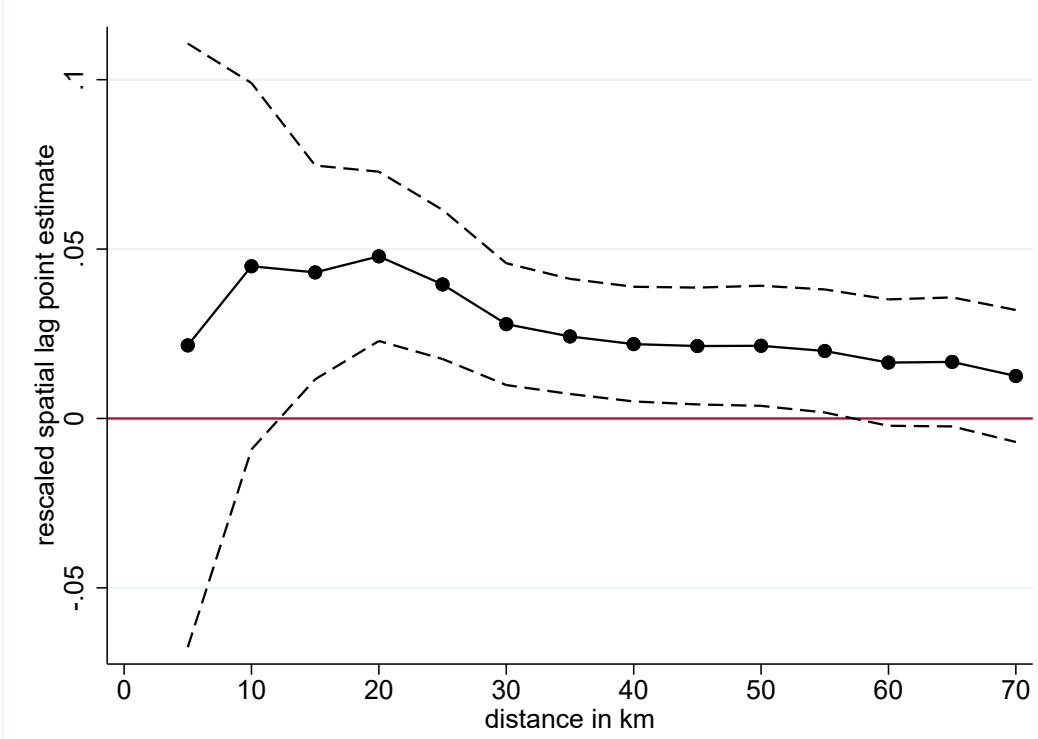
Spillovers from initiatives one and two years before appear to be even stronger than the simultaneous spillovers.[18] This is plausible given the time constraints involved in the process of launching an initiative (e.g., collection of signatures), as well as the possible delays in the flow of information between neighboring municipalities.

[17] The test replaces the Cragg-Donald weak identification test in the case of heteroskedastic standard errors.

[18] As a placebo test, we also regressed the initiative dummy on forwarded spatial lags. The idea is that spillover effects from future initiatives on initiatives today cannot happen. As expected, the effects from pre-trends are statistically insignificant.

An alternative explanation, however, is that these effects include so-called feedback effects (Anselin 2003), when a change in the signature requirement in the neighborhood affects municipality i's initiatives through the neighbors' initiatives, which then again affect the neighbors' initiatives and feed into i's initiatives. The contemporaneous specification does not capture these dynamics.[19] In any case, the estimates of column 5 provide evidence consistent with the interpretation that spillover effects vanish eventually when going further back into time. In the following analyses, we will rely on effects from the second lagged spatial lag.

Figure 3.1: Spillover effects over space



Notes: This graph shows the point estimates with 95% confidence intervals of the spatial lag estimated from Equation 3.1. The spatial lag contains municipalities within different radii. The point estimates and the standard error are rescaled by the respective standard deviation of each spatial lag. We use the spatial lag in $t-2$. All regressions include time and municipality fixed effects as well as the control variables as defined before. Standard errors are clustered at the county level.

[19]Relatedly, in Figure 3.A.2, we estimate average spillover effects of a shock on the right-hand side by taking the sum of initiatives in municipality i over several years. The estimate for the aggregated initiatives over one year (rescaled by the standard deviation) corresponds to the specification of column (6) in Table 3.1. The following point estimates increase until the aggregate over three years and then decrease for aggregated initiatives of more than three years.

In addition to estimating the effects over time, we also study spillover effects from neighbors over space, that is for radii ranging from 5 to 70 km, in steps of 5 km. The results are shown in Figure 3.1. The effects are not identified for small radii. A reason could be that the number of initiatives for small radii is rather little.[20] Another explanation is that larger municipalities are dropped from this sample as they do not have neighbors within these radii.[21] Spillover effects decline for increasing radii from 20 km on. This confirms that spillover effects are regionally restricted, e.g., due to limited information flows across space, and close-by municipalities being more relevant for each other than further apart ones.

3.4.2 Extension of results

In this sub-section, we extend our main results by three empirical tests to shed more light on the spillover mechanisms. Firstly, we ask whether the spillovers are due to learning about the possibility of holding initiatives or due to learning about specific policies. Secondly, we are interested to see whether informational channels – such as newspaper circulation or direct personal contacts across municipalities – play a role in these patterns of interactions. Thirdly, we test whether the average spillover effects estimated in baseline are driven by successful initiatives only, or also by unsuccessful ones.

Up to now, we have demonstrated that all initiatives in the neighborhood positively affect the likelihood of hosting any initiative. This result allows arguing that the use of direct democratic instruments might have spillovers itself as voters learn about a new political tool they can exercise in general.[22]

In Table 3.2, we advance a step further by dividing the initiatives into three main public policy areas – traffic, economy, and infrastructure – over which initiatives can be held. These topics are by far the most frequent ones and jointly cover 83% of all. We then estimate the baseline regressions with controls and fixed effects both within and across these topics. Two out of three within-topic coefficients reported on the diagonal of Table 3.2 show significant effects. These

[20] For radii of 5 and 10 km, the average amount of initiatives per neighborhood is less than 2. For larger radii, this number increases, e.g., for 50 km it is 52 initiatives per neighborhood.

[21] Note that distances are measured from the centroid of a town.

[22] Parallels can be drawn to the literature that studies the cross-border diffusion of democracy (Elkink 2011), regime change (Brinks and Coppedge 2006), and riots (Aidt and Franck 2015).

results broadly support the hypothesis that the interaction effects are largely driven by spillovers in specific public policies. We do not detect evidence for spillovers across different topics.

Next, we test whether the cross-municipal spillovers in initiatives are conditional on the availability and exchange of information. One of the important spillover mechanisms we have in mind is the media channel. Vetoing or inaugurating a certain policy in one municipality may have the highest impact on neighboring town's voters when they are sufficiently informed. To test this hypothesis, we divide the municipalities according to whether households purchase daily regional newspapers above or below the median town. Regional newspapers are a primary source of information for citizens on local issues.[23] Results for the baseline specifications for the two sub-samples are collected in Table 3.3 (columns 1-2). We find evidence of significant spatial interactions in towns with above-median number of newspapers but not for those with below-median newspapers. This exercise reveals that information is one important transmission channel through which spillovers in initiatives spread.

Relatedly, we test whether more direct channels of information diffusion play a role for direct democratic activities having effects on neighbors. Direct channels refer to personal contacts of citizens from different municipalities. To test this hypothesis, we divide our sample according to whether a municipality 'hosts' incoming commuters above or below the median town.[24] Results for the baseline specification for the two sub-samples are collected in Table 3.3 (columns 3-4). We find evidence of significant spatial spillovers into towns with above-median number of commuters per capita but not for those with below-median commuters.[25] This indicates that direct channels of information diffusion between municipalities play a role for spillovers in direct democracy.

Finally, we test whether the success of an initiative in the election matters

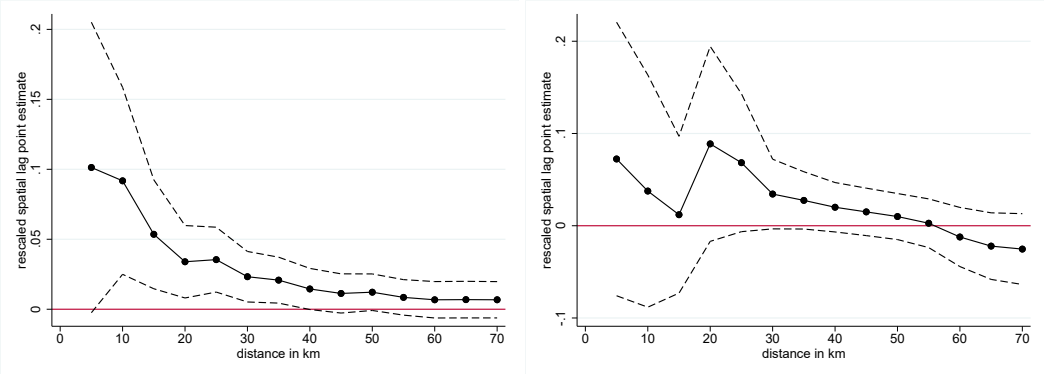
[23]We rely on the data on 2008's newspaper circulation by Falck et al. (2014) which in turn rely on data from the German Audit Bureau of Circulations (Informationsgemeinschaft zur Feststellung der Verbreitung von Werbeträgern, IVW 2011). The data provides the annual number of daily newspapers per household for each municipality in 2008.

[24]We use commuters data from the Federal Statistical Office ("Regionaldatenbank").

[25]The number of commuters as well as the number of newspapers are not correlated with other variables that affect the probability of hosting an initiative. Means and standard deviations of the control variables are similar for the two sub-samples.

Figure 3.2: Effects of spillovers from neighbors' successful or failed initiatives

(a) *Successful initiatives* (b) *Failed initiatives*



Notes: These graphs show the point estimates with 95% confidence intervals of the spatial lag estimated from Equation 3.1. The spatial lag is defined as the average amount of successful (sub-figure (a)) and failed (sub-figure (b)) initiatives in a referendum and contains municipalities within different radii. We use the spatial lag in $t-2$. The point estimates and the standard error are rescaled by the respective standard deviation of each spatial lag. All regressions include time and municipality fixed effects as well as the control variables as defined before. Standard errors are clustered at the county level.

for the spillover effects. We restrict the sample to those initiatives that collected enough signatures in the first stage and that were subsequently put to vote.[26] We then regress our dependent variable, i.e. the initiative dummy, on a spatial lag that, on the one hand, only includes initiatives that gained a majority in favor of the initiative and, on the other hand, only includes initiatives that failed to gain a simple majority. The estimation results are shown for a range of radii for both of the cases in Figure 3.2. We can see that spillover effects from failed initiatives are not statistically significant, while successful initiatives show evidence of spillovers. This result indicates that citizens rather try to mimic fruitful direct democratic activity than unsuccessful initiatives.

[26] Another possibility for an initiative to succeed is for the city council to accept the proposal before it is put to vote, or even before it reaches the minimum threshold of signature requirements. We discard these cases, since their success is not readily observable by the public.

Table 3.2: Spillover effects of neighbors' initiatives by topic

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	initiative traffic			initiative economy			initiative infrastructure		
Spatial lag traffic	**-0.6822**			-0.0856			-0.4835		
	(0.8685)			(1.1312)			(1.0843)		
Spatial lag economy		0.2074			**0.8396****			-0.0195	
		(0.2129)			(0.4059)			(0.4293)	
Spatial infrastructure			-0.1266			0.0953			**0.5986***
			(0.2587)			(0.3273)			(0.3496)
Signature requirement	-0.1132	-0.1158	-0.1154	-0.1358	-0.1596*	-0.1312	-0.2093**	-0.2063**	-0.1806*
	(0.0973)	(0.0946)	(0.1025)	(0.0970)	(0.0943)	(0.1030)	(0.0963)	(0.0956)	(0.1043)
Ln population	-0.0033	-0.0044	-0.0036	0.0074	0.0010	0.0081	-0.0081	-0.0076	-0.0039
	(0.0035)	(0.0033)	(0.0039)	(0.0081)	(0.0073)	(0.0082)	(0.0070)	(0.0064)	(0.0075)
Unemployment rate	0.0168	0.0227	0.0207	0.0034	-0.0030	0.0072	-0.0102	-0.0046	0.0131
	(0.0159)	(0.0217)	(0.0177)	(0.0218)	(0.0251)	(0.0224)	(0.0219)	(0.0261)	(0.0233)
Share of population over 65	0.0090	0.0011	0.0064	0.0040	-0.0157	0.0032	0.0482**	0.0464**	0.0433**
	(0.0174)	(0.0144)	(0.0157)	(0.0233)	(0.0216)	(0.0221)	(0.0229)	(0.0213)	(0.0217)
Left share	-0.0031	-0.0040	-0.0050	-0.0071	-0.0022	-0.0076	-0.0234***	-0.0251***	-0.0263***
	(0.0060)	(0.0050)	(0.0048)	(0.0102)	(0.0092)	(0.0091)	(0.0079)	(0.0076)	(0.0071)
Observations	80,994	80,994	80,994	80,994	80,994	80,994	80,994	80,994	80,994
Number of ags	9,432	9,432	9,432	9,432	9,432	9,432	9,432	9,432	9,432
Hansen-J p-value	0.361	0.424	0.349	0.0832	0.240	0.0664	0.0638	0.0604	0.261
Kleibergen-Paap F	7.551	13.54	25.28	7.551	13.54	25.28	7.551	13.54	25.28
Kleibergen-Paap LM	43.26	42.16	73.46	43.26	42.16	73.46	43.26	42.16	73.46

*** p<0.01, ** p<0.05, * p<0.1. Notes: The table presents second stage estimates of the linear probability model specified in Equation 3.1. Spatial lags contain municipalities within a radius of 50 km. We use the spatial lag in $t-2$. All regressions include time and municipal fixed effects as defined before. Standard errors are clustered at the county level.

Table 3.3: Spillover effects of neighbors' initiatives by number of newspapers per household and commuters per capita

VARIABLE	(1)	(2)	(3)	(4)
	Citizen initiative dummy			
	local newspapers		**commuters p.c.**	
	below median	above median	below median	above median
Spatial lag (neighbor initiatives)	0.7209	1.1689***	0.4437	0.8438*
	(0.6154)	(0.4409)	(0.5895)	(0.5074)
Signature requirement	-0.1960	-0.0461	-0.0906	-0.2067**
	(0.1407)	(0.0734)	(0.1203)	(0.0951)
Ln population	-0.0023	0.0114	-0.0023	0.0019
	(0.0130)	(0.0156)	(0.0178)	(0.0192)
Unemployment share	0.0289	0.0454	0.0418	0.0654
	(0.0531)	(0.0534)	(0.0537)	(0.0769)
Share of population over 65	0.0200	0.0142	0.0611	0.0669
	(0.0476)	(0.0566)	(0.0578)	(0.0700)
Left share	-0.0205	-0.0210	-0.0628***	-0.0031
	(0.0168)	(0.0207)	(0.0231)	(0.0236)
Observations	37,095	37,109	37,007	36,907
Number of municipalities	3,787	3,637	7,209	6,210
Hansen-J p-value	0.275	0.793	0.767	0.377
Kleibergen-Paap F	19.11	14.50	6.379	27.27
Kleibergen-Paap LM	48.66	40.46	38.94	48.04

*** p<0.01, ** p<0.05, * p<0.1. Notes: The table presents second stage estimates of the linear probability model specified in Equation 3.1. All spatial lags contain municipalities within a radius of 50 km. We use the spatial lag in $t-2$. All regressions include time and municipal fixed effects. Standard errors are clustered at the county level.

3.5 Conclusions

To the best of our knowledge, previous literature has not yet tested or conceptualized the proposed hypothesis that voters of related jurisdictions may mimic each others behavior through the means of direct democracy. The channels of such interactions may be quite different and complex, going from spillovers in specific policies to the process of learning to exploit direct democratic rights. What this analysis adds to the literature is that it is plausible that mimicking between jurisdictions takes place not only through government-level interactions, but also through voters' direct actions in vetoing and inaugurating policies through binding initiatives. Such interactions are, of course, conditional on the existence of some institutions of direct democracy, which are currently not any more rare especially in sub-national levels of high-income countries.

If this reasoning is true, then it is important to recognize and quantify such interactions, because direct democracy matters for policy outcomes. Studies traditionally concentrating on the United States and Switzerland, but more recently also extending to Germany and other countries, find empirical support that direct democratic instruments affect policies in the public sector in general, and fiscal decisions in particular. Although, we find evidence for spatial spillovers in initiatives, it is left to future work to assess to what extent such mimicking behavior drives policy outcomes.

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3.A Appendix A: Additional figures and tables

Figure 3.A.1: Geographical distribution of initiatives in German towns from 2002 to 2014

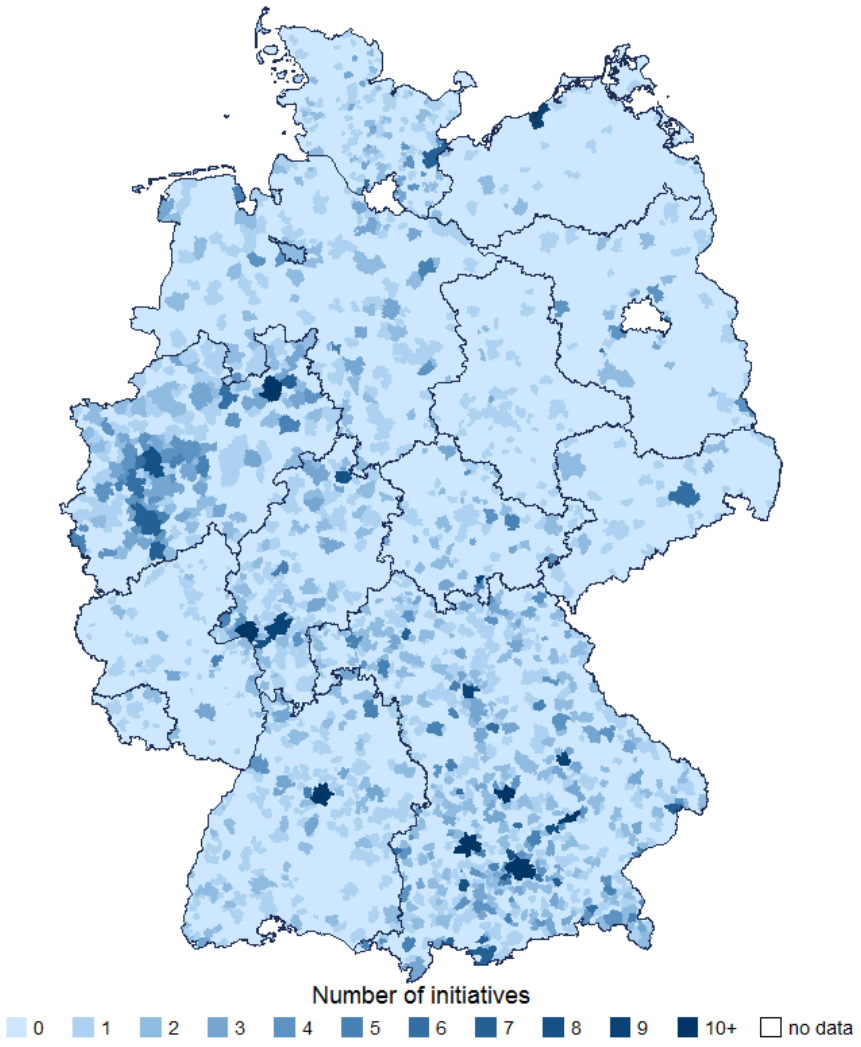
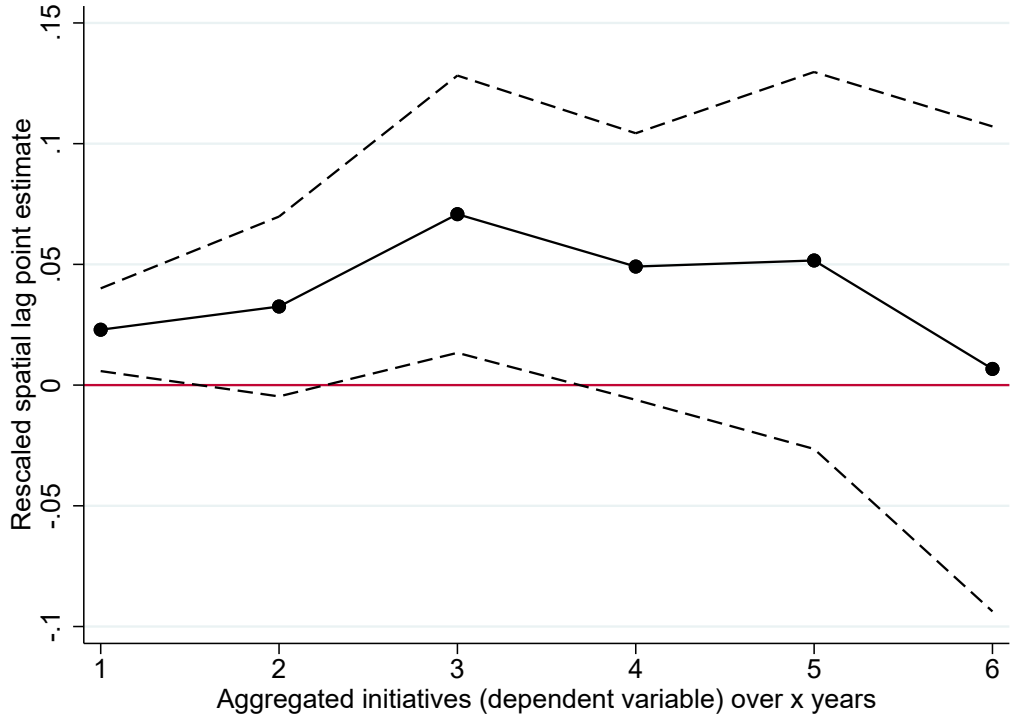


Figure 3.A.2: Spillover effects over years



Notes: This graph shows the point estimates with 95% confidence intervals of the spatial lag estimated from Equation 3.1. The dependent variable is the sum of initiatives over x amount of years. The point estimates and the standard error are rescaled by the respective standard deviation of each spatial lag. All regressions include time and municipality fixed effects as well as the control variables as defined before. Standard errors are clustered at the county level.

Table 3.A.1: Institutions and summary statistics across states, 2002-2014

STATE	Year direct democracy adopted	Signature requirement (mean) in % of pop.	(min-max)	Number of initiatives	Total annual population (mean)	Unemployment rate (mean)	Share of population over 65 (mean)	Left share (mean)	Number of towns in the sample
Baden-Württemberg	1956	0.0983	(0.0327-0.1)	261	9671	0.0335	0.181	0.378	1112
Bayern	1995	0.0983	(0.03-0.1)	1272	6083	0.0349	0.181	0.295	2056
Brandenburg	1993	0.1	(0.1)	80	5514	0.12	0.206	0.602	929
Bremen	1994	0.0971	(0.075-0.1)	2	330418	0.113	0.21	0.622	2
Hessen	1993	0.0985	(0.03-0.1)	250	14160	0.0492	0.201	0.484	430
Mecklenburg-Vorpommern	1993	0.0997	(0.0196-0.1)	49	1975	0.134	0.188	0.507	1829
Niedersachsen	1996	0.0999	(0.0708-0.1)	223	7671	0.0563	0.2	0.487	1061
Nordrhein-Westfalen	1994	0.0794	(0.03-0.1)	426	45121	0.0577	0.191	0.446	406
Rheinland-Pfalz	1994	0.13	(0.0601-0.15)	143	1748	0.0412	0.201	0.434	2311
Saarland	1997	0.1	(0.1)	13	19777	0.0532	0.217	0.527	52
Sachsen	1990	0.143	(0.1-0.15)	116	8552	0.108	0.225	0.428	1042
Sachsen-Anhalt	1990	0.142	(0.0043-0.15)	109	3215	0.137	0.205	0.565	2311
Schleswig-Holstein	1990	0.0999	(0.04-0.1)	232	2519	0.0504	0.192	0.468	1142
Thüringen	1993	0.0887	(0.0339-0.1)	131	2396	0.0912	0.202	0.511	1032

Notes: The data on observed initiatives (column: 5) is available at http://www.mehr-demokratie.de/bb-datenbank.html. The data on the signature requirement (columns: 3-4) is collected from the municipal codes. The control variables (columns: 6, 8 and 9) come from the Federal Statistical Office ("Regionaldatenbank"). The data on unemployment is taken from the Bundesagentur für Arbeit.

Table 3.A.2: First-stage results of Table 3.1 - determinants of citizen-initiatives

	(1)	(2)	(3)	(4)	(5)
VARIABLE	Spatial lag citizen initiatives				
	t		$t-1$	$t-2$	$t-3$
$\sum_{j\neq i}^{N}(w_j \times$signature requirement $j)$	-0.1233***	-0.1366***	-0.0677***	-0.1374***	-0.0758***
	(0.0169)	(0.0341)	(0.0245)	(0.0185)	(0.0218)
$\sum_{j\neq i}^{N}(w_j \times$ln population $j)$	0.0022	0.0051*	0.0017	-0.0017	0.0025
	(0.0032)	(0.0030)	(0.0032)	(0.0025)	(0.0028)
$\sum_{j\neq i}^{N}(w_j \times$unemployment share $j)$	-0.0876***	-0.0812***	-0.1363***	-0.1095***	-0.2003***
	(0.0308)	(0.0315)	(0.0310)	(0.0331)	(0.0378)
$\sum_{j\neq i}^{N}(w_j \times$share of population over 65 $j)$	0.1997**	0.1423	0.1668*	0.3584***	0.2882**
	(0.0866)	(0.0872)	(0.0950)	(0.1021)	(0.1157)
$\sum_{j\neq i}^{N}(w_j \times$left share $j)$	-0.0125	-0.0149	-0.0032	0.0121	0.0365*
	(0.0107)	(0.0135)	(0.0108)	(0.0132)	(0.0202)
Signature requirement		0.0222	-0.0319	0.0007	0.0735***
		(0.0320)	(0.0253)	(0.0223)	(0.0237)
Ln population		0.0025	0.0040*	0.0022	0.0038
		(0.0019)	(0.0024)	(0.0027)	(0.0036)
Unemployment rate		-0.0044	0.0092	-0.0036	0.0193
		(0.0085)	(0.0093)	(0.0115)	(0.0136)
Share of population over 65		0.0037	0.0063	0.0119	-0.0080
		(0.0048)	(0.0063)	(0.0077)	(0.0089)
Left share		-0.0048*	-0.0041	0.0061	0.0171***
		(0.0026)	(0.0033)	(0.0052)	(0.0052)
Observations	101,673	100,481	90,767	80,994	71,167
Number of municipalities	9,944	9,939	9,716	9,432	8,897

*** p<0.01, ** p<0.05, * p<0.1. Notes: The table presents the results for the OLS first stage estimation of the model specified in Equation 3.2. All regressions include time and municipal fixed effects. Standard errors are clustered at the county level.

3.B. Appendix B: Data

- The paper uses panel data on the population of German municipalities, the sample:
  - starts in 2002 and ends in 2014
  - includes 11,000 to 13,000 municipalities every year
  - and has 155,676 observations in total
- The data includes the following sets of variables (summary statistics in the online appendix):
  - Town identifiers including the name and the official key ("Amtlicher Gemeindeschlüssel"), and county and state identifiers
  - Data on around 3,300 initiatives and signature requirements
  - Fiscal data, including spending and its structure and taxes including tax rates and tax revenues
  - Socioeconomic and demographic data, including population size, population over 65 years old, unemployment, commuters, local newspaper consumption, etc.
  - Results and turnouts for federal, European and state elections
  - Other municipal characteristics, like building permits and area, including residential, industry, agriculture
- The sources of data are:
  - Statistik Lokal
  - Mehr Demokratie e.V.
  - Municipal codes
  - Arbeitsagentur
  - Falck et al. (2014)

- The data is available for download at:
  http://ftp.zew.de/pub/persons/ZarehAsatryan/LocalGovDataDE/

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Biases in Fiscal Multiplier Estimates (with Zareh Asatryan, Friedrich Heinemann and Justus Nover), 2020, European Journal of Political Economy 63, 101861

Euro Area Reform Preferences of Central and Eastern European Economic Experts (with Sebastian Blesse and Friedrich Heinemann), 2020, Empirica, Journal of European Economics

Vetoing and inaugurating policy like others do: evidence on spatial interactions in voter initiatives (with Zareh Asatryan and Frank Streif), 2017, Public Choice 172, 525-544