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Antoine Boucher

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Three Essays in Applied Political Economy

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Antoine Boucher

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Three Essays in Applied Political Economy

Antoine Boucher

Under the supervision of L. Chauvet and M. Ferry

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General Introduction

This thesis presents the results of three research projects relating to two wider topics: development assistance for developing countries and the consequences of political repression. The three chapters presented are self-contained research articles and can be read separately. While they may appear unrelated at first, they link back to the power relationship behind policies and their effects on a wide array of outcomes, including economic results or individuals' political perceptions. Consequently, these three chapters fall within the domain of political economy, since the primary objective of these three studies is to assess how political decisions, which are motivated by political interests and power dynamics, impact both economic and political outcomes.

The first two chapters delve into the political determinants and economic repercussions of both bilateral and multilateral development assistance. Official Development Assistance (ODA) refers to financial aid and resources provided by governments and international organizations to support the economic and social development of low and middle-income countries. Multilateral aid is provided by multiple countries through international organizations to support development initiatives in recipient countries, while bilateral aid is given directly from one country to another. Following the aid legitimacy crisis during the 1990s decade, the formulation of the Millennium Development Goals prompted an increase in official development assistance. Along with the rise of both multilateral and bilateral aid, the 2000s also witnessed the emergence of so-called non-traditional donors, with China being a prominent example.

Despite the relatively recent increase in its official development assistance activities, China is not a newcomer to this sector. The first Chinese aid projects started in 1949 at the end of the Chinese Civil War. During this early period, similar to many other bilateral donors, China's aid was primarily motivated by strategic considerations. These included providing assistance to socialist and nonaligned countries and countering Taiwan's diplomatic recognition. One representative

example of China's early aid efforts is the construction of the TAZARAM railway linking Tanzania and Zambia in 1975, as this substantial transport project was executed in collaboration with non-aligned and socialist governments. As noted by Brautigam (2011), Chinese aid's substantial increase during the 2000s can be attributed to several key factors, including China's growing demand for primary resources due to its rapid economic growth, its aspiration to be seen as a responsible global actor, the diplomatic rivalry with Taiwan, and the strategic objective of securing new markets for its industries that have saturated the domestic one.

As China's influence grew on the global stage, its aid provision became increasingly controversial. It faced accusations of supporting authoritarian regimes, throwing recipient countries into debt crises, being deployed as a means to secure access to natural resources, and lacking environmental and social safeguards. Consequently, a growing part of the economic development literature has explored this emerging donor, notably its aid determinants and developmental impacts.

Based on the findings of Dreher and Fuchs (2015) and Dreher et al. (2018b), which explore the determinants of Chinese aid, Chinese overseas economic intervention does not seem to display a significantly higher degree of political motivation compared to Western donors. Furthermore, it is not primarily explained by the natural resource endowments of recipient nations. However, since China is not part of the Donor Assistance Committee, its aid does not follow international standards. Consequently, its cross-border financial flows need a closer examination to understand its specific features. When China's overseas economic intervention is broken down into two categories, ODA-like (i.e. loans with a grant element of at least 25%) and other official financial flows (OOF), a more nuanced pattern emerges. Dreher and Fuchs (2015) and Dreher et al. (2018b) find that China's ODA allocation is influenced by foreign policy considerations, such as the recognition of Taiwan, whereas OOF are driven by economic factors, including the presence of oil resources, a lower debt burden (indicating a higher likelihood of repayment), and preexisting trade connections with China. Considering the results of these studies, the sulfurous reputation of Chinese aid seems unjustified, as its allocation aligns with the same political factors as other major bilateral donors (Alesina and Dollar, 2000; Faye and Niehaus, 2012).

Regardless of China's motivations to provide aid, several studies have found that Chinese ODA seems to have an overall positive impact on development, as measured across various dimensions. For instance, Chinese ODA would generate

economic growth (Dreher et al., 2021b), despite concerns about potential capture by recipient elites (Dreher et al., 2021a). It has also been shown to be associated with improvements in health and education (Martorano et al., 2020; Cruzatti et al., 2023), a reduction in spatial inequality by decentralizing economic activities (Bluhm et al., 2018), a boost in firms' sales (Marchesi et al., 2021), without undermining recipients' state stability (Gehring et al., 2022). However, the picture is not entirely bright, as Chinese aid has also been linked to an increase in corruption around its projects (Isaksson and Kotsadam, 2018a), discouraged trade union involvement (Isaksson and Kotsadam, 2018b), and has fueled government repression in recipient countries (Gehring et al., 2022), all through a supplier-to-recipient norm transmission.

The first chapter aims to contribute to these two literature strands. More precisely, I examine how transport infrastructure projects funded by Chinese overseas economic intervention influence outcomes at the firm level in developing countries. Based on Mueller (2022) findings, I employ local political motivations in China as an instrument for Chinese-funded transport infrastructure, given that China tends to utilize its overseas interventions to stimulate the activities of local firms during periods of social unrest within China. I contribute by testing the instrument proposed by Mueller (2022) in a more aggregated setting and by complementing Marchesi et al. (2021) findings by narrowing down the analysis to transport projects only, hence focusing on the relief of infrastructure constraint's mechanism. This emphasis on transport projects, encompassing the construction of roads, railways, ports, and airports, aligns with China's aid strategies, as they are inclined toward undertaking substantial projects and employing a large-scale approach, which differs from the practices of other major bilateral donors (Brautigam, 2011). In addition, recent data from AidData regarding Chinese aid projects between 2000 and 2014 indicates that almost a third of China's foreign economic involvement is dedicated to constructing transport infrastructure.

Along with China's emergence as a prominent donor, the 2000s also saw various pledges to enhance aid effectiveness among "official" donors by promoting good practices through several forums, with a notable example being the 2005 Paris Declaration. In essence, these conferences aimed to shift the focus from the donor interest model, which implies that aid allocation is driven by donors' strategic objectives like securing access to natural resources, building political alliances, or advancing their economic interests, towards a recipient need model, in which aid is distributed based on the development priorities of the recipient country. Aligned

with these principles emerged a need to place greater reliance on multilateral aid, as it tends to be more influenced by recipients' needs rather than the donors' interests, as suggested by [Maizels and Nissanke \(1984\)](#) and [Neumayer \(2003\)](#). However, since the funding for multilateral aid is contingent on financial contributions from developed nations (i.e., traditional donors who typically allocate bilateral aid based on their own interests ([Maizels and Nissanke, 1984](#); [Alesina and Dollar, 2000](#))), it becomes crucial to investigate the political motives driving the allocation of multilateral aid. This need is also more pronounced when we take into account that a growing portion of multilateral aid is allocated towards recipient public procurement. This allows for the recipient's own interests to play a role, including the electoral interests of the recipient government.

Several researchers investigated this question, notably regarding the influence of the United States and other major bilateral donors within these organizations. For instance, [Kilby \(2013\)](#) and [Kersting and Kilby \(2021\)](#) have shown that domestic U.S. politics play a role in the allocation of aid by the World Bank. [Kersting and Kilby \(2016\)](#) and [Dreher et al. \(2008\)](#) also emphasized that U.S. foreign interests impact the World Bank's interventions, with World Bank loans disbursed faster and lower inflation forecasts from the IMF when the recipient government is undergoing a competitive election and geopolitically aligns with the U.S. Moreover, recipient countries tend to receive more multilateral aid from various international organizations if they hold a seat on the United Nations Security Council ([Kuziemko and Werker, 2006](#); [Dreher et al., 2009b,a](#); [Kersting and Kilby, 2019](#)) or have representation on the institution's executive board ([Dreher et al., 2019](#); [Kaja and Werker, 2010](#)). To expand upon this body of literature, one could explore further the most substantial aspect of multilateral development aid, namely, the procurement contracts allocated by the World Bank.

The World Bank was established in 1944 at the Bretton Woods Conference. Its primary objective was to assist the post-World War II reconstruction efforts in Europe by providing financial and technical assistance. Over time, the World Bank expanded its mission to support economic development and poverty reduction worldwide by providing loans and grants to developing countries. The World Bank has emerged as the foremost multilateral contributor, with its assistance comprising 31% of the overall aid disbursed by multilateral organizations since the 2000s (according to OECD statistics). In the 2000s, in accordance with the aid ownership principles declared in various aid effectiveness forums (such as Paris in

2005, Accra in 2008, and Busan in 2011), a growing portion of the Bank's aid has been channeled through recipients' procurement systems. More precisely, aid allocated through recipient procurement entails the direct channeling of funds into the recipient country's own procurement or purchasing procedures, as opposed to the donor country or organization buying goods or services and then delivering them to the recipient. This financing mechanism has gained increasing prominence in the realm of multilateral aid provision, as it involves private sector firms and generates fiscal resources, both of which contribute to the recipient's economic growth. Consequently, the second chapter, co-authored with Lisa Chauvet and Marin Ferry, focuses on the political cycles involved in the allocation of World Bank procurement contracts. Specifically, it explores how donor and recipient countries tend to skew the allocation of these contracts in favor of their domestic companies to align with electoral interests.

The third chapter adopts a distinct approach by examining how politicization and repression have influenced electoral results from a historical perspective. It leverages the natural experiment involving Savoy and Nice change of country between 1815 and 1860. More precisely, it exploits variations in electoral outcomes along the 1815-1860 border for similar populations that experienced varying degrees of politicization processes and exposure to repression.

The substantial damages of political repression have been widely documented. As political repression often involves traumatic incidents, one of its most evident consequences is on mental health. [Munczek and Tuber \(1998\)](#), [Sales et al. \(2000\)](#), and [Stammel et al. \(2013\)](#) have all established a link between being a victim of or having a family member affected by political repression and the development of enduring psychological disorders. These individuals experience conditions such as depression, anxiety, post-traumatic symptoms, and prolonged grief disorder for decades following the traumatic events.

Political repression can also have detrimental effects on economic outcomes by eroding trust. [Lichter et al. \(2021\)](#), [Booth et al. \(2022\)](#), [Nikolova et al. \(2022\)](#), and [Pronkina et al. \(2023\)](#) found that various forms of political repression, such as surveillance by the Stasi in East Germany, the Cultural Revolution in China, or simply being aware of the existence of forced labor camps in the USSR, diminish social trust across both interpersonal and institutional aspects. These studies demonstrate a lasting impact, as this lack of trust is transmitted across generations through the process of parental socialization.

And as highlighted by [Nunn \(2009\)](#), trust plays a crucial role in shaping long-term economic development, notably through productivity ([Bjørnskov and Méon, 2015](#)), education, and the quality of institutions ([Bjørnskov and Méon, 2013](#)). [Algan and Cahuc \(2010\)](#) empirically validated this relationship on a global scale, using a measure of inherited trust based on the trust levels of early U.S. immigrants in their countries of origin. Their findings underscore the significant influence of trust on economic development, as they attribute 45% of the variations in income per capita to inherited trust. This insight is confirmed by [Lichter et al. \(2021\)](#), who suggest that regions in former East Germany characterized by a higher density of Stasi surveillance currently experience lower income levels and higher unemployment due to reduced trust.

Despite all its negative consequences on mental health, trust, and economic development, a Machiavellian vision could still justify the use of political repression if the latter meets one of its main purposes, which is to eliminate the politicization previously carried out by the opposition. Several studies explored the impact of political repression on electoral outcomes. [Kapelko and Markevich \(2014\)](#), [Lupu and Peisakhin \(2017\)](#), and [Rozenas et al. \(2017\)](#) found that areas or ethnic groups repressed during the Soviet era are less likely to vote for pro-Russian parties nowadays. In the context of political repression during China's cultural revolution, [Wang \(2021\)](#) suggests that individuals living in more repressed localities have more anti-regime attitudes. In the context of Alsace-Lorraine, [Dehdari and Gehring \(2022\)](#) and [Gehring \(2021\)](#) indicate that relatively less violent repression during the nation-building process led to a heightened inclination towards regionalism, decentralization, and support for the European Union. Based on this literature, the end doesn't seem to justify the means as political repression seems to be either ineffective or even counterproductive regarding its political goals

Most of the existing research examining the influence of political repression on electoral outcomes primarily focuses on the repercussions of large-scale repression under communist regimes. Such repression tended to be extensive and frequently targeted entire geographical regions or ethnic groups. Could the effects on victims' political opinions differ when political repression is less indiscriminate and more targeted? To explore this query, the 1851 political repression in Southeastern France can provide valuable insights. Following Napoléon III's coup, Republicans initiated an uprising and faced severe repression. According to sources from [Margadant \(1979\)](#) and [Devos \(1992\)](#), 21,000 individuals were sentenced, with 9,530 of them deported,

including 239 to the infamous *Bagne* in French Guiana. This case also offers a convenient setting to address the main challenge in estimating the impact of political repression, namely, the non-random nature of repression events. In this case, since Savoy and Nice, two regions with populations similar in characteristics were not in France for exogenous reasons, the difference among the frontier separating these regions from France can be used to quantify the repression’s impact on electoral outcomes. In other words, the main distinction between these two groups lies in the fact that Savoy and Nice avoided repression due to external factors, despite the similarities with their neighbors who remained in France. This enables an assessment of the impact of repression on electoral outcomes using variations in results along the border.

The remainder of this introduction will provide a more detailed overview of the three articles comprising this thesis.

0.1 Chinese Transport Infrastructure Projects and Firms’ Export Probability

The first chapter explores the effects of China-funded transport infrastructure on firm-level development, specifically examining its potential influence on the export activities of companies located in developing nations. I evaluate how Chinese overseas economic investments in roads, railways, ports, and airports affect firms’ export probability (*i.e.* the extensive margin) by relying on an instrumental strategy that leverages the political factors underlying China’s aid distribution. In fact, based on the findings of Mueller (2022), the Chinese government would use its aid efforts as a means to address labor and social unrest within its own borders.

Transport projects financed by China play a key role in recent infrastructure construction in the developing world. According to the 2018 report from the Infrastructure Consortium for Africa, China’s investment in African infrastructure over the past decade has surpassed the combined contributions of all G8 nations¹. The construction of transport infrastructure also represents a substantial share of China’s overseas intervention, with almost a third of the entire projects financed by China falling within this category². China’s approach to development assistance differs notably from that of traditional donors and multilateral institutions. In contrast, they are more inclined to embrace a large-scale approach, channeling funds into substantial projects (Brautigam, 2011), which is particularly striking

¹Source: <https://www.icafrica.org/en/topics-programmes/2018-overview/>

²Source: AidData

when it comes to transport infrastructure. Given China’s unique approach to providing development assistance for transport infrastructure and its reduced level of conditionality compared to DAC bilateral donors, leaders in developing nations are increasingly inclined to view China as their preferred development partner for the construction of transportation infrastructure, as highlighted by [Horigoshi et al. \(2022\)](#).

Building on China’s growing prevalence as a provider of transportation infrastructure in developing countries, I employ an empirical evaluation of Chinese-financed transport projects on firm-level development, with a particular emphasis on firms’ export probability. This focus is both motivated by the intuitions of export-led growth models ([Adelman, 1984](#)) as well as the composition of the study’s sample, which comprises firms located in developing countries. Indeed, studies by [Minot and Goletti \(1998\)](#) and [Bas \(2012\)](#) have indicated that a growing number of exporting firms can foster economic development, notably by easing the adoption of skill-biased technology and increasing household income. Therefore, investigating whether Chinese transportation projects enhance the likelihood of firms in developing countries to engage in export is motivated by economic development considerations.

I evaluate the impact of Chinese-financed transport infrastructure on firms’ export probability by relying on two data sources: the World Bank Enterprise Survey for firm-level variables and AidData’s Geocoded Global Chinese Official Finance Dataset for Chinese transport projects. The first source provides both panel and cross-section data on various firm-level information for companies located in developing countries, including the firms’ ADM1 region, its sector, and whether the firm engaged in exports during a given year. The second data source provides detailed information about projects funded by China, including their sector categorization, precise location, and the year when these projects were completed. Both data precise dimensions enable the exploration of firm, sector, and spatial heterogeneity.

After matching these two data sources, I assess whether the implementation of a Chinese-funded transport infrastructure project in a given region³ leads to a higher export probability for firms located in that region, as compared to firms in regions where no such projects were undertaken. Assessing this potential impact of Chinese-funded transport infrastructure presents several empirical challenges, as

³Defined as ADM1 region.

the relation between the two main variables may be subject to endogeneity. In order to tackle these biases, I employ a shift-share instrument developed by [Mueller \(2022\)](#) which draws upon the domestic political determinants of Chinese aid. More precisely, I use the interaction between the regions' probability of receiving Chinese transport infrastructure and the number of labor unrest in China to create an exogenous source of variation for the number of transport projects received by a given region. The instrument's intuition relies on the fact that Chinese aid projects exclusively involve Chinese companies, in the sense that only Chinese companies are going to construct transport infrastructure funded by China. Awarding large construction contracts to domestic companies could potentially lead to enhanced working conditions and increased employment, thereby contributing to the resolution of labor-related disputes.

The first-stage results suggest that such a relation is verified since regions favored by China received significantly more transport projects during years when the Chinese government needed to calm labor unrest. The reliability of this finding is verified through a series of robustness checks, which encompass sample dependence and interactions with usual aid confounders. Continuing this instrumental strategy, second-stage results suggest that, on average, Chinese-funded transport infrastructure does not appear to have a significant impact on firms' export probability. This lack of significant results is surprising as compared to the overall positive developmental effect of Chinese aid identified in the literature ([Bluhm et al., 2018](#); [Baniya et al., 2020](#); [Martorano et al., 2020](#); [Dreher et al., 2021b](#)). It is also puzzling when compared to [Marchesi et al. \(2021\)](#) findings, which suggest that Chinese aid projects (across all sectors) have an average positive impact on firms' sales, particularly when they lack transport infrastructure.

A series of robustness checks confirm that the lack of statistically significant results does not stem from sample dependence or model misspecification. Similar results are found when using alternative outcomes, such as whether firms identify transportation as an obstacle to their activity, their sales figures, and the amount exported. The lack of significant findings is further explored, notably by exploiting China's intervention heterogeneity in terms of financial flow and transport project categories. Results are not different whether the projects' funding is considered as ODA-like (*i.e.* if the loan has a grant element of under 25%) or classified as Other Official Finance. The construction of roads, rails, ports, or airports does not exhibit a distinct effect on firms' export probability. Additional heterogeneity at the

region, sector, and firm level is addressed. Surprisingly, transport projects have no particular impact on the probability of exporting for firms operating in transport-intensive sectors, *i.e.* firms that rely structurally on transportation and would typically be expected to benefit the most from additional transport infrastructure. Conversely, when it comes to regions with relatively low population density, Chinese transport projects appear to increase firms' export probability. The latter seems intuitive, as sparsely populated regions are typically presumed to have limited market accessibility and pre-existing transportation infrastructure. Firms situated in such regions are therefore expected to derive greater advantages from these transport projects.

In summary, relying on a two-stage instrumentation strategy, Chinese-funded transport infrastructure seems to have no significant impact on firms' export probability. Theoretically, this absence of significant results could be attributed to insufficient additional investment in infrastructure, as suggested by [Bougheas et al. \(1999\)](#), to the absence of trade reforms supporting this infrastructure improvement, as denoted by [Baniya et al. \(2020\)](#), or to suboptimal placement of transport networks, as proposed by [Graff \(2019\)](#). However, Chinese transport projects seem to have a positive effect on firms' extensive margins when undertaken in regions characterized by low population density.

0.2 Foreign Aid and Power Play: Political Cycle in World Bank's Procurement Allocation

The second chapter, co-authored with Lisa Chauvet and Marin Ferry, investigates the existence of a political cycle in the World Bank's allocation of procurement contracts. Specifically, we investigate whether companies secure larger procurement contracts from the World Bank during election periods, which could suggest the existence of a certain form of clientelism.

Previous literature established that multilateral aid allocation is less driven by donor interests ([Maizels and Nissanke, 1984](#); [Neumayer, 2003](#)). Yet a growing literature body is investigating the political-driven distortions for international development organizations. For instance, the allocation of multilateral aid is impacted by major bilateral donors' interests like the United States ([Kilby, 2013](#); [Kersting and Kilby, 2016, 2021](#)), recipient tend to receive more multilateral aid when they hold a seat on the United Nations Security Council ([Kuziemko and Werker, 2006](#); [Dreher et al., 2009b,a](#); [Kersting and Kilby, 2019](#)) or have representation on the institution's executive board ([Dreher et al., 2019](#); [Kaja and Werker, 2010](#)).

Multilateral aid allocation can also follow recipient countries' political interests (McLean, 2017), and could even be partly captured by the recipient's elite (Andersen et al., 2022). A major contributor to official multilateral development aid, namely the World Bank, allocates a significant portion of its assistance through procurement. This process entails the recipient government selecting the company responsible for executing the project. To be more specific, after the World Bank approves funding for a project in a particular country, the government of that country has to select the supplying company using one of the allocation methods outlined by the World Bank⁴. The aid allocation through procurement became more important over the years, as it empowers the aid recipient and consequently aligns with the ownership principles articulated in aid effectiveness forums.

However, there is an established body of research indicating that public procurement can be influenced by political interests fulfilled by public-private agreements, thus encouraging cronyism. More precisely, companies that contributed to the victorious candidates observed an augmentation in the worth of their procurement contracts, thereby establishing procurement contracts as a central component of a kickback arrangement. (Daniele and Bennedsen, 2010; Goldman et al., 2013; Titl and Geys, 2019; Schoenherr, 2019; Baltrunaite, 2020). This article aims to link the literature on political determinants for multilateral aid and electoral cycles in procurement. In fact, given the substantial amount of multilateral aid provided by the World Bank, its predominant reliance on procurement procedures for aid allocation, and the importance of developed countries, where cronyism in procurement can be prevalent, within this institution, it raises the question of whether similar patterns are observed in World Bank procurement contracts and if the average contract amount varies around elections in the supplier and the recipient country. As emphasized by Dreher et al. (2018a) and Lehne et al. (2018), the potential cycle in procurement contract allocation could be an important concern since it could hinder the effectiveness and efficiency of the World Bank interventions.

We distinguish two distinct political cycle processes whether the election occurs in the recipient country (i.e. the country that receives a World Bank project) or in the supplier country (i.e. the country of origin of the foreign supplying firm). In the first situation, the recipient government is approaching an upcoming election and is striving for re-election. To achieve this objective, the incumbent government may decide to grant larger procurement contracts to domestic companies. This

⁴The main allocation procedures are the International Competitive Bid, Quality and Cost Based Selection, Single Source Selection, and National Competitive Bid

would impulse the image of an active government promoting economic growth among domestic firms and generating job opportunities. Domestic companies might also provide financial support for the electoral campaign in exchange for receiving World Bank procurement contracts. We refer to this first case as the domestic political cycle. In the second case, the supplier government is also approaching an upcoming election with the same electoral objective. Knowing that the recipient country is responsible for allocating World Bank contracts, the supplier government may employ diplomatic or economic leverage on the recipient government in order to tilt the allocation process toward a resident firm coming from its country. The awarded foreign firm might ensure job creation in its home country or offer financial support for the incumbent's electoral campaign in return for this favor. This particular situation is referred to as the cross-border political cycle. The main difference between the domestic and cross-border political cycles is the supplier-to-recipient influence at work in the second configuration.

In order to test these two hypotheses, we use the information taken from the World Bank's Contract Awards Database, a highly detailed database on major contracts awarded between 1993 and 2019. It contains information on the name of the supplying firm, its country of origin, the precise date of contract signature, contract amount (in US\$), recipient country, contract category, and allocation method. This dataset is then matched with the National Elections across Democracy and Autocracy database, which contains the precise election dates of elections along with highly detailed information about the election's context.

Thanks to an econometric model estimated with Poisson Pseudo Maximum Likelihood estimators and a fine-grained set of fixed effects, our findings indicate that domestic firms win significantly larger contracts around election semesters in the recipient country. This not only supports the findings of [McLean \(2017\)](#) but also extends them by suggesting that the domestic preference appears to align with an electoral cycle. Alongside the domestic political cycle, we also observe confirmation of the cross-border political cycle, as foreign firms win significantly larger contracts before the election semester in their country of origin.

The existence of both cycles is ensured by a series of robustness checks, addressing potential sample selection, endogeneity in the electoral calendar, and alternative specifications. In order to investigate whether these domestic and cross-border political cycles might indicate cronyism in the allocation of World Bank contracts, we explore further the elections' heterogeneity. Both cycles occur only when firms

are legally allowed to make donations to candidates in their respective countries, when elections are competitive, when unemployment figures are increasing, and when the incumbent government is seeking re-election. We then examine the influence exerted by suppliers on recipients in the cross-border political cycle context. Foreign firms win significantly larger contracts around elections only if the firms' country of origin is a significant aid partner for the recipient country, hence highlighting the importance of economic ties in the supplier-to-recipient influence. The intensity of this cross-border political cycle increases if the foreign firm's home country reduces its tied aid, indicating that this cycle may be emerging as a tool for developed countries to retrieve their contributions to multilateral development institutions. The cross-border political cycle is also more pronounced when there is a shared colonial history between the supplier and recipient countries, underscoring the importance of historical connections in the influence exerted from supplier to recipient.

In summary, we found the existence of domestic and cross-border political cycles in the allocation of World Bank contracts, as both domestic and foreign firms win significantly larger procurement contracts around election semesters in their respective countries of origin. Heterogeneity analyses suggest strong hints of cronyism since these cycles occur especially when incumbent governments need to boost their image and when legal conditions facilitate kickback arrangements.

0.3 The Political Legacy of 19th Century Politicization and Repression in Southeastern France

This third chapter aims to assess how politicization and political repression have influenced electoral results, notably by exploiting the natural experiment along the 1815-1860 border separating the Savoy Duchy and the Nice County from France.

Political repression has a wide range of harmful consequences, as it erodes the mental well-being of those targeted and their families, (Munczek and Tuber, 1998; Sales et al., 2000; Stammel et al., 2013), and as it can spoil economic development through lowered trust (Nunn, 2009; Pronkina et al., 2023; Lichter et al., 2021; Booth et al., 2022; Nikolova et al., 2022). Despite its dramatic consequences, political repression continues to be employed as a strategy, and could even increase in the coming years, as suggested by indicators measuring its prevalence. As indicated in the Political Terror Scale Index, political repression has deteriorated in low-income

countries in recent decades. The situation may become even more concerning when we take into account the norm-transmission effect from autocratic aid providers to the recipients (Gehring et al., 2022; Isaksson and Kotsadam, 2018a), materialized by an increase in government repression and corruption around Chinese aid projects. This concerning trend is not limited to developing countries and could also be a matter of concern for developed nations. In fact, Freedom House’s data index, which assesses press freedom (a specific instance of political repression), has shown a decline in recent years across all income categories.

Despite all its disastrous consequences, the utilization of political repression as a tactic can be explained by its central objective: to counter political opposition and retain power by any means necessary. It is worth considering whether political repression effectively achieves its core objective, and the long-run perspective offered by a historical natural experiment seems appropriate to conduct such an evaluation. In 1815, the regions of Savoy and Nice located in southeastern France were returned to the Kingdom of Piedmont-Sardinia during the Congress of Vienna, aiming to dismantle the recently defeated French Empire. In 1860, these two regions underwent another exogenous change in their political affiliation, as the King of Piedmont-Sardinia gifted them to Napoleon III after the Second French Empire provided assistance to Italian armies during a conflict against Austria. From 1815 to 1860, the southeastern regions of France (i.e. excluding Savoy and Nice) saw a politicization process characterized by the emergence of Republican secret societies, which are the ideological ancestors of contemporary left-wing movements. The southeastern region of France also witnessed one of the most violent episodes of political repression in French history, which occurred in the aftermath of the uprising against Napoleon III’s coup in 1851. In total, According to the archives work of Margadant (1979) and Devos (1992), 26,884 people were arrested. 21,000 of them were sentenced, of which 9,530 were deported to penitentiary establishments in Algeria or French Guiana. In the meantime, populations in Savoy and Nice went through a different politicization process, less influenced by Republican ideologies, and were spared from instances of political repression.

Considering the exogenous switch of Savoy and Nice during a crucial period in the formation of modern political ideologies, I exploit the differences in terms of political opinions along the 1815-1860 border. More precisely, I conduct a spatial regression discontinuity model that compares electoral outcomes for communes located within a 15km band along the border. The treatment group comprises

communes that remained within France between 1815 and 1860, thus experiencing Republican politicization and political repression. The population residing within the 15km area exhibits homogeneous geographic features and current socio-economic characteristics. Ex-ante analysis of political opinions before 1815 also indicates that both treatment and control populations shared similar political opinions before the treatment, measured with social conflicts data collected by [Chambru and Maneuvrier-Hervieu \(2022\)](#).

Through data collection from election records in the French National Archives, results from the 1871 legislative election (i.e. the first free election after Savoy and Nice returns) are retrieved at the commune level. The voting patterns on the French side of the border showed a significant preference for radical Republican candidates, indicating an influence of Republican politicization efforts undertaken in these regions.

The long-term legacies of politicization and political repression are then explored with recent election results at the commune level for presidential and legislative elections held between 1995 and 2022, obtained from the ministry of interior's data. Left-wing candidates, which are the ideological heirs of 19th-century Republicans, still display significantly higher results in municipalities located on the French side of the border. The role of political repression is then investigated thanks to hand-collected data from the French National archives. More precisely, information from the book of [Devos \(1992\)](#) was retrieved and enabled a highly comprehensive database on 1851 repression in Southeastern France. This first-hand original data comprises details about individuals who applied for a pension due to their experiences of repression in 1851, as part of the National Reparation Law of 1881. To be more specific, it encompasses information on the name of the person who was repressed, the place and date of birth, as well as the municipality where the person was in 1851, and his or her occupation. The number of repressed individuals by commune is added to the model and reveals that repression had a significantly diminishing effect on left-wing preference. However, it's important to note that repression's impact wasn't substantial enough to completely overturn the initial left-wing preference. This result suggests the relatively limited effectiveness of the political repression strategy, as it was unable to counteract the initial effects of politicization.

Repressed political dynasties, forced emigration, and a relatively unmixed population could explain the persistent effects of politicization and political repression over the decades. Indeed, municipalities where a mayor shared his or

her name with a repressed citizen within a 20km radius exhibit a significantly lower preference for the left. Similar results were found when including the number of repressed citizens who left the commune due to forced transportation or exile. It also appears that discontinuity results observed in contemporary election outcomes are influenced by the treated and control communes situated in distinct employment zones and educational academies. This finding implies that enduring effects persist when the population does not mix at work and during higher education.

In summary, a political supply with familial memories of the repression, as well as a mechanic reduction of the number of Republican activists that emigrated and a relatively unmixed population can explain the persistent effect of politicization and political repression on electoral outcomes. This research adds to the body of literature that explores the influence of political repression on electoral results, by considering another context than Soviet regimes or communist China (Kapelko and Markevich, 2014; Lupu and Peisakhin, 2017; Rozenas et al., 2017; Zhukov and Talibova, 2018; Wang, 2021). It also provides a broader temporal perspective, since the events considered here date back over a century, whereas most studies in this field have focused on events from the mid-20th century. Additionally, it introduces a novel and detailed individual-level database, enhancing the granularity of the analysis.

Chapter 1

Chinese Transport Infrastructure Projects and Firms' Export Probability

This article is an augmented version of A. Boucher (2022): Connective infrastructure and firms' export: China vs. the World Bank. Different approaches, different results? *Revue d'Economie du Développement*.

1.1 Introduction

In 2001, the China Communications Construction Company started to build a deep water port in the city of Gwadar in Pakistan. This Chinese-funded project is a section of the China-Pakistan Economic Corridor, one of the major components of the Belt and Road Initiative. The port's construction was finalized in 2007 and is anticipated to substantially decrease freight time and transportation costs between Pakistan and the rest of the world. Projects of this nature represent a substantial share of China's overseas economic intervention and may have multiple consequences on development outcomes, notably on the market access and international trade performance of firms situated in regions influenced by these constructions. One could therefore question the impact of Chinese-funded transport infrastructure¹ on firms' development in recipient countries, and more precisely the effect of Chinese transport projects on enterprises' probability to export (*i.e.* selling their products or services in another country).

The focus on transport infrastructure, rather than other aid project categories, is made because the impact of such projects on firms' export activity (both the extensive and intensive margins²) is straightforward and largely documented in the literature. [Bougheas et al. \(1999\)](#) were the first to model the theoretical impact of infrastructure on trade. The creation of infrastructure would decrease the "iceberg" variable cost for transport, as defined by [Samuelson \(1954\)](#) and [Krugman \(1991\)](#), which ultimately leads to an increase in traded volumes (*i.e.* the intensive margin). They however precise that additional infrastructure must be large enough in order to preserve positive marginal effects. The construction of transport infrastructure can also expand the probability of exporting (*i.e.* the extensive margin). In a [Melitz \(2003\)](#) framework, a reduction of the variable cost would increase the expected profit from entering the export market, making it easier to cover the fixed sunk costs of exporting and ultimately fostering firms' export probability. The impact of infrastructure on both the intensive and the extensive margin has been demonstrated empirically. First, [Limao and Venables \(2001\)](#), [Portugal-Perez and Wilson \(2012\)](#), [Coşar and Demir \(2016\)](#), [Donaldson \(2018\)](#) and [Martincus and Blyde \(2013\)](#) highlighted empirical evidence (at respectively country, sub-national region, district, and firm levels) of transport infrastructure's impact on both trade

¹In this study, transport projects or transport infrastructure corresponds to the construction of roads, rail, ports, or airports.

²At the firm level, the extensive margin is defined as the firm participation to the export market (whether the firm exports part of its production or not), while the intensive margin reflects the volumes exported.

costs and volume exported in a quite substantial magnitude. Then, several empirical studies (Djankov et al., 2010a; Coşar and Demir, 2016; Albarran et al., 2013) have investigated the influence of infrastructure on the likelihood of exporting at different levels – country, region, and firm. Their findings suggest that improved transport infrastructure contributes to an increase in the probability of exporting.

This paper focuses on the transport infrastructure developed by China for several reasons. First, China emerged as a major provider of transport infrastructure to developing countries during the 2000s, as reported in the ICA annual report of 2018 and by Bluhm et al. (2018). Second, a significant portion of China’s foreign economic intervention involves the construction of transport infrastructure. This component constitutes nearly a third of China’s overseas economic involvement³ (Official Development Assistance + Other Official Flows) and accounts for 24.6% of China’s total ODA flow between 2000 and 2014 (Dreher et al., 2022a). Third, China is willing to finance substantial and expensive projects at a time when multilateral and Western donors are less enthusiastic about this large-scale approach (Brautigam, 2011; Swedlund, 2017; Gehring et al., 2022; Boucher, 2022). This divergence in approach can be particularly noticeable in the context of transportation projects. Lastly, according to Horigoshi et al. (2022), African leaders show a preference for China as a development partner when it comes to the construction of transport infrastructure.

Considering export probability as our primary firm-level outcome is also motivated by the context of our study, which focuses only on firms in developing countries. In line with export-led growth models (Adelman, 1984), an increasing number of exporting firms would foster economic development. According to the literature, exporting firms indeed facilitate the adoption of skilled-biased foreign technology (Bas, 2012), sustain employment in case of recession (Das et al., 2007), increase households’ real income, and reduce the severity of poverty (Minot and Goletti, 1998). Therefore, investigating whether Chinese transport projects enhance the likelihood of firms in developing countries to engage in export is motivated by economic development considerations.

Using the World Bank Enterprise Survey⁴, combined with AidData’s Geocoded Global Chinese Official Finance Dataset (Version 1.1.1)⁵ (Bluhm et al., 2018; Dreher

³Source: AidData

⁴Source: Enterprise Surveys, The World Bank, <http://www.enterprisesurveys.org>

⁵AidData Research and Evaluation Unit, (2017), Geocoding Methodology, <https://www.aiddata.org/data/geocoded-chinese-global-official-finance-dataset>

et al., 2022a), I investigate the impact of China-financed transport infrastructure on firms' probability to export. Specifically, I assess whether the presence of Chinese transport infrastructure in a firm's region leads to a higher export probability compared to firms located in regions that did not receive similar projects.

However, concerns regarding endogeneity may emerge from various sources, which can introduce estimation biases and potentially lead to misinterpretations. To address and mitigate these biases, and based on Mueller (2022) findings, I employ a shift-share instrumental variable strategy that relies on an interaction between the regions' probability of receiving Chinese-funded transport projects (calculated as the ratio of years with at least one transport project to the total number of years in the sample) and instances of labor unrest in China. Considering that Chinese aid projects exclusively involve Chinese companies, it is plausible to suggest that China utilizes its aid as a means to alleviate labor unrest. By awarding large aid contracts to domestic companies, this approach could potentially lead to enhanced working conditions and increased employment, thereby contributing to the resolution of labor-related disputes. The mechanism explored by Mueller (2022) can provide an exogenous source of variation in order to explain the construction of Chinese transport projects.

I find that, on average, Chinese transport projects do not have a significant impact on firms' probability to export. This lack of effect could potentially be attributed to insufficient additional investment in infrastructure, as underlined in the Bougheas et al. (1999) framework, the absence of trade reforms supporting this infrastructure improvement, as denoted by Baniya et al. (2020), or suboptimal placement of transport networks, as suggested by Graff (2019). However, further heterogeneity analyses suggest that Chinese transport projects have a positive impact on the extensive margins for firms in regions with relatively low population density.

This research contributes to two literature strands: the effects of Chinese aid and the influence of infrastructure on firms' export activities.

For the first one, the developmental impact of Chinese aid has been recently investigated at the country (Baniya et al., 2020; Dreher et al., 2021b), sub-national region (Bluhm et al., 2018; Gehring et al., 2022), local (Isaksson and Kotsadam, 2018a,b; Guo and Jiang, 2020), and household levels (Martorano et al., 2020; Perrot, 2022). Chinese foreign economic intervention seems to have an overall positive impact on development, as it would enhance growth measured by nightlight, boost

country-level trade, reduce within-country inequality, diminish the occurrence of conflict, generate jobs, and improve household welfare. However, it is worth noting that this intervention may also foster local corruption, which could impede growth enhancement (Méon and Sekkat, 2005), lead to an increase in government repression, and discourage participation in trade unions. This globally positive impact is also confirmed by Mandon and Woldemichael (2023)’s meta-regression analysis.

However, firm-level development impact has been under-studied and this paper aims to complete the literature. To the best of my knowledge, the study conducted by Marchesi et al. (2021) is the only existing analysis that seeks to assess the impact of Chinese aid at the firm level. Their study examines the effects across all sectors of Chinese and World Bank aid projects, thereby capturing the impacts of various mechanisms, exploiting the regional-sectoral heterogeneity, and exploring the impact on firms’ sales growth. In contrast, the present analysis specifically zooms in on the realm of Chinese transport infrastructure, consequently focusing on the mechanism presented by Bougheas et al. (1999), and examining the impact on firms’ export probability. Drawing on the findings of Dreher et al. (2021b) and Marchesi et al. (2021), it appears that World Bank aid, unlike China’s assistance, does not exhibit a significant impact on growth or firm performance. As a result, I chose not to compare Chinese transport projects with those funded by the World Bank, but to focus solely on the former.

I also contribute to the Chinese aid literature by testing the instrument proposed by Mueller (2022) at the subnational level. Mueller (2022) found that China, as so-called traditional donors, appears to allocate aid based on local political motives. More precisely, they tend to provide more aid in years when there is increased labor unrest within China. Since China directs its aid exclusively to firms from its own country, the Chinese government’s aid contracts with these firms are used to address social tensions by creating jobs and improving working conditions. In Mueller (2022)’s approach, this finding is employed as an instrument to gauge Chinese aid allocation in recipient countries. The instrument involves an interaction between local labor unrest shocks in a particular Chinese prefecture and the probability of a recipient country receiving Chinese aid projects contracted by a company coming from that same prefecture. However, in this study, I use the instrument in a slightly different manner. Instead of focusing on the prefecture of origin of the contracting Chinese firm, I aggregate the number of labor unrest incidents in China at the yearly level.

For the second one, this paper contributes to the literature on infrastructure and trade by conducting a more granular analysis. Most of the studies in this domain are conducted at the country or sub-national level (Limao and Venables, 2001; Portugal-Perez and Wilson, 2012; Coşar and Demir, 2016; Donaldson, 2018). To the best of my knowledge, only Albarran et al. (2013) and Martincus and Blyde (2013) examine the effect of infrastructure on trade at the firm level. They however both focus on firms coming from one country (respectively Spain and Chile), as this analysis would encompass a wider and more heterogeneous sample. The lack of a significant impact observed in this study regarding Chinese transport projects may serve as further empirical evidence supporting the predictions of the Bougheas et al. (1999) model. In fact, this model suggests that without reaching a certain threshold of infrastructure stock, the construction of new infrastructure may not effectively reduce trade costs⁶.

The paper is organized as follows: Section 2 introduces the data. Section 3 discusses the empirical strategy used in order to estimate the effect of Chinese transport infrastructure projects on firms' export probability. Section 4 presents the main results and robustness checks. Section 5 explores heterogeneity at the project, region, sector, and firm levels. Section 6 concludes.

1.2 Data

1.2.1 Firm level data

In line with previous works investigating the impact of aid on firm-level development (Chauvet and Ehrhart, 2018; Marchesi et al., 2021; Bomprezzi and Marchesi, 2023), this paper relies on the World Bank Enterprise Survey. This database provides various information for a sample of firms representative of an economy's manufacturing and services sector. Data were retrieved through repeated face-to-face interviews with business owners thanks to a standardized questionnaire, which allowed for the creation of a detailed dataset that includes both firm panels and repeated cross-sections. It contains information such as interviews' year, sub-national region of location⁷, sector (4 digits ISIC code), sales, percentage of sales exported, number of employees, foreign ownership, and other various characteristics.

⁶In their model, infrastructure can have no effect on variable trade costs if $\lambda * D < k$, with λ as the total amount of input allocated to infrastructure development, D an index of geographic factors and k the importance of the infrastructure projects

⁷Most of the cases, the sub-national region available is at the first administrative level (ADM1), but WBES has sometimes provided aggregated sub-national regions, such as Western Kazakhstan, which corresponds to the current Mangistau, Atyrau, Aktyubin and Zapadno regions (ADM1) of the country.

Overall, there is information for 147,270 firms (20,750 observed in panel data and 126,520 observed in cross-section), located in 133 developing countries, 711 sub-national regions, operating in 30 two-digit ISIC sectors, and interviewed either once or twice between 2001 and 2019. As Table A1 in the appendix presents, this sample includes both exporting and non-exporting firms. More precisely, the dataset includes information on firms categorized into different groups: firms that always exported, never exported, started, or stopped exporting between two interviews. Table A2, which shows the distribution of our sample in terms of sector, and indicates that a large majority of firms perform either in the manufacturing or service sectors (94.8% of the sample). Firms operating in mining or construction represent a minor share of the sample. Table A3 presents the share of exporters by sector.

From this raw data, I constructed a dummy taking the value one if firm f located in the country c , sub-national region r , operating in sector s exported during the year t . The latter is going to be the dependent variable in the main analysis, enabling us to investigate the extensive margin. The intensive margin could be explored thanks to the amount exported, constructed through sales multiplied to the percentage of sales exported. I converted the sales expressed in local currency to 2014 USD using World Bank data on exchange rates and GDP deflator⁸. As a large majority of firms constituting my sample are not exporters (80%), the distribution of the amount exported skewed to the right. I consequently performed an inverse hyperbolic sine transformation on the amount exported, as described by Bellemare and Wichman (2020). But as Table 1.1 shows, this measure needs to be used with caution due to its abnormally high values (even after taking away the top 1% values), its non-negligible share of missing values (14.4%), and the possibility of measurement errors in sales or the proportion of sales exported. Consequently, results for the intensive margin must be interpreted with caution, and it is preferable to focus the main analysis exclusively on the extensive margin.

1.2.2 Aid level data

The variable of interest was constructed thanks to AidData’s Geocoded Global Chinese Official Finance Dataset (Version 1.1.1). Since China is a so-called non-traditional donor, they do not participate in the global reporting systems and do not provide extensive official information about their aid. But Custer et al. (2021) provided a Tracking Underreported Financial Flows (TUFF) methodology

⁸While being careful if the amount of sales reported corresponds to the year t , $t-1$, or $t-2$ in order to apply the correct exchange rate. Check the page <https://www.enterprisesurveys.org/en/survey-datasets> and download the sample description for more information.

that enables the collection of information on projects financed by China, resulting in the creation of this database. More precisely, AidData relies on publicly available information provided by the Chinese government, which may not encompass the complete range of Chinese aid activities. Hence, there is no guarantee that this dataset is representative of Chinese overseas interventions. However, to the best of my knowledge, AidData remains the most complete and geographically precise data source on this subject. The latter reports the development projects' precise location, flow type, amount committed in 2014 USD, and replicates OECD-DAC aid sector classification. Since this study focuses on the potential effects of transport infrastructure, only Transport and Storage (210) projects were kept.

Since the most precise level of firm location is the ADM1 region, it was not possible to retain the geolocation information of aid projects, which prevented the use of more refined analyses such as spatial regression discontinuity design. Consequently, the ADM1 region where the infrastructure projects were implemented became the spatial dimension of the aid dataset. Overall, there is information on 244 Chinese projects, allocated toward 68 countries, 236 regions, and completed between 2000 and 2019.

As the estimated amount for Chinese projects is only available for commitment and may occasionally be missing, the main variable of interest is the number of Chinese transport infrastructure projects conducted in country c , region r completed during the last four years (*i.e.* between t and $t-4$). Taking into account this time gap instead of using the number of projects completed during year t seems appropriate for the firm-level dataset, mainly because transport infrastructure projects are expected to have lasting effects, and because the WBES interviews are not repeated every year⁹. Since there are on average four years between two WBES waves, this variable of interest can therefore be interpreted as the number of completed Chinese transport infrastructure projects in the region since the last WBES interview.

Having information on completed infrastructure projects over the last four years in a given ADM1 region, the aid dataset was then merged with the World Bank Enterprise Survey at the region year level.

Table 1.1 below presents some descriptive statistics for the main variables. Tables A1, A4, and A5 in the appendix present more precise descriptions of firms' export status, region treatment status regarding Chinese projects, and Chinese transport project categories. Maps A1, A2, and A3 in the appendix show the ADM1 region

⁹A transport project may have been completed in a given region but may not be finished the same year as a WBES survey.

observed in the WBES, the number of firm-level observations per country, and the locations of Chinese transport infrastructure projects.

Table 1.1: Descriptive Statistics - Main Variables

	Observations	Mean	Std. Dev.	Min	Max
Exporter	168,020	0.20	0.40	0	1
Amount Exported	143,812	724,430.6	3,460,204	0	44,391,596
Amount Exported (IHT)	143,812	3.00	5.79	0	18.30
Chinese Aid	168,020	0.12	0.52	0	4

1.3 Empirical Strategy

1.3.1 Main regression

In order to measure the impact of Chinese transport infrastructure on firms' export probability, I resort to a shift-share instrumental variable approach. The equation below displays the second stage of this strategy:

$$Exporter_{f,s,c,r,t} = \alpha + \beta Aid_{c,r,t,t-4} + \gamma X_{c,r,t} + \sigma H_{f,s,c,r,t} + \mu_{s,t} + \phi_{c,t} + \theta_f \text{ or } \theta_r + \varepsilon_{f,s,c,r,t}$$

The variable $Exporter_{f,s,c,r,t}$ captures the extensive margin and is a dummy taking the value one if the firm f , located in the country c , sub-national region r , operating in sector s participates in the export market during year t . As presented in Table 1.1, a significant share of firms do not participate in the export market. Following the work of [Berman and Héricourt \(2010\)](#), the characteristics of the sample of firms that do not export versus those that engage in exporting are presented in Table A6. Exporters are as expected larger, more productive, and more foreign-owned.

The effect of transport infrastructure is captured by the $Aid_{c,r,t}$ variable, which is the number of Chinese transport projects completed during the last four years in country c , sub-national region r , and year t . Ideally, it would be preferable to exploit the heterogeneity in terms of project size, as it is reasonable to assume that larger transport projects could have a more substantial impact on firms' export probability. As the provided amount is an estimation of the commitment rather than the actual disbursement, and since this imperfect information was missing for approximately 10% of Chinese projects, I opted for the number of completed projects as it represents the most reliable variable of interest, and as it still allows to exploit the heterogeneity in terms of the number of infrastructure received.

Concerns may arise regarding the staggered nature of the treatment since these projects are implemented at different times for different regions within the sample. According to Callaway and Sant’Anna (2021), such settings can complicate the analysis because the treatment effect may vary over time and across different regions, potentially resulting in estimations with an opposite sign to the Average Treatment Effect on Treated (ATT). To address this, I have chosen to focus my analysis on firms’ first and last observations (in the case of firms observed in panel data), hence excluding the intermediate periods. The attrition caused by this solution should not be of great concern, as the observations between the first and last ones account for only 1.6% of the sample. It is reasonable to assume that the first and last observations of firms should be representative and that excluding the intermediate periods is unlikely to introduce bias into the estimates. Robustness checks will be conducted to examine the results when including the intermediate observations.

Another potential issue in this model may arise due to the presence of endogeneity. Numerous factors can simultaneously explain 1) a firm’s f participation in the export market; and 2) the number of Chinese transport infrastructure received in a given region r . There could be time-varying economic or political shocks at the country level, such as the emergence of a conflict, natural disaster (Martincus and Blyde, 2013), ending the recognition of Taiwan (Dreher et al., 2018b), welcoming Chinese foreign officials (Lavallée and Lochard, 2022), or even hosting the Dalai-Lama (Fuchs and Klann, 2013). Time-varying sector factors, such as the dynamics within specific industries, can also potentially influence both firms’ decisions to export and donors’ motivations to invest (Dreher et al., 2018b; Hochman et al., 2013); and even time-invariant region characteristics, such as one region’s remoteness or distance to the coast (Moore, 2018). These biases can however be contained thanks to the inclusion of fixed effects.

Tackling first the country and sector time variant heterogeneity, and following the empirical strategy of Berman and Héricourt (2010), a set of country-year and sector-year fixed effects $\phi_{c,t}$ and $\mu_{s,t}$ are included to the specification, which allows controlling for factors impacting similarly firms operating within the same country and year, or the same sector and time group.

The fixed effect θ_f aims to control for time-invariant firm characteristics that may explain a different level of performance in the export market. As only 1.31% of the firms in the sample changed their region of establishment between waves, the latter were excluded from the analysis. By adding this restriction, the variability of regions

across enterprises is eliminated, and the firm fixed effect controls for time-invariant region-specific characteristics. The inclusion of this fixed effect however restrains the sample to a panel dimension, since firms observed only once are absorbed. Only firms that have been interviewed at least twice are retained in this specification. Consequently, in order to preserve the information provided by the cross-section dimension of this dataset, this specification will be complemented by a second one with a region fixed effect θ_r instead of the firm one.

By including this set of fixed effects, the reverse causality bias is relatively addressed, as it is difficult to imagine the time-varying characteristics of a given firm influencing aid allocation at the regional level. But this specification still remains subject to endogeneity threats, such as omitted variable issues. Indeed, there may be time-varying regional factors that are correlated with both the dependent variable and the variable of interest. For instance, the discovery of a mineral deposit may impact both the region's aid received and the firms' international business environment. Lastly, since the number of Chinese projects is not officially reported but estimated, the main independent variable may not fully capture the Chinese foreign intervention, consequently making the specification sensitive to measurement errors.

Given the inability to add a time-varying region fixed effect (as it would absorb the variable of interest), I attempt to reduce these biases as much as possible by including controls in the specification. First, time-varying region controls $X_{c,r,t}$ are included. They intend to grasp one region's ability to attract Chinese infrastructure projects, and the potential regional economic dynamic explaining a different proportion of exporting firms. They encompass the GDP estimated by night light data (\log)¹⁰, and the estimated population (\log)¹¹. Second, time-varying firm controls $H_{f,s,c,r,t}$ are also included. They aim to explain a given firm's participation in the export market during a given year. Following the findings of the firm-level trade literature (Idson and Oi, 1999; Melitz, 2003; Bernard and Jensen, 2004), firm foreign ownership and size are therefore included. They are respectively a dummy equal to one if the firm is owned by a foreigner, and a categorical value equal to one, two, or three if the firm has less than 20, between 20 and 100, or more than

¹⁰30 arcs second DMSP-VIIRS stable nightlight data from 2001 to 2013 (source: <https://www.ngdc.noaa.gov/eog/dmsp/downloadV4composites.html>), and Li et al. (2020) harmonized nightlight for years post-2013

¹¹Center for International Earth Science Information Network - CIESIN - Columbia University. 2018. Gridded Population of the World, Version 4 (GPWv4): Population Density, Revision 11. Palisades, NY: NASA Socioeconomic Data and Applications Center (SEDAC). <https://doi.org/10.7927/H49C6VHW>. Accessed 10/12/2020.

100 employees. Table A7 displays the descriptive statistics of all variables included in the main specification.

Weights provided by the WBES are applied in the specification so the sample is representative at the country and sector level.

Finally, to address potential heteroskedasticity within regions and over time, standard errors are clustered at the region-year level. This clustering allows for the correction of potential correlation within those groups in the error term. As underlined by Moulton (1990), standard errors should indeed be clustered at the variable of interest’s level in case one attempts to measure the impact of an aggregated shock on a smaller unit.

Including this supplementary set of controls can partially account for time-varying region factors that may influence both firms’ export probability and the allocation of transport infrastructure projects. However, as endogeneity concerns may persist, the safest option is to instrument the number of transport projects funded by China. The following subsection will outline the instrumentation strategy.

1.3.2 Instrumenting Chinese transport infrastructure projects

The equation presented below illustrates the first stage regression, which aims to estimate the construction of Chinese transport infrastructure:

$$Aid_{CHN,c,r,t,t-4} = \beta ProbaCHN_r * LaborUnrest_{t-3} + \gamma X_{c,r,t} + \sigma H_{f,s,c,r,t} + \mu_{c,s,t} + \theta_f + \varepsilon_{f,s,c,r,t}$$

The number of Chinese transport infrastructure completed during the last four years in a given country c , sub-national region r , and year t is estimated thanks to a Bartik instrument (Goldsmith-Pinkham et al., 2020). The latter is an interaction between the region r ’s probability to receive transport projects from China; measured as the number of years when the region receives at least one transport project from China divided by the total number of years in the sample; and the number of labor unrest events, such as strikes or workers protest, that occurred in China in a given year t . A three-year lag is applied since what matters here is the number of labor unrest incidents in the year preceding the project’s decision, which occurs on average two years before its completion.

The intuition of this instrument relies on the following findings of Mueller (2022): As Chinese aid projects are carried out exclusively by domestic companies, the

Chinese government uses its aid to stimulate employment and improve working conditions in the country, helping to calm labor-related social tensions in China. Like [Mueller \(2022\)](#), the number of labor unrest was retrieved from two unofficial sources, *China Strikes Crowdmap* and the *China Labor Bulletin*¹². This shift dimension is multiplied by the share dimension that is commonly employed in the existing literature ([Dreher et al. \(2021b\)](#)), *i.e.* the regions' probability to receive Chinese transport projects. In other words, regions favored by China would receive a higher number of transport projects when China seeks to temper labor unrest within its own borders. For example, in 2012, employees of the Jiuha Aluminum Corporation in Foshan staged a protest demanding the return of their social security funds¹³. This protest, possibly along with other similar events, might have influenced the Chinese government's decision to sign contracts the following year, such as the Addis Ababa-Djibouti railway project signed in 2013. Such contracts would boost the order books of companies like Jiuha Aluminum Corporation, hence leading to potential improvements in working conditions and a reduction in social tensions.

There could be doubts regarding the satisfaction of the exclusion restriction with the current instruments. First, the probability of receiving Chinese-financed transport infrastructure could be correlated with unobserved regional characteristics that directly influence firms' export probability. The inclusion of either firm and region fixed effects¹⁴ should take into account the impact of one region's time-invariant characteristics on the outcome variable. Second, labor unrest in China may be correlated with periods of global economic recession, thereby explaining both the emergence of labor-related social conflicts within China and firms' export probability in developing countries. However, by including country-year fixed effects, the impact of global events is captured.

As [Dreher et al. \(2021b\)](#) mentioned, this strategy is comparable to a difference-in-difference approach. The occurrence of a shock, in this case, the emergence of labor unrest in China, is compared between two groups: treated regions with a probability greater than zero; and untreated regions, with a probability equal to zero. One must ensure that treated and control regions had similar trends in the share of exporters before the start of the treatment. Figure [A4](#) presents the variation of labor unrest in China, the variation in the number of Chinese transport projects, and the share

¹²Available at: <https://chinastrikes.crowdmap.com/> and <https://clb.org.hk/en>

¹³<https://chinastrikes.crowdmap.com/reports/view/815>

¹⁴The firm fixed effect being equal to a region one since moving firms are dismissed

of exporters¹⁵ for the two groups. The evolution of the exporter variable before the treatment (*i.e.* prior 2005) gives little reason to believe that treated and control regions followed non-parallel trajectories.

In summary, potential endogeneity concerns are mitigated with an instrumentation strategy based on Chinese local political motives. The effect of labor unrest in China is compared between treated and control regions, hence providing an exogenous source of variation for Chinese transport projects.

Table 1.2: Instruments Statistics

	Observations	Mean	Std. Dev.	Min	Max
Proba CHN	168,020	0.03	0.07	0	0.53
Labor Unrest (log)	168,020	3.94	2.38	0	7.94
Proba CHN x Labor Unrest	168,020	0.14	0.35	0	2.91

1.4 Results

1.4.1 Baseline results

The following section displays the results of the specifications exposed above. Table 1.3 presents the estimated impact of Chinese transport infrastructure on firms' probability to export.

Results presented in columns (1) and (2) display the estimation outcomes when the model includes a firm fixed effect (*i.e.* with panel data only, hence explaining the smaller number of observations); and columns (3) and (4) demonstrate the results obtained when the model incorporates a region fixed effect (*i.e.* with both panel and cross-section data). Columns (1) and (3) display results when running simple Ordinary Least Square regressions, as columns (2) and (4) present the estimations when instrumenting the variable of interest. Columns (a) and (b) below columns (2) and (4) report the first stages.

Focusing first on columns (1) and (3), Chinese transport infrastructure seem to have no significant impact on firms' extensive margin. However, as specified in the previous section, these estimations may be influenced by endogeneity biases.

In columns (a) and (b), representing the first stages, the instrument appears to be a significant and substantial predictor of Chinese transport infrastructure. The robustness of these instruments is reflected in the Kleibergen-Paap F-statistics (Kleibergen, 2007), which exceed the threshold of 15¹⁶. The *ProbaCHN* *

¹⁵Three years moving average since WBES interviews are not conducted every year.

¹⁶The 1st stage F-test had the same values.

Table 1.3: Chinese Transport Infrastructure on Firms' Export Probability

Second Stages: Dep. Var.:	(1)	(2)	(3)	(4)
	Firm FE		Region FE	
<i>Exporter</i> _{<i>f,s,c,r,t</i>}	No IV	IV	No IV	IV
Aid _{<i>c,r,t</i>}	-0.088 (0.056)	-0.096 (0.080)	-0.039 (0.021)	-0.036 (0.045)
Population (log) _{<i>c,r,t</i>}	0.049 (0.055)	0.050 (0.056)	0.012 (0.025)	0.012 (0.026)
GDP (log) _{<i>c,r,t</i>}	-0.035 (0.042)	-0.037 (0.046)	-0.007 (0.016)	-0.006 (0.017)
Foreign Owned _{<i>f,s,c,r,t</i>}	-0.012 (0.043)	-0.012 (0.043)	0.146 (0.018)***	0.146 (0.018)***
Size _{<i>f,s,c,r,t</i>}	0.083 (0.021)***	0.083 (0.021)***	0.099 (0.006)***	0.099 (0.006)***
<i>N</i>	37,516	37,516	151,577	151,577
<i>R</i> ²	0.85	0.01	0.23	0.04
Country x Year FE	Yes	Yes	Yes	Yes
Sector x Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	No	No
Region FE	No	No	Yes	Yes
Weights	Yes	Yes	Yes	Yes
N region year (clusters)	1,324	1,324	1,595	1,595
Kleibergen-Paap LM stat (p-value)		0.000		0.000
Kleibergen-Paap F-stat		27.29		73.46
First Stages: Dep. Var:	Firm FE		Region FE	
<i>Aid</i> _{<i>c,r,t</i>}	(a)		(b)	
Proba CHN x Labor Unrest _{<i>c,r,t</i>}	2.389 (0.457)***		1.654 (0.193)***	
<i>R</i> ²	0.97		0.97	
Controls	Yes		Yes	
Country x Year FE	Yes		Yes	
Sector x Year FE	Yes		Yes	
Firm FE	Yes		No	
Region FE	Yes		No	
Weights	Yes		Yes	

Standard errors in parentheses, clustered at the region-year level.

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

$LaborUnrest_{c,r,t-3}$ coefficient exhibits the expected sign: the recipient regions favored by China would receive significantly more Chinese transport infrastructure during the years when there were relatively more labor-related social conflicts in China. On average, as displayed by Figure A5, regions relatively favored by China (*i.e.* the top quartile in terms of $ProbaCHN_r$) would receive from 0.4 to 4.9 additional transport projects financed by China in years when China experiences an average number of social conflicts ¹⁷.

Focusing on columns (2) and (4) displaying the second stages, Chinese transport infrastructure seem to have overall no significant effect on firms' extensive margins. Relying on Bougheas et al. (1999) framework, Chinese investment in transport infrastructure would not be sufficient in order to reduce transport costs and foster firms' export probability. It is worth noting that the coefficients are quite similar between columns (1) and (2), as well as between columns (3) and (4), indicating that the potential endogeneity bias may be relatively minor. Yet, the absence of significant and positive outcomes might still raise questions.

To investigate this absence of impact, one can examine whether Chinese transport projects have any influence on transportation as a declared obstacle for operations, sales, and sales growth. These variables would logically be affected first before potentially resulting in an increase in export market participation. Table A8 presents results with alternative dependent variables: transport obstacle, which is a categorical value ranging from 0 (if the firm's owner stated that transport is not an obstacle to their operations) to 4 (if it is considered a very severe obstacle to their business); sales (log)¹⁸; and sales growth (inverse hyperbolic sine transformation)¹⁹. Similar to results in Table 1.3, Chinese transport projects would have no impact on transport as an obstacle to operations, or sales. The last columns even suggest that Chinese projects would have a negative impact on firms' sales growth. But this result must be taken with great care since the attrition due to missings in sales growth is not negligible (almost a third of the sample).

Following the absence of significant impact, one can check whether Chinese transport projects have an effect on other variables that could capture an improvement in international market access. Tables A9, A10, and A11 present

¹⁷As the mean number of transport projects funded by China is 0.12, favored regions would receive 3 to 41 times more transport infrastructure projects from China in years when China experiences an average number of social conflicts.

¹⁸With the top 1% being removed as they reported abnormally high values.

¹⁹Constructed as the difference between sales and sales three years ago. The top 1% fastest-growing firms were removed.

results when using alternative dependent variables capturing other measures of trade activity, respectively the amount exported (i.e. the intensive margin), a dummy variable taking the value one if the firm imports foreign input, and the percentage of imported inputs. Alternative measures of trade activities seem to indicate that Chinese transport infrastructure do not seem to impulse firms' export or import. Chinese projects (in the region fixed effect specification of Tables A9 and A11) would actually have a negative and significant effect on the amount exported and the amount of foreign input used, which is difficult to explain based on the Bougheas et al. (1999) framework and is not in line with Baniya et al. (2020) country-level findings. Once again, these results must be interpreted with great caution since the sample is affected by the non-negligible share of missing values in the dependent variables.

1.4.2 Robustness checks

Before conducting heterogeneity analyses, it is essential to verify the validity of the instruments and rule out the possibility that the lack of significant results is due to sample dependence, confounding factors, or misspecification.

First stage

One may legitimately worry that instruments' quality could be entirely driven by a small pool of countries. Robustness checks are consequently required for the first stages. First, Figures A6 and A7 exhibit the first stages when countries are dropped one by one. The instruments' coefficient always remains significant at a 5% level. Second, Following the approach of Burnside and Dollar (2000), Figure A8 presents the instrument's coefficient variation when dropping each country-year pair one by one in the first stage. Once again, there is not a particular country-year outlying combination that entirely explains the instruments' quality²⁰.

Dubious readers may still worry about the external validity of the instruments, hence raising concerns about the genuine efficiency of these first stages. To address this concern, several tests are conducted. First, a test of plausible exogeneity, as defined by Conley et al. (2012) and Van Kippersluis and Rietveld (2018), is performed. The latter consists of the following: first-stage regressions are run country by country. The sample is then restrained to countries for which the instrument is not a significant determinant of Chinese transport projects. In other

²⁰The two observations around -1 in Figure A8 represent the changes in coefficients when Turkey is excluded from the sample. While these observations may initially appear problematic, it is worth noting that even when excluding these firms, the k-Paap statistics remain at a high level.

words, it is a sample for which there is no correlation between the instrument and the instrumented variable. A regression with the main dependent variable is computed with both the instrument and the instrumented variable on the right-hand side. As Table A12 exhibits, $ProbaCHN * LaborUnrest$ is non-significant on a sub-sample where the instrument is not correlated to *Aid* (i.e. a subset where the results should not be driven by collinearity).

In addition to this test of plausible exogeneity, Table A13 presents results when time-varying country factors that may act as confounders between transport projects and firms' exports are added to the controls. More precisely, received remittances, net FDI inflows, total imports, total exports, aid received from the World Bank, and trade with China at the country level are added; since these variables could be correlated to Chinese transport projects and could explain firms' export probability. Because the country-year fixed effect would absorb them, the potential confounding factors are interacted with the share dimension of the instrument (i.e. $ProbaCHN_r$). It seems that adding these potential confounders does not change the coefficients or the quality of the instrument.

The absence of significance in the plausible exogeneity test and the stability of the coefficients when confounders are added suggests that the instrument does not explain directly or indirectly the dependent variable. It can therefore be considered plausibly exogenous.

Second stage

Similar to what was done for the first stage of the specification, one should ensure that the results of the second stage are not influenced by sample dependence or misspecification.

Addressing first the potential sample dependence, one concern with the WBES is the potential survivor bias. In fact, the survey can only recontact firms that survived, which can potentially affect the results. Such a phenomenon would be an issue if firms' age distribution between panel and cross-section (i.e. observed at least twice or only once) is significantly different. Figure A9 shows that the two distributions are very similar. Another possible issue with the WBES is the important variation in the number of firms across countries (as illustrated in Figure A2), leading to some regions being over-represented in the sample. To mitigate this bias, and as other studies using the same firm data (Chauvet and Ehrhart, 2018; Marchesi et al., 2021; Bomprezzi and Marchesi, 2023), I conduct a randomization of the sample. 50 or 250

firms²¹ by region were randomly picked, thus giving equal weight for each region. This process is repeated 500 times. The coefficients reported in Table A14 do not vary much compared to those in Table 1.3. More than 90% of the time, the *Aid* coefficient is not significant. Additionally, it is worth noting that the instrument’s quality remains satisfactory. Another concern regarding sample dependence resides in the influence of post-conflict countries, since the latter capture an important part of aid and are rapidly growing. Table A15 shows the results when excluding these countries from the sample²². The coefficient remains relatively stable.

Addressing now the potential misspecification concern, it is important to verify that 1) the current model is not too sensitive to modifications; and 2) that alternative specifications yield similar results. First, Table A16 exhibits similar results when employing alternative lags for the variable of interest and the instrument. Specifically, a more conventional two-year lag is used for the *Aid* variable (similar to Dreher et al. (2021b) and Marchesi et al. (2021) studies), and the mean labor unrest in China between year $t - 3$ and $t - 7$ is considered (representing the timing of project decisions for projects implemented between years t and $t - 4$). Then, Table A17 displays similar results when using alternative measures of aid, respectively a dummy taking the value one if a given region received at least one project in the last four years and the estimated amount of commitment received on the last four years (inverse hyperbolic sine transformation). Finally, Results remain consistent when incorporating the intermediate observations (Table A18), when adding firms that changed regions (Table A19), and they are not influenced by bad controls (Table A20). Results presented in Table A21 indicate a significant negative impact when adding additional controls (such as state ownership and sales from three years ago). However, this result should be cautiously interpreted because the presence of missing variables in sales leads to a substantial reduction in the sample size.

Tackling then alternative strategies, one may wonder whether the results differ when employing standard instruments from the Chinese aid literature, such as $ProbaCHN * Steel_{c,r,t-3}$, $ProbaCHN * Factor_{c,r,t-3}$, and $ProbaCHN * Reserves_{c,r,t-3}$, Table A22 indicates either the same absence of effects or a negative effect entirely driven by one country²³. Additionally, Table A23 presents an absence of significant results when employing an alternative approach employed by Isaksson and Kotsadam (2018a) and Isaksson and Kotsadam (2018b), which involves

²¹For respectively the firm and region fixed effect analyses

²²Afghanistan, Bangladesh, Burundi, Colombia, Nigeria, Philippines, Sri Lanka, and Uganda

²³significant results and the strength of the instruments are driven by Turkey.

comparing treated observations to yet-to-be-treated observations in order to address selection bias.

In summary, Chinese transport projects would have in overall no impact on firms' export probability. Instruments seem to be robust and exogenous, and 2nd stage results are robust to modification and consistent across methods.

1.5 Heterogeneity analysis

Chinese transport projects are diverse in terms of financial flow and categories; as much as enterprises composing the sample are widely heterogeneous in terms of region, sectors, and individual characteristics. One needs to investigate whether the effect differs in terms of projects, region, sector, and firm characteristics.

1.5.1 Chinese transport projects heterogeneity

Since China is not part of the Development Assistance Committee (DAC) donor group, the financial flow rules of the OECD do not apply to its foreign economic interventions. Consequently, the OECD's guideline regarding the grant element, which requires a loan to have a grant element of under 25% to be considered concessional enough for aid, does not apply to Chinese economic overseas interventions. Therefore, it is necessary to differentiate between transport projects that are considered ODA-like (with a grant element under 25%) and those classified as Other Official Finance (OOF, where the grant element exceeds 25%). This distinction is important because ODA-like projects may be more development-oriented and potentially offer greater benefits to local firms. Conversely, since OOF-like projects are funded through commercial loans, China is providing more substantial funding per project under this flow type (Dreher et al., 2022a). These larger projects could offer greater advantages to local firms. Table 1.4 presents results with either the number of ODA-like or OOF transport projects. Both flow types have no significant impact on firms' export probability.

It is also worth considering, as shown in Table A5, whether different infrastructure categories within transport projects have distinct impacts. Table 1.5 shows results where the variable of interest denotes either the number of roads, rail, or port and airport constructed within a given region during the last four years. None of these types of projects appear to have a different impact on firms' export probability.

Table 1.4: Chinese Transport Infrastructure on Firms' Export Probability - By type of financial flow

Dep. Var.:	Firm FE		Region FE	
	ODA	OOF	ODA	OOF
<i>Exporter</i> _{<i>f,s,c,r,t</i>}				
Aid _{<i>c,r,t</i>}	-0.367 (0.342)	-0.131 (0.107)	-0.116 (0.145)	-0.051 (0.067)
<i>N</i>	37,516	37,516	151,577	151,577
<i>R</i> ²	0.00	0.01	0.04	0.04
Country x Year FE	Yes	Yes	Yes	Yes
Sector x Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	No	No
Region FE	No	No	Yes	Yes
Weights	Yes	Yes	Yes	Yes
N region year (clusters)	1,324	1,324	1,595	1,595
Kleibergen-Paap LM stat (p-value)	0.000	0.000	0.000	0.000
Kleibergen-Paap F-stat	10.29	14.32	13.72	39.97

Standard errors in parentheses, clustered at the region-year level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table 1.5: Chinese Transport Infrastructure on Firms' Export Probability - By category

Dep. Var.:	Firm FE			Region FE		
	Road	Rail	Port/airport	Road	Rail	Port/airport
<i>Exporter</i> _{<i>f,s,c,r,t</i>}						
Aid _{<i>c,r,t</i>}	-0.567 (0.535)	-0.158 (0.129)	-3.011 (3.501)	-0.149 (0.190)	-0.068 (0.089)	-2.157 (2.902)
<i>N</i>	37,516	37,516	37,516	151,577	151,577	150,870
<i>R</i> ²	0.02	0.01	0.08	0.04	0.04	0.03
Country x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Sector x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	No	No	No
Region FE	No	No	No	Yes	Yes	Yes
Weights	Yes	Yes	Yes	Yes	Yes	Yes
N region year (clusters)	1,324	1,324	1,324	1,595	1,595	1,595
Kleibergen-Paap LM stat (p-value)	0.000	0.000	0.050	0.000	0.000	0.000
Kleibergen-Paap F-stat	7.76	9.41	1.77	13.51	22.49	3.36

Standard errors in parentheses, clustered at the region-year level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

1.5.2 Regions' population density

The impact of Chinese infrastructure may differ regarding the initial characteristics of the recipient regions, notably in terms of existing infrastructure. Firms located in regions with relatively scarce transport networks may indeed benefit more from the construction of transport infrastructure. Therefore, the heterogeneous impact of Chinese projects can be estimated thanks to an interaction with initial population density as a proxy for existing infrastructure. One can safely assume that relatively uninhabited and rural regions will consequently lack infrastructure. The region's initial population density $Density_r$ is constructed thanks to the estimated population (Gridded population of the world); and with the regions' area in square kilometers, computed thanks to the ADM1 shapefiles (GADM). Table 1.6 below presents the results when interacting $Aid_{c,r,t}$ and the instrument with $Density_r$:

Table 1.6: Chinese Transport Infrastructure on Firms' Export Probability - Interaction with Regions' Population Density

Dep. Var.: $Exporter_{f,s,c,r,t}$	(1) Firm FE	(2) Region FE
$Aid_{c,r,t}$	-0.197 (0.149)	0.056 (0.070)
$Aid_{c,r,t} \times Density_r$	0.017 (0.015)	-0.016 (0.007)**
N	37,516	151,577
R^2	0.01	0.04
Country x Year FE	Yes	Yes
Sector x Year FE	Yes	Yes
Firm FE	Yes	No
Region FE	No	Yes
Weights	Yes	Yes
Controls	Yes	Yes
N region year (clusters)	1,324	1,595
Kleibergen-Paap LM stat (p-value)	0.000	0.000
Kleibergen-Paap F-stat	18.29	38.08

Standard errors in parentheses, clustered at the region-year level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Columns (1) and (2) present the estimated impact of Chinese transport infrastructure on firms' probability to export, with respectively firm and region-fixed effects. Results in column (1) suggest that Chinese projects have no impact on firms' export probability, no matter the region's population density. However,

results are different when applying a region-fixed effect in column (2), since Chinese projects would have a positive impact on firms' export probability if they are located in regions with low population density (i.e. areas with supposedly few transport infrastructure). On average, one additional Chinese transport infrastructure project would increase the probability of exporting from 0.1% to 5% for firms located in the 10% least densely populated regions. This result seems intuitive, as new infrastructure are expected to have a greater impact in areas with the lowest market access.

1.5.3 Sector structural needs in transports

The effect of Chinese transport infrastructure may also differ in terms of the sector's structural needs, more precisely in terms of transport infrastructure requirements. Firms operating in sectors that disproportionately depend on transport should indeed gain the most with the construction of new transport infrastructure. Following the work of [Chauvet and Ferry \(2021\)](#), the sector's intensity in transport is defined as the share of transportation expenses (inland, water, rail, and transportation support activities) over the total intermediate consumption. As stated by [Rajan and Zingales \(1998\)](#), the US economy can be considered a frictionless market, and sectors are expected to exhibit high transport intensity due to structural factors rather than imperfect transport provision. Therefore, the intensity index is derived from the 2014 input-output table for US industries²⁴. This $TransportIntensity_s$ is then multiplied with the $Aid_{c,r,t}$ variable and the instrument. Table 1.7 presents the estimated interactions of these variables²⁵:

This table follows the same organization as the previous one. Surprisingly, Chinese transport infrastructure would have no impact on firms' extensive margin, no matter the sectors' transport dependence. Column (2) even suggests a negative impact of Chinese transport projects over firms' export probability if the latter operates in transport-intensive sectors. However, one should exercise great caution when interpreting this result since coefficients with a 10% significance level in large samples are generally not considered reliable for interpretation.

²⁴From the World Input-Output Database, [Timmer et al. \(2015\)](#).

²⁵the sample size for these estimations is smaller due to the removal of firms that changed sectors in order to reduce noise in the estimation.

Table 1.7: Chinese Transport Infrastructure on Firms' Export Probability - Interaction with Sectors' Intensity in Transport

Dep. Var.: <i>Exporter_{f,s,c,r,t}</i>	(1) Firm FE	(2) Region FE
Aid $_{c,r,t}$	-0.113 (0.111)	-0.024 (0.049)
Aid $_{c,r,t}$ x Transport s	0.024 (0.134)	-0.063 (0.037)*
<i>N</i>	24,302	136,975
<i>R</i> ²	0.01	0.04
Country x Year FE	Yes	Yes
Sector x Year FE	Yes	Yes
Firm FE	Yes	No
Region FE	No	Yes
Weights	Yes	Yes
Controls	Yes	Yes
N region year (clusters)	1,210	1,528
Kleibergen-Paap LM stat (p-value)	0.000	0.000
Kleibergen-Paap F-stat	25.83	33.56

Standard errors in parentheses, clustered at the region-year level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

1.5.4 Firm labor productivity

Chinese projects may also have a differentiated impact on firms' export probability depending on individual characteristics. In a Melitz (2003) framework, non-exporting firms with high productivity (*i.e.* near the productivity cutoff) may indeed see their probability to enter the export market increase, as the infrastructure lead to a reduction in variable costs, hence making the entrance in the export market less costly. Table 1.8 below displays the effect of $Aid_{c,r,t}$ interacted with firms' initial labor productivity $LaborProductivity_{f,s,c,r}$ ²⁶

Table 1.8: Chinese Transport Infrastructure on Firms' Export Probability - Interaction with Firms' Labor Productivity

Dep. Var.: $Exporter_{f,s,c,r,t}$	(1) Firm FE	(2) Region FE
$Aid_{c,r,t}$	-0.097 (0.358)	-0.193 (0.084)**
$Aid_{c,r,t}$ x Productivity f	-0.000 (0.027)	0.011 (0.007)
N	36,414	134,490
R^2	0.01	0.05
Country x Year FE	Yes	Yes
Sector x Year FE	Yes	Yes
Firm FE	Yes	No
Region FE	No	Yes
Weights	Yes	Yes
Controls	Yes	Yes
N region year (clusters)	1,313	1,576
Kleibergen-Paap LM stat (p-value)	0.000	0.000
Kleibergen-Paap F-stat	13.08	40.46

Standard errors in parentheses, clustered at the region-year level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Results in column (2) suggest that Chinese transport projects would increase firms' export probability if they have relatively high labor productivity, which is in line with Melitz (2003) theory. However, this result should be taken with great caution since the export probability would increase for a very small sample of highly productive firms (less than 1% most productive firms).

²⁶Labor productivity is estimated as the sales per employee in 2014 USD (log).

1.6 Conclusion

Using a shift-share instrumental variables strategy, this study suggests that, on average, Chinese projects seem to have no effect on firms' export probability. This result could be theoretically explained by insufficient investment in infrastructure or sub-optimal placement of transport networks. Exploiting the project, region, sector, and firm dimensions of this database, results suggest that Chinese transport infrastructure increases firms' probability to export if companies are located in regions with low population density. Future research should consider utilizing datasets that provide more precise firm information, such as location data or details on transportation spending. This would allow for the investigation of two key aspects: 1) assessing whether Chinese transport infrastructure truly have no impact on firms' trade costs, and 2) examining whether enterprises located in proximity to these projects exhibit no significant effects on their export performances.

Appendix

Table A1: Export Status Statistics

	Never Exports	Always Exports	Starters	Stoppers	Total
N	130,904	29,306	4,164	3,646	168,020
Percent	77.91%	17.44%	2.48%	2.17%	100%
Cumulative %	77.91%	95.35%	97.83%	100%	

Table A2: Sector Distribution Statistics

Sector	Obs.	Percent	Cumulative %
<i>Manufacture</i>	86,839	53.90	53.90
Food	18,294	21.07	21.07
Garments	10,618	12.23	33.30
Fabricated metal products	7,969	9.18	42.48
Metallic and non-metallic mineral products	7,757	8.93	51.41
Chemicals	6,077	7.00	58.41
Textile	5,54	6.38	64.79
Furniture	5,270	6.07	70.86
Machinery equipment	5,106	5.88	76.74
Rubber and plastics	4,795	5.52	82.26
Publishing	3,354	3.86	86.12
Electronics	3,330	3.83	89.95
Wood	2,916	3.36	93.31
Leather	2,036	2.34	95.65
Motor vehicles	1,877	2.16	97.81
Paper	1,147	1.32	99.13
Tobacco	296	0.34	99.47
Refined Petroleum	229	0.27	99.74
Recycling	227	0.26	100
<i>Services</i>	65,893	40.90	94.80
Wholesales	22,479	34.11	34.11
Other services	11,559	17.54	51.66
Retail	10,559	16.02	67.68
Hotel	7,437	11.29	78.97
Transport and communication	6,265	9.51	88.48
Sales of motor vehicles	5,086	7.72	96.19
IT	2,408	3.65	99.85
Energy and water supply	59	0.09	99.94
Financial services	41	0.06	100
<i>Construction</i>	7,917	4.91	99.72
<i>Mining</i>	449	0.28	100
Mining	415	92.43	92.43
Petroleum and gas extraction	34	7.57	100
<i>Total</i>	161,098	100	100

Table A3: Sector Statistics - Share of exporters

Sector	Obs.	Exporter (%)	Non exporter (%)
<i>Manufacture</i>	86,839	28.37	71.63
Food	18,294	22.23	77.77
Garments	10,618	35.68	64.32
Fabricated metal products	7,969	27.36	72.64
Metallic and non-metallic mineral products	7,757	19.67	80.33
Chemicals	6,077	33.47	66.53
Textile	5,54	37.88	62.12
Furniture	5,270	20.34	79.66
Machinery equipment	5,106	36.70	63.30
Rubber and plastics	4,795	30.03	69.97
Publishing	3,354	16.40	83.60
Electronics	3,330	38.14	61.86
Wood	2,916	27.91	72.09
Leather	2,036	39.69	60.31
Motor vehicles	1,877	32.18	67.82
Paper	1,147	26.42	73.58
Tobacco	296	32.43	67.57
Refined Petroleum	229	28.82	71.18
Recycling	227	20.70	79.30
<i>Services</i>	65,893	11.64	88.36
Wholesales	22,479	7.43	92.57
Other services	11,559	10.72	89.28
Retail	10,559	17.30	82.70
Hotel	7,437	9.14	90.86
Transport and communication	6,265	22.09	77.91
Sales of motor vehicles	5,086	7.61	92.39
IT	2,408	20.02	79.98
Energy and water supply	59	5.08	94.92
Financial services	41	0	100
<i>Construction</i>	7,917	6.47	93.53
<i>Mining</i>	449	38.75	61.25
Mining	415	40.72	59.28
Petroleum and gas extraction	34	14.71	85.29
<i>Total</i>	161,098	19.77	80.23

Table A4: Region Treatment Status Statistics

	Never Treated	Always Treated	No to Yes	Yes to No	Total
<i>Chinese Aid</i>					
N	1,554	25	120	17	1,716
Percent	90.56%	1.46%	6.99%	0.99%	100%
Cumulative %	90.56%	92.02%	99.01%	100%	

Table A5: Chinese transport project - category

<i>Categories:</i>	Road, Bridge or Tunnel	Rail	Airport	Port	Other	Total
<i>Number of projects</i>						
N	721	289	29	26	34	1,099
Percent	65.61%	26.3%	2.61%	2.37%	3.09%	100%
Cumulative %	65.61%	91.91%	94.54%	96.91%	100%	
<i>Amount</i>						
Amount (billion USD)	22.35	25.66	3.57	5.11	1.32	58.01
Percent amount	38.52%	44.23%	6.16%	8.80%	2.29%	100%
Cumulative % amount	38.52%	82.75%	88.91%	97.71%	100%	

Chinese aid database before merging to the WBES.

Other projects encompass donation of aircraft, construction of collective transport, donation of road signs, and road maintenance.

Table A6: Export Status Statistics

Variable	Obs.	Mean	Std. Dev.	Q1	Med.	Q3
<i>Continuous Exporters</i>						
State Owned	28,487	0.03	0.18	0	0	0
Foreign Owned	28,955	0.22	0.42	0	0	0
Size	28,973	2.15	0.79	2	2	3
Labor Pdy (log)	28,666	10.22	2.78	9.43	10.59	11.55
<i>Starters</i>						
State Owned	4,161	0.05	0.22	0	0	0
Foreign Owned	4,159	0.15	0.36	0	0	0
Size	4,103	1.86	0.78	1	2	2
Labor Pdy (log)	4,122	10.18	1.89	9.21	10.29	11.30
<i>Continuous Non-Exporters</i>						
State Owned	124,135	0.02	0.13	0	0	0
Foreign Owned	128,032	0.07	0.25	0	0	0
Size	124,744	1.51	0.69	1	1	2
Labor Pdy (log)	106,998	9.73	2.17	8.77	9.86	10.91
<i>Stoppers</i>						
State Owned	3,644	0.03	0.16	0	0	0
Foreign Owned	3,645	0.15	0.36	0	0	0
Size	3,623	1.89	0.78	1	2	3
Labor Pdy (log)	3,204	10.11	2.92	9.29	10.46	11.41
<i>All Observations</i>						
State Owned	160,427	0.02	0.14	0	0	0
Foreign Owned	164,791	0.10	0.30	0	0	0
Size	161,443	1.64	0.75	1	1	2
Labor Pdy (log)	138,758	9.79	2.53	8.85	10.01	11.07

Table A7: Descriptive Statistics - All Variables

Variable	Obs.	Mean	Std. Dev.	Min	Max
<i>Dependent Variables</i>					
Exporter	168,020	0.20	0.40	0	1
Amount Exported (IHT)	168,020	3.00	5.79	0	18.30
Obstacle Transport	159,078	1.21	1.27	0	4
Sales (log)	143,772	12.85	3.1	-19.07	20
Sales growth (IHT)	117,900	0.044	4.22	-5.23	10.95
Importer	168,020	0.18	0.38	0	1
Import (% input)	168,020	18.54	32.40	0	100
<i>Variable of Interest</i>					
Chinese Aid	168,020	0.12	0.52	0	4
<i>Region-Year Controls</i>					
GDP (log)	167,598	1.63	1.60	-6.01	4.25
Population (log)	167,902	15.06	1.52	9.19	19.15
<i>Firm-Year Controls</i>					
Foreign Owned	164,791	0.10	0.30	0	1
Size	161,443	1.64	0.75	1	3

Figure A1: Sub-national regions interviewed in the World Bank enterprise survey

World Bank Enterprise Survey by ADM1

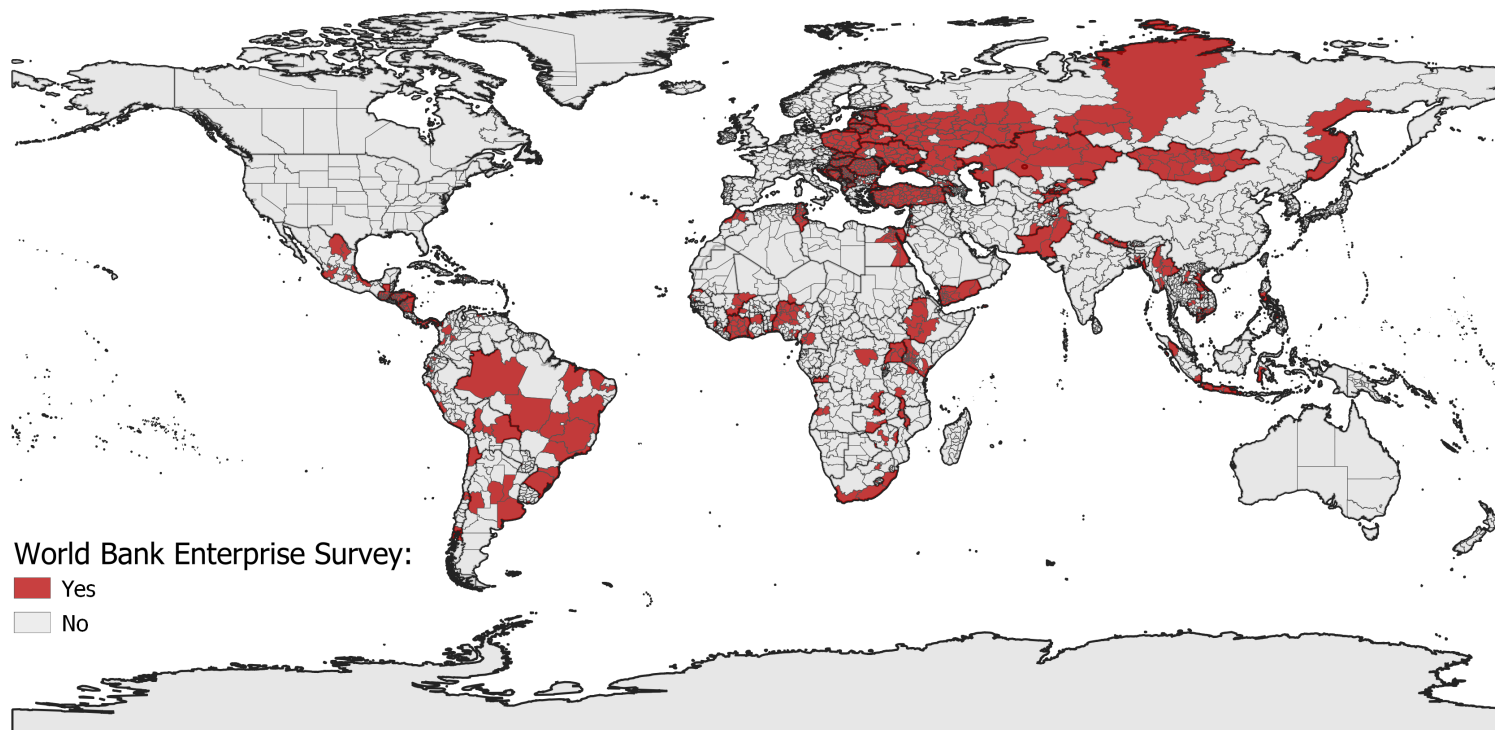


Figure A2: Number of firms observed in the World Bank enterprise survey by country

Number of firms in the World Bank Enterprise Survey by country

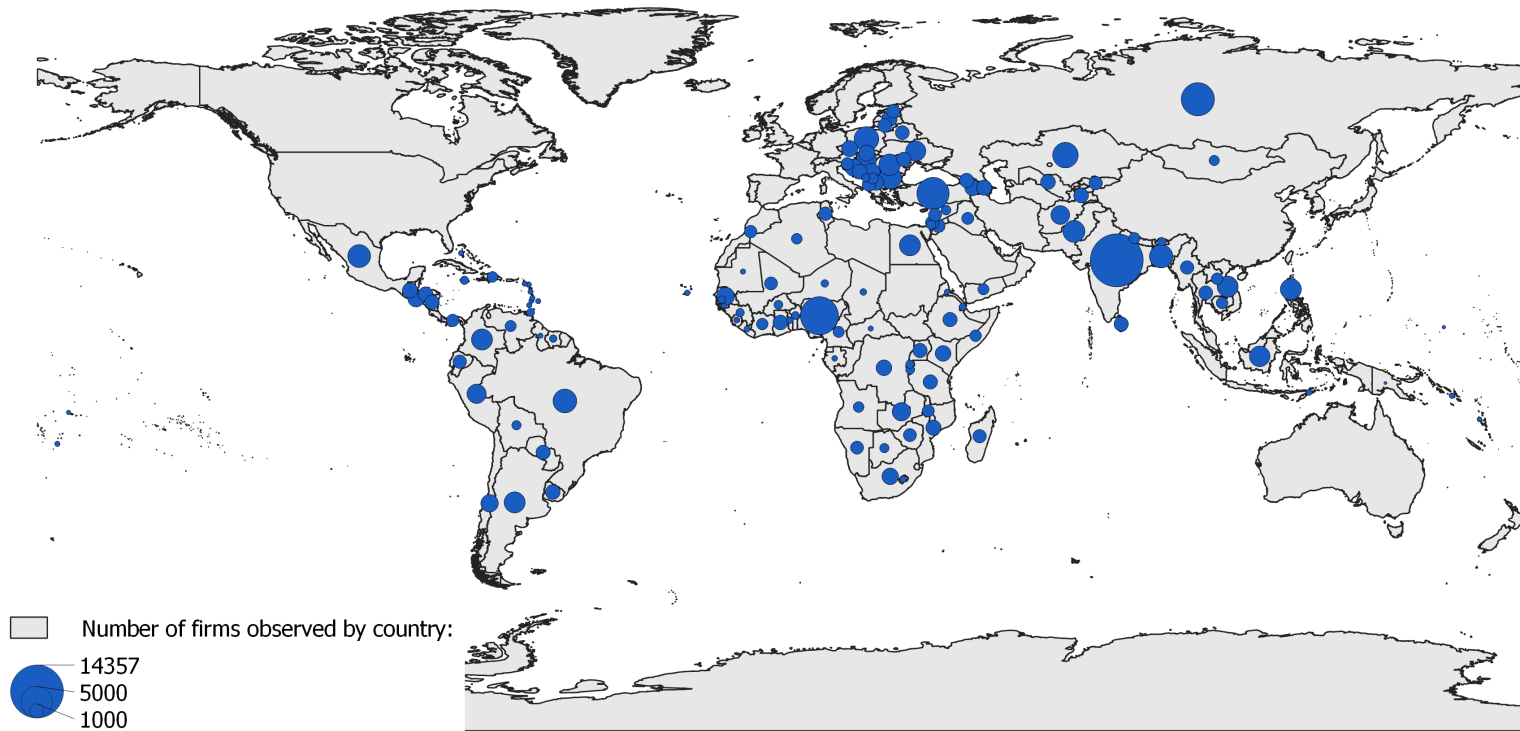


Figure A3: Sub-national regions interviewed in the WBES and Chinese connective infrastructure projects

World Bank Enterprise Survey and Chinese Transport Projects by ADM1

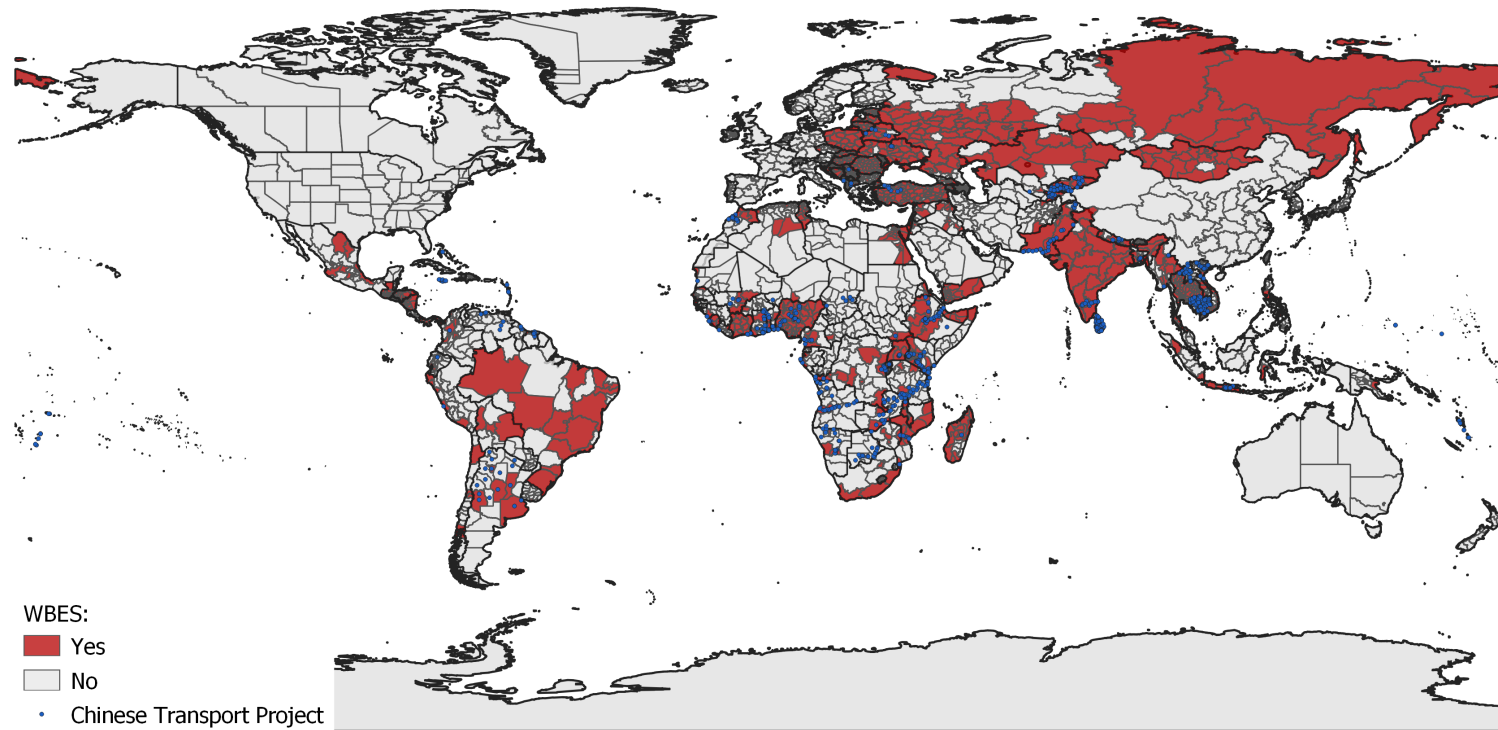


Figure A4: Parallel trends: labor unrest and Chinese transport projects

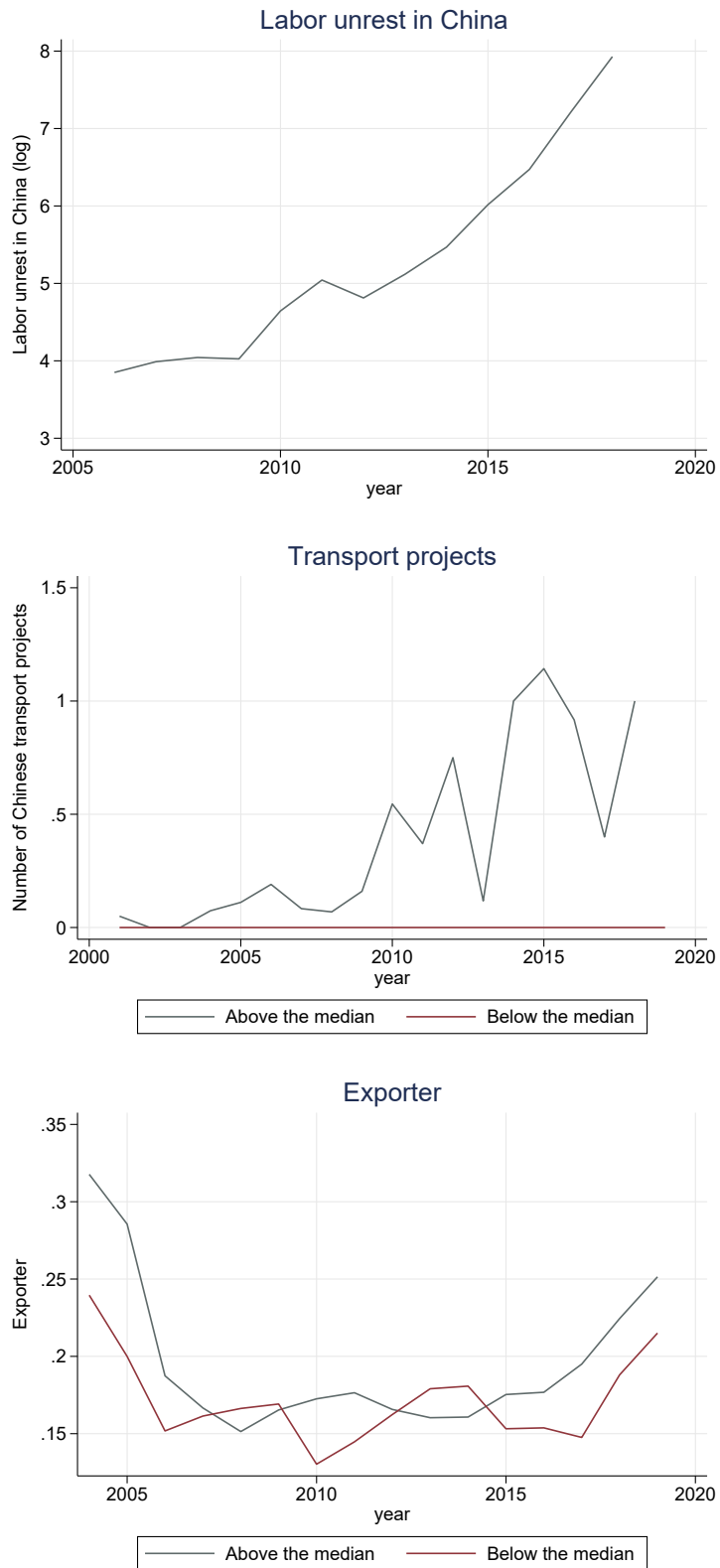


Figure A5: First stage marginal effects

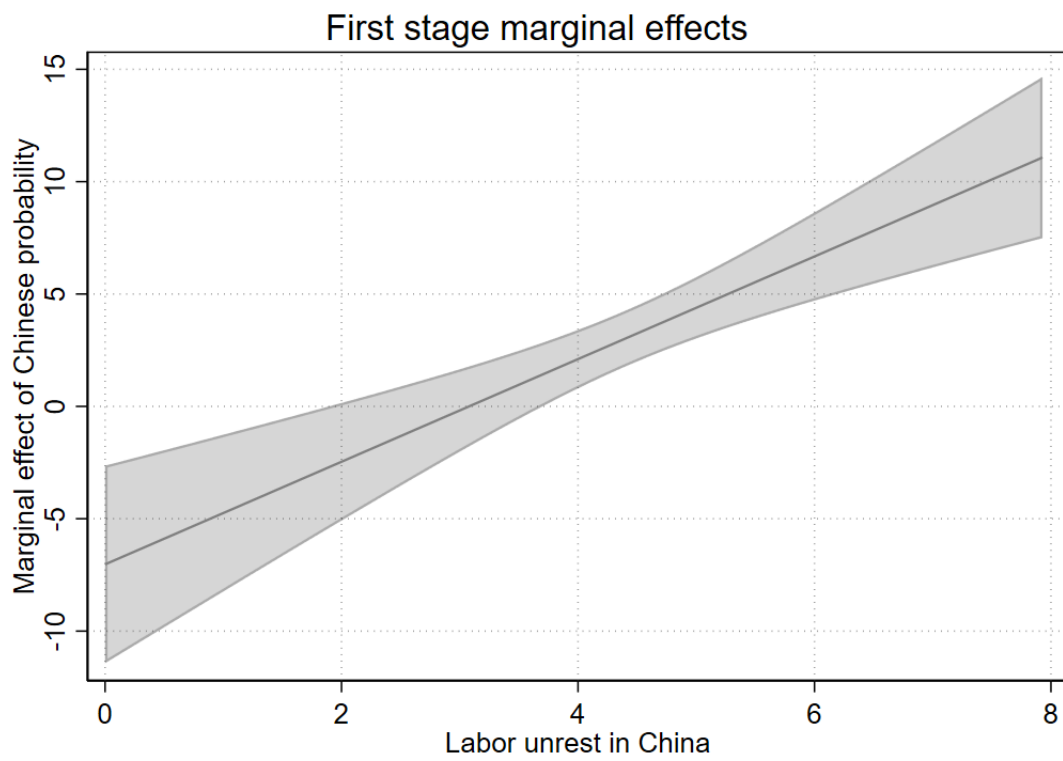


Table A8: Chinese Transport Infrastructure on various Firms' Outcomes

Dep. Var.:	Firm FE			Region FE		
	Transport Obst.	Sales (log)	Sales growth (IHT)	Transport Obst.	Sales (log)	Sales growth (IHT)
$Exporter_{f,s,c,r,t}$						
Aid _{c,r,t}	0.197 (0.271)	-0.180 (0.510)	-0.962 (0.866)	0.074 (0.218)	0.076 (0.244)	-1.712 (0.581)***
<i>N</i>	37,498	29,002	20,308	149,540	130,323	107,388
<i>R</i> ²	0.00	0.09	0.00	0.00	0.29	0.00
Country x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Sector x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	No	No	No
Region FE	No	No	No	Yes	Yes	Yes
Weights	Yes	Yes	Yes	Yes	Yes	Yes
N region year (clusters)	1,324	1,266	1,177	1,590	1,558	1,508
Kleibergen-Paap LM stat (p-value)	0.000	0.000	0.000	0.000	0.000	0.000
Kleibergen-Paap F-stat	27.27	37.49	44.96	75.60	77.82	52.47

Standard errors in parentheses, clustered at the region-year level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A9: Chinese Transport Infrastructure on Firms' Amout Exported

Dep. Var.:	(1)	(2)
$Export_{f,s,c,r,t}$	Firm FE	Region FE
Aid $_{c,r,t}$	-0.638 (1.249)	-1.295 (0.447) ^{***}
N	29,016	130,175
R^2	0.01	0.07
Country x Year FE	Yes	Yes
Sector x Year FE	Yes	Yes
Firm FE	Yes	No
Region FE	No	Yes
Weights	Yes	Yes
Controls	Yes	Yes
N region year (clusters)	1,282	1,571
Kleibergen-Paap LM stat (p-value)	0.000	0.000
Kleibergen-Paap F-stat	37.63	78.39

Standard errors in parentheses, clustered at the region-year level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A10: Chinese Transport Infrastructure on Firms' Import Probability

Dep. Var.:	(1)	(2)
$Importer_{f,s,c,r,t}$	Firm FE	Region FE
Aid $_{c,r,t}$	0.081 (0.062)	-0.014 (0.022)
N	37,516	151,577
R^2	0.00	0.03
Country x Year FE	Yes	Yes
Sector x Year FE	Yes	Yes
Firm FE	Yes	No
Region FE	No	Yes
Weights	Yes	Yes
Controls	Yes	Yes
N region year (clusters)	1,324	1,595
Kleibergen-Paap LM stat (p-value)	0.000	0.000
Kleibergen-Paap F-stat	27.29	73.46

Standard errors in parentheses, clustered at the region-year level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A11: Chinese Transport Infrastructure on Firms' Imported Input (%)

Dep. Var.:	(1)	(2)
$Import_{f,s,c,r,t}$	Firm FE	Region FE
Aid $_{c,r,t}$	-2.077 (5.646)	-6.194 (2.507)**
N	37,516	151,577
R^2	0.00	0.01
Country x Year FE	Yes	Yes
Sector x Year FE	Yes	Yes
Firm FE	Yes	No
Region FE	No	Yes
Weights	Yes	Yes
Controls	Yes	Yes
N region year (clusters)	1,324	1,595
Kleibergen-Paap LM stat (p-value)	0.000	0.000
Kleibergen-Paap F-stat	27.29	73.46

Standard errors in parentheses, clustered at the region-year level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A12: Chinese Projects - Test of Plausible Exogeneity

Dep. Var.:	(1)	(2)
$Exporter_{f,s,c,r,t}$	Firm FE	Region FE
Aid $_{c,r,t}$	-0.169 (0.121)	-0.061 (0.036)*
Proba CHN x Labor Unrest $_{c,r,t}$	0.153 (0.115)	-0.143 (0.099)
N	26,658	119,937
R^2	0.87	0.23
Country x Year FE	Yes	Yes
Sector x Year FE	Yes	Yes
Firm FE	Yes	No
Region FE	No	Yes
Weights	Yes	Yes
Controls	Yes	Yes
N region year (clusters)	1,037	1,271

Standard errors in parentheses, clustered at the region-year level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Figure A6: Robustness check for the first stage of Chinese projects, firm fixed effect

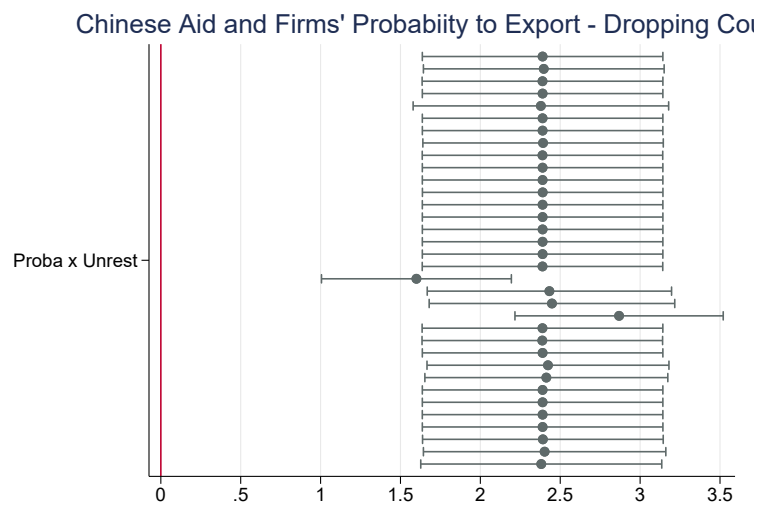
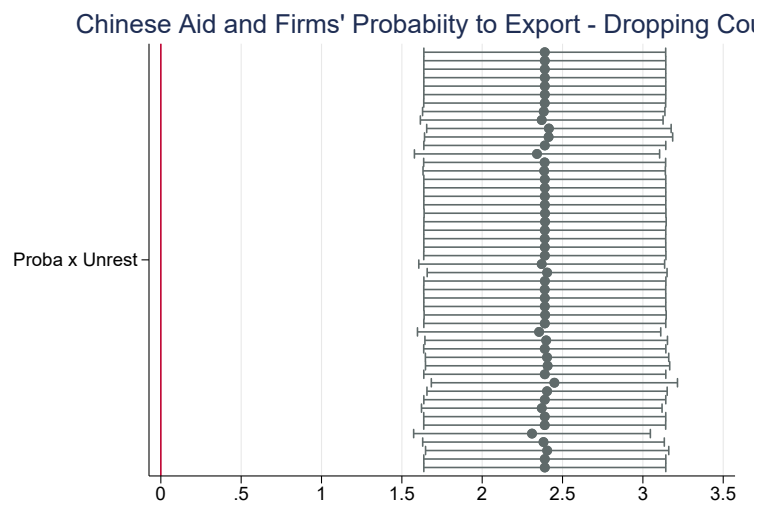
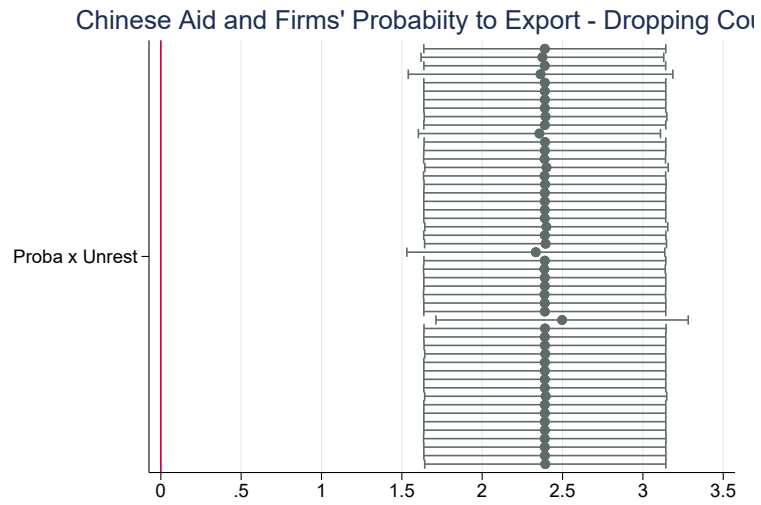


Figure A7: Robustness check for the first stage of Chinese projects, region fixed effect

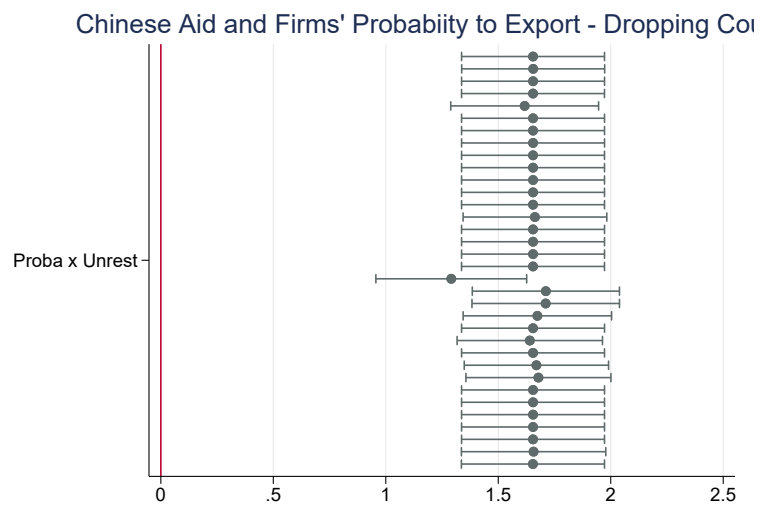
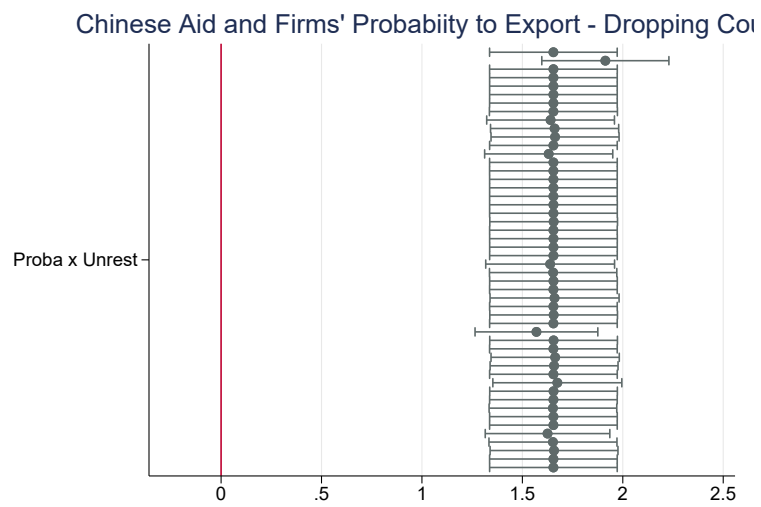
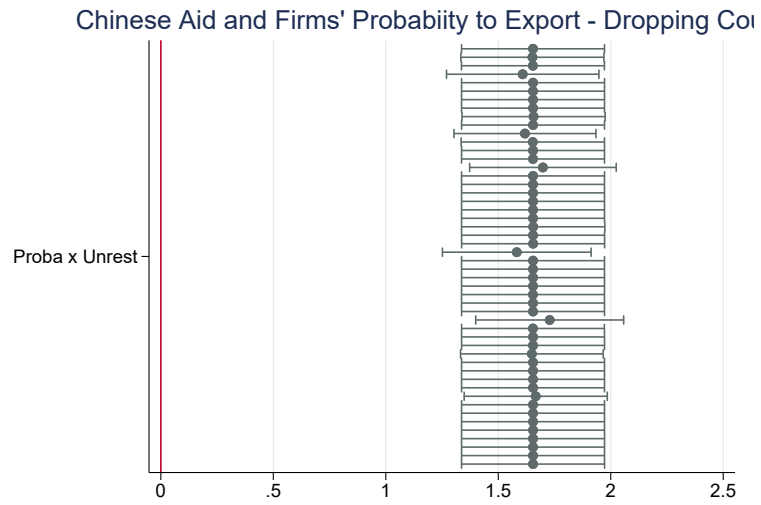


Figure A8: Robustness check for the first stage of Chinese projects:
Country-Year influence on instrument's coefficient (firm and region fixed effects)

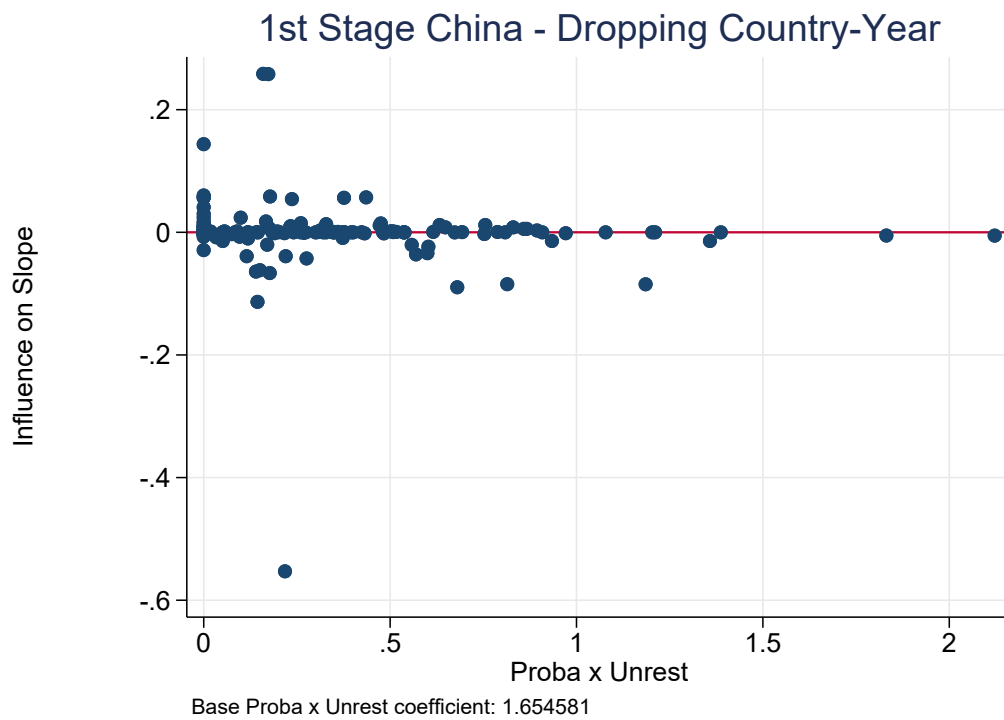
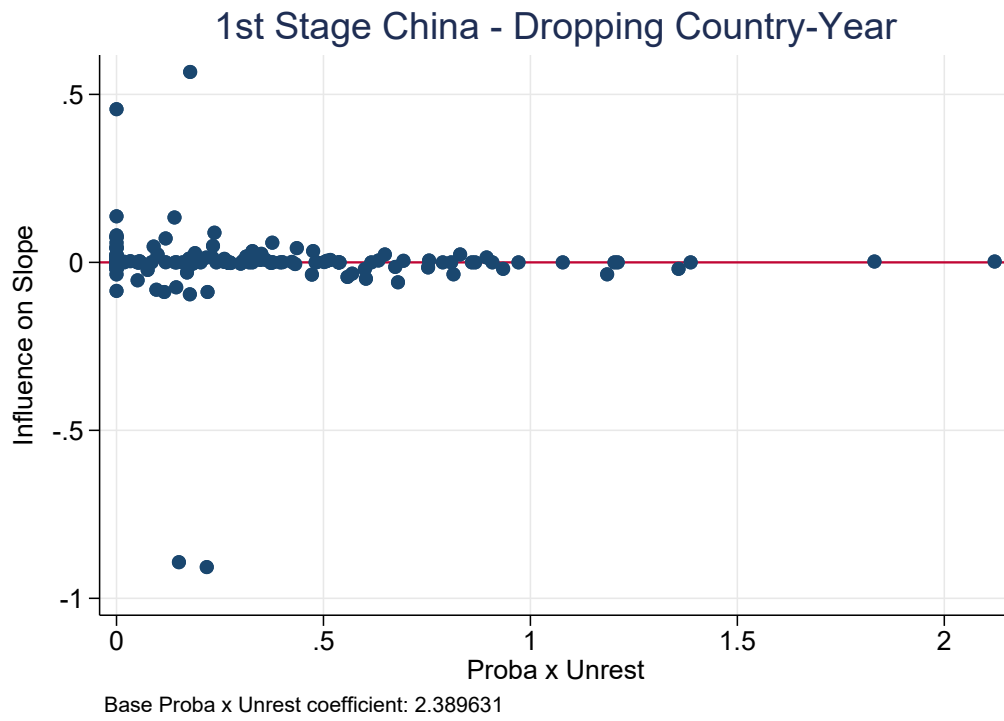


Table A13: Chinese projects - Exclusion restrictions

Dep. Var.: <i>Exporter_{f,s,c,r,t}</i>	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
	Firm fixed effect						Region fixed effect							
Aid c,r,t	-0.087 (0.079)	-0.083 (0.081)	-0.096 (0.079)	-0.095 (0.078)	-0.094 (0.079)	-0.113 (0.095)	-0.215 (0.199)	-0.022 (0.043)	-0.042 (0.048)	-0.037 (0.044)	-0.036 (0.045)	-0.026 (0.041)	-0.028 (0.052)	0.026 (0.085)
Remittances c,t x Proba CHN r	0.480 (0.242)*						0.598 (0.672)	0.309 (0.116)**						0.280 (0.176)
FDI c,t x Proba CHN r		-0.081 (0.041)*					-0.036 (0.066)		0.032 (0.024)					0.005 (0.036)
Import c,t x Proba CHN r			0.342 (0.581)				-7.790 (5.265)			0.049 (0.401)				0.201 (1.954)
Export c,t x Proba CHN r				0.202 (0.323)			3.250 (2.212)				0.138 (0.209)			0.166 (0.746)
WB Aid c,t x Proba CHN r					-0.084 (0.069)		-0.061 (0.103)					0.088 (0.029)***		0.080 (0.031)**
Import CHN c,t x Proba CHN r						0.097 (0.387)	1.949 (1.726)						-0.231 (0.264)	-0.457 (0.549)
Export CHN c,t x Proba CHN r						0.465 (0.236)**	0.862 (0.546)						0.258 (0.164)	0.169 (0.196)
<i>N</i>	37,048	37,408	37,516	37,516	37,516	36,778	36,418	150,839	151,263	151,577	151,577	151,577	149,038	148,614
<i>R</i> ²	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.04	0.04	0.04	0.04	0.04	0.04	0.04
Country x Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector x Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No	No	No	No	No
Region FE	No	No	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Weights	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N region year (clusters)	1,308	1,314	1,324	1,324	1,324	1,270	1,264	1,587	1,590	1,595	1,595	1,595	1,548	1,545
Kleibergen-Paap LM stat (p-value)	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Kleibergen-Paap F-stat	37.36	29.69	38.40	27.94	28.55	77.47	11.48	65.00	70.58	106.00	78.91	76.33	49.54	14.40

Standard errors in parentheses, clustered at the region-year level.

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

Figure A9: Survivor bias

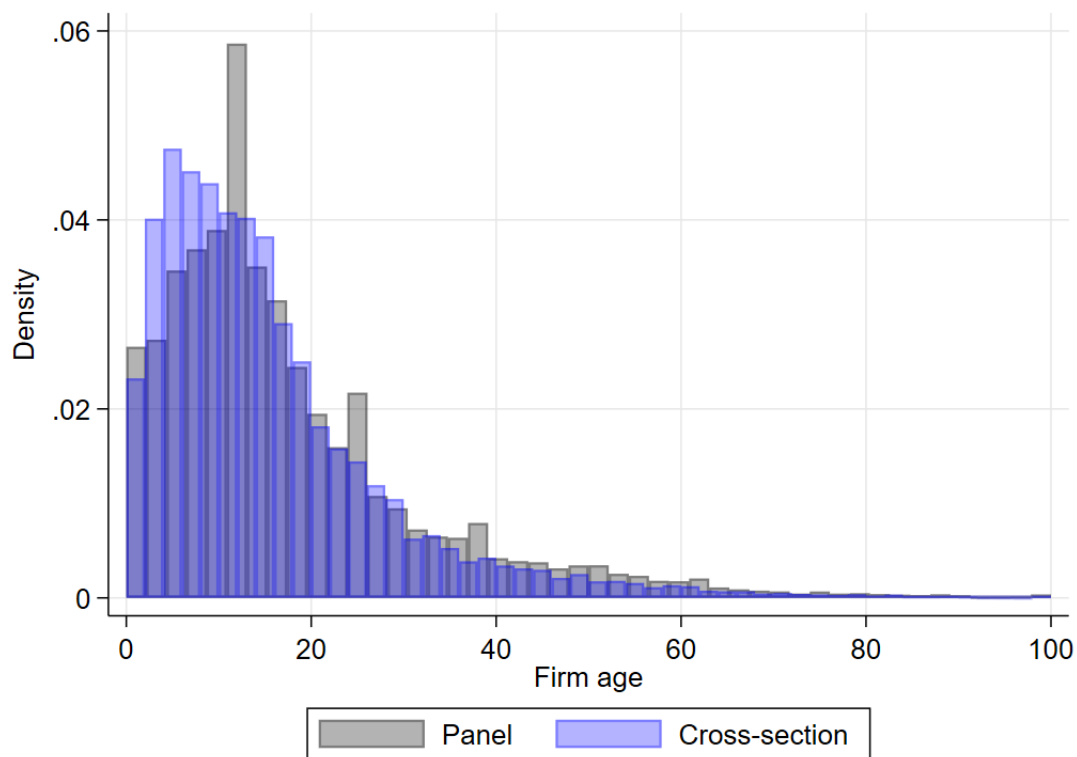


Table A14: Randomization of firms

$Exporter_{f,s,c,r,t}$	(1) Firm FE	(2) Region FE
Aid Coefficient, 500 replications:		
Mean	-0.036	-0.049
Standard deviation	0.168	0.010
% not significant	94.8	98.8
Number of firms randomly drawn	50	250
Country x Year FE	Yes	Yes
Sector x Year FE	Yes	Yes
Firm FE	Yes	No
Region FE	No	Yes
Weights	Yes	Yes
Mean K-Paap F-stat	16.76	71.98
sd K-Paap F-Stat	7.38	0.61

Table A15: Chinese Transport Infrastructure on Firms' Export Probability -
Without countries in conflict

Dep. Var.: <i>Exporter_{f,s,c,r,t}</i>	Firm FE		Region FE	
	Baseline	No conflict	Baseline	No conflict
Aid c,r,t	-0.096 (0.080)	-0.105 (0.089)	-0.036 (0.045)	-0.022 (0.052)
<i>N</i>	37,516	33,412	151,577	132,852
<i>R</i> ²	0.01	0.01	0.04	0.04
Country x Year FE	Yes	Yes	Yes	Yes
Sector x Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	No	No
Region FE	No	No	Yes	Yes
Weights	Yes	Yes	Yes	Yes
N region year (clusters)	1,324	1,232	1,595	1,470
Kleibergen-Paap LM stat (p-value)	0.000	0.000	0.000	0.000
Kleibergen-Paap F-stat	27.29	22.96	73.46	60.40

Standard errors in parentheses, clustered at the region-year level.

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

Table A16: Chinese Transport Infrastructure on Firms' Export Probability - Alternative lags

Dep. Var.: <i>Exporter</i> _{<i>f,s,c,r,t</i>}	Firm FE			Region FE		
	Baseline	Unrest t-3 to t-7	Aid t-2	Baseline	Unrest t-3 to t-7	Aid t-2
Aid _{<i>c,r,t</i>}	-0.096 (0.080)	-0.103 (0.083)	-0.963 (1.019)	-0.036 (0.045)	-0.032 (0.046)	-0.269 (0.347)
<i>N</i>	37,516	37,516	37,516	151,577	151,577	151,577
<i>R</i> ²	0.01	0.01	-0.03	0.04	0.04	0.04
Country x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Sector x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	No	No	No
Region FE	No	No	No	Yes	Yes	Yes
Weights	Yes	Yes	Yes	Yes	Yes	Yes
N region year (clusters)	1,324	1,324	1,324	1,595	1,595	1,595
Kleibergen-Paap LM stat (p-value)	0.000	0.000	0.001	0.000	0.000	0.003
Kleibergen-Paap F-stat	27.29	21.07	3.62	73.46	72.78	5.03
Aid lags	t to t-4	t to t-4	t-2	t to t-4	t to t-4	t-2
Unrest lags	t-3	t-3 to t-7	t-3	t-3	t-3 to t-7	t-3

Standard errors in parentheses, clustered at the region-year level.

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

Table A17: Chinese Transport Infrastructure on Firms' Export Probability - Alternative variables of interest

Dep. Var.: <i>Exporter</i> _{<i>f,s,c,r,t</i>}	Aid dummy		Aid amount	
	Firm FE	Region FE	Firm FE	Region FE
Aid _{<i>c,r,t</i>}	-0.118 (0.097)	-0.043 (0.056)	-0.006 (0.005)	-0.003 (0.003)
<i>N</i>	37,516	151,577	37,516	151,577
<i>R</i> ²	0.01	0.04	0.01	0.04
Country x Year FE	Yes	Yes	Yes	Yes
Sector x Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	No	Yes	No
Region FE	No	Yes	No	Yes
Weights	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
N region year (clusters)	1,324	1,595	1,324	1,595
Kleibergen-Paap LM stat (p-value)	0.000	0.000	0.000	0.000
Kleibergen-Paap F-stat	16.99	52.81	13.83	38.18

Standard errors in parentheses, clustered at the region-year level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A18: Chinese Transport projects - With intermediate observations

Second Stages: Dep. Var.: <i>Exporter</i> _{<i>f,s,c,r,t</i>}	Firm FE		Region FE	
	No Inter. obs.	Inter. Obs.	No Inter. obs.	Inter. Obs.
Aid _{<i>c,r,t</i>}	-0.096 (0.080)	-0.080 (0.079)	-0.036 (0.045)	-0.036 (0.045)
<i>N</i>	37,516	40,285	151,577	153,962
<i>R</i> ²	0.01	0.01	0.04	0.04
Country x Year FE	Yes	Yes	Yes	Yes
Sector x Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	No	No
Region FE	No	No	Yes	Yes
Weights	Yes	Yes	Yes	Yes
N region year (clusters)	1,324	1,332	1,595	1,597
Kleibergen-Paap LM stat (p-value)	0.000	0.000	0.000	0.000
Kleibergen-Paap F-stat	27.29	25.25	73.46	73.09

Standard errors in parentheses, clustered at the region-year level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A19: China Transport Infrastructure on Firms' Export Probability - With firm that changed regions

Dep. Var.: <i>Exporter_{f,s,c,r,t}</i>	Firm FE		Region FE	
	Baseline	With moving	Baseline	With moving
Aid $_{c,r,t}$	-0.096 (0.080)	-0.064 (0.062)	-0.036 (0.045)	-0.038 (0.045)
<i>N</i>	37,516	39,502	151,577	153,769
<i>R</i> ²	0.01	0.01	0.04	0.04
Country x Year FE	Yes	Yes	Yes	Yes
Sector x Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	No	No
Region FE	No	No	Yes	Yes
Weights	Yes	Yes	Yes	Yes
N region year (clusters)	1,324	1,370	1,595	1,617
Kleibergen-Paap LM stat (p-value)	0.000	0.000	0.000	0.000
Kleibergen-Paap F-stat	27.29	39.31	73.46	74.12

Standard errors in parentheses, clustered at the region-year level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A20: Chinese Transport Infrastructure on Firms' Export Probability - Dropping controls

Dep. Var.: <i>Exporter_{f,s,c,r,t}</i>	Firm FE					Region FE				
	Baseline	No GDP	No pop.	No size	No Foreign	Baseline	No GDP	No pop.	No size	No Foreign
Aid _{c,r,t}	-0.096 (0.080)	-0.081 (0.070)	-0.092 (0.079)	-0.089 (0.082)	-0.097 (0.079)	-0.036 (0.045)	-0.032 (0.042)	-0.035 (0.045)	-0.030 (0.046)	-0.043 (0.046)
<i>N</i>	37,516	37,516	37,516	37,976	37,536	151,577	151,577	151,577	152,546	153,735
<i>R</i> ²	0.01	0.01	0.01	0.00	0.01	0.04	0.04	0.04	0.01	0.03
Country x Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector x Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	No	No	No	No	No
Region FE	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Weights	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N region year (clusters)	1,324	1,324	1,324	1,326	1,324	1,595	1,595	1,595	1,596	1,602
Kleibergen-Paap LM stat (p-value)	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Kleibergen-Paap F-stat	27.29	29.64	27.43	27.81	27.34	73.46	77.97	73.26	74.11	72.99

Standard errors in parentheses, clustered at the region-year level.

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

Table A21: China Transport Infrastructure on Firms' Export Probability - More controls

Dep. Var.: <i>Exporter</i> _{<i>f,s,c,r,t</i>}	Firm FE		Region FE	
	Baseline	More Ctrls	Baseline	More Ctrls
Aid _{<i>c,r,t</i>}	-0.096 (0.080)	-0.187 (0.084)**	-0.036 (0.045)	-0.143 (0.052)***
Population (log) _{<i>c,r,t</i>}	0.050 (0.056)	0.039 (0.057)	0.012 (0.026)	-0.017 (0.029)
GDP (log) _{<i>c,r,t</i>}	-0.037 (0.046)	-0.042 (0.048)	-0.006 (0.017)	-0.011 (0.021)
Foreign Owned _{<i>f,s,c,r,t</i>}	-0.012 (0.043)	-0.045 (0.073)	0.146 (0.018)***	0.158 (0.017)***
Size _{<i>f,s,c,r,t</i>}	0.083 (0.021)***	0.038 (0.027)	0.099 (0.006)***	0.093 (0.007)***
Sales _{<i>f,s,c,r,t-3</i>}		0.009 (0.004)**		0.012 (0.002)***
State Owned _{<i>f,s,c,r,t</i>}		0.049 (0.126)		-0.064 (0.026)**
<i>N</i>	37,516	22,454	151,577	109,497
<i>R</i> ²	0.01	0.01	0.04	0.06
Country x Year FE	Yes	Yes	Yes	Yes
Sector x Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	No	No
Region FE	No	No	Yes	Yes
Weights	Yes	Yes	Yes	Yes
N region year (clusters)	1,324	1,207	1,595	1,510
Kleibergen-Paap LM stat (p-value)	0.000	0.000	0.000	0.000
Kleibergen-Paap F-stat	27.29	36.16	73.46	53.33

Standard errors in parentheses, clustered at the region-year level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A22: Chinese Transport projects - Alternative Instruments

Dep. Var.: <i>Exporter</i> _{<i>f,s,c,r,t</i>}	Proba x Steel		Proba x Factor		Proba x Reserves	
	Firm FE	Region FE	Firm FE	Region FE	Firm FE	Region FE
Aid _{<i>c,r,t</i>}	-0.156 (0.104)	-0.160 (0.074)***	-0.160 (0.105)	-0.132 (0.070)*	-0.021 (0.066)	-0.070 (0.059)
<i>N</i>	37,516	151,577	37,516	151,577	37,516	151,577
<i>R</i> ²	0.01	0.04	0.01	0.04	0.01	0.04
Country x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Sector x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	No	Yes	No	Yes	No
Region FE	No	Yes	No	Yes	No	Yes
Weights	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N region year (clusters)	1,324	1,595	1,324	1,595	1,324	1,595
Kleibergen-Paap LM stat (p-value)	0.000	0.000	0.000	0.000	0.000	0.000
Kleibergen-Paap F-stat	35.74	21.88	43.07	21.70	31.09	23.76
First Stages: Dep. Var: Aid _{<i>c,r,t</i>}						
Proba CHN x Steel _{<i>c,r,t</i>}	-22.391 (3.745)***	-0.159 (0.073)**				
Proba CHN x Factor _{<i>c,r,t</i>}			-3.769 (0.574)***	-2.648 (0.568)***		
Proba CHN x Reserves _{<i>c,r,t</i>}					3.682 (0.660)***	2.290 (0.470)***
<i>R</i> ²	0.97	0.96	0.98	0.96	0.97	0.96
Country x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Sector x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	No	Yes	No	Yes	No
Region FE	No	Yes	No	Yes	No	Yes
Weights	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at the region-year level.

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

Table A23: Chinese Projects on Firms' Export Probability - Active Inactive difference

Dep. Var.: <i>Exporter</i> _{<i>f,s,c,r,t</i>}	(1) Firm FE	(2) Region FE
Active <i>c,r,t</i>	-0.032 (0.062)	-0.008 (0.026)
Inactive <i>c,r,t</i>	0.114 (0.054)**	0.081 (0.019)***
<i>N</i>	37,516	151,577
<i>R</i> ²	0.85	0.23
Country x Year FE	Yes	Yes
Sector x Year FE	Yes	Yes
Firm FE	Yes	No
Region FE	No	Yes
Weights	Yes	Yes
Controls	Yes	Yes
N region year (clusters)	1,324	1,595
Active - Inactive	-0.145	-0.088
F test: Active-Inactive=0	3.19	8.32
p-value	0.07	0.00

Standard errors in parentheses, clustered at the region-year level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A24: China Transport Infrastructure on Firms' Export Probability - By macro regions

Dep. Var.: <i>Exporter_{f,s,c,r,t}</i>	Firm FE					Region FE				
	Asia	Sub-Sah. Africa	Europe	Latin America	MENA	Asia	Sub-Sah. Africa	Europe	Latin America	MENA
Aid c,r,t	0.192 (0.138)	0.318 (0.501)	0.013 (0.150)	-0.012 (0.071)	-0.325 (0.106)***	-0.241 (0.172)	-0.006 (0.201)	0.039 (0.090)	-0.033 (0.047)	0.006 (0.125)
<i>N</i>	6,104	8,288	8,408	10,818	3,732	37,359	34,698	34,505	31,061	13,886
<i>R</i> ²	0.05	0.03	0.01	0.01	0.07	0.05	0.05	0.03	0.07	0.06
Country x Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector x Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	No	No	No	No	No
Region FE	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Weights	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N region year (clusters)	248	207	646	157	66	328	296	707	178	86
Kleibergen-Paap LM stat (p-value)	0.000	0.112	0.000	0.000	0.002	0.003	0.044	0.000	0.008	0.006
Kleibergen-Paap F-stat	9.37	0.97	4.48	22.31	27.69	6.49	3.47	21.42	27.23	14.28

Standard errors in parentheses, clustered at the region-year level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Chapter 2

Foreign Aid and Power Play Political Cycle in World Bank's Procurement Allocation

Co-authored with Lisa Chauvet (Université Paris 1 Panthéon-Sorbonne, CNRS, Centre d'Economie de la Sorbonne) and Marin Ferry (Université Gustave Eiffel, ERUDITE (EA 437))

1.1 Introduction

As pointed out recently by an article published in *Mediapart* (a French investigative journal),¹ French bilateral aid has sometimes been used to the benefit of national companies. To be more precise, the Agence Française de Développement (AFD) has reportedly favored French firms for large contracts allocated by public procurement procedures. One case in point is the 2016 granting of a €24.4 million renovation contract for Douala Airport to French firm SOGEA-SATOM, a subsidiary of Bolloré’s civil engineering firm Vinci. *Mediapart*’s investigations found that this is not an isolated case and that French firms are very often chosen for contracts allocated by a procurement process. In this paper, we investigate whether such favoritism: 1) is also prevalent in the case of a multilateral aid agency, here the World Bank; 2) is driven by electoral prospects by examining political cycles in World Bank procurement around elections; 3) varies depending on whether the election is held in the country where the contract is performed (recipient country) or in the country of origin of the firm winning the contract (supplier country). Some examples point to a potential link between the electoral cycle and World Bank contract allocation. In 2011, COLAS, French leader in road construction and part of the Bouygues conglomerate, won a World Bank procurement contract to build three bridges in Madagascar. In 1995, Nagarjuna Limited, a large Indian public construction company, won two World Bank contracts to build roads in Tamil Nadu State. At first glance, those two examples have little in common aside from being civil works contracts in developing countries funded by the World Bank. The comparison ends there. Yet the two contracts have a further point in common: they were all signed around elections in the recipient or supplier countries.² Although those illustrations appear to be anecdotal, they lead us to our main research question: Do companies win larger World Bank contracts during election periods? And if so, can we find evidence of the mechanisms contributing to these political cycles?

The potential existence of a political cycle in the awarding of World Bank procurement contracts is an important issue because it calls into question the effectiveness and efficiency of Bank interventions. Indeed, there is no guarantee that the contracting firm chosen for electoral purposes is the most suitable and efficient to perform the project financed by those contracts. [Lehne et al. \(2018\)](#)

¹<https://www.mediapart.fr/journal/international/270921/les-derives-de-l-aide-francaise-au-developpement>

²They were allocated a year before a national election in the firms’ countries of origin for contracts in Madagascar and India.

undertake to assess the costs of such election-related procurement and find that public road contracts awarded to politically connected firms are the most likely to never be built. Taking a more aggregated approach, [Dreher et al. \(2018a\)](#) show that aid effectiveness tends to be reduced when aid is allocated for political purposes. Their findings conclude that the effect of aid on growth is reduced when aid is committed in years when the recipient country had a representative on the United Nations Security Council.

In addition to undermining the effectiveness of World Bank interventions, a political cycle can also impair the fairness of elections. Indeed, the ability of an incumbent government to use procurement to augment its campaign funds or enhance its public image through favoring local industry and supporting domestic employment bestows it with an unfair advantage over other candidates. This advantage arises from the fact that increased financial resources can elevate the probability of winning an election ([Epstein and Franck \(2007\)](#) and [Rekkas \(2007\)](#)), in addition to fostering a positive perception among voters.

Using both the World Bank's Contract Awards Database and the National Elections across Democracy and Autocracy (Nelda) dataset, we assess whether the allocation of World Bank procurement contracts is subject to such a political cycle. To this end, we use a gravity model with three-dimensional data (recipient countries, supplier countries, and years) in the same vein as [Starosta De Waldemar and Mendes \(2018\)](#) who look at the cross-country determinants of the European Union's procurement, and [Dreher et al. \(2019\)](#) who investigate the political economy of IFC lending.³ We differentiate between domestic and cross-border political cycles. A domestic political cycle refers to situations in which local firms win larger World Bank contracts around elections in the country where the contract is performed (recipient country). A cross-border political cycle relates to situations where foreign firms win larger World Bank contracts around elections in their country of origin (*i.e.* the supplier country), the procurement being then implemented in the recipient country. Our results support the existence of both a domestic and a cross-border political cycle.

More specifically and with respect to the domestic political cycle, we find that local firms win, on average, significantly larger World Bank contracts around election semesters in their home country (respectively 96% and 82% larger procurement contracts one semester before and during an election semester). These findings are

³International Finance Corporation, which is the World Bank's window in charge of financing developing countries' private sectors.

in line with McLean (2017) and Zhang and Gutman (2015), who find a preference for local firms in World Bank procurement allocation. We add to the literature by showing that the domestic preference is particularly acute around elections. Our results also suggest the existence of a cross-border political cycle as foreign firms win on average 43% larger contracts one semester before an election in their home country (the supplier country).

We then explore the mechanisms underlying both domestic and cross-border political cycles. This investigation reveals that political environment and election characteristics matter. Indeed, both political cycles are observed mainly in countries where corporate donations to electoral candidates are allowed and when the incumbent is running for re-election, thereby providing indirect evidence of cronyism. Moreover, these political cycles also appear to be more noticeable when the election outcome is relatively uncertain (when elections are competitive, or when polls are rather unfavourable to the incumbent), and where additional campaign funding or actions that influence voters' perceptions of the candidates could have more significant impacts on the ballot box results. In addition to the political characteristics of election countries, we also acknowledge the influence of economic factors on these political cycles, particularly their exacerbation in the presence of rising unemployment in countries approaching upcoming elections. One reason behind this relationship might be that competing candidates may influence the allocation of large World Bank procurement in favor of companies from their own homeland, as a means to demonstrate their capacity to tackle unemployment and provide tangible evidence to voters of their commitment to reducing joblessness.

Lastly, regarding the cross-border political cycle alone, *i.e.* when elections are held in the foreign firm's country of origin, results suggest that such a political cycle is more likely to be observed when the firm's country of origin: (1) shares historical ties, (2) is a significant aid partner of the recipient country, and (3) can easily meet with recipient country in international organizations. We also find that foreign firms from supplier countries displaying a significant reduction in tied aid are more likely to win larger contracts around election semesters. This suggests that procurement constitutes an alternative to tied aid as it existed before the Paris Declaration and could thus be a way for developed countries to recuperate the contributions they make to the World Bank.

Our paper contributes to two different strands of the political economy literature. First we add to the literature on the political economy of international organizations,

which finds politically-driven distortions 1) in the allocation of aid funds by those institutions⁴ or 2) in the way these funds could be used by elites in receiving countries as shown in Djankov et al. (2008); Bjørnskov (2010) and most recently by Andersen et al. (2022) who identified an increase in financial flows from recipients to tax havens a few months after World Bank disbursements. We do so by focusing on the World Bank procurement allocation process and relating that process to elections. Second, the paper ties in with the literature on public procurement allocation, which has highlighted that firms are more likely to win public procurement contracts when they have close political connections and contribute to campaign financing⁵. Our contribution shows that such arrangements may occur across countries when the election is held in the country of origin of a firm winning a contract abroad. It also provides an analysis of transnational influence drivers.

The rest of the paper is organized as follows: section 2 provides an overview of the literature and discusses the domestic and cross-border political cycle mechanisms driven by elections in the recipient and supplier countries, respectively. Section 3 introduces the data used to assess the existence of a political cycle in World Bank procurement contracts. Section 4 presents the empirical strategy and the main results for both the domestic and cross-border political cycles. Section 5 explores the channels, and section 6 concludes.

1.2 Electoral returns on World Bank procurements?

Our primary hypothesis is that firms secure larger World Bank procurement contracts around election periods, which could be characterized as a political cycle and likely involves diverse mechanisms such as campaign financing, kickback arrangements, and actions aimed at enhancing candidates' public image among voters. Before describing these mechanisms in detail, we need to briefly explain the World Bank procurement contract allocation process: Once the recipient's main priorities have been identified by the Country Partnership Framework, the Bank agrees to fund a project in a given place. The recipient country then chooses the firm in charge of project implementation.⁶ After choosing the supplier, the recipient

⁴Kaja and Werker (2010); Dreher et al. (2019); McLean (2017); Zhang and Gutman (2015); Kersting and Kilby (2016, 2019, 2021); Faye and Niehaus (2012); Kuziemko and Werker (2006); Dreher et al. (2009a,b)

⁵Titl and Geys (2019); Goldman et al. (2013); Daniele and Bennesen (2010); Kapur and Vaishnav (2013); Mironov and Zhuravskaya (2016); Schoenherr (2019); Baltrunaite (2020)

⁶Delegation to the recipient country of the choice of aid contract implementing firm has developed over time and across countries. Delegation has been the norm since the mid-2000s,

transfers the World Bank funds to the selected firm and the project can start. For the purpose of our research question, the effect of elections in recipient country r first needs to be differentiated from the effect of elections in supplier country s (the supplier firm's country of origin).

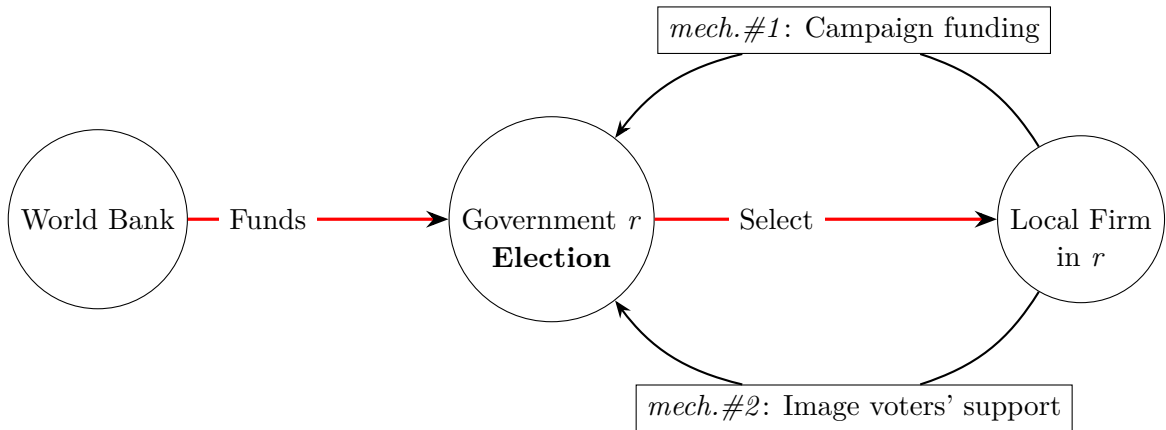
Let's first assume that an election is coming up in recipient country r and that the incumbent government is running for re-election and seeks to maximize its likelihood of winning the upcoming election. Considering that the recipient government is responsible for choosing the supplier to execute the World Bank contract, it can utilize the allocation process strategically to favor domestic firms that are friendly to the government. This could occur through two mechanisms: In the first mechanism, domestic firms willing to finance the government's campaign in exchange for a procurement contract could be favored (*mechanism #1*). In the second mechanism, the allocation of contracts could present a timely opportunity for competing candidates, particularly the incumbent, to demonstrate their support for the national industry and their capacity to generate employment. This showcase of support could potentially help them gain voters favor and support (*mechanism #2*). This causal chain of events as well as the potential mechanisms at play are summarized in Figure 1.1 below.

Let's now assume that an election is coming up in country s , which does not receive World Bank funds but has firms that may act as suppliers for contracts performed in recipient country r . The incumbent government here also wants to be re-elected and is consequently looking for funds (*mechanism #1*) and/or wants to enhance its public image towards voters (*mechanism #2*). One possible way to obtain such financial support or to improve public image could be to help a domestic firm in supplier country s to win a World Bank procurement contract abroad, *i.e.* in country r , which is responsible for choosing the supplier firm. To this end, s could use its economic or diplomatic influence with r 's government to encourage it to choose a supplier firm from s . If this pressure is effective, the selected company will be grateful for the contract and agree to finance the incumbent's campaign. Furthermore, the incumbent government will also be able to extol the competitiveness of domestic companies abroad and its merits for the national economy, particularly in terms of jobs. These cross-border relationships, necessary for such political cycle to occur, as well as its underlying mechanisms are summarized in Figure 1.2 below.

The mechanisms underlying the effect of elections on procurement allocation

but still varies from country to country depending on the quality of its institutions.

Figure 1.1: Domestic Political Cycle



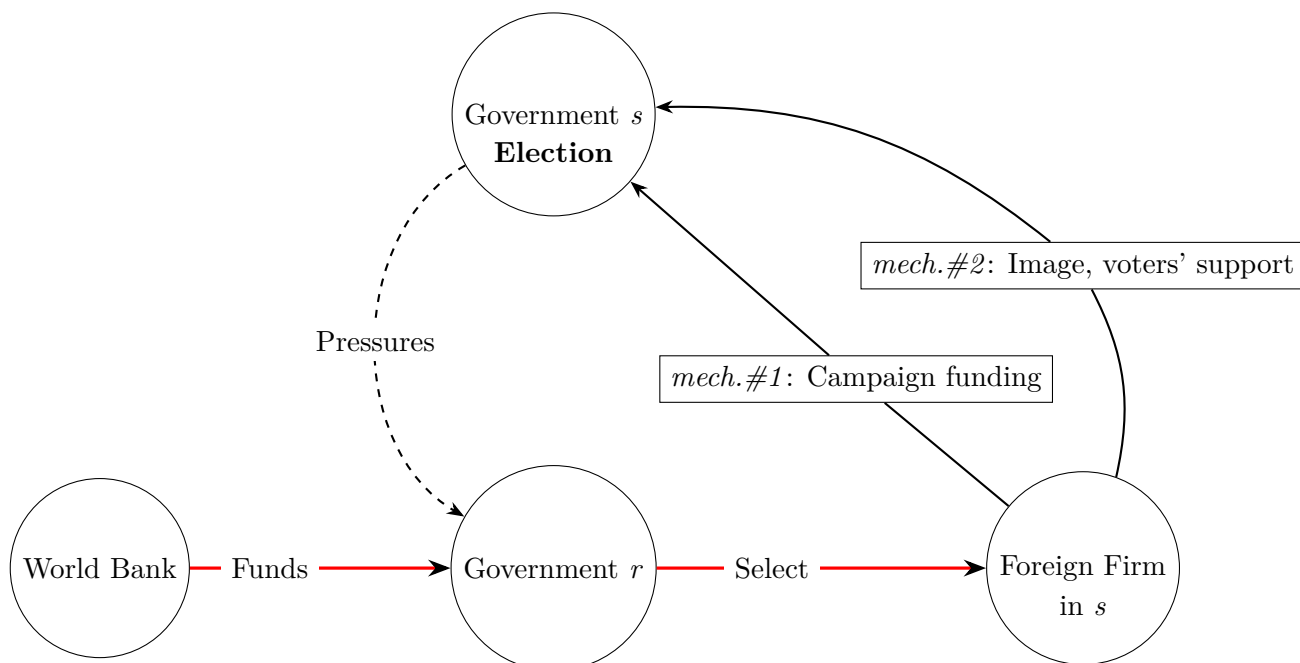
Note: Allocation and selection processes are indicated by the red arrows, while the black arrows describe the mechanisms at work in the political cycle.

differ slightly depending on where the election is held. However, all the types of political cycle feature key electoral motives to choose or push for a certain firm to win a World Bank procurement contract, implying that the selected company returns the favor to the government. The two cases also point to a potentially sub-optimal choice of supplier firm. The chosen firm may indeed be the favorite for the incumbent’s purpose, but it might not be the best to conduct the World Bank project in terms of quality of implementation or cost.

Yet, a number of assumptions are required for these political cycles to occur. The aid recipient’s entirely independent choice of contractor, irrespective of allocation method, is the main assumption required for our mechanism to hold. However, the World Bank can review the choice of supplier firm and veto it if it finds any irregularities. Nevertheless, the existing literature bears out the likelihood that both political cycles exist. Focusing on World Bank civil works procurement contracts and international competitive bidding, [Zhang and Gutman \(2015\)](#) show that only 30% of contracts are reviewed by the World Bank. Hence this limited audit scope combined with discretionary allocation makes it feasible that there could be a distortion in procurement contract allocation.

Our hypotheses also imply a flaw in the World Bank’s procurement process. It would not be the first time that World Bank funds have not been awarded in

Figure 1.2: Cross-Border Political Cycle



Note: Allocation and selection processes are indicated by the red arrows, while the black arrows describe the mechanisms at work in the political cycle. Dotted arrows represent transnational means of influence.

accordance with the optimal process developed by the institution and have depended on criteria that should not come into play. First, [Kaja and Werker \(2010\)](#) find that a country receives more World Bank projects in a year when it has a representative on the World Bank Board of Directors. [Dreher et al. \(2019\)](#) find similar results in the case of the International Finance Corporation. [McLean \(2017\)](#) and [Kersting and Kilby \(2021\)](#) highlight the influence of the US on the World Bank, as the US government replaces bilateral funds with multilateral funds in years when Congress is uncooperative. Findings from [Kersting and Kilby \(2016\)](#) even suggest faster loan disbursements before elections in the recipient country when it is politically aligned with the United States (*i.e.* when votes in the UN General Assembly are aligned with those of the US).

Another underlying assumption is the existence of a firm connection between governments and corporations, particularly in terms of funding candidates and political parties in return for procurement contracts. This kind of kickback arrangement has already been established in the literature. [Titl and Geys \(2019\)](#) evidence this sort of connection for public procurement contracts in the Czech

Republic between 2007 and 2014. To be more precise, they find that firms donating 10% more to a political party winning (losing) power see a 0.5–0.6% increase (decrease) in the value of their public procurement contracts. Likewise, [Goldman et al. \(2013\)](#) identify that US companies connected to the winning (losing) party secure significantly more (fewer) procurement contracts after the election. [Daniele and Bennedsen \(2010\)](#) find similar results in what they describe as the world’s least corrupt society: Denmark. Similar findings are also observed in Lithuania ([Baltrunaite, 2020](#)) and South Korea ([Schoenherr, 2019](#)).

Those kickback arrangements between governments and corporations are also expected to be more pronounced around election years. [Kapur and Vaishnav \(2013\)](#) suggest that construction firms in India experience a short-term liquidity crunch around election years, reflected by a decrease in their consumption of cement. They posit that those firms encounter this situation as they spend their cash flow to fund electoral campaigns. [Mironov and Zhuravskaya \(2016\)](#) observe an increase in tunneling around election years for firms with procurement contracts in Russia.⁷ This tunneling is interpreted as an increase in corruption in the allocation of public procurement around regional election years, as cash flows channel from firms to politicians in return for procurement contracts. In short, these different results reinforce the idea that public procurement could be an object of exchange between companies and politicians in order to influence the outcome of future elections.

Focusing on the cross-border political cycle, another key assumption is that supplier countries will use their influence with recipients in order to get what they want (in our context, having their national firms selected for procurement contracts abroad). Such behavior has already been observed in the existing literature. [Kuziemko and Werker \(2006\)](#) find that the amount of ODA received from the US and the United Nations grows significantly (by respectively 59% and 8%) in years when the recipient country has a seat on the UN Security Council. Those results suggest vote buying by developed countries from recipient countries via aid, since the effect increases during years in which key diplomatic events occur (*i.e.* when the Security Council’s vote is crucial). [Dreher et al. \(2009a\)](#), [Kersting and Kilby \(2019\)](#) present a similar pattern for World Bank aid and IMF loans as the number of projects, loans and supplemental grants received is higher in years when the recipient has a seat on the UN Security council. Taking a larger donor sample, [Faye and Niehaus \(2012\)](#) find that bilateral aid can be used not only to influence the recipient’s vote at the UN,

⁷Tunneling is the transfer of assets and profits out of firms for the benefit of those who control them.

but also to affect the recipient’s domestic election results. They show that donors give more aid to politically aligned recipients in the lead-up to competitive elections. In keeping with the findings for votes at the UN and recipient election results, we can consider that supplier countries (particularly those in the OECD) have means of pressure that can influence the recipient country’s choice of contractor.

Therefore, consistent with the existing literature and above findings, there is good reason to believe that the award of procurement contracts may be biased in respect of individual and electoral prospects. We hence propose to test the following hypotheses:

Hypothesis 1 - Domestic political cycle: *Local firms from r win larger World Bank procurement contracts around election semesters in the recipient country r .*

Hypothesis 2 - Cross-Border political cycle: *Foreign supplier firms from country $s \neq r$ win larger World Bank procurement contracts in country r around election semesters in their country of origin s .*

1.3 World Bank procurement and election data

In line with McLean (2017) and Zhang and Gutman (2015), our paper builds on the World Bank’s Contract Awards Database on major contracts awarded between 1993 and 2019.⁸ In view of the patchiness of data for 1993 and 1994, we focus on the 1995-2019 period. The information presented is highly detailed: name of supplying firm, its country of origin (supplier country), date of contract signature, contract amount (in US\$), recipient country, contract category and allocation method are all available. These data contain information on contracts that have been reviewed and approved by the World Bank, suggesting that the identification of a political cycle would probably underestimate this phenomenon.

This dataset reveals that the two most common procurement allocation methods for the period studied (*i.e.* 1995-2019) are Quality and Cost-Based Selection (27.9%) and International Competitive Bidding (27%). The World Bank *Procurement Regulations for Investment Project Financing Borrowers* (2016) state that Quality and Cost-Based Selection, “is a competitive process among shortlisted consulting firms under which the selection of the successful firm takes into account the quality of the proposal and the cost of the services”. This process is used solely for

⁸<https://www.worldbank.org/en/projects-operations/products-and-services/brief/summary-and-detailed-borrower-procurement-reports>

consultancy contracts. Under the rules of International Competitive Bidding, the recipient government has to advertise the procurement opportunity. Firms from all over the world can apply provided they meet the World Bank’s prerequisites. This procedure is used mainly for goods and civil works contracts. The third procurement allocation method is Single-Source Selection whereby the choice of supplier is at the discretion of the recipient government. This procedure obviously lacks transparency and the World Bank’s *Guidelines: Selection and Employment of Consultants* (2014) advocate using it only in exceptional circumstances. Yet, this method was used for 22% of World Bank contracts between 1995 and 2019. It was used mainly for consultancy missions, but also for some goods and civil works contracts. Finally, the fourth procurement allocation method is National Competitive Bidding (20.8%). This is similar to International Competitive Bidding with the main difference being that only firms from the recipient country can answer the call for tenders. Like its international equivalent, this method is used mainly for civil works and goods contracts. There are other procurement allocation methods (1.5%), which remain marginal and do not enter into those four main categories. Table 1.1 below presents the distribution of allocation methods across our sample:

Table 1.1: Procurement allocation method - Descriptive Statistics

	Observations	Percent	Cumulative
Quality And Cost-Based Selection	95,838	27.9	27.9
International Competitive Bidding	93,212	27.1	55.0
Single Source Selection	77,275	22.5	77.5
National Competitive Bidding	71,666	20.8	98.4
Other	5,353	1.5	100.0
Total	343,344	100.0	-

Notes: Procurement database, authors’ computation.

From this raw data, we calculated the average amount in US\$ won by firms from country s in year t and semester k ⁹ for World Bank contracts performed in recipient country r . This dependent variable¹⁰ allows us to construct a three-dimensional (gravity) panel dataset at the recipient-supplier-time level. Overall, we have 179,187 World Bank contracts¹¹ won by 132,762 firms from 197 supplier countries for projects

⁹Corresponding to the signature date of the contract.

¹⁰The average amount is calculated as follows: total amount in US\$ won by firms from s for contracts in country r , year t and semester k divided by the total number of contracts won by firms from s in country r , year t and semester k .

¹¹The number of contracts does not match the total observations in Table 1.1 since there can

in 153 recipient countries between 1995 and 2019. The sample of recipients is made up of developing countries benefiting from at least one World Bank procurement contract during the period of study. The sample of supplier countries includes developed and developing economies whose firms won at least one World Bank procurement contract. This leads us to define two different panel databases: one two-dimensional panel (recipient-time level) including 7,888 observations and one three-dimensional panel (at the recipient-supplier-time level) reporting 1,543,760 observations.

Upon a closer examination of the data, with a specific focus on the distinction between local and foreign firms (i.e., firms from recipient (r) and supplier (s) countries, respectively), the domestic preference of recipient governments becomes apparent as (and as shown by McLean (2017)) about three quarters of the procurement contracts are secured by firms based in the country receiving the funding for procurement (cf. Table 1.2 below). This share experiences a slight reduction when narrowing down to procurement dedicated to the provision of goods, as well as when calls for tender are opened to international competition (ICB) which is expected given the broader scope of potential suppliers in such cases. Conversely, most of civil work procurement contracts seem to be won and implemented by companies from receiving countries. Local firms won 93% of the civil works contract, but it represents 65% of the total amount allocated, which suggests that foreign firms tend to win larger civil work contracts. In fact, for the 5% most expensive civil works contracts, the share of local firms falls to 67%, and foreign firms tend to win more for the top 1% most expensive civil work contracts (52% of contracts).

Shifting then attention to the geographical distribution of World Bank procurement, Figure 1.3 illustrates that a considerable portion of these contracts is awarded to firms originating from China (the largest provider of winning firms)¹², India, but also other emerging countries such as Brazil, Argentina, Vietnam, and the biggest traditional donors (USA, UK, France, Germany). The spatial distribution of the average amount of procurement won by countries reveals that the most substantial contracts are secured by Chinese firms, as well as companies from Turkey, Iran, Venezuela, and several European countries (including Spain, Greece, and Switzerland). The prevalence of China in most of the procurement statistics, including the number of contracts, average contract amounts, and total contract

be more than one supplying firm within a contract.

¹²Of which firms have won 19% of all the World Bank funding for procurement over 1995-2019, but increasingly so, ending up with 27% at the end of the period.

amounts, raises concerns about the influence of outlier countries on our main results. However, our findings remain robust to various tests that assess sample dependence, supporting the credibility of our results.

Table 1.2: Racing for procurement: Local vs. Foreign firms

	Type of contract							
	All		Consultancy		Goods		Civil Works	
	Obs.	%	Obs.	%	Obs.	%	Obs.	%
Local	252,752	73.61	106,649	71.42	67,881	68.27	73,107	92.29
Foreign	90,592	26.39	43,880	28.58	31,546	31.73	6,105	7.71
Total	343,344	100	150,529	100	99,427	100	79,212	100

	Allocation method							
	QCBS		ICB		SSS		NCB	
	Obs.	%	Obs.	%	Obs.	%	Obs.	%
Local	62,309	71.42	59,774	68.27	58,886	78.97	71,226	100
Foreign	31,053	28.58	32,288	31.73	15,147	21.03	0	0
Total	93,362	100	92,062	100	72,033	100	71,226	100

Notes: Procurement database, authors' computation.

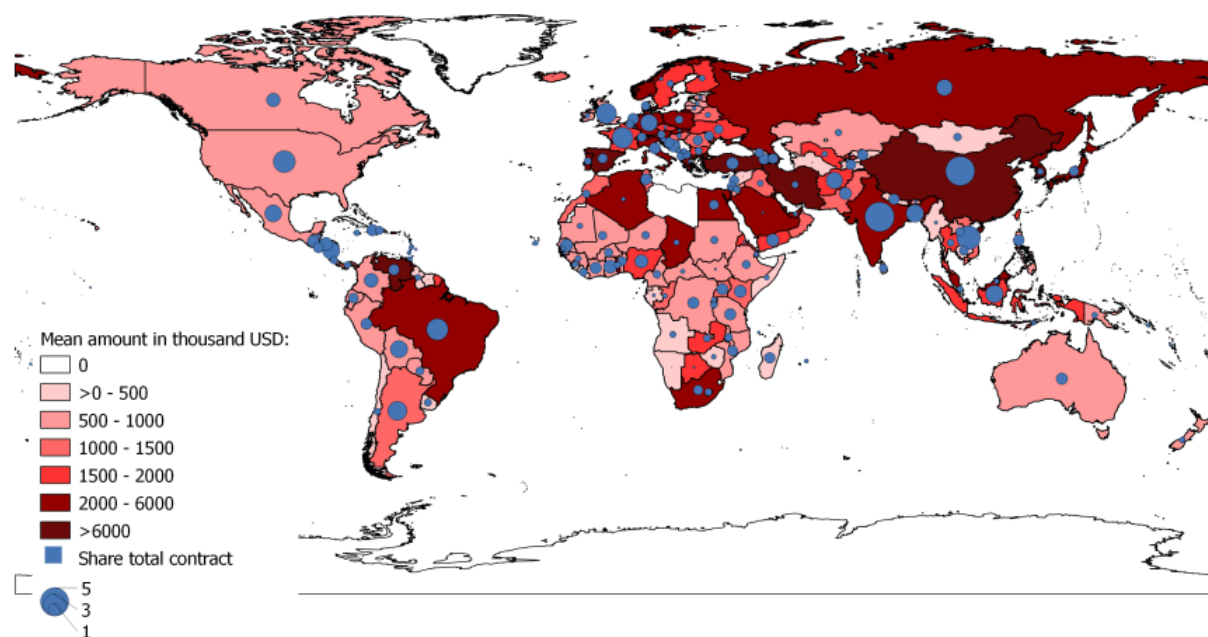
The sum of categories and methods does not match the total since some contracts could not be classified in these categories.

Focusing next on election data, we constructed a set of five variables of interest from the National Elections across Democracy and Autocracy (Nelda) dataset (Hyde and Marinov, 2012). This dataset on elections between 1945 and 2020 provides highly detailed information such as precise election date, incumbent participation, whether the election was held early or late, and type of election in a given country. The election considered may be legislative or presidential depending on whether the political system is respectively parliamentary or presidential.¹³ We used the election date to build the election semester variable (a dummy variable equal to one if there is an election in year t , semester k in a given country r or s). As the average term of office in our sample is 4.4 years, we built four additional election variables ranging from two semesters before the election to two semesters after the election. In other words, our political cycle variables range from one year prior the election to one year after the election, thereby preventing our set of dummies from overlapping. We believe that using the semester rather than the annual dimension, offers several

¹³Indirect elections (*i.e.* where there is no mass voting) are not included in this dataset. Given that our mechanism may also be found in cases where elections are indirect, countries with this kind of election were added in (Source: Wikipedia).

advantages. Firstly, it enables us to control for confounding factors at the country-year (either recipient or supplier) level by incorporating fixed effects. Secondly, adopting a lower time dimension would result in a significantly larger number of observations, which could unnecessarily prolong computation time, considering that the contract awarding process typically occurs over months rather than days. As such, the semester dimension strikes a balance between capturing relevant variations and ensuring computational efficiency.

Figure 1.3: Spatial distribution of World Bank contracts won by companies



Notes Mean amount in thousand USD won on World Bank contracts by firms from the reported country. Share total contract categories: $\geq 5\%$ of total number of contracts financed by the World Bank, $\geq 3\%$, $\geq 1\%$:

Table 1.3 below presents some descriptive statistics for several procurement variables (including the dependent variable in bold font) over the whole sample considered and differentiate between election and non-election periods. It shows that the average amount of contracts won is \$52,322 around election semesters¹⁴ while it reduces down to 36,783 outside of this period. On average, and regardless of the election calendar, the average amount for a procurement contract is of around \$45,000, with some countries receiving zero (no contract) and others over \$800,000,000.¹⁵ The discrepancy in the average amount of procurement won by

¹⁴In both recipient and supplier countries.

¹⁵The largest average amount corresponds to two observations, respectively Spanish and Brazilian firms winning civil works contracts to build the Quito Metro subway in Ecuador in 2015.

companies during and outside elections is the most significant among all the measures of procurement computed. Specifically, the number of contracts remains relatively similar during elections or non-election periods. The difference becomes slightly larger when considering the share of total amounts or of total number of contracts. Lastly, although the total amount won by firms from a particular country is higher during elections, the gap observed for periods without upcoming elections remains lower compared to the difference in the average amount.

Table 1.3: Procurement variables - Around vs. Outside elections

	Elec. Semesters (k-2 to k+2)		Outside Elec. (realm)	
	Observations	Mean	Observations	Mean
Average Amount	823,239	52,322.13	720,521	36,783.18
Number Contracts	823,239	0.12	720,521	0.11
Total Amount	823,239	189,761.1	720,521	152,905.3
Share Amount	823,239	0.41	720,521	0.36
Share Contracts	823,239	0.42	720,521	0.37

Notes: Three-dimensional panel dataset, authors' computation.

1.4 Empirical strategy and main results

This section examines the two main hypotheses introduced above: H1 regarding the domestic political cycle, and H2 concerning the cross-border political cycle.

1.4.1 Domestic political cycle

We first test whether more substantial procurement contracts are awarded around election semesters in recipient countries to local firms (H1). As explained above, given the structure of the World Bank procurement data and the possibility of isolating the semester in which the contract was won, we use an econometric specification based on this precise time decomposition to include multiple fixed effects which minimizes the omitted variable bias considerably. In order to first test H1, *i.e.* “Local supplier firms from recipient country r win larger World Bank procurement contracts around election semesters in r ”, we use the following model, which relies solely on the recipient-time dimension of our gravity database:

$$Procurement_{r,k,t} = \alpha + \sum_{k \in -2,+2} \beta_k Election_{r,k,t} + \omega_{r,t} + \mu_{k,t} + \varepsilon_{r,k,t} \quad (2.1)$$

where $Procurement_{r,k,t}$ denotes the average amount of World Bank procurement won by firms from recipient country r (local firms) in semester k of year t (in which the contract was signed). Variables of interest thus consist in a set of dummy variables $\sum_{k \in -2,+2} Election_{r,k,t}$ flagging semesters around the election semester in recipient country r (*i.e.* the semester in which the election is held). More specifically, we are interested in the two semesters before and after the election, which amounts to looking at one year before and after the election. Considering the large number of zeroes in the dependent variable (coinciding with years and semesters when recipient countries did not receive World Bank procurement funds), we follow [Mullahy and Norton \(2022\)](#) and [Bellemare and Wichman \(2020\)](#) in choosing not to transform the dependent variable (*i.e.* average amount per contract won) as it could lead to substantial differences in elasticities, and therefore estimate specification 2.1 by means of Poisson Pseudo Maximum Likelihood estimators (PPML).

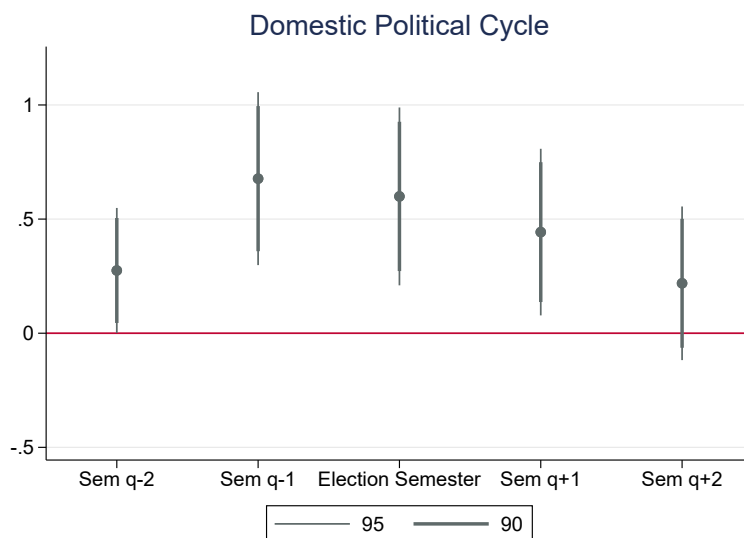
As election dates are generally set by the national constitution, reverse causality does not threaten the identification of a causal effect running from elections to average amounts of procurement won, *i.e.* the set of β_k . However, the incumbent government could still influence the date of the next election (by advancing or postponing it) to coincide more or less with World Bank procurement funding. We discuss in the robustness checks section this potential limitation to our empirical strategy and show in the supplementary appendix that removing election for which dates might have been shifted does not affect our main findings. However, although reverse causality is of no great concern here, our estimates might still suffer from omitted variable biases affecting the trend in the average value of procurement won around elections. To handle this potential estimation bias, we extend our specification to include a set of fixed-effects to control for: 1) time-varying factors at recipient-year level ($\omega_{r,t}$), 2) global events (common to all sample countries) that could affect the timing of World Bank procurement funding ($\mu_{k,t}$). As a consequence, regression results provide estimates of the contribution of election semesters to the average amount of procurement won by firms of a given recipient country when elections are taking place, with respect to periods without upcoming elections in this given country as well as in other countries receiving procurement over the study period.

Given the large set of fixed-effects, we employ the `ppmlhdfe` command developed by [Correia et al. \(2020\)](#) in order to reduce computation time by resorting to a procedure for multiple dimensions demeaning. This procedure leads to absorb higher

dimensional fixed effects such as recipients' invariant characteristics, θ_r , or common time-varying factors, δ_t , which are then included in (and thus controlled by) $\omega_{r,t}$ and $\mu_{k,t}$, respectively. Lastly, we cluster the standard errors at the recipient \times year level to control for potential error correlation in a given recipient country within a given year, as there might be unobserved factors causing observations to be correlated at this level (such as civil protest movements, new laws, etc.). Figure 1.4 reports on the impact of elections in the recipient country on the average amount of procurement when winning companies are from the recipient country.¹⁶

Results suggest the existence of a domestic political cycle in the World Bank procurement allocation process. On average, recipient countries see their local firms winning 78.2% larger contracts around their elections. In other words, contracts won by local firms are found to increase by more than half around elections in the recipient country. In addition, results suggest that recipient countries appear to favor (on average) their local firms (by awarding them larger government contracts) half a year before an election, as well as during the election period and the following semester.

Figure 1.4: Domestic political cycle - Effect of election in recipient countries



Notes: Coefficients estimated with recipient \times year and semester \times year fixed effects. Observations: 5,902. R^2 : 0.84. Robust standard errors clustered at the recipient \times year level (2,951).

¹⁶The regression corresponding to these figures is also reported in Table S.A2 in the supplementary appendix.

1.4.2 Cross-border political cycle

We then explore the second type of political cycle that might be at play in the allocation of these procurement contracts, referred to as the cross-border political cycle (H2). The existence of such a cycle assumes that the allocation process might be manipulated in order to serve electoral interests, this time in supplier countries, *i.e.* in countries whose domestic companies win contracts abroad. The following section thus examines whether firms from supplier country s are awarded with larger World Bank procurement contracts in recipient country r around election semesters in their country of origin s . Testing hypothesis H2 necessitates the formulation of another model, which draws upon the three-dimensional database.

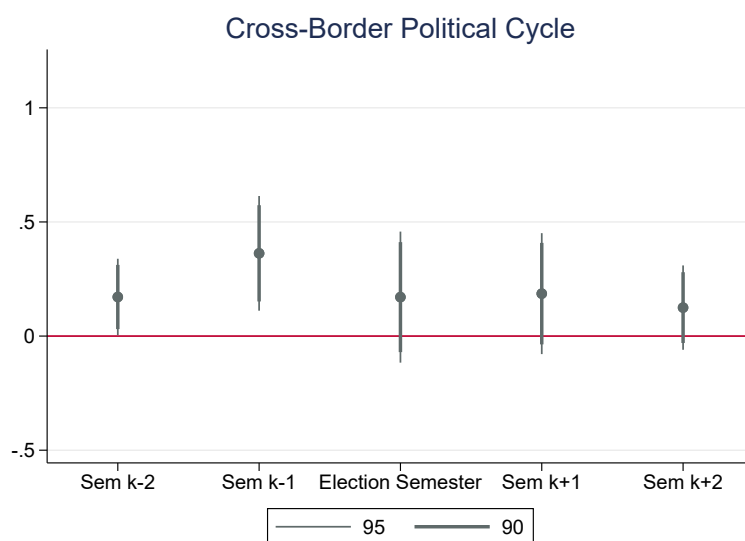
The structure of our data becomes three-dimensional (with time, recipient and supplier dimensions) consisting in a dyadic (recipient-supplier) panel dataset, which allows for the inclusion of different types of fixed effects to control for unobserved factors that might lead foreign firms from a given country (the supplier country) to win larger procurement contracts around elections in their country of origin (*i.e.* the supplier country). A three-dimensional panel also allows us to explore the mechanisms behind this cross-border political cycle by using the heterogeneity of the recipient-supplier dyads. In view of this dyadic structure and (as with H1) a significant number of zeroes in the dependent variable, specification 2.2 is also estimated by PPML estimators proven to perform better in the estimation of gravity models (Tenreyro and Silva, 2006; Sun and Reed, 2010; Gómez-Herrera, 2013; Larch et al., 2019). This takes the following form: relatively similar to Equation 2.1, but with some changes to our variables of interest and the set of fixed effects, thus taking the following form:

$$Procurement_{s,r,k,t} = \alpha + \sum_{k \in -2,2} \beta_k Election_{s,k,t} + \omega_{r,k,t} + \rho_{s,r,t} + \varepsilon_{s,r,k,t} \quad (2.2)$$

The main difference with this specification compared with the former specification is the set of electoral dummy variables, $Election_{s,k,t}$ which is now based on the electoral calendar of the country of origin of the winning firms (the foreign firms from the supplier country). Another difference relates to the set of fixed effects. Since the focus is on the supplier country's political cycle, $\omega_{r,t}$ from Equation 2.1 can be replaced with $\omega_{r,k,t}$, which controls for any recipient country factors that vary by semester in a given year, such as the domestic political cycle. Furthermore, we extend this specification by incorporating a time-varying dyad fixed effect at the annual level, denoted as $\rho_{s,r,t}$. This additional component controls for events that

occur at this level of variability, captures annual phenomena common to the supplier countries, and accounts for the contribution of the invariant structural characteristics of each dyad. As with the previous specifications, we challenge this specification with inconsistent election dates, providing suggestive evidence that the reverse causality issue is not so much of a concern. Only omitted variables threaten the identification of a causal effect running from election dates to the average amount of procurement contracts won by foreign companies. However, we do believe that the fine-grained set of fixed effects helps minimize such a concern.

Figure 1.5: Cross-border political cycle - Effect of election in supplier countries



Notes: Coefficients estimated with recipient x year x semester, and supplier x recipient x year fixed effects. Observations: 41,966. R²: 0.88. Robust standard errors clustered at the supplier x year x semester level (5,818).

Figure 1.5 above displays the estimated coefficients for our five dummy variables capturing semesters around the election semester in supplier countries, with both levels of confidence (5% and 10%) reported.¹⁷ Winning foreign firms tend to be awarded more lucrative World Bank contracts in recipient countries as their home country draws closer to the election semester. On average, foreign firms win 43.7% larger contracts one semester before an election in their home country. Contracts won by foreign companies increase by just under 50% around elections in their home countries.

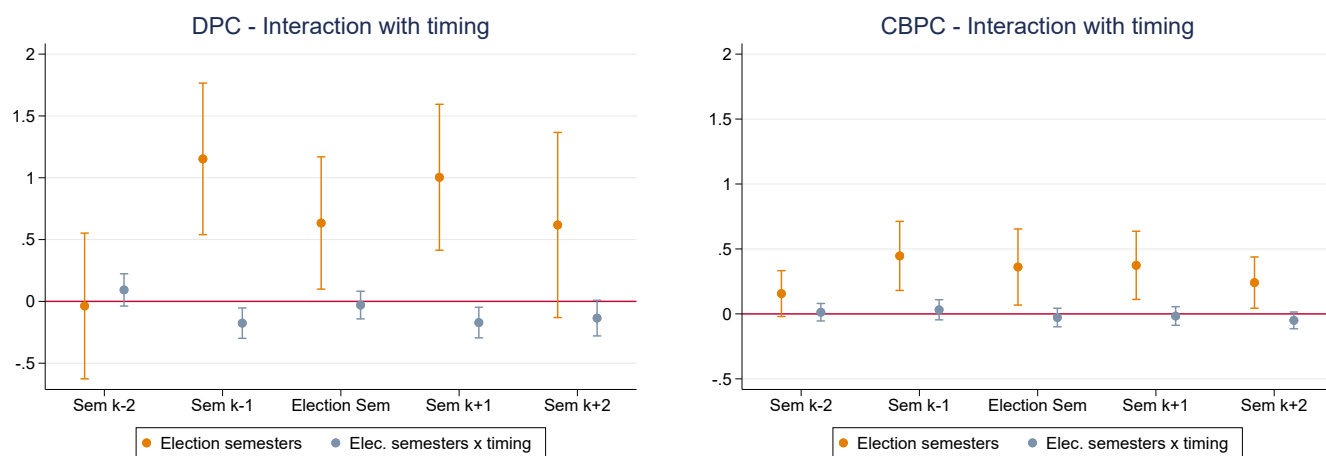
Overall, these results, both on the domestic and cross-border political cycles, provide initial support for the two hypotheses tested, and complement previous

¹⁷The corresponding regression is available in the supplementary appendix (see Table S.A2).

findings on the World Bank’s procurement process. It adds the dimension of electoral interests to the findings of McLean (2017) and Zhang and Gutman (2015), and also reveals potential motives behind the results of Kersting and Kilby (2016).

Following the latter, whom uncover faster disbursements around elections in recipient countries, we test whether the above identified cycles are reinforced or attenuated with respect to the average time from project approval to contract award. Results from Figures 1.6 below, indicate that, indeed, expedited contracts tend to be more prone to political cycles since the latter is attenuated when the duration from the contract approval to its award increases.¹⁸ However, this effect appears to be observed primarily within the context of domestic political cycles, specifically for the two semesters surrounding the election semester.

Figure 1.6: Interaction of political cycles with the duration of contract award (from contract approval to contract award)



Notes:

Left graph: Coefficients estimated with recipient x year and semester x year fixed effects. Observations: 5,902. R²: 0.85. Robust standard errors clustered at the recipient x year level (2,951).

Right Graph: Coefficients estimated with supplier x year x semester, and supplier x recipient x year fixed effects. Observations: 41,966. R²: 0.90. Robust standard errors clustered at the recipient x year x semester level (5,818).

1.4.3 Robustness checks

In order to ensure the existence of such political cycles (both domestic and cross-border) in World Bank procurement, the results reported above are subjected to a series of robustness tests, the rationale and results of which are presented in details in the Supplementary Appendix. Overall, these tests show that our main results are not affected by *i*) more funding for procurement contracts around election semesters

¹⁸We thank Bob Rijkers for suggesting this test to us.

in recipient or supplier countries (see Figure S.A2, Tables S.A3 and S.A4), *ii*) the possibility that some election dates could have been changed to match World Bank procurement contract award dates (see Tables S.A5 and S.A6), nor by *iii*) the way we supplemented the NELDA dataset, adding information on indirect elections (see Tables S.A7, S.A8 and S.A9). Our results are also found to be robust to alternative standard-errors clustering (see Table S.A10) as well as to the sample composition as they do not seem to be influenced by potential outliers (see Figure S.A3). Lastly, the results are also valid when we include in the regression the electoral variables simultaneously for the recipient countries and the supplier countries (see Table S.A11).

In view of our main findings, one could assume that such cycles are not common in international cooperation, regardless of the form of aid considered. Others might even wonder to what extent these political cycles in World Bank procurement are not mitigated by potential omitted variables such as the annual amounts of official development assistance (ODA) that countries receive.¹⁹ Such a situation would lead us to different conclusions about the contribution of foreign aid to electoral interests, as it would in fact help to smooth out the political cycle of procurement. We test this view by interacting the set of semester variables around the election with the amount of ODA disbursed (from different donors) to recipient countries r in year t .²⁰ Tables S.A12 and S.A13 in the supplementary appendix suggest that such a mitigating effect is likely to be true, but in an extremely small proportion that never reverses nor absorbs the election-induced cycle on the average amount per contract won (the thresholds for reversing the effect corresponding to colossal ODA amounts).

1.4.4 Analysis of contract heterogeneity

Next, we examine whether some types of procurement allocation method are more prone to political cycles. As explained above, developing countries granted World Bank procurement funds are responsible for selecting the company that will perform the contract in the country, which can be done in four different ways: (1) International Competitive Bidding (ICB) where domestic and foreign companies compete for the contract, (2) Quality and Cost-Based Selection (QCBS) where the recipient government selects the company based on the quality of its proposal and cost, (3) Single-Source Selection (SSS) which is a type of private (over-the-counter)

¹⁹We again thank Bob Rijkers for providing this idea.

²⁰The annual amount of ODA disbursements received (expressed as $\log(\text{ODA}+1)$) thus overlapping two semesters.

award (with fewer quality requirements), and (4) National Competitive Bidding (NCB) where only domestic companies are entitled to compete for the procurement contract. We first re-examine the domestic political cycle by allocation method and run the same regressions as those presented in Figure 1.4, but focusing on contract allocation method sub-samples. Hence, we compare the probability of winning larger procurement contracts (on average) around election semesters under one of the four allocation methods to winning larger procurement contracts under the same allocation method in semesters further away from elections.

Looking first at the Table A1 in the appendix reporting estimates of the domestic political cycle, results suggest that none of the four allocation methods has the advantage, leaving it unclear which process might most facilitate arrangements between politicians and firms and might therefore be most prone to foster political cycle. Results are more clear-cut for cross-border political cycles. Quite intuitively, Table A2 in the appendix shows that international competitive bidding is the main allocation method by which supplier countries' companies win (on average) larger contracts abroad in countries holding elections, as they get closer from their home country's elections. Procurement contracts secure through the two other allocation methods appear to be smaller, on average, around election periods as compared to situations outside of elections.

We then repeat the exercise, but this time differentiating between procurement by main activity sector. The World Bank finances three categories of procurement: for the supply of goods, for civil works and for consultancy services. Left graph of Figure A1 in the appendix shows that local companies win (on average) larger contracts for civil works around the election semester in the recipient country. The timing of the effect closely matches the one of the main regression. Moreover, one can observe that the political cycle persists for up to two semesters after the election when considering contracts not designated for consultancy purposes. While the coefficient for civil works contracts in the second semester following the election semester is non-significant, it is plausible that this positive and significant effect could be influenced by goods provision procurement, which constitutes lucrative contracts (see Figure S.A1 in the supplementary appendix). Conversely, the impact of consultancy procurement on the domestic political cycle diminishes, as this type of contract typically remains smaller (on average) than those focused on goods provision or civil works. These results somehow illustrate our above mentioned main assumption as these two categories of procurement are the most lucrative.

Consequently, they are more likely to be strategically allocated to friendly companies that can potentially support campaign financing or engage in kickback arrangements. Moreover, these types of contracts are also more likely to generate a significant number of jobs due to the substantial amounts involved in them.

Turning to the cross-border political cycle, right graph in Figure A1 in the appendix suggests that supplier countries are also more likely to see their multinational firms win more lucrative civil works procurement contracts in the semester before the election, which may again support the cronyism hypothesis since civil works contracts are larger (in terms of amount) than consultancy and goods procurement contracts.

1.5 The rationale for political cycles in World Bank procurement: indirect evidence

The above findings potentially raise important ethical questions which would seriously challenge the primary purpose of this form of aid, which is to serve recipient countries' collective interests and not individual/electoral interests, least of all in aid-provider countries. We therefore attempt in the following section to provide more evidence on the mechanisms driving the effects found previously.

The existence of a political cycle in the award of World Bank procurement contracts could suggest cronyism between politicians and the supplier companies that win these contracts, as depicted in Figures 1.1 and 1.2. However, such a claim calls for more direct evidence of on-lending arrangements between these stakeholders. Yet such an exercise inevitably has its limitations when it comes to revealing behavior that is probably covert and distorts competition. We therefore endeavor to provide evidence of cronyism by refining our results based on other factors such as the economic and political environment of both recipient and supplier countries.

1.5.1 Mechanism #1: Political cycles for campaign financing?

As explained above, one of the mechanism in support of the existence of a domestic political cycle is that recipient governments select firms to help finance their election campaigns. Firms might be selected by a government in advance of upcoming elections in exchange for a commitment to fund the government's election campaign. This would create a political cycle prior to the election. Alternatively, firms might finance the recipient government's campaign in exchange for the award of a future public contract. This would create a political cycle after the election.

Yet a close eye is kept on contributions made by private firms to the funding of candidates' election campaigns and the practice is even banned in many OECD countries and some emerging countries (see Figure A2 in the appendix). Although many developing, and thereby recipient, countries allow private companies (both domestic and foreign) to contribute funds to candidates' campaigns, some of them such as Mozambique, Ecuador, Uzbekistan, Egypt, Tunisia, Guinea-Bissau and Liberia prohibit such donations.²¹ Therefore, if campaign financing is not one of the mechanisms underlying the political cycle in the procurement allocation process, then these countries' likelihood of obtaining larger contracts should not differ from those that allow private donations to election candidates.

Drawing on the Political Finance Database, we define two sub-samples of countries: one where private donations to candidates are allowed, and the other one where such donations are banned. We then test our empirical model on these two sub-samples. Figure 1.7 below shows the coefficient estimates for the semesters around the election when Equations 2.1 (domestic awarded firms and elections in recipient countries) and 2.2 (foreign awarded firms and elections in supplier countries) of Table S.A16 are estimated for each of these two sub-samples (donations authorized or banned).

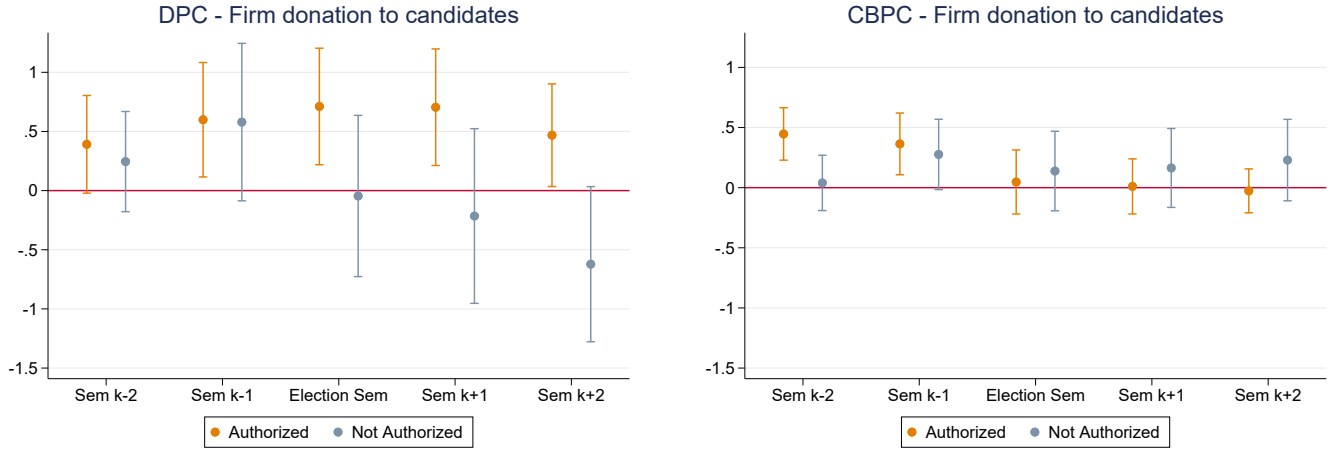
Results are clear-cut: both political cycles (domestic and cross-border) are only observed in countries where private donations to candidates are authorized, thus providing hints of cronyism in the allocation of World Bank procurement contracts around election semesters in recipient and supplier countries. In the sub-sample of countries that allow donations to candidates, the political cycle manifests around the election period in the case of local firms and elections in recipient countries. However, for foreign firms, the political cycle occurs before elections (as observed in the main regression) when the elections are taking place in their country of origin.

We complement these results by using data on politicians' public disclosure rules and practices collected by Djankov et al. (2010b).²² Again, according to our assumptions about the mechanisms for kick-back arrangements in the context of World Bank procurement, one could expect to see such cycles to occur in context where accountability towards taxpayers is weakly enforced. According to Djankov et al. (2010b) and their findings, public disclosure can thus be considered as a good

²¹At least, such was the case in 2018 as reported by the Political Finance Database produced by the Institute for Democracy and Electoral Assistance.

²²Where the politicians studied in Djankov et al. (2010b) are the members of the lower house of parliament, for 175 countries.

Figure 1.7: Political cycle where campaign financing is “allowed”



Notes:

Left graph: Coefficients estimated with recipient x year and semester x year fixed effects. Observations: 3,986 (authorized), 1,254 (banned). R²: 0.86 (authorized), 0.85 (banned). Robust standard errors clustered at the recipient x year level (1,993 authorized, 627 banned).

Right Graph: Coefficients estimated with recipient x year x semester, and supplier x recipient x year fixed effects. Observations: 23,960 (authorized), 12,084 (banned). R²: 0.89 (authorized), 0.92 (banned). Robust standard errors clustered at the supplier x year x semester level (3,658 authorized, 1,460 banned).

proxy for the (perceived) prevalence of corruption among politicians, as the latter is shown to decrease when disclosure of personal assets to the public helps identifying conflicts of interest as well as source of incomes. Consequently, we again divide our sample of recipient and supplier countries according to whether they have to (and do) disclose publicly their sources of income and conflicts of interest, and test our two hypotheses of political cycle on these sub-samples. Results of Table A4 in the Appendix first suggest that political cycle in World Bank procurement is observed in recipient countries where there is mandatory public disclosure but also where there is no practices or enforcement of public disclosure for politicians. Yet, the timing of the political cycle in countries where it is not mandatory seems to be more aligned with previous results about private donations to candidate. The existence of the cross-border political cycle is then only observed for countries where politicians do not need to report their personal assets (or usually do not do it) albeit, the timing significantly departs from previous findings, leaving therefore unclear the contribution of public disclosure practices to the realization of such a cycle. Nevertheless one can notice that where there is strong stringency regarding politicians’ assets transparency, the political cycle is negative, especially in the two semesters following the election semester. This pattern could be attributed to a more

stringent scrutiny of elected officials immediately after elections, aimed at preventing any suspicions of cronyism. This might lead them to distance themselves from the bidding process in recipient countries during the immediate post-election period. Such cautionary measures could be put in place to uphold transparency and avoid any appearance of favoritism or conflicts of interest.

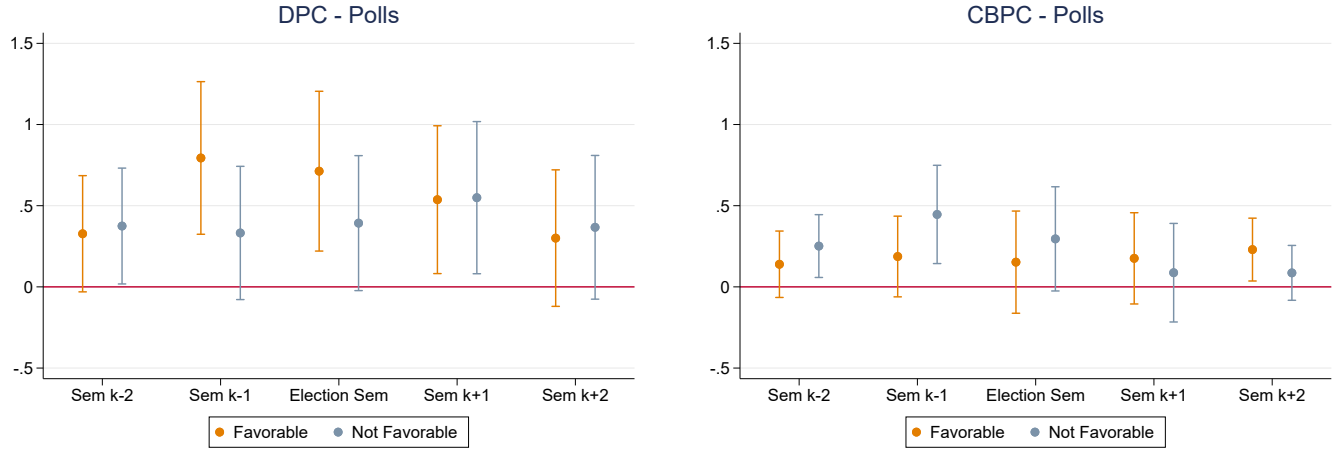
1.5.2 Mechanism #2: Political cycles to boost public image?

In addition to campaign financing, another factor that may account for the occurrence of these political cycles is the potential benefits that new public contracts could bestow in shaping voters' perceptions of candidates' competencies and capacities to stimulate economic activity and enhance their overall quality of life. The awarding of significant World Bank procurement contracts to domestic companies just prior to elections could prompt candidates, particularly the incumbent, to highlight their contribution, notably through the economic policies they have implemented thus far, to bolster the international competitiveness of national firms.

As a result, we can anticipate that these political cycles are likely to manifest predominantly in elections where there is a necessity to bolster the public image, such as when pre-election polls are not favorable to the incumbent candidate. Fortunately, the NELDA database provides this information, allowing us to partition our main sample into two sub-samples based on whether the elections were linked to unfavorable polls for the incumbent prior to the election. Figures 1.8 below displays the results. Regarding, the effect of elections in recipient countries, results are quite unexpected as the political cycle seems to occur primarily in context where prior-election polls are favourable to the incumbent. However, shifting attention to the cross-border political cycle, one can notice that this cycle appears mostly when polls are unfavourable to incumbent in the country of origin of foreign firms winning World Bank procurement abroad.

The improvement in the public image that incumbent candidates may gain from witnessing their national firms winning larger procurement contracts could potentially translate into additional job creation over the short to medium term, contingent upon the type of contracts secured. As a result, one might anticipate these political cycles to emerge in situations of escalating unemployment, especially prior to the election. In order to test such hypothesis, we retrieve annual figures for unemployment rates for both recipient and supplier countries and extend

Figure 1.8: Political cycle where public image is deteriorated



Notes:

Left graph: Coefficients estimated with recipient x year and semester x year fixed effects. Observations: 5,208 (favorable), 5,066 (not favorable). R^2 : 0.85 (favorable), 0.84 (not favorable). Robust standard errors clustered at the recipient x year level (2,604 favorable, 2,533 not favorable).

Right Graph: Coefficients estimated with recipient x year x semester, and supplier x recipient x year fixed effects. Observations: 29,232 (favorable), 30,004 (not favorable). R^2 : 0.89 (favorable), 0.90 (not favorable). Robust standard errors clustered at the supplier x year x semester level (4,732 favorable, 4,722 not favorable).

our specification 2.1 and 2.2 with interaction terms between dummy variables denoting semesters around election and the annual unemployment rate. Results are reported in Table A5 in the Appendix. Both estimate results indicate that political cycles, whether domestic or cross-border, are observed in the presence of increasing unemployment in countries where the election is being held. This phenomenon is particularly evident during the election semester in the recipient countries and two semesters before the election when it concerns elections taking place in the countries of origin of the supplying firms.

Lastly, one could also expect to find such political cycles in countries and political environments where there is more of a need to stand out from the other candidates. In a context such as autocracies (and to a lesser extent anocracies) where there is not much suspense regarding the outcome of the upcoming election, politicians would be expected to go to less trouble to tip the World Bank procurement process in favor of companies that can provide financial support to the election campaign. To test this assumption, we again divide our sample into different sub-categories by type of political system based on the Polity 2 assessment of democracy in the Polity V dataset. Table S.A17 in the supplementary appendix reports on results for both the domestic and cross-border political cycles regarding the average amount of

procurement won by local and foreign firms depending on the recipient and supplier country's political system, respectively.

We first observe that the domestic political cycle favoring local firms around elections is stronger in democratic recipient countries, *i.e.* where the outcome of the election is more uncertain and where election candidates would therefore need funds to improve their chances of being elected. This political cycle is also observed in countries that are not fully democratic (as defined in the Polity V dataset), albeit with significance at the 10% level only.

Turning then to the cross-border political cycle, Table S.A17 also supports our initial intuition of a stronger political cycle in countries where incumbents (and other candidates) need campaign financing because of the uncertainty of the upcoming election, which is most likely to be the case in democracies as opposed to autocracies.

Still on the idea of a distortion in the allocation of World Bank procurement where such is needed, Tables S.A18 and in the supplementary appendix display estimate results when the sample is divided depending on whether the election (in recipient and supplier countries respectively) was close or not, using the NELDA definition of a competitive election (whether the ruling party was confident or not before the election). While the results are not clear-cut for the domestic political cycle, those for the cross-border political cycle again lend support to the above arguments and point up the fact that politicians might be quicker to skew the allocation process when they need a favor in return from the firms winning procurement contracts or to use their success to restore their public image, which is probably more likely to be the case in a competitive election environment rather than for elections where one candidate significantly outstrips the others.

All in all, the above findings show that the allocation of the World Bank procurement contracts is liable to align with the electoral interests of the recipient and supplier countries, as both domestic and foreign companies appear to win on average larger contracts around elections in those countries, especially when private donations to candidates are allowed, when incumbents need to improve their public image, and when election results are uncertain.

1.6 Pressure tactics: how to tip the allocation process?

Yet, the mechanisms described above can only be at play if there are ways of influencing the World Bank's procurement allocation process. This section therefore focuses on these potential levers and attempts to assess their contribution to the

achievement of such political cycles.

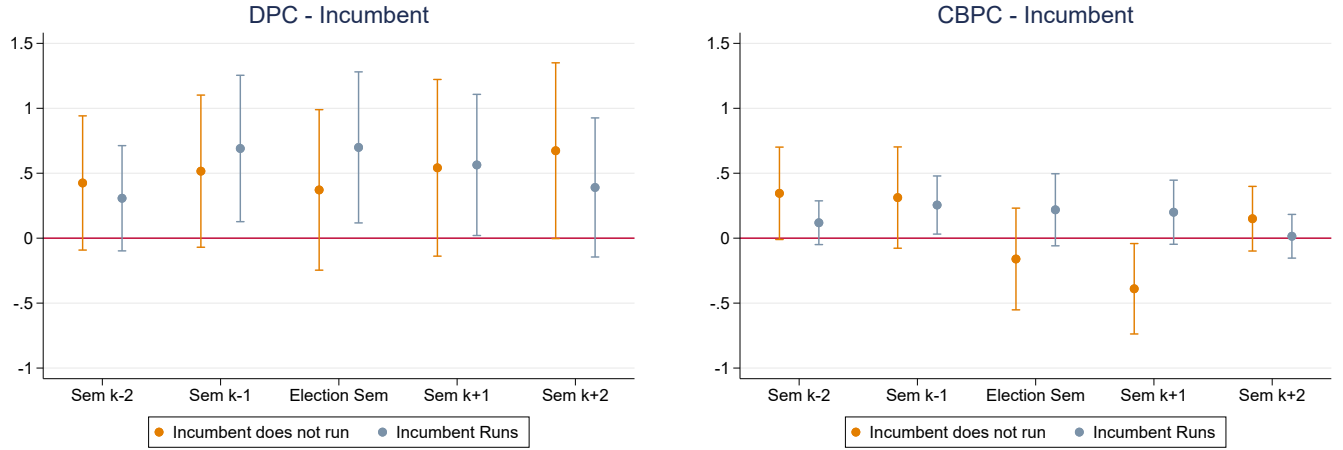
1.6.1 Political networks: the advantage of incumbency

One would first expect incumbents to have more power and the networks to influence the award of public contracts in contrast to elections where incumbents do not stand for another term. We consequently split the sample into two sub-groups depending on whether the incumbent runs or not. Given that these electoral environment characteristics (campaign funding and incumbency) are exogenous to the World Bank procurement allocation process, we believe that there could be no good reason, aside from cronyism, that could explain our observation of different effects on these two sets of sub-samples. Any variation in the effect of semesters around elections on the above-defined sub-samples would therefore let the proverbial cat out of the bag by revealing suggestive evidence of cronyism. The coefficient estimates reported in Figure 1.9²³ confirm this intuition for the domestic political cycle. A larger and more persistent political cycle is observed when the incumbent is running in the next election. The timing of the domestic political cycle is in line with our previous findings, with local companies awarded (on average) larger procurement contracts in the semester prior to and during the election.

Similarly, we also test whether the effect of an election on the average amount of a procurement contract won differs depending on the incumbent's electoral prospects in the supplier country. We run Equation 2.2 on the same two sub-samples. Our intuition is fairly similar to what we suspected for the domestic political cycle. Incumbents in supplier countries potentially benefit from wider networks that could facilitate their interference in the procurement contract allocation process. On an international scale, we believe that this assumption makes even more sense, since incumbents are more likely to have met recipient country officials in person over the course of their previous term of office and should thus be in a better position than their electoral competitors to reach the people in charge of selecting the winning company. Results reported in Figure 1.9 provide some support for the above assumptions as it can be observed that the cross-border political cycle emerges solely when the incumbent is running for another term, thus lending support to the hypothesis regarding the need for enough connections to skew the procurement contract allocation process abroad in favor of local companies.

²³Detailed regression results displayed in Table S.A19 in the supplementary appendix.

Figure 1.9: Political cycle where it is “easier”



Notes:

Left graph: Coefficients estimated with recipient x year and semester x year fixed effects. Observations: 3,630 (no incumb.) 4,534 (incumb.). R²: 0.84 (no incumb.), 0.84 (incumb.). Robust standard errors clustered at the recipient x year level (1,815 (no incumb.), 2,267 (incumb.)).

Right Graph: Coefficients estimated with recipient x year x semester, and supplier x recipient x year fixed effects. Observations: 19,610 (no incumb.) 32,012 (incumb.). R²: 0.90 (no incumb.), 0.89 (incumb.). Robust standard errors clustered at the supplier x year x semester level (3,344 (no incumb.) 4,340 (incumb.)).

1.6.2 Aid and colonial ties: the power of close partnerships

We next review other means of influence that could drive the cross-border political cycle (*i.e.* Equation 2.2). Among these alternative means, historical and current aid connections may also be considered as a way for supplier countries to exert influence. Identifying dyads of recipient and supplier countries by the amount of aid received and provided, respectively, we divide the entire sample into pairs of countries in which supplier countries are defined as significant aid partners of recipient countries.²⁴ Column (1) of Table A6 in the appendix displays a significant positive effect for the two semesters preceding the election in the supplier country, as in the main regressions, but that effect seems to hold mostly when recipient and supplier countries are characterized as significant aid partners.

The importance of aid partnership also emerges when looking at historical ties between countries. We interact the election variables with a dummy equal to one if the supplier- recipient pair shares a colonial history (built using the CEPII GeoDist

²⁴In keeping with Frot (2009), we define pairs of recipient and supplier countries as significant aid partners when the share of foreign aid provided by supplier country s in the total amount of aid granted to recipient country r is larger than the share of supplier country s in the total amount of aid provided worldwide by all donors.

database, Mayer and Zignago (2011)).²⁵ Table A7 in the appendix displays a significant positive effect for the two semesters preceding the election in the supplier country, especially when the supplier-recipient pair shares a colonial history. Firms from a supplier country without historical links with the recipient won on average 29% larger contracts two semesters before the election, whereas suppliers from a country sharing a colonial history with the recipient won 161% larger contracts over the same period. Given these results, it thus appears that supplier countries may be able to use their development cooperation and historical partnership to tip the award process in their favor, particularly as their elections approach.

Drawing on the findings of McLean (2015), who suggests that donors are more likely to support multilateral aid when their firms stand to gain from intervention by international organizations, the second channel we can find to explain the higher probability of winning more lucrative procurement contracts around elections concerns trade interests of bilateral donors. Focusing on the second configuration, where foreign firms (on average) win larger contracts in the semesters following elections in their country of origin, bilateral donors could be suspected of intervening (unofficially) in this award process by encouraging the recipient government to choose a firm from the donor country.

All sovereign states contribute to the World Bank's funds, but high-income countries contribute more than others given their greater financing capacities. Yet, although bilateral donors' contributions to the World Bank might be considered as altruistic, this view has been largely challenged by the existing literature. Indeed, while the literature has shown that the allocation of bilateral aid is to some extent driven by diplomatic interests (especially during the Cold War and War on Terror periods) and trade interests (particularly after the fall of the Soviet bloc) (Berthélemy and Tichit, 2004; ?; Fleck and Kilby, 2010), it has also highlighted similar evidence regarding multilateral aid, where funds are strategically allocated to countries in keeping with the interests of the largest bilateral donors (Kuziemko and Werker, 2006; Dreher et al., 2019, 2022b). Moreover, since 2005 and the Paris Declaration, most of the bilateral donors have committed to significantly reduce tied and partially tied aid, a type of development assistance that was commonplace throughout the 1970s, 1980s and 1990s (albeit starting to decline in the early 1990s). Given the academic evidence discussed above and the international context of a reduction in tied aid, it would be reasonable to assume that bilateral donors are

²⁵Only the main colonial empires were considered here: Belgium, France, Germany, Netherlands, Portugal, Russia, Spain and United Kingdom.

looking for other ways to obtain returns from their official development assistance, whether provided on a bilateral or multilateral basis. Moreover, it could also be argued that a supplier country's quest for trade would more likely be served in a context where the supplier countries' governments also have individual interests, especially electoral interests, since the above findings point to greater room for maneuver around these periods.

In order to test the above assumption, we re-run our gravity model (*i.e.* Equation 2.2), extending the model with interaction terms between the political cycle dummy variables and a variable measuring the annual share of tied aid in the total aid committed by supplier countries (where the awarded foreign firms are from).²⁶ Results in Table A8 suggest that such tied aid-for-procurement substitution is at play around election semesters in the firms' home country. To be more precise, the larger contracts won around election semesters in the supplier country are much smaller when winning firms are from countries with a large share of tied bilateral aid in their total official development assistance. In other words, firms from supplier countries that reduced the share of tied bilateral aid won even larger procurement contracts around their election semesters. This is suggestive evidence that procurement in developing countries could be used by traditional donors to offset the loss of economic returns due to the reduction in their tied aid. In addition to substitution, this result implies that traditional donors (*i.e.* countries with the most tied aid) are likely to drive the cross-border political cycle.

1.6.3 Board memberships: the importance of high-level meetings

Lastly, and as discussed in the first sections of the paper, such a political cycle would be unlikely if there were no opportunities for negotiation or means of influencing the award process for the foreign companies of interest to politicians standing in the next election. In line with this idea, the above results show that the likelihood of winning a larger procurement contract increases when the incumbent in the supplier country is running for another term. This suggests that international political connections, whether direct or indirect (discussion forums in international institutions, for example), could also constitute a way of tipping the allocation process in favor of companies from the supplier country.

World Bank Boards could be places where just such connections and influence

²⁶Note that in order to match annual tied aid commitments with the semester dimension of our data, we report the same amount of tied aid commitments for two consecutive semesters of the same year.

can be used. Indeed, the literature on the political economy of foreign aid provides ample evidence that membership of international institutions is often accompanied by certain ‘privileges’ (Dreher et al., 2009b; Vreeland, 2011; Dreher et al., 2019). In line with this literature, it would be reasonable to assume that membership of the board of the institution financing the procurement contracts could be one of the transmission channels. Executive directors are elected or appointed (for the largest World Bank contributors) every two years, and each candidate is elected by a country or sub-group of countries²⁷ (e.g. in 2003, the elected Austrian board representative obtained the majority of votes from Austria, Belarus, Belgium, Czech Republic, Hungary, Kazakhstan, Luxembourg, Slovak Republic, Slovenia, and Turkey). Given the relatively short term of office, membership of the board would therefore provide a small window of opportunity to negotiate and arbitrate decisions in favor of the country represented. Therefore, it is tempting to think that countries not receiving World Bank funds could take advantage of this private discussion arena to tip the award of public contracts in favor of their national companies, especially if they shared their term of office with representatives of recipient countries. We drew information from World Bank Annual Reports from 1995 to 2019 on the composition of World Bank Boards of Executive Directors. From this, we identified board membership for each country and for each year in the period of study. We then re-ran our main specification designed to capture the cross-border political cycle (*i.e.* Equation 2.2 on a number of sub-samples: 1) one where both recipient and supplier country sat on the board of executive directors; 2) one where only the supplier country sat on the board; 3) one where only the recipient country sat on the board; and 4) one where neither recipient nor supplier country sat on the board. Table 1.4 below shows the results for these sub-sample estimates.

The results in the first column show a strong political cycle when both recipient and supplier country (the country from which the winning firms originate) had an elected representative on the World Bank Board of Directors in the same period of time, thus lending more weight to our hypothesis regarding the Board as a “place for arrangements”. However, these large semester effects around elections are not found to be as strong in the case of other sub-sample estimates, except in column (4) where none of the stakeholders sat on the Board. This suggests that in the absence of this discussion arena, the supplier country may find other ways of tipping the

²⁷If a country is a large contributor to the World Bank budget, its vote carries a greater weight and it can choose a director directly. If the country is not a large contributor, it cannot choose directly and has to team up with other countries in order to choose a director.

Table 1.4: Cross-border political cycle - By presence at the Board of Executive Directors

Dep. var.:	(1)	(2)	(3)	(4)
	<i>AverageAmount_{s,r,k,t}</i>			
	Recip and Supp	Just Supp	Just Recip	None
Semester k-2 $_{s,k,t}$	0.114 (0.321)	-0.064 (0.119)	0.832 (0.412)**	0.508 (0.207)**
Semester k-1 $_{s,k,t}$	1.330 (0.505)***	-0.020 (0.157)	0.677 (0.437)	0.662 (0.286)**
Election Semester $_{s,k,t}$	0.849 (0.492)*	0.073 (0.202)	0.543 (0.474)	0.431 (0.301)
Semester k+1 $_{s,k,t}$	1.430 (0.476)***	0.296 (0.193)	0.735 (0.459)	-0.257 (0.243)
Semester k+2 $_{s,k,t}$	1.012 (0.302)***	0.309 (0.134)**	0.672 (0.398)*	-0.181 (0.192)
<i>N</i>	2,156	20,196	1,358	15,096
<i>R</i> ²	0.89	0.90	0.86	0.91
Recip x Year x Sem Fixed Effect	Yes	Yes	Yes	Yes
Supp x Recip x Year Fixed Effect	Yes	Yes	Yes	Yes
N Supp x Year x Sem (clusters)	692	1,062	992	4,348

Notes: Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$.

award process in their favor, as evidenced by previous results on historical ties and significant aid partnership, the latter being supported by results in Figure A4 in the appendix, where we specifically differentiate countries that are not board members together, according to the importance of their aid relationship.

1.7 Conclusion

Building on two- to three-dimensional models using data from the World Bank's Contract Awards Database and the National Elections across Democracy and Autocracy dataset, we put forward the existence of both a domestic and cross-border political cycles for World Bank procurement contracts. To be more precise, local firms are found to win significantly larger World Bank contracts around election semesters in recipient countries. Further heterogeneity analysis suggests that this domestic political cycle occurs in particular when local firms are allowed to donate to candidates and when the incumbent government is running for re-election. In addition, foreign firms are found to win significantly larger World Bank contracts in a given recipient country one semester before an election in their country of origin, thereby suggesting an cross-border political cycle. This cross-border political

cycle is found in particular when corporations are allowed to fund candidates, when the incumbent is running for re-election and when the elections are uncertain. The supplier-to-recipient influence is at play when both can easily meet on the World Bank Board of Directors, and when the supplier has significant economic and historical influence over the recipient.

Our results contribute to the existing literature on the political economy of international organizations. They show that development projects funded through World Bank procurement contracts are indeed likely to be used as an arrangement between private companies and both recipient and supplier governments to serve electoral interests.

Nevertheless, our results do not reveal cronyism, albeit they strongly suggest it. Future research on this subject should thus refine the analysis at the firm-level in order to see whether politically connected firms are indeed those which benefit the most from such domestic and cross-border political cycles. Furthermore, the results of our study highlight the need for further research on the political and economic impacts of these distortions in the allocation of World Bank procurement.

Appendix

Table A1: DPC - by allocation method

	(1)	(2)	(3)	(4)
Dep. var.:	<i>AverageAmount_{r,k,t}</i>			
	Local firms			
	ICB	QCBS	SSS	NCB
Semester k-2 r,k,t	-0.711 (0.310)**	0.527 (0.373)	0.007 (0.263)	0.140 (0.292)
Semester k-1 r,k,t	0.625 (0.437)	0.422 (0.417)	-0.355 (0.381)	0.511 (0.357)
Election Semester r,k,t	0.682 (0.475)	0.574 (0.448)	-0.494 (0.412)	0.067 (0.367)
Semester k+1 r,k,t	0.154 (0.440)	-0.248 (0.481)	0.465 (0.392)	0.228 (0.355)
Semester k+2 r,k,t	0.173 (0.335)	-0.967 (0.434)**	0.669 (0.365)*	-0.103 (0.254)
N	2,074	2,142	2,474	1,804
R^2	0.84	0.70	0.73	0.66
Year x Sem. FE	Yes	Yes	Yes	Yes
Recip. x Year FE	Yes	Yes	Yes	Yes
N Supp. x Year (clusters)	1,037	1,071	1,237	902

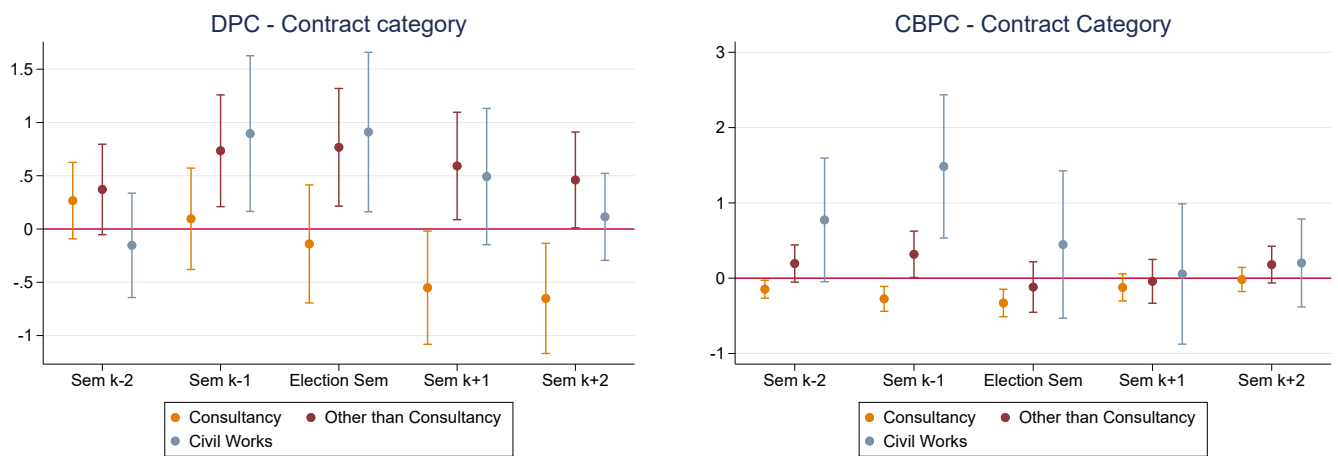
Notes: Robust standard errors in parentheses, clustered at the supplier×year level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$.

Table A2: CBPC - by allocation method

	(1)	(2)	(3)
Dep. var.:	<i>AverageAmount_{s,r,k,t}</i>		
	Foreign firms		
	ICB	QCBS	SSS
Semester k-2 s,k,t	0.196 (0.178)	-0.139 (0.097)	-0.695 (0.261)***
Semester k-1 s,k,t	0.768 (0.264)***	-0.218 (0.130)*	-1.203 (0.344)***
Election Semester s,k,t	0.344 (0.283)	-0.254 (0.134)*	-0.664 (0.355)*
Semester k+1 s,k,t	0.217 (0.255)	-0.066 (0.124)	0.112 (0.293)
Semester k+2 s,k,t	0.228 (0.174)	-0.003 (0.109)	0.078 (0.255)
N	12,104	16,016	9,432
R^2	0.87	0.81	0.84
Recip. x Year x Sem. FE	Yes	Yes	Yes
Supp. x Recip. x Year FE	Yes	Yes	Yes
N Supp. x Year x Sem. (clusters)	2,874	3,654	3,150

Notes: Robust standard errors in parentheses, clustered at the supplier×year×semester level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$.

Figure A1: Political cycle by contract category

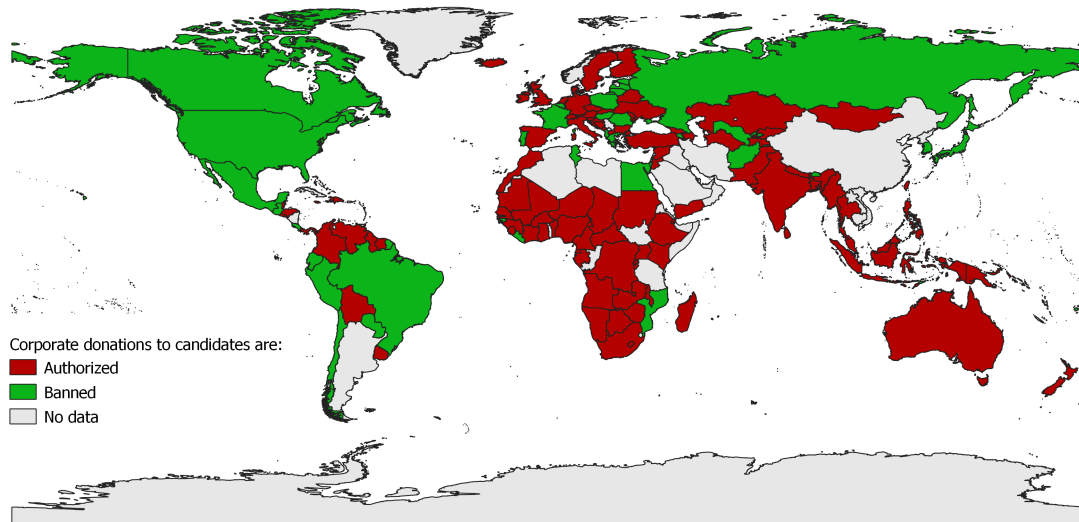


Notes:

Left graph: Coefficients estimated with recipient x year and semester x year fixed effects. Observations: 3,920 (consultancy), 4,078 (other than consultancy), 2,192 (civil works). R^2 : 0.73 (consultancy), 0.77 (other than consultancy), 0.82 (civil works). Robust standard errors clustered at the recipient x year level (1,960 consultancy, 2,039 other than consultancy, 1,096 civil works). See Table S.A14 in the supplementary appendix for detailed regression table.

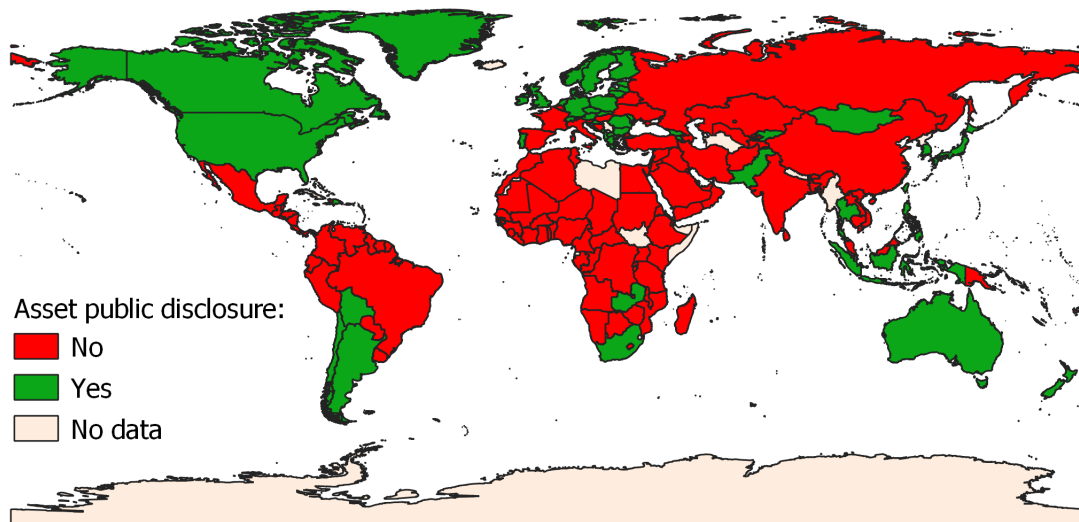
Right Graph: Coefficients estimated with recipient x year x semester, and supplier x recipient x year fixed effects. Observations: 26,116 (consultancy), 15,248 (other than consultancy), 1,460 (civil works). R^2 : 0.82 (consultancy), 0.88 (other than consultancy), 0.80 (civil works). Robust standard errors clustered at the supplier x year x semester level (4,936 consultancy, 3,518 other than consultancy, 972 civil works). See Table S.A15 in the supplementary appendix for detailed regression table.

Figure A2: Map of countries authorizing/banning corporate donations to candidate



Source: Political Finance Database

Figure A3: Map of countries where disclosure of politicians' assets is mandatory or common



Source: Djankov et al. (2010b)

Table A3: Interaction with mean timing

Dep. Var.:	DPC <i>MeanAmount</i> _{r,t,q}	CBPC <i>MeanAmount</i> _{s,r,t,q}
Semester q-2 <i>r,t,q</i> or <i>s,t,q</i>	-0.037 (0.358)	0.156 (0.107)
Semester q-1 <i>r,t,q</i> or <i>s,t,q</i>	1.153 (0.373)***	0.446 (0.162)***
Election Semester <i>r,t,q</i> or <i>s,t,q</i>	0.634 (0.325)*	0.361 (0.178)**
Semester q+1 <i>r,t,q</i> or <i>s,t,q</i>	1.004 (0.359)***	0.374 (0.160)**
Semester q+2 <i>r,t,q</i> or <i>s,t,q</i>	0.618 (0.455)	0.241 (0.120)**
Mean Timing <i>s,t,q</i>	0.063 (0.045)	0.394 (0.030)***
Mean Timing <i>s,r,t,q</i> x q-2	0.093 (0.079)	0.013 (0.041)
Mean Timing <i>s,r,t,q</i> x q-1	-0.176 (0.075)**	0.032 (0.047)
Mean Timing <i>s,r,t,q</i> x Elec	-0.030 (0.068)	-0.028 (0.043)
Mean Timing <i>s,r,t,q</i> x q+1	-0.171 (0.075)**	-0.016 (0.044)
Mean Timing <i>s,r,t,q</i> x q+2	-0.135 (0.088)	-0.050 (0.039)
<i>N</i>	5,902	41,966
<i>R</i> ²	0.85	0.90
Recipient x Year FE	Yes	Yes
Year x Sem FE	Yes	No
Recip x Year x Sem FE	No	Yes
Supp x Recip x Year FE	No	Yes
N Recip x Year (clusters)	2,951	
N Supp x Year x Sem (clusters)		5,818

Robust standard errors in parentheses, clustered at the level reported in each column.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A4: Domestic Political Cycle and Politicians' assets public disclosure

Dep. Var.:	<i>AverageAmount_{r,k,t}</i>		<i>AverageAmount_{s,r,k,t}</i>	
	Local firms		Foreign firms	
Elections in:	Recipient countries Semester _{r,k,t}		Supplier countries Semester _{s,k,t}	
Public disclosure:	Yes	No	Yes	No
Semester k-2	-0.251 (0.334)	0.615 (0.209)***	0.055 (0.091)	-0.108 (0.282)
Semester k-1	0.841 (0.459)*	0.545 (0.235)**	0.085 (0.136)	0.133 (0.319)
Election Semester	1.087 (0.500)**	0.390 (0.241)	-0.031 (0.159)	0.228 (0.335)
Semester k+1	-0.133 (0.372)	0.476 (0.258)*	-0.278 (0.154)*	0.091 (0.290)
Semester k+2	-0.861 (0.287)***	0.411 (0.251)	-0.375 (0.131)***	0.529 (0.211)***
N	3,504	2,102	22,120	12,224
R ²	0.87	0.85	0.88	0.91
Recip. × year FE	Yes	Yes	No	No
Sem. × Year FE	Yes	Yes	No	No
Recip. × Sem. <i>times</i> Year FE	No	No	Yes	Yes
Supp. × Recip. <i>times</i> Year FE	No	No	Yes	Yes
N Recip. × Year (clusters)	592	2,171		
N Supp. × Sem. <i>times</i> Year (clusters)			2,032	3,084

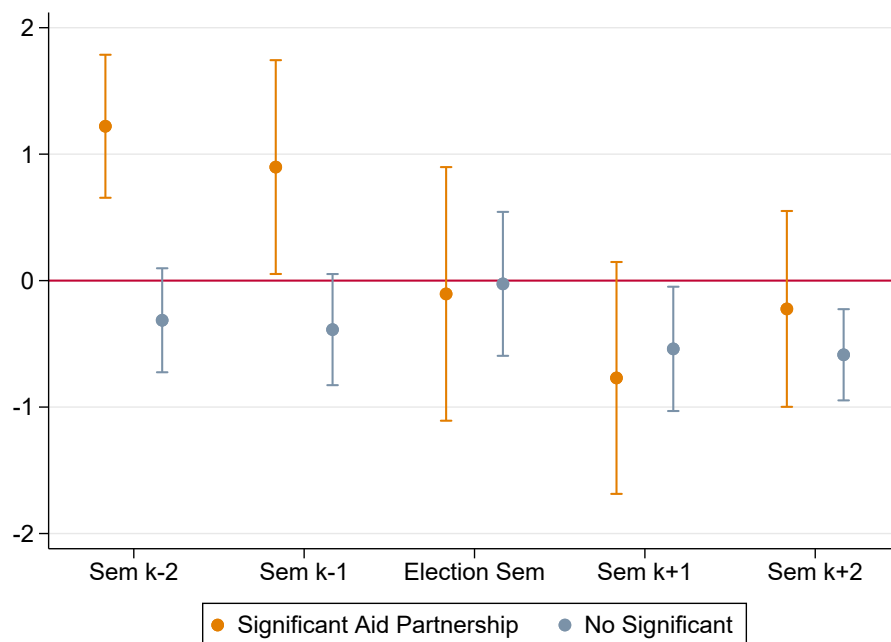
Notes: Robust standard errors in parentheses, clustered at the level reported in each column.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$.

Table A5: Political Cycles - interaction with unemployment

Dep. Var.:	<i>AverageAmount</i> _{r,k,t}	<i>AverageAmount</i> _{s,r,k,t}
	Local firms	Foreign firms
Elections in:	Recipient countries	Supplier countries
	Semester _{r,k,t}	Semester _{s,k,t}
Semester k-2 x Unemployt	0.007 (0.028)	0.066 (0.021)***
Semester k-1 x Unemployt	0.082 (0.044)*	0.034 (0.023)
Election Semester x Unemployt	0.139 (0.040)***	0.046 (0.024)*
Semester k+1 x Unemployt	0.059 (0.033)*	0.031 (0.024)
Semester k+2 x Unemployt	0.058 (0.023)**	-0.027 (0.017)
Semester k-2	0.206 (0.338)	-0.312 (0.170)*
Semester k-1	-0.060 (0.392)	0.085 (0.212)
Election Semester	-0.663 (0.361)*	-0.189 (0.237)
Semester k+1	-0.136 (0.342)	-0.046 (0.221)
Semester k+2	-0.344 (0.218)	0.332 (0.165)**
<i>N</i>	5,718	41,820
<i>R</i> ²	0.85	0.88
Year x Sem. FE	Yes	No
Recip. x Year FE	Yes	No
Recip. x Year x Sem. FE	No	Yes
Supp. x Recip. x Year FE	No	Yes
N Recip. x Year (clusters)	2,859	-
N Supp. x Year x Sem. (clusters)	-	5,720

Notes: Robust standard errors in parentheses, clustered at the level reported in each column. Note that **Unemployt** is the annual unemployment rate of recipient and supplier country *i* committed for year *t*, in estimates of domestic and cross-border political cycles, respectively. The variable in level (not in interaction) is therefore captured by the set of Recip. x Year in the domestic political cycle regression and by Supplier x recipient x Year fixed effects in the estimates of the cross-border political cycle. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Figure A4: CBPC - Supplier and recipient without board memberships, by aid partnership status



Notes: Observations: 2,104 (sign. partnership), 10,926 (no sign.). R^2 : 0.95 (sign. partnership), 0.89 (no sign.). Robust standard errors clustered at the supplier x year x semester level (954 sign. partnership, 4,014 no sign.).

Table A6: CBPC - by significance of aid partnership

Dep. var.:	<i>AverageAmount</i> _{s,r,k,t}	
	Significant aid partner	Not significant aid partner
Semester k-2 _{s,k,t}	0.279 (0.164)*	0.032 (0.102)
Semester k-1 _{s,k,t}	0.624 (0.197)***	-0.098 (0.121)
Election Semester _{s,k,t}	-0.112 (0.202)	-0.136 (0.142)
Semester k+1 _{s,k,t}	-0.314 (0.204)	-0.065 (0.125)
Semester k+2 _{s,k,t}	-0.122 (0.175)	0.003 (0.108)
<i>N</i>	10,158	28,782
<i>R</i> ²	0.92	0.89
Recip x Year x Sem Fixed Effect	Yes	Yes
Supp x Recip x Year Fixed Effect	Yes	Yes
N Supp x Year x Sem (clusters)	1,828	5,538

Notes: Robust standard errors in parentheses, clustered at the supplier×year×semester level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$.

Table A7: CBPC - Interaction with former colonial history

Dep. var.:	<i>Average Amount</i> _{<i>s,r,k,t</i>}
Former Colony _{<i>s,r</i>} x Semester k-2 _{<i>s,k,t</i>}	0.681 (0.296)**
Former Colony _{<i>s,r</i>} x Semester k-1 _{<i>s,k,t</i>}	0.981 (0.377)***
Former Colony _{<i>s,r</i>} x Election Semester _{<i>s,k,t</i>}	-0.179 (0.478)
Former Colony _{<i>s,r</i>} x Semester k+1 _{<i>s,k,t</i>}	0.372 (0.473)
Former Colony _{<i>s,r</i>} x Semester k+2 _{<i>s,k,t</i>}	-0.161 (0.171)
Semester k-2 _{<i>s,k,t</i>}	0.167 (0.086)*
Semester k-1 _{<i>s,k,t</i>}	0.344 (0.134)***
Election Semester _{<i>s,k,t</i>}	0.179 (0.152)
Semester k+1 _{<i>s,k,t</i>}	0.168 (0.139)
Semester k+2 _{<i>s,k,t</i>}	0.125 (0.097)
<i>N</i>	41,966
<i>R</i> ²	0.88
Recip. x Year x Sem. FE	Yes
Supp. x Recip. x Year FE	Yes
N Supp. x Year x Sem. (clusters)	5,818

Notes: Robust standard errors in parentheses, clustered at the supplier×year×semester level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A8: CBPC - Interaction with share tied aid

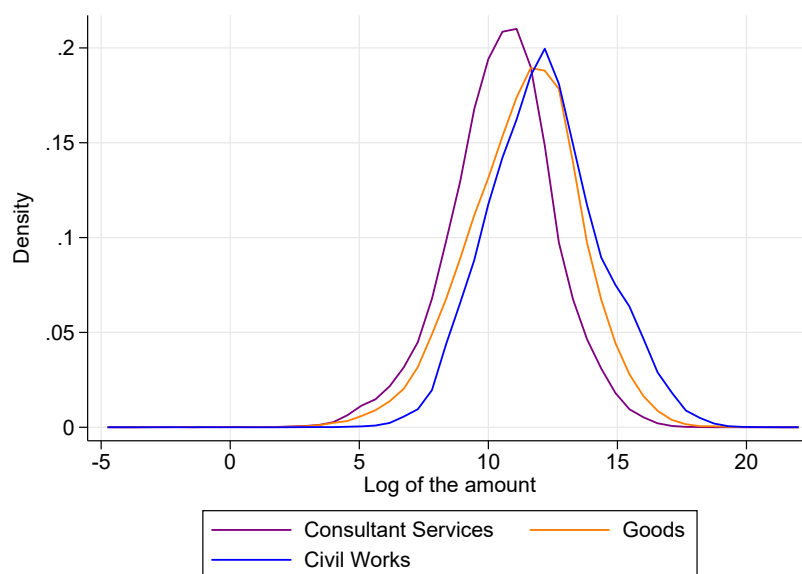
Dep. var.:	<i>AverageAmount</i> _{<i>s,r,k,t</i>}
Election in	Supplier: Semester k _{<i>s,k,t</i>}
Semester k-2 x Unemploy	0.016 (0.380)
Semester k-1 x Tied Aid _{<i>s,t</i>}	-1.175 (0.473)**
Elec Semester x Tied Aid _{<i>s,t</i>}	-1.909 (0.570)***
Semester k+1 x Tied Aid _{<i>s,t</i>}	-1.972 (0.532)***
Semester k+2 x Tied Aid _{<i>s,t</i>}	-1.838 (0.401)***
Semester k-2	0.171 (0.101)*
Semester k-1	0.442 (0.142)***
Election Semester	0.321 (0.158)**
Semester k+1	0.351 (0.143)**
Semester k+2	0.281 (0.101)***
<i>N</i>	41,966
<i>R</i> ²	0.88
Supp. x Recip. x Year Fixed Effect	Yes
Recip. x Year x Sem. Fixed Effect	Yes
N Supp. x Year x Sem. (clusters)	5,818

Notes: Robust standard errors in parentheses, clustered at the supplier×year×semester level. Note that Tied Aid_{*s,t*} is the annual share of foreign aid of supplier country *i* committed for year *t*. The variable in level (not in interaction) is therefore captured by the set of Supplier x recipient x Year fixed effects. Recipient x year x semester; and supplier x recipient x year fixed effects are included. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$.

Supplementary Appendix

Statistics and main regressions

Figure S.A1: Density function of USD amounts per contract with respect to contract's category



Source: Authors' calculation.

Table S.A1: Summary of the dimension used

Election		
Firm	<i>Recipient</i>	<i>Supplier</i>
<i>Recipient</i>	Domestic political cycle; local firms	-
<i>Supplier</i>	Domestic political cycle; foreign firms	International political cycle

Table S.A2: Domestic and Cross-border Political Cycles - Main regression

Dep. Var.:	<i>AverageAmount</i> _{<i>r,k,t</i>}	<i>AverageAmount</i> _{<i>s,r,k,t</i>}
	Local firms	Foreign firms
Elections in:	Recipient countries Semester _{<i>r,k,t</i>}	Supplier countries Semester _{<i>s,k,t</i>}
Semester k-2	0.275 (0.189)	0.171 (0.085)**
Semester k-1	0.677 (0.260)***	0.362 (0.128)***
Election Semester	0.599 (0.266)**	0.171 (0.146)
Semester k+1	0.443 (0.244)*	0.186 (0.135)
Semester k+2	0.219 (0.221)	0.125 (0.094)
<i>N</i>	5,902	41,966
<i>R</i> ²	0.84	0.88
Year x Sem FE	Yes	No
Recip x Year FE	Yes	No
Recip x Year x Sem FE	No	Yes
Supp x Recip x Year FE	No	Yes
N Recip x Year (clusters)	2,951	-
N Supp x Year x Sem (clusters)	-	5,818

Notes: Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Robustness checks

Figure S.A2: Total amount received by recipient countries around their elections

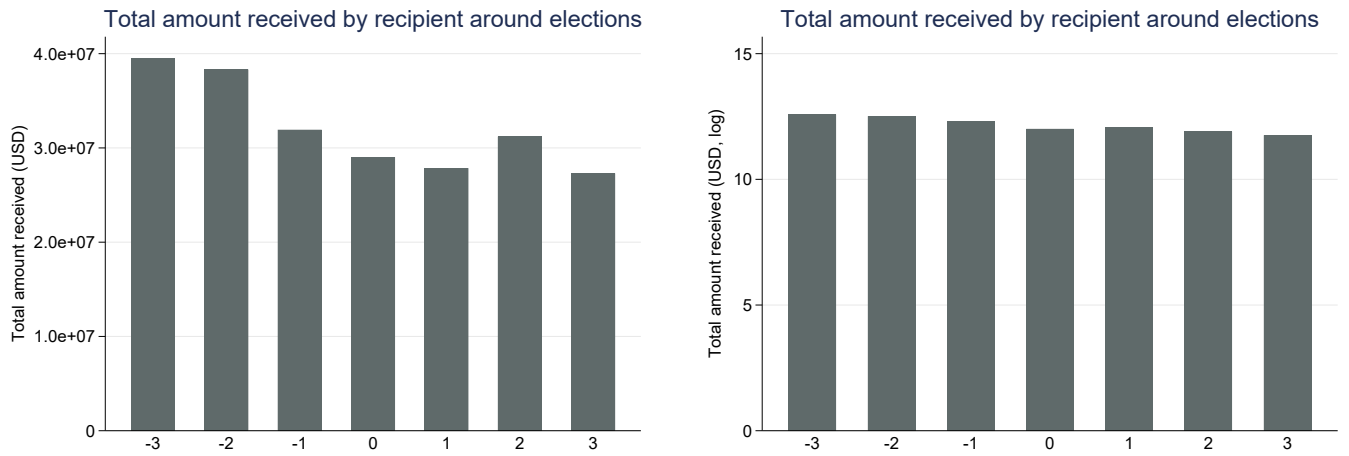


Table S.A3: DPC - Effect of election in recipient countries - Other dep. variables

	(1)	(2)	(3)
Dep. Var.:	$TA_{r,k,t}$	$NC_{r,k,t}$	$AA_{r,k,t}$
	Local firms		
Semester k-2 r,t,k	0.294 (0.134)**	0.037 (0.044)	0.275 (0.189)
Semester k-1 r,t,k	0.127 (0.161)	0.004 (0.056)	0.677 (0.260)***
Election Semester r,t,k	0.000 (0.181)	-0.049 (0.060)	0.599 (0.266)**
Semester k+1 r,t,k	-0.035 (0.170)	-0.137 (0.058)**	0.443 (0.244)*
Semester k+2 r,t,k	0.001 (0.118)	-0.094 (0.042)**	0.219 (0.221)
N	5,902	5,902	5,902
R^2	0.94	0.84	0.84
Year x Sem FE	Yes	Yes	Yes
Recip x Year FE	Yes	Yes	Yes
N Supp x Year (clusters)	2,951	2,951	2,951

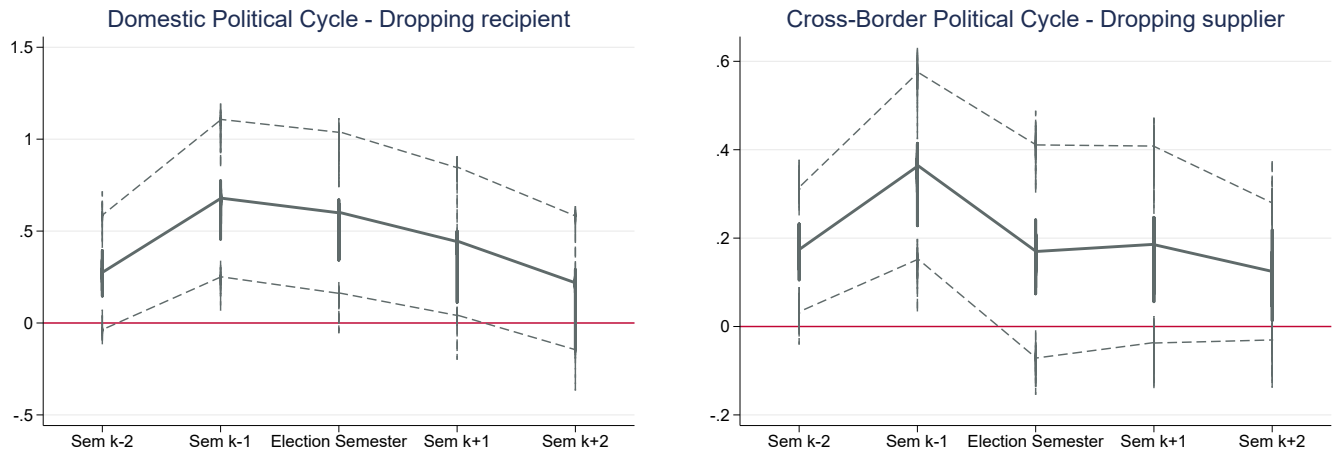
Notes: Robust standard errors in parentheses. TA , NC , AA stand for Total Amount of procurement (in USD), Number of Contracts, and Average Amount per contract, respectively. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table S.A4: CBPC - Effect of election in supplier countries - Alternative dependent variables

	(1)	(2)	(3)
Dep. Var.:	$TA_{s,k,t}$	$NC_{s,k,t}$	$AA_{s,k,t}$
	Foreign firms		
Semester k-2 s,t,k	0.072 (0.086)	0.019 (0.024)	0.171 (0.085)**
Semester k-1 s,t,k	0.161 (0.129)	0.008 (0.029)	0.362 (0.128)***
Election Semester r,t,k	0.007 (0.146)	-0.012 (0.030)	0.171 (0.146)
Semester k+1 s,t,k	0.019 (0.142)	-0.007 (0.029)	0.186 (0.135)
Semester k+2 s,t,k	0.018 (0.098)	0.004 (0.022)	0.125 (0.094)
N	41,966	41,966	41,966
R^2	0.90	0.30	0.88
Recip x Year x Sem Fixed Effect	Yes	Yes	Yes
Supp x Recip x Year Fixed Effect	Yes	Yes	Yes
N Supp x Year x Sem (clusters)	5,818	5,818	5,818

Notes: Robust standard errors in parentheses. TA , NC , AA stand for Total Amount of procurement (in USD), Number of Contracts, and Average Amount per contract, respectively. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Figure S.A3: DPC - Checking for Outliers



Notes:

Left graph: Coefficients estimated with recipient x year and semester x year fixed effects. Robust standard errors are clustered at the recipient x year level.

Right Graph: Coefficients estimated with recipient x year x semester, and supplier x recipient x year fixed effects. Robust standard errors are clustered at the supplier x year x semester level.

Table S.A5: DPC - Dropping inconsistent election

Dep. Var.: $AvAmount_{r,k,t}$	Baseline	No Inconsistent
Semester-2 $_{r,k,t}$	0.275 (0.189)	0.296 (0.207)
Semester-1 $_{r,k,t}$	0.677 (0.260)***	0.760 (0.281)***
Election Semester $_{r,k,t}$	0.599 (0.266)**	0.694 (0.294)**
Semester+1 $_{r,k,t}$	0.443 (0.244)*	0.483 (0.276)*
Semester+2 $_{r,k,t}$	0.219	0.301
N	5,902	5,232
R^2	0.85	0.84
Year x Sem FE	Yes	Yes
Recip \times Year FE	Yes	Yes
N Supp \times Year (clusters)	2,951	2,616

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table S.A6: CBPC - Dropping inconsistent election

Dep. Var.: $AvAmount_{s,r,k,t}$	Baseline	No Inconsistent
Semester-2 $_{s,k,t}$	0.171 (0.085)**	0.120 (0.106)
Semester-1 $_{s,k,t}$	0.362 (0.128)***	0.354 (0.141)**
Election Semester $_{s,k,t}$	0.171 (0.146)	-0.027 (0.147)
Semester+1 $_{s,k,t}$	0.186 (0.135)	0.067 (0.129)
Semester+2 $_{s,k,t}$	0.125 (0.094)	0.256 (0.108)**
N	41,966	33,004
R^2	0.90	0.89
Recip \times Year \times Sem FE	Yes	Yes
Supp \times Recip \times Year FE	Yes	Yes
N Supp \times Year \times Sem (clusters)	5.818	4,922

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table S.A7: Countries not covered in NELDA, added by the authors

Country	Election Year	Semester	Date (dd/mm)	Country	Election Year	Semester	Date (dd/mm)
Anguilla	1994	S1	16/03	Hong Kong	1994	S2	18/11
	1999	S1	04/03		1999	S2	28/11
	2000	S1	03/03		2003	S2	23/11
	2005	S1	21/02		2007	S2	18/11
	2010	S1	15/02		2011	S2	06/11
	2015	S1	22/04		2015	S2	22/11
	2020	S1	29/06		2019	S2	24/11
Cayman Islands	1996	S2	20/11	Montserrat	1996	S2	11/11
	2000	S2	08/11		2001	S1	02/04
	2005	S1	11/05		2006	S1	31/05
	2009	S1	20/05		2009	S2	08/09
	2013	S1	22/05		2014	S2	11/09
	2017	S1	24/05		2019	S2	18/11
China	1998	S1	05/03	Puerto Rico	1996	S2	05/11
	2003	S1	05/03		2000	S2	07/11
	2008	S1	05/03		2004	S2	02/11
	2013	S1	05/03		2008	S2	04/11
	2018	S1	05/03		2012	S2	06/11
Cook Islands	1999	S1	16/06	2016	S2	08/11	
	2004	S2	07/09	2020	S2	03/11	
	2006	S2	27/09	Somalia	2012	S2	10/09
	2010	S2	17/11		2017	S1	08/02
	2014	S2	09/07	UAE	2006	S2	16/12
	2018	S1	14/06		2011	S2	24/09
Gibraltar	1996	S1	16/06	2015	S2	03/10	
	2000	S1	10/02	2019	S2	05/10	
	2003	S2	28/11	Virgin Islands	1995	S1	20/02
	2007	S2	11/10		1999	S1	17/06
	2011	S2	08/12		2003	S1	16/06
	2015	S2	26/11		2007	S2	20/08
	2019	S2	17/10		2011	S2	07/11
Guam	1998	S2	03/11	2015	S1	08/06	
	2002	S2	05/11	2019	S1	25/02	
	2006	S2	07/11	Palestine	1996	S1	20/01
	2010	S2	02/11		2005	S1	09/01
	2014	S2	04/11				
	2018	S2	06/11				

Source: Wikipedia

Table S.A8: DPC - Without added elections

	(1)	(2)
Dep. var.:	<i>AverageAmount_{r,k,t}</i>	
	Local firms	
	Baseline	Without
Semester k-2 r,k,t	0.275 (0.189)	0.269 (0.190)
Semester k-1 r,k,t	0.677 (0.260) ^{***}	0.713 (0.269) ^{***}
Election Semester r,k,t	0.599 (0.266) ^{**}	0.625 (0.276) ^{**}
Semester k+1 r,k,t	0.443 (0.244) [*]	0.496 (0.256) [*]
Semester k+2 r,k,t	0.219 (0.221)	0.265 (0.237)
N	5,902	5,902
R^2	0.84	0.84
Year x Sem FE	Yes	Yes
Recip x Year FE	Yes	Yes
N Supp x Year (clusters)	2,951	2,951

Notes: Robust standard errors in parentheses. * $p < 0.10$,
** $p < 0.05$, *** $p < 0.010$

Table S.A9: Cross-Border Political Cycle - Without added elections

	(1)	(2)
Dep. var.:	<i>MeanAmount</i> _{<i>s,r,k,t</i>}	
	Baseline	Without
Semester k-2 _{<i>s,k,t</i>}	0.171 (0.085)**	0.178 (0.089)**
Semester k-1 _{<i>s,k,t</i>}	0.362 (0.128)***	0.210 (0.122)*
Election Semester _{<i>s,k,t</i>}	0.171 (0.146)	0.089 (0.140)
Semester k+1 _{<i>s,k,t</i>}	0.186 (0.135)	0.078 (0.133)
Semester k+2 _{<i>s,k,t</i>}	0.125 (0.094)	0.036 (0.106)
<i>N</i>	41,966	41,966
<i>R</i> ²	0.88	0.88
Supplier Fixed Effect	Yes	Yes
Recip Fixed Effect	Yes	Yes
Recip x Supp Fixed Effect	Yes	Yes
Year x Sem Fixed Effect	Yes	Yes
Recip x Year x Sem Fixed Effect	Yes	Yes
Supp x Year Fixed Effect	Yes	Yes
Supp x Recip x Year Fixed Effect	Yes	Yes
N Supp x Year x Sem (clusters)	5,818	5,818

Standard errors in parentheses clustered at supplier, year, semester level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table S.A10: Main regressions - multiple-way clustering

Dep. Var.:	<i>AverageAmount</i> _{r,k,t}	<i>AverageAmount</i> _{s,r,k,t}
	Local firms	Foreign firms
Elections in:	Recipient countries Semester _{r,k,t}	Supplier countries Semester _{s,k,t}
Semester k-2	0.275 (0.256)	0.171 (0.087)**
Semester k-1	0.677 (0.355)*	0.362 (0.174)**
Election Semester	0.599 (0.341)*	0.171 (0.186)
Semester k+1	0.443 (0.381)	0.186 (0.162)**
Semester k+2	0.219 (0.354)	0.125 (0.141)
N	5,902	41,966
R ²	0.84	0.86
Recip. × Year FE	Yes	No
Sem. × Year FE	Yes	No
Recip. × Sem. × Year FE	No	Yes
Supp. × Recip. × Year FE	No	Yes
N Recip. (clusters)	150	-
N Supp. (clusters)	-	187
N Year (clusters)	26	26
N Semester (clusters)	-	2

Note: Robust standard errors in parentheses, two-way clustered at both the recipient country and supplier country in column (1) and three-way clustered at the recipient, year and semester levels in column (2). *, **, *** denote significance at the 10, 5, and 1% level, respectively.

Table S.A11: Political Cycle on World Bank procurement contract - DPC and CBPC together

Dep. Var.:	Dom. Pol. Cycle	Cross-Border Pol. Cycle	Simultaneously
	<i>AverageAmount</i> _{r,k,t}	<i>AverageAmount</i> _{s,r,k,t}	<i>AverageAmount</i> _{s,r,k,t}
Semester k-2	0.2748 (0.140)**	0.1712 (0.085)**	-0.0614 (0.103)
Semester k-1	0.6773 (0.193)***	0.3625 (0.128)***	0.2991 (0.148)**
Election Semester	0.5994 (0.199)***	0.1707 (0.146)	0.2194 (0.182)
Semester k+1	0.4430 (0.186)**	0.1860 (0.135)	0.0812 (0.183)
Semester k+2	0.2187 (0.172)	0.1247 (0.094)	0.1205 (0.127)
<i>N</i>	5,902	41,966	44,534
<i>R</i> ²	0.84	0.88	0.79
Year x Sem FE	Yes	No	Yes
Recip x Year FE	Yes	No	No
Recip x Year x Sem FE	No	Yes	No
Supp x Recip x Year FE	No	Yes	Yes
N Recip x Year (clusters)	2,951	-	-
N Supp x Year x Sem (clusters)	-	5,818	5,996

Notes: Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table S.A12: Domestic Political Cycle - Interactions with aid

Dep. var.:	(1)	(2)	(3)	(4)	(5)
	<i>Average Amount_{r,k,t}</i>				
ODA :	Baseline	All Aid	Bilateral	Multi. - WB	WB
Semester k-2 r,k,t	0.275 (0.189)	0.231 (0.246)	0.160 (0.248)	0.257 (0.250)	0.290 (0.212)
Semester k-1 r,k,t	0.677 (0.260)***	0.650 (0.312)**	0.568 (0.313)*	0.734 (0.314)**	0.696 (0.277)**
Election Semester r,k,t	0.599 (0.266)**	0.522 (0.307)*	0.391 (0.303)	0.582 (0.314)*	0.661 (0.286)**
Semester k+1 r,k,t	0.443 (0.244)*	0.550 (0.304)*	0.492 (0.302)	0.615 (0.309)**	0.531 (0.267)**
Semester k+2 r,k,t	0.218 (0.221)	0.279 (0.293)	0.249 (0.299)	0.285 (0.295)	0.239 (0.240)
ODA r,t × Semester k-2 r,k,t		0.0001 (0.000)	0.0001 (0.000)	0.0001 (0.000)	-0.0002 (0.001)
ODA r,t × Semester k-1 r,k,t		0.0001 (0.000)	0.0001 (0.000)	-0.0001 (0.000)	-0.0003 (0.001)
ODA r,t × Election Semester r,k,t		0.0001 (0.000)	0.0002 (0.000)**	0.0001 (0.000)	-0.0009 (0.001)
ODA r,t × Semester k+1 r,k,t		-0.0002 (0.000)	-0.0000 (0.000)	-0.0005 (0.000)*	-0.0014 (0.001)*
ODA r,t × Semester k+2 r,k,t		-0.0001 (0.000)	-0.0000 (0.000)	-0.0002 (0.000)	-0.0003 (0.000)
<i>N</i>	5,902	5,902	5,902	5,902	5,902
<i>R</i> ²	0.84	0.84	0.84	0.84	0.84
Year × Sem FE	Yes	Yes	Yes	Yes	Yes
Recip × Year FE	Yes	Yes	Yes	Yes	Yes
N Recip × Year (clusters)	2,951	2,951	2,951	2,951	2,951

Notes: Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table S.A13: Cross-Border Political Cycle - Interactions with aid

Dep. var.:	(1)	(2)	(3)	(4)	(5)
	<i>AverageAmount_{s,r,k,t}</i>				
ODA :	Baseline	All Aid	Bilateral	Multi. - WB	WB
Semester k-2 s,k,t	0.171 (0.085)**	0.341 (0.103)***	0.197 (0.088)**	0.452 (0.117)***	0.315 (0.101)***
Semester k-1 s,k,t	0.362 (0.128)***	0.511 (0.149)***	0.396 (0.134)***	0.783 (0.182)***	0.605 (0.156)***
Election Semester s,k,t	0.171 (0.146)	0.166 (0.163)	0.157 (0.149)	0.424 (0.198)**	0.151 (0.167)
Semester k+1 s,k,t	0.186 (0.135)	0.083 (0.150)	0.201 (0.138)	0.368 (0.187)**	0.088 (0.156)
Semester k+2 s,k,t	0.125 (0.094)	-0.003 (0.117)	0.101 (0.096)	0.136 (0.127)	-0.031 (0.108)
ODA r,t × Semester k-2 s,k,t		-0.0001 (0.000)	0.0001 (0.000)	-0.0003 (0.000)	-0.0014 (0.001)**
ODA r,t × Semester k-1 s,k,t		-0.0002 (0.000)**	-0.0002 (0.001)	-0.0004 (0.000)**	-0.0016 (0.001)**
ODA r,t × Election Semester s,k,t		0.0001 (0.000)	-0.0001 (0.001)	-0.0004 (0.000)	-0.0010 (0.001)
ODA r,t × Semester k+1 s,k,t		-0.0002 (0.000)**	-0.0016 (0.001)**	-0.0010 (0.000)**	-0.0006 (0.001)
ODA r,t × Semester k+2 s,k,t		-0.0001 (0.000)	-0.0021 (0.001)**	-0.0002 (0.000)	0.0002 (0.000)
<i>N</i>	39,982	39,982	39,982	39,982	39,982
R ²	0.86	0.86	0.86	0.86	0.86
Recip × Year × Sem FE	Yes	Yes	Yes	Yes	Yes
Supp × Recip × Year FE	Yes	Yes	Yes	Yes	Yes
N Recip × Year × Sem (clusters)	5,654	5,654	5,654	5,654	5,654

Allocation method and contract category

Table S.A14: DPC by contract category

	(1)	(2)	(3)
Dep. var.:	<i>AverageAmount_{r,k,t}</i>		
	Local firms		
	Consultancy	Other than Consultancy	Civil Works
Semester k-2 r,k,t	0.267 (0.218)	0.371 (0.258)	-0.153 (0.298)
Semester k-1 r,k,t	0.096 (0.289)	0.734 (0.319)**	0.896 (0.444)**
Election Semester r,k,t	-0.140 (0.337)	0.767 (0.336)**	0.910 (0.455)**
Semester k+1 r,k,t	-0.551 (0.323)*	0.592 (0.307)*	0.492 (0.389)
Semester k+2 r,k,t	-0.651 (0.314)**	0.460 (0.274)*	0.114 (0.248)
<i>N</i>	3,920	4,078	2,192
<i>R</i> ²	0.73	0.77	0.82
Year x Sem FE	Yes	Yes	Yes
Recip x Year FE	Yes	Yes	Yes
N Supp x Year (clusters)	1,960	2,039	1,096

Notes: Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table S.A15: CBPC by contract category

	(1)	(2)	(3)
Dep. var.:	<i>AverageAmount_{s,r,k,t}</i>		
	Foreign firms		
	Consultancy	Other than Consultancy	Civil Works
Semester k-2 <i>s,k,t</i>	-0.146 (0.072)**	0.194 (0.150)	0.773 (0.499)
Semester k-1 <i>s,k,t</i>	-0.274 (0.100)***	0.317 (0.187)*	1.484 (0.578)**
Election Semester <i>s,k,t</i>	-0.330 (0.111)***	-0.118 (0.204)	0.447 (0.595)
Semester k+1 <i>s,k,t</i>	-0.123 (0.109)	-0.042 (0.177)	0.055 (0.567)
Semester k+2 <i>s,k,t</i>	-0.017 (0.097)	0.180 (0.148)	0.202 (0.355)
<i>N</i>	26,116	15,248	1,460
<i>R</i> ²	0.82	0.88	0.80
Recip x Year x Sem FE	Yes	Yes	Yes
Supp x Recip x Year FE	Yes	Yes	Yes
N Supp x Year x Sem (clusters)	4,936	3,518	972

Notes: Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Channels

Firms' donation to candidates

Table S.A16: DPC - Political Finance, firm donation to candidates

Dep. Var.:	<i>AverageAmount_{r,k,t}</i>		<i>AverageAmount_{s,r,k,t}</i>	
	Local firms		Foreign firms	
Elections in:	Recipient countries		Supplier countries	
	Semester _{r,k,t}		Semester _{s,k,t}	
Donation authorized:	Yes	No	Yes	No
Semester k-2	0.391 (0.251)	0.245 (0.257)	0.447 (0.133)***	0.039 (0.140)
Semester k-1	0.599 (0.294)**	0.579 (0.405)	0.364 (0.156)**	0.277 (0.178)
Election Semester	0.711 (0.299)**	-0.045 (0.414)	0.047 (0.162)	0.138 (0.201)
Semester k+1	0.705 (0.300)**	-0.215 (0.449)	0.010 (0.139)	0.164 (0.199)
Semester k+2	0.468 (0.264)*	-0.622 (0.399)	-0.027 (0.111)	0.229 (0.206)
<i>N</i>	3,986	1,254	23,960	12,084
<i>R</i> ²	0.86	0.85	0.87	0.88
Year x Sem FE	Yes	Yes	No	No
Recip x Year FE	Yes	Yes	No	No
Recip x Year x Sem. FE	No	No	Yes	Yes
Supplier x Year FE	No	No	Yes	Yes
Supplier x Recip x Year FE	No	No	Yes	Yes
N Supp x Year (clusters)	1,993	627	-	-
N Supp x Year x Sem. (clusters)	-	-	3,658	1,460

Notes: Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Political regime

Table S.A17: Political Cycles - by political regime

	(1)	(2)	(3)	(4)
Dep. var.:	<i>MeanAmount_{r,k,t}</i>		<i>MeanAmount_{s,r,k,t}</i>	
	Dom. Pol. Cycle		Cross-Border Pol. Cycle	
	Democracy	No democracy	Democracy	No democracy
Semester k-2	0.178 (0.204)	0.458 (0.238)*	0.219 (0.087)**	-1.654 (0.520)***
Semester k-1	0.781 (0.313)**	0.476 (0.252)*	0.354 (0.115)***	-2.145 (0.613)***
Election Semester	0.640 (0.322)**	0.326 (0.278)	0.082 (0.131)	0.267 (0.763)
Semester k+1	0.051 (0.244)	0.529 (0.308)*	0.154 (0.129)	0.421 (0.770)
Semester k+2	-0.408 (0.188)**	0.569 (0.296)*	0.082 (0.113)	0.285 (0.453)
<i>N</i>	3,160	2,400	35,324	3,956
<i>R</i> ²	0.84	0.87	0.88	0.92
Year x Sem FE	Yes	Yes	No	No
Recip x Year FE	Yes	Yes	No	No
Recip x Year x Sem FE	No	No	Yes	Yes
Supp x Recip x Year FE	No	No	Yes	Yes
N Supp x Year (clusters)	1,580	1,200	-	-
N Supp x Year x Sem (clusters)	-	-	3,852	1,472

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Competitive election

Table S.A18: Political Cycles - Competitive election

Dep. Var.:	<i>AverageAmount_{r,k,t}</i>		<i>AverageAmount_{s,r,k,t}</i>	
	Local firms		Foreign firms	
Elections in:	Recipient countries Semester _{r,k,t}		Supplier countries Semester _{s,k,t}	
	Not competitive	Competitive.	Not competitive	Competitive
Semester k-2	0.476 (0.255)*	0.247 (0.248)	-0.042 (0.181)	0.211 (0.099)**
Semester k-1	0.650 (0.295)**	0.522 (0.288)*	0.175 (0.211)	0.346 (0.154)**
Election Semester	0.499 (0.310)	0.559 (0.294)*	0.208 (0.286)	0.023 (0.163)
Semester k+1	0.482 (0.322)	0.578 (0.323)*	0.268 (0.263)	-0.094 (0.153)
Semester k+2	0.496 (0.306)	0.318 (0.317)	0.220 (0.153)	-0.138 (0.086)
<i>N</i>	4,412	4,538	21,412	34,634
<i>R</i> ²	0.85	0.84	0.89	0.89
Year x Sem. FE	Yes	Yes	No	No
Recip. x Year FE	Yes	Yes	No	No
Recip. x Year x Sem. FE	No	No	Yes	Yes
Supp. x Recip. x Year FE	No	No	Yes	Yes
N Supp. x Year (clusters)	2,206	2,269	-	-
N Supp. x Year x Sem. (clusters)	-	-	3,854	4,582

Notes: Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Incumbent

Table S.A19: DPC - Incumbent

Dep. Var.:	<i>AverageAmount</i> _{r,k,t}		<i>AverageAmount</i> _{s,r,k,t}	
	Local firms		Foreign firms	
Elections in:	Recipient countries Semester _{r,k,t}		Supplier countries Semester _{s,k,t}	
	No Incumbent	Incumbent	No Incumbent	Incumbent
Semester k-2	0.425 (0.314)	0.307 (0.246)	0.345 (0.216)	0.119 (0.102)
Semester k-1	0.516 (0.356)	0.691 (0.343)**	0.313 (0.237)	0.255 (0.136)*
Election Semester	0.372 (0.376)	0.699 (0.354)**	-0.160 (0.238)	0.218 (0.169)
Semester k+1	0.542 (0.414)	0.564 (0.330)*	-0.389 (0.212) *	0.200 (0.150)
Semester k+2	0.673 (0.411)	0.390 (0.326)	0.150 (0.151)	0.015 (0.102)
<i>N</i>	3,630	4,534	19,610	32,012
<i>R</i> ²	0.84	0.84	0.90	0.89
Year x Sem FE	Yes	Yes	No	No
Recip x Year FE	Yes	Yes	No	No
Supplier x Year x Sem. FE	No	No	Yes	Yes
Supplier x Recip x Year FE	No	No	Yes	Yes
N Supp x Year (clusters)	1,815	2,267	-	-
N Supp. x Year x Sem. (clusters)	-	-	3,344	4,340

Notes: Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Chapter 3

The Political Legacy of 19th Century Politicization and Repression in Southeastern France

1.1 Introduction

December 1851. Jean-Marie Labruyère, also named “Chacaille”, was a mailman in the French department of Ain. “Chacaille” was also known to be a staunch defender of the Republic, and was very likely to be part of a secret *Montagnard* club¹. A few days after Napoleon III’s coup that ended the 2nd Republic, “Chacaille” was frightened of the repression suffered by people who shared his ideas and went into exile in Switzerland. His fate was however less tragic than some of his companions: some of them were sent into penal colonies (*i.e.* *bagne*), expelled from their communes, or even executed. This particular example taken from the archive work of Devos (1992) leads to question the legacy of politicization² and the effectiveness of political repression³. Did “Chacaille”’s disappearance generate fear among his companions, prompting them to moderate their political stances? Or did it trigger indignation and drive them towards more extreme positions? Perhaps “Chacaille”’s departure simply reduced the number of Republican activists, consequently diminishing local support for their cause. One might wonder about the effect of the early 19th-century politicization and the 1851 political repression in southeastern France on election results. This question can be addressed by using the natural experiment along the 1815-1860 border separating France from the Duchy of Savoy and Nice County, and comparing homogeneous communes in a spatial regression discontinuity setting.

Broadly speaking, understanding the impact of political repression on electoral outcomes is important as it causes substantial damage. Since political repression is usually linked to traumatic events, one of its most obvious effects is on psychological health. Munczek and Tuber (1998), Sales et al. (2000), and Stammel et al. (2013) found that being a victim of political repression leads to long-run psychological disorders⁴. Political repression can also harm economic outcomes by eroding trust. As denoted by Nunn (2009), trust is a determinant of long-term economic development, notably through productivity (Bjørnskov and Méon, 2015), education, and the quality of institutions (Bjørnskov and Méon, 2013). Lichter et al. (2021), Booth et al. (2022), Nikolova et al. (2022), and Pronkina et al. (2023) found that

¹*Montagnard* clubs were clandestine Republican organizations during the first half of the 19th century.

²Politicization is defined as the process of becoming politically aware.

³Political repression is defined by Davenport (2007) as “the act of a state entity controlling a citizenry by force for political reasons, particularly for the purpose of restricting or preventing the citizenry’s ability to take part in the political life of a society”.

⁴such as depression, anxiety, aggressive feelings, post-traumatic symptoms, and prolonged grief disorder.

exposure to diverse forms of political repression ⁵ reduces trust. They show a persistent effect over time, as this mistrust is transmitted over generations through parental socialization.

Questioning the impact of political repression on electoral outcomes is also a contemporary issue. When looking at current global trends, one may expect political repression to intensify in the future. According to the political terror scale index (Gibney et al., 2022), low-income countries⁶ went from a context where “There is a limited amount of imprisonment for non-violent political activity. (...) Political murder is rare.” to a situation where “Execution or other political murders and brutality may be common. Unlimited detention, with or without a trial, for political views is accepted.”⁷. A similar trend can be observed with the Freedom House’s data, where political liberty for the press scores (*i.e.* a specific form of political repression) deteriorated for every income category between 2001 and 2016. This information suggests that political repression may increase in the near future, even in developed countries.

Despite having all these negative effects, is political repression at least efficient in one of its main purposes, *i.e.* to eliminate the politicization previously carried out by the opposition⁸? According to the psychological literature, the answer is not obvious. When confronted with political repression, individuals can have opposite reactions. On the one hand, political repression may suppress the opposition by generating fear among the dissidents (Young, 2019). The latter would become risk-averse and temper their political opposition. On the other hand, political repression may increase the perceived distance between the oppressed group and the prosecutors, making the dissidents more attached to their social group (Nugent, 2020). In this situation, political repression would generate a more united and polarized opposition.

Answering this question is also not easy from a statistical standpoint. As mentioned by Walden and Zhukov (2020), estimating the effect of political repression presents methodological issues. Since political repression is most of the time directed toward a specific group, such studies can be subject to selection bias, hence making it difficult to have a suitable counterfactual. However, the political repression in southeastern France during the 19th century can be a natural experiment that

⁵such as being spied on by the Stasi in East Germany, undergo the cultural revolution in China or simply knowing about the gulag forced labor camps in USSR

⁶According to World-Bank income classification

⁷Source: <https://www.politicalterroryscale.org/Data/Documentation.html>

⁸another purpose of repression could be to increase the opportunity cost of engaging a conflict, hence reducing the incentive to rebel.

bypasses such issues. I make the argument that the Duchy of Savoy and the Nice County switched exogenously between France and the Kingdom of Piedmont Sardinia in both 1815 and 1860. During these 45 years, a population with homogeneous political and geographical characteristics (even though they were previously part of separate political territories, cf. Figure A2), went through different historic paths in these two distinct political entities, all within an era when contemporary political opinions were shaped. They notably lived different exposure to politicization and political repression: The French side politicized in part through secret Republican clubs and faced political repression, while the Italian side politicized rather on the basis of affiliation or opposition to France/Italy, and managed to avoid repression.

Using the election results at the municipality level (retrieved from the French ministry of the interior) between 1995 and 2022, I resort to a spatial regression discontinuity design by comparing the electoral outcomes of homogeneous communes along the 1815-1860 Savoy and Nice border. This econometric approach appears well-suited to the topic since it enables the comparison of communes with similar characteristics while also accounting for geographic spillovers. Results indicate that Republican politicization in the 19th century led to a significantly stronger preference for radical Republican candidates on the French side during the 1871 legislative election. It also had long-run impacts since the French side votes significantly more toward left-wing candidates, *i.e.* the ideological heirs of 19th-century republicanism (Julliard, 2014), during legislative and presidential elections between 1995 and 2022. This result on its own provides little information about the effects of political repression. Therefore, using archives work from Devos (1992), I introduce the number of repressed individuals by communes in the specification. Results indicate that municipalities where citizens went through tougher repression have a significantly lower preference for left-wing candidates. This negative impact is however not large enough to offset the initial politicization effect. I then try to explain how and why the effects of politicization and repression persist over the long run. The long-term legacy of repression is explored with repressed political dynasties and repression-related emigration. Both communes where a mayor shared its name with a repressed citizen within a 20km radius and communes with repressed citizens that emigrated⁹ have a significantly lower preference for left-wing candidates. The persistence of the effects can be explained by the lack of population mixing since

⁹Either self-exile or forced emigration to detention in French Guiana or Algeria.

there is less difference in terms of voting behavior between communes that are located in the same academy and employment area, *i.e.* areas where individuals from both sides of the border have mingled through higher education and/or at work.

This study contributes to the literature addressing the impact of political repression on electoral outcomes. The majority of papers composing this strand investigated the repression during the Soviet era and came to different conclusions. [Kapelko and Markevich \(2014\)](#), [Lupu and Peisakhin \(2017\)](#), [Rozenas et al. \(2017\)](#), and [Rozenas and Zhukov \(2019\)](#) found that Soviet repression had long-term impacts, since the oppressed areas or ethnic groups are less likely to vote for pro-Russian parties until nowadays, hence validating the polarization hypothesis of [Nugent \(2020\)](#). Regarding these studies, political repression would be counter-productive and would not reach its primary purpose. However, based on the same context of Stalin-era repression, [Zhukov and Talibova \(2018\)](#) came to different conclusions. They found that more heavily repressed communities had a lower turnout in recent elections, validating the [Young \(2019\)](#) fear hypothesis. In the context of political repression during China's cultural revolution, [Wang \(2021\)](#) even reconciles both intuitions, as individuals living in more repressed localities have more anti-regime attitudes, but less contentious behaviors (measured by protests). Investigating relatively less violent state repressions in the context of Alsace-Lorraine, [Dehdari and Gehring \(2022\)](#) and [Gehring \(2021\)](#) indicate that less severe repressive policies resulted in an increased political preference for regionalism, decentralization, and support for the European Union, hence illustrating once again the [Nugent \(2020\)](#) polarization hypothesis. In summary, the literature suggests that more or less violent repression can deter and inflame opposition, depending on the post-repression political context ([Rozenas and Zhukov, 2019](#)). However, the overall body of research confirms that political repression has effects that span decades and generations, mainly through parental socialization mechanisms ([Bisin and Verdier, 2001](#); [Avdeenko and Siedler, 2017](#)).

The findings of this paper confirm that past political repression has a long-run impact, and brings additional support for the [Young \(2019\)](#) hypothesis. It also complements the literature in a number of ways. First, it adds original first-hand data from [Devos \(1992\)](#)'s archival work, which allows a more granular individual dimension to be exploited in the measurement of repression. Then, it considers another context than Soviet or Communist China, *i.e.* 19th century France's, in which political repression was less indiscriminate and more targeted. Finally, this

study also offers a broader perspective since the events considered here date back more than a century, whereas most studies in this literature have considered events that occurred in the mid-20th century.

The rest of the paper is organized as follows: Section 2 provides an overview of the historical context of southeastern France in the 19th century. Section 3 introduces the data, the empirical strategy, and discontinuity tests. Section 4 presents the main results. Section 5 explores the channels, and Section 6 concludes.

1.2 Historical context

1.2.1 1815: Congress of Vienna's separation

In 1815, after years of incessant wars against European coalitions, Napoleon I was defeated. This loss marked the end of the French Empire, whose borders were redrawn at the Congress of Vienna. During the latter, the Duchy of Savoy and the Nice County were given back to the Kingdom of Piedmont Sardinia¹⁰. Figure A1 presents the boundaries of the mentioned regions, as well as the 1815-1860 border separating them from France.

From then on, the two regions and France experienced two different historical paths during a few decades that were crucial to the structuration of contemporary political opinions.

1.2.2 Southeastern France (1815-1860)

Between 1815 and 1830, France experienced a return to the monarchy with the Second Restoration. The regime then evolved into a constitutional monarchy with the July Monarchy between 1830 and 1848. Since censal suffrage¹¹ was the rule during this period, very few republican (*i.e.* left-wing) politicians were present in the government. But southeastern France became a fertile ground for republican ideas, as local politicians formed *Montagnard* clubs. The latter were particularly present in rural areas and took the form of discussion clubs, more or less formally declared as political clubs, where mostly young men met and discussed politics.

¹⁰Wars, treaties, and alliances made Savoy and Nice either an independent political entity (the Savoy Duchy, evolving in the Piedmont Sardinia Kingdom), part of the French kingdom, or even occupied by the Spanish Kingdom. In total, Savoy and Nice have been occupied or annexed seven times by France (1536 to 1559, 1600 to 1601, 1630 to 1631, 1690 to 1696, 1703 to 1713, 1792 to 1815, and 1860 to nowadays. Figure A2 in the appendix shows a timeline of the various political entities to which the Duchy of Savoy and Nice County have belonged over the course of history. Table A1 presents the changes in political entities over the territory and provides a summary of the reasons behind these modifications.

¹¹A form of voting rights in which eligibility to vote is determined by an individual's economic or property status. In this period, the right to vote was linked to the payment of a tax.

Margadant (1979) explains the development of these clubs in southeastern France for two reasons. First, this region is home to more Protestants, historically skeptical towards the monarchy and therefore more inclined to be republican. The second is linked to the social character of these clubs: once established, they evolved into hubs for the informal socialization of young men within the village, inflating the importance of these political clubs. These clubs gave Republican politicians the opportunity to present the main lines of their programs, including the abolition of the wine tax, the shift of the taxation's burden from the poor to the rich, low-interest loans through state banks, or free usage of common lands. This program appealed to both the middle and working classes, from farm workers and craftsmen to agricultural landowners, and gave these rural populations their first form of politicization (Margadant, 1979).

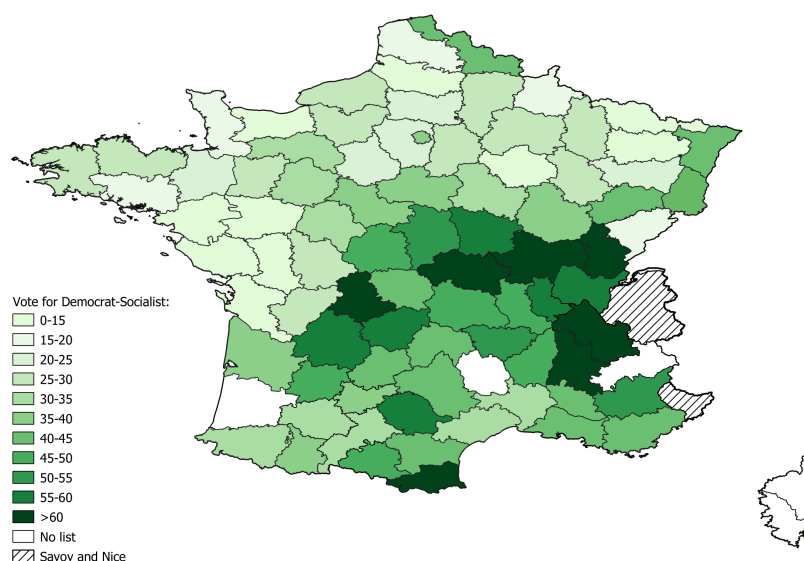
Their hopes were almost fulfilled with the Revolution of 1848 and the installation of the Second Republic. Figure 1.1 presents the vote for democrat-socialists per department for the legislative election of 1849¹², retrieved from Bouillon (1956) (democrat-socialists were the most radically republican political offer at the time). It seems that *Montagnard* societies in the southeast had short-run effects since these departments mostly voted for democrat-socialist deputies. However, the conservative Party of Order won the majority of parliament's seats. The state of political freedom is back to conditions that prevailed under the July Monarchy, with political reunions once again banned. Republicans had to go back to underground organizations and formed secret societies, once again particularly in the southeastern part of France. However, with the opportunity of winning elections, they intensified their propaganda and became what can today be considered the precursor of a mass political party¹³ (Vigier, 1963; Margadant, 1979; Agulhon, 1979, 1992).

This new organization gave them enough leverage to react to the 1851 event. Louis Napoleon-Bonaparte, the president of the Second Republic at the time and nephew of Napoleon I, staged a coup. He granted himself dictatorial powers and established the French Second Empire. According to the archives work of Margadant (1979), 100,000 men from 900 communes participated in protests against the coup. 70,000 from 775 communes mobilized in arms against the government, and 27,000 in 270 communes participated in violent clashes against the army. This event still remains the largest uprising in the history of the French provinces (*i.e.* outside

¹²With universal male suffrage.

¹³Generally, the political leadership was located in urban centers. It delegated to small committees or local branches, formed by clubs and activists in rural areas

Figure 1.1: 1849 legislative election results - vote for Democrat-Socialists

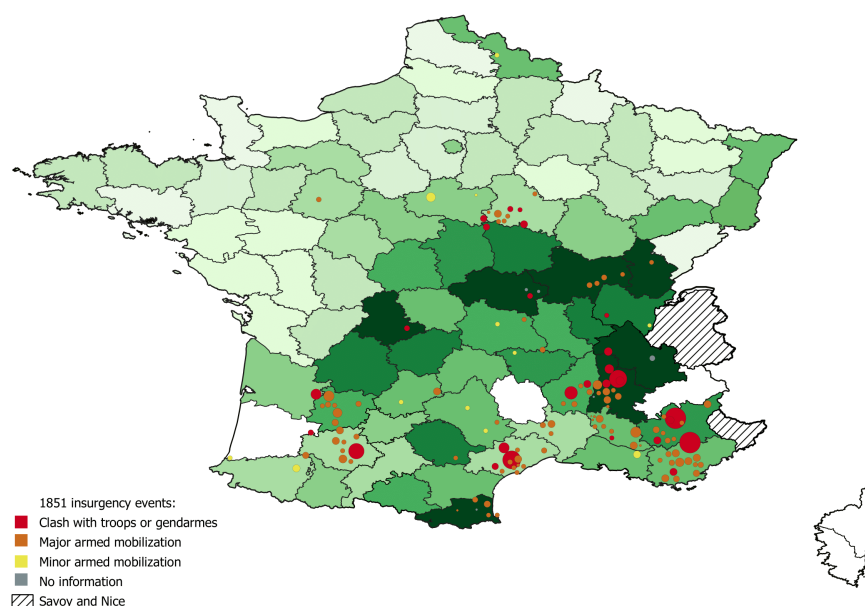


of Paris). Figure 1.2 presents the data retrieved by Margadant (1979), with information on the location, nature, and size of insurgency events in December 1851. Unsurprisingly, most of them occurred in the southeastern part of France, which is the region where *Montagnard* clubs and secret societies were particularly active. Margadant (1979) underlines that the main determinant of participation in the insurrection was the politicization through *Montagnard* clubs as almost no region without such organizations participated in the insurrection. He also adds that rebellious areas did not have different economic foundations or social compositions¹⁴.

Despite their relatively modern political structure, the surprise of the coup d'état caught the Republicans off guard, affording them insufficient time to adequately orchestrate the insurrection, resulting in its failure. Consequently, Republicans underwent the greatest political purge in France between the Reign of Terror (1793-1794) and the end of the Second World War. According to the archives work of Margadant (1979) and Devos (1992), 26,884 people were arrested. 21,000 of them were sentenced, of which 9,530 were deported (notably 239 of them were sent to the *Bagne* in French Guiana).

¹⁴More precisely, similar to areas that did not experience rebellion, rebellious regions witnessed a rise in agricultural output, growth in industrial activities, and could be viewed as semi-urban.

Figure 1.2: 1851 insurgency events



1.2.3 Savoy and Nice and the return to France

In the meantime, the Duchy of Savoy and the Nice County were part of the Piedmont-Sardinia Kingdom. The latter has also experienced revolutionary events, but mostly in the context of Italy’s unification, such as the first Italian War of Independence in 1848 (*i.e.* the *Risorgimento*). Since the two regions were not culturally close with Piedmont, the Italian insurgency events did not have great echoes in Savoy and Nice, as the many declaration of Savoy deputies in the Torino assembly can suggest¹⁵. As for the politicization of the populations of Savoy and Nice, it mainly concerned the political entity that the territory should adopt: joining France, Italy, Switzerland or even becoming independent (Milbach, 2008; Courrière, 2019). Consequently, and unlike their French fellows, Republican politicians had less

¹⁵“A high and powerful barrier separates Savoy from Italy, its customs, its habits, its language, its trade do not assign it any place in the great Italian family which must be constituted one day” “*Une haute et puissante barrière sépare la Savoie et l’Italie, ses mœurs, ses habitudes, sa langue, son commerce ne lui assignent aucune place dans la grande famille italienne qui doit se constituer un jour*” Pantaléon Costa de Beauregard; “If the war of Independence is popular in Piedmont, it is not in Savoy. You fight for your independence and your nationality: but we, why do we fight?” “*Si la guerre de l’Indépendance est populaire en Piémont, elle ne l’est pas en Savoie. Vous combattez pour votre indépendance et votre nationalité : mais nous, pourquoi combattons-nous ?*” Gustave de Martinel; “Your cause is not ours” “*Votre cause n’est pas la nôtre*” Charles Dufayard (sources: (Avezou, 1939; Dufayard, 1914)).

echo in these regions¹⁶, did not organize in secret clubs, and did not face political repression.

In 1860, Savoy and Nice went back to France in an exogenous manner. Victor-Emmanuel II, King of Italy and Duke of Savoy, gave the two regions back in exchange for France’s help during Italy’s war against the Austrian Empire in Lombardy. A plebiscite was organized and the population was asked if they wanted to join the French Empire. In Savoy, out of 130,839 voters, 130,533 voted yes. In Nice, 25,743 over 25,933 voted yes (Ménabréa, 2009; Varaschin, 2009). This abnormally high proportion of “yes” demonstrates the undemocratic nature of the vote. The cession of Savoy and Nice had in fact already been officially enacted in the Treaty of Turin signed a few weeks earlier, with the French army already present in the territory before the vote. The vote also lacked an alternative choice: staying in Italy, joining Switzerland, or becoming independent was not an option. And as noted by Baud and Binz (1985), the influence of the parish priests was determinant, directing the vote of religious and less-educated populations towards the yes side¹⁷.

In summary, Savoy and Nice exogenously changed political entities between 1815 and 1860. Consequently, these territories followed distinct historical paths notably marked by different politicization and an absence of exposure to political repression.

1.3 Empirical strategy and Data

1.3.1 Data

The sample considered for this study is composed of nine departments, more precisely Jura, Ain, Haute-Savoie, Savoie, Isère, Hautes-Alpes, Alpes-de-Haute-Provence, Alpes-Maritimes, and Var. For the main analyses, I focus on the smallest administrative units, which is the municipality (communes) level. Overall, there are 2,685 communes in the sample. Geographic boundary data are retrieved from GADM shapefiles (the Database of Global Administrative Areas).

In order to estimate the impact of politicization and political repression, electoral outcomes for the legislative election of 1871 (i.e. the first post-treatment-free election) are retrieved. To the best of my knowledge, no data is accessible on commune-level election results during the Third Republic (1870-1940).

¹⁶although the border was not totally impervious to the circulation of political ideas, as witnessed by the protests in Chambéry in 1848 (Milbach, 2008)

¹⁷“*Si les six cents curés savoyards eussent fait opposition à l’annexion, la presque unanimité eût été en sens inverse*” “If the six hundred Savoyard parish priests had opposed annexation, the almost unanimous decision would have been in the opposite direction.” (Source: Baud and Binz (1985))

Consequently, I collected commune-level election records from French National Archives¹⁸.

In order to test for the long-run persistence, electoral results such as the vote for left and right-wing candidates or abstention during both presidential and legislative elections are retrieved from the French ministry of the interior¹⁹. The latter are available at the commune level, but unfortunately only for recent elections. The 1995 presidential election and 1997 legislative election were the first ones for which the ministry of the interior published commune-level results²⁰. Consequently, there is information for commune-level electoral results for the presidential elections of 1995, 2002, 2007, 2012, 2017, and 2022; and legislative elections of 1997, 2002, 2007, 2012, 2017, and 2022²¹.

1.3.2 Empirical strategy

In order to test the effect of different historic trajectories on electoral outcomes, I resort to a spatial regression discontinuity design, illustrated in Figure 1.3. More precisely, the goal is to estimate whether the early 19th-century politicization and 1851 political repression impacted both 1871 and long-run election results for homogeneous municipalities along the Savoy and Nice border. The geographical discontinuity design uses the following estimation:

$$Y_{m,t} = \alpha + \beta Treatment_m + [\lambda_1 D_m + \lambda_2 (D * Treat)_m + \lambda_3 D_m^2 + \lambda_4 (D^2 * Treat)_m] + \theta X_m + \phi_t + \varepsilon_{m,t}$$

$Y_{m,t}$ is the electoral outcome for municipality m during an election t . It can be the vote share in percent for left, far-left, moderate-left, right, far-right, or moderate-right candidates during a given presidential or legislative election. $Treatment_m$ is the variable of interest and corresponds to a binary variable taking the value 1 if a given municipality m was located in France between 1815 and 1860. As suggested by Gelman and Imbens (2019), since the effect might differ based on proximity to the border, the distance between the 1815-1860 border and the municipality's

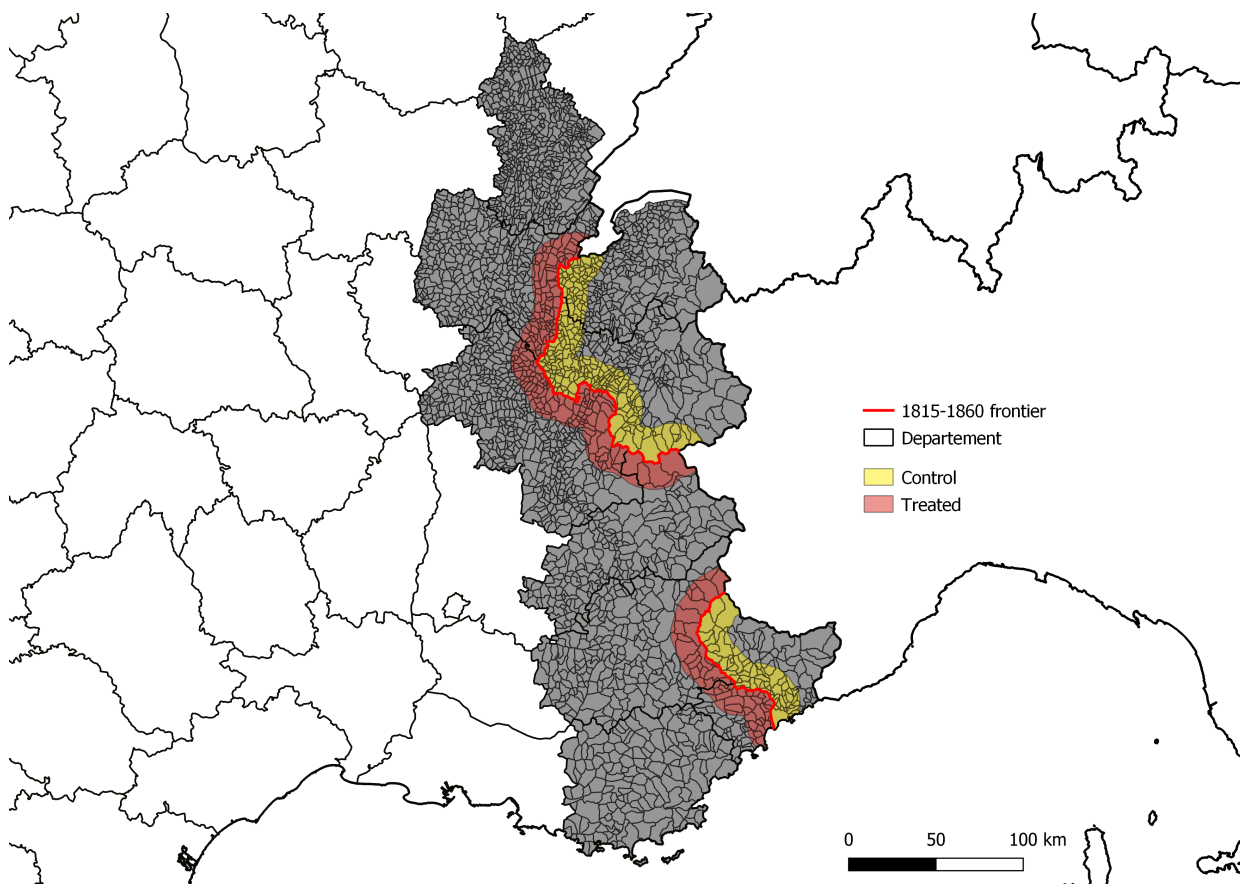
¹⁸Documents are accessible at the *Archives Nationales de France* in Pierrefitte-sur-Seine. Document number: C//3517-C//3564,C//3566-C//3606.

¹⁹<https://www.data.gouv.fr/fr/pages/donnees-des-elections/>

²⁰Using the 2017 commune structure. Results at the polling station level in 2022 were used to reconstitute results at the 2017 commune level for communes that merged between 2017 and 2022.

²¹Older election results from the entire Fifth Republic (1958 to nowadays) were collected by Sciences Po, but at the legislative district, which does not allow for the preferred empirical strategy.

Figure 1.3: 1815-1860 border - 15km bandwidth



chief town is included (retrieved from French National Institute of Statistics and Economic Studies (INSEE) statistics), as well as an interaction between the distance and the treatment, and second-degree polynomial interactions. In order to account for commune-level particularities that can explain a tendency to vote for a particular political color, a vector X of time-invariant municipality-level controls is included. First, as [Chambro et al. \(2021\)](#) underlined, proximity to the department's prefecture was an important determinant for economic development in the 19th century (*i.e.* potentially a determinant of current voting behavior). Consequently, distances between a given municipality chief town and department prefectures or other large cities are included²². Second, as suggested by [Dell \(2010\)](#), in order to capture other unobserved time-invariant commune-level characteristics, I include the interaction between a given municipality chief town's latitude and longitude. Third, as in [Dehdari and Gehring \(2022\)](#), X comprises border-segment fixed effects, which

²²Precisely, distances to Annecy, Bourg-en-Bresse, Chambéry, Gap, Digne-les-Bains, Geneva, Grenoble, Lons-le-Saunier, Lyon, Marseille, Nice, Toulon, Torino, and Genoa are included.

allows comparison between treated and control municipalities that are geographically close. Finally, in order to take into account one election's particularity, an election fixed effect ϕ is added²³. Standard errors are clustered thanks to Conley (2010) spatial standard errors, with a radius of 10km in order to account for neighboring municipalities' spillovers²⁴. On average, there are 22 municipalities per cluster.

The 1815-1860 frontier partially follows the current departmental boundaries²⁵. In fact, the 1815-1860 border, which was used to draw the borders of today's departments, is the result of centuries of history and has been traced through wars, treaties, alliances, and exchanges of territory (summarized in Table A1). It may raise concerns about the exogeneity of this border. But as suggested by the map of Boula De Mareüil et al. (2017), the 1815-1860 border does not mark the division between different linguistic areas²⁶. In fact, both sides of the Duchy of Savoy frontier were speaking *Franco-Provençal* dialects; and both sides of the Nice County border were speaking *Langue d'Oc* dialects. The 1815-1860 border also does not translate to any religious frontier, as both sides are mostly catholic and have a similar number of Protestant churches. In summary, in spite of the fact that the 1815-1860 border follows the line of the current departments and stems from previous historic events, it does not represent any sort of major cultural division, since the inhabitants on each side of the border speak similar languages and practice the same religion. As Figure A3 suggests, the frontier mostly follows geographic features such as the Rhône River between Haute-Savoie and Ain, the Guiers River between Isère and Savoie, the Var and Estéron rivers in the Alpes-Maritimes; or such as mountain ridges like the *Massif des Cerces* separating the Hautes-Alpes and Savoie. Considering the absence of cultural differentiation and its mostly geographic features, the 1815-1860 border can be considered exogenous.

1.3.3 Discontinuities

Only the communes whose chief town is located within a 15 km band along the 1815-1860 border are taken into account in this specification since this threshold is close to the estimated optimal bandwidth (Calonico et al., 2014)²⁷ and allows for comparing communes that are homogeneous in characteristics. One first needs

²³Except on the analysis of the 1871 election since it concerns only one election.

²⁴Regressions were run thanks to Colella et al. (2019) Stata package.

²⁵Only partially, the south border of the Nice country does not follow the department boundary of the Alpes-Maritimes.

²⁶Source: <https://atlas.limsi.fr/>

²⁷Thanks to the `rdbwselect` Stata command, the optimal bandwidth was estimated to 18.24km and 14.5km for respectively presidential and legislative electoral outcomes.

to ensure that communes are indeed similar in terms of pre and post-treatment characteristics that can act as confounding factors.

Pre-treatment characteristics

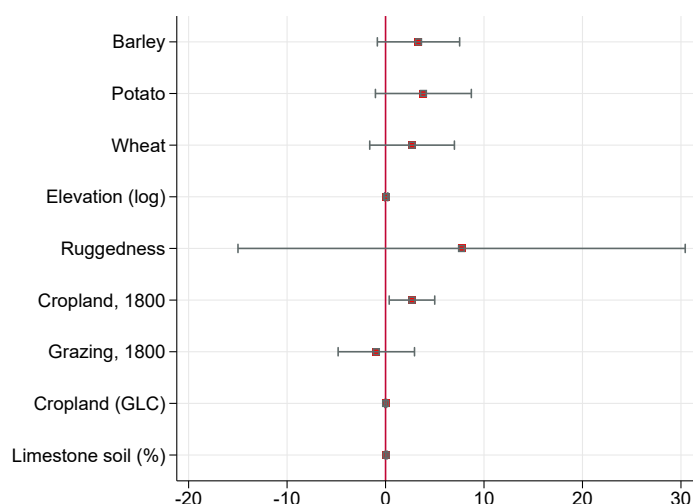
Figure 1.4 presents discontinuities for geographic characteristics that can be determinants of political preferences. More precisely, the above-presented empirical strategy is used with various geographic parameters as dependent variables. As Nunn and Qian (2011) and Alesina et al. (2011) suggested, soil suitability for certain crops and early usage of the plow are determinants of long-run economic development and fertility preferences, which can affect political preferences. In addition, Nunn and Puga (2012) and Gooch (2019) found that rough terrain protected respectively remote African and Chinese areas from slavers raids and political repressions during the Great Leap Forward. One can extrapolate these results by suggesting that rough terrain may have protected some areas from 1851 political repression. Finally, Siegfried (1913) suggested that soil composition impacted long-run political preferences in the French department of Vendée. More precisely, he found that limestone soil leads to smaller farms and denser villages, which consequently reduces the influence of the church, hence leading to a less conservative vote.

Based on these findings, I test for discontinuities in soil suitability for wheat, barley, and potato (FAO/GAEZ), elevation and terrain ruggedness (CGIAR-SRTM), the estimated share of crops and grazing land in 1800 (History database of the Global Environment, Klein Goldewijk et al. (2011)), and the share of limestone soil per commune (BRGM, *Bureau de recherches géologiques et minières*)²⁸. From the results presented in Figure 1.4, it seems that both treated and controls are not significantly different in terms of geographic characteristics, except for the estimated share of cropland per commune in 1800, as the treated communes (*i.e.* those who were in France between 1815 and 1860) would have significantly more land dedicated to crops. However, based on the current and more precise land cover data (Global Land Cover), the share of cropland is not significantly different between treated and control communes. Additionally, thanks to French census data from 1861, 1866, and 1872 at the arrondissement level²⁹, treated and controls had a similar share of farmers. The share of cropland in 1800 is nonetheless added as a control in the robustness checks.

²⁸The pixel resolution is 6x9km for FAO/GAEZ, 90m for CGIAR-SRTM, 6x9km for HYDE, and 25x50m for BRGM. Given the relatively low resolution, results for soil suitability and land use in 1800 should be taken with caution

²⁹Administrative level smaller than department but larger than communes.

Figure 1.4: Pre-treatment - Geographic characteristics



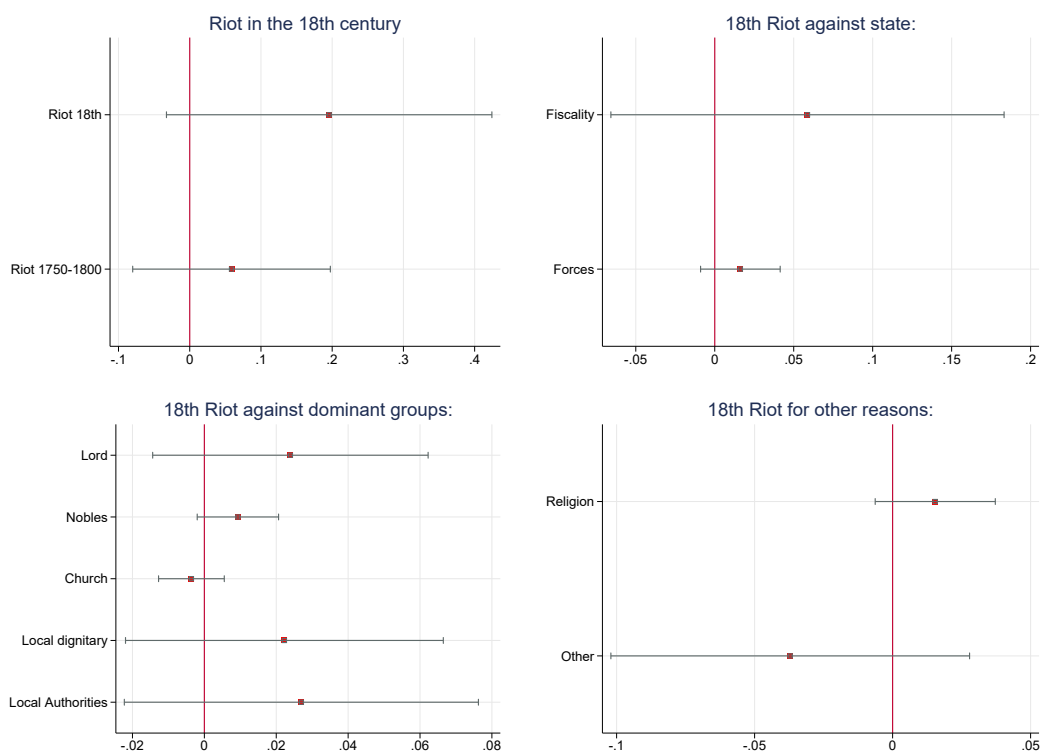
Notes: Coefficients estimated with a 15km bandwidth with polynomial interaction terms, commune-level controls, and border-segment fixed effects. Bars represent the 95% confidence interval, with spatial standard errors clustered at a 10km radius. Observations: 561.

Testing for potential discontinuities on pre-treatment political preferences is also essential. One may worry that I simply capture already pre-existing differences in political preferences, potentially stemming from the several changes of political entities that Savoy and Nice have known throughout their history. In fact, the perception of monarchy in Savoy and Nice might have varied among the population since the Piedmont Sardinia Kingdom, which previously governed these territories, was regarded as a relatively enlightened political system. Consequently, the population along the border in Savoy and Nice could potentially harbor less aversion to monarchy, resulting in a diminished inclination towards left-leaning and Republican ideologies. As the pre-treatment period was before 1815, carrying out these tests is not an easy task, since there were very few universal suffrage elections at that time. The only election to have occurred before 1815 under universal suffrage was the legislative election of 1792. But turnout was very low and close to censal suffrage levels³⁰. One can hardly consider these votes as representative of the population's opinion. Consequently, I resort to the [Chambru and Maneuvrier-Hervieu \(2022\)](#) Historical Social Conflict Database as a proxy for pre-treatment political preferences. The latter is a dataset constituted from archival sources and provides information on social conflicts that occurred in France between the 12th and 19th centuries. It contains highly detailed information, such as the date and precise

³⁰According to [Dupuy \(2014\)](#), turnout was between 10 and 11% only

location of the conflict, as well as the estimated number of people involved and the type of social conflict. Figure A4 shows a map of the social conflicts between 1700 and 1789 in the area, and Table A2 presents descriptive statistics for social conflict variables in the sample.

Figure 1.5: Pre-treatment - Political characteristics



Notes: Coefficients estimated with a 15km bandwidth with polynomial interaction terms, commune-level controls, and border-segment fixed effects. Bars represent the 95% confidence interval, with spatial standard errors clustered at a 10km radius. Observations: 561.

Figure 1.5 displays coefficients for the treatment variable. I focus solely on social conflicts that occurred between 1700 and 1789, as they are more likely to capture contemporary political opinions, especially with the events related to the French Revolution. Treated and control communes do not seem to differ significantly in terms of social conflict in the 18th century. Further analyses are conducted by types of conflicts. Once again, no side experienced significantly more conflicts of any kind, which is reassuring, especially for conflicts against dominant groups.

In addition, based on the French National Assembly dataset, I compare the share of left-wing representatives by departments across three regimes: the National Convention (1792-1795), the Council of Five Hundred (1795-1799), and

the Napoleonic Era (1800-1814)³¹. Results in Table A3 indicate that treated departments did not elect significantly more left-wing representatives.

Finally, based on the work of Vovelle (2002), the presence of refractory priests³² during the French Revolution turned their parishioners towards political conservatism. Based on data retrieved from Vovelle (2002) and Biard et al. (2015), Figure A5 presents the share of refractory priests per department. It seems that both treated and control areas had a high rate of *Jureur* priests³³, but no side seems to experience a higher prevalence. Consequently, despite changes in political entities before 1815, no side was more revolutionary than the other. In other words, treated and control communes did not differ in terms of pre-treatment political opinions.

Post-treatment characteristics

One must ensure that control areas did not have different economic and cultural paths between 1815 and 1860, which could explain future discontinuities in terms of electoral outcomes. Using census data from 1861 at the *arrondissement* level (*i.e.* one year after Savoy and Nice return to France), results in Figure A6 suggest that treated and control areas have homogeneous characteristics even after 45 years of different administrations. They have a similar population in terms of nationalities (even though treated may have significantly fewer Swiss), religion, and socio-economic structure.

But many things could have happened between 1860 and 1995 (the first year for which commune-level recent election results are available) that might have influenced political preferences on both sides of the border in distinct ways.

First, both sides may have experienced different paths in terms of early migration to large cities, such as Paris. And according to Barsbai et al. (2017), migrants can affect stayers' political preferences. Consequently, if the treated side migrated more to Paris, it may explain its tendency to vote more for left-wing candidates. As done by Daudin et al. (2019), I constructed the number of migrants to Paris by *arrondissement*, for each decade between 1860 and 1900. The latter was constructed thanks to the TRA survey (Bourdieu et al., 2014) and the French census. TRA survey, also known as the *Enquêtes de 3,000 familles*, presents information on the birth, marriage, and death place of roughly 25,000 individuals whose name

³¹The notion of the political left was not clearly established at the time. I based my classification on Julliard (2014). Consequently, *Montagnard* and moderate are considered as the most left-wing political offer during the National Convention, the Council of Five Hundred, and the Napoleonic Era

³²Priest who had refused to take the oath to the civil constitution during the French Revolution.

³³Priests who agreed to take the oath to the civil constitution.

starts with the letters “TRA”. From this information, the share of migrants to Paris per arrondissement is built and matched with the population information of the French census in order to have the estimated number of migrants to Paris per arrondissement. While the TRA survey’s representativeness is limited when examined at the arrondissement level and might exhibit selection bias for control areas, as people born in Savoy and Nice before 1860 are not included since these territories were not part of France, it is the most detailed geographic data available concerning early migration to Paris. I also used the department-name census from the INSEE in order to generate the occurrence of typically local names from the treated department in overall Parisian birth for each decade between 1890 and 2000.

Table A4 presents the discontinuity results for three measures of migration to Paris. Regarding the TRA survey and migration to Paris as the occurrence of local names in Paris between 1890 and 1910, there should be no significant differences between treated and control arrondissements in terms of migration to Paris during the 19th century. When the occurrence of local names in Paris between 1890 and 2000 is considered, the treated side would have migrated less. This result is not such a great concern since this should have increased the left vote share for control communes stayers (based on the findings of Barsbai et al. (2017)), yet the main finding of this paper suggests that treated communes vote significantly more for left-wing candidates.

One may also worry that a self-selected migration based on political criteria occurred from control to treated areas after the return of Savoy and Nice to France in 1860, potentially increasing the number of left-wing voters in treated areas. According to French census data, the evolution of the population in both treated and control departments remained stable after 1860 (Figure A7).

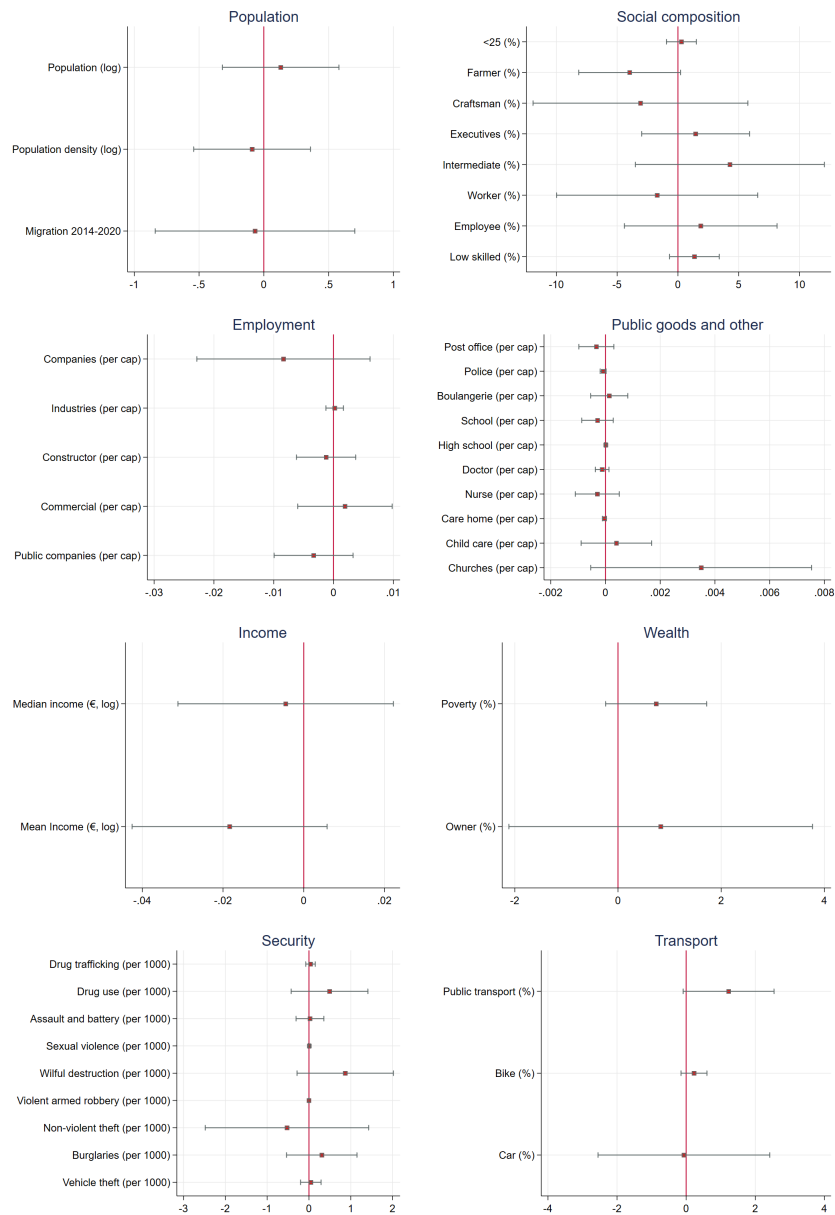
Second, treated and control communes were potentially hit differently by the First and Second World Wars. One side may have suffered more from the Allied bombing or may have had more casualties, potentially changing attitudes toward politicians (Adena et al., 2021). Using the THOR (Theater History of Operations), the *Mémoire des Hommes* French Ministry of Army databases and The Morts Pour la France database (Gay and Grosjean, 2023) on respectively allied bombing, military and civil loss during the Second World War, and military loss during the First World War, results in Figure A8 suggest that no side went through tougher conditions during World Wars.

Finally, other unobserved events may have potentially impacted differently

treated and control communes, translating into different socio-economic current characteristics. According to electoral sociology literature (Lewis-Beck, 1983; Gaxie, 1985; Gonthier, 2021), income and socio-professional category are relevant determinants of French voters' electoral behavior. Consequently, using data from the INSEE, I test whether treated communes had a significantly different population, immigration, social composition, employment, public goods distribution, income, wealth, security, and transport in 2017. According to the results presented in Figure 1.6, communes composing the sample are homogeneous in characteristics. They notably have a similar share of young people, share of workers, and median/mean income.

In summary, treated and control communes are homogeneous in pre and post-treatment characteristics. More precisely, they did not have different political preferences before the treatment, they have similar geographic features, migration patterns, Second World War experience, and current socio-economic characteristics. Consequently, one can safely attribute a discontinuity in electoral outcomes as the result of different historic trajectories.

Figure 1.6: Post-treatment - INSEE statistics (2017)



Notes: Coefficients estimated with a 15km bandwidth with polynomial interaction terms, commune-level controls, and border-segment fixed effects. Bars represent the 95% confidence interval, with spatial standard errors clustered at a 10km radius. Observations: 561.

1.4 Results

1.4.1 Republican Politicization's effects

First post-treatment election: 1871

One first needs to examine the impact of different historic trajectories, notably marked by different politicization, on the electoral outcomes during the first free

election after treatment, *i.e.* the legislative election of 1871.

Table 1.1: Treatment on 1871 legislative election results - Absolute effect

Dep. Var.:	Non	Republican		
	Rep.	All	Moderate	Radical
$Vote_m$				
Treatment $_m$	-1.289 (3.053)	1.289 (3.053)	-14.823 (3.310)***	7.821 (3.499)**
N	326	326	326	326
R^2	0.80	0.80	0.71	0.83
Commune controls	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes
Election FE	No	No	No	No

Standard errors in parentheses, clustered at 10km radius

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table 1.1 presents the baseline results of the treatment’s effect (*i.e.* to be in France between 1815 and 1860) on the commune-level election results for the 1871 legislative election. According to the work of Julliard (2014), Republicans were a heterogeneous political family during the end of the 19th century, that could be divided into two groups: moderate republicans (also named opportunists), willing to form alliances with moderate conservatives³⁴; and radical Republicans, who held more socialist positions³⁵. Consequently, votes for Republicans are divided into two subsamples: vote for moderate or radical Republicans³⁶. Communes that were located in France between 1815 and 1860 voted significantly more for radical Republican candidates (*i.e.* the ideological ancestors of the current left-wing) in the 1871 election. On average, radical Republican candidates had 7.8 additional percentage points in the treated municipalities³⁷.

In summary, different historical paths involving different politicization had a relatively short-run impact, since the treated side who was politicized through Republican clubs voted significantly more for radical Republican candidates in the 1871 legislative election.

³⁴group that included as notable representatives Jules Ferry, Jules Grévy, Adolphe Thiers.

³⁵Notable representatives: Léon Gambetta, Louis Blanc, Victor Hugo

³⁶Classification was made through group membership retrieved from the National Assembly website (<https://www2.assemblee-nationale.fr/sycomore/recherche>)

³⁷The first post-treatment vote was a Napoleonic plebiscite in 1870. Even though it cannot be considered a free election, I also collected electoral-district-level results through newspaper archives. Results seem to depict greater opposition to the Empire on the treated side, which can be interpreted as a Republican electoral behavior (Table A5, Lacroix (2018))

Persistence of the effect

One can now verify whether the previously observed pattern has been maintained throughout history. Table 1.2 presents the baseline results of the treatment’s effect on the commune-level first-round presidential election results³⁸. Communes that were located in France between 1815 and 1860 voted significantly more for left-wing candidates between 1995 and 2022. On average, left-wing candidates had 1.3 additional percentage points in the treated municipalities. This result seems to be mainly driven by moderate left-wing candidates (*i.e.* mostly the *Parti Socialiste*)³⁹.

Table 1.2: Treatment on 1st round presidential election results (1995-2022) - Absolute effect

Dep. Var.:	Left			Right			Abstention
	All	Far	Moderate	All	Far	Moderate	
$Vote_{m,t}$							
Treatment m	1.334 (0.594)**	0.384 (0.450)	0.920 (0.412)**	-0.391 (0.587)	-0.364 (0.553)	-0.027 (0.598)	0.485 (0.443)
N	3,335	3,335	3,335	3,335	3,335	3,335	3,335
R^2	0.54	0.56	0.83	0.58	0.69	0.54	0.55
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table 1.3 presents the baseline results for commune-level first-round legislative election results⁴⁰. The trends are more pronounced as treated-side communes significantly voted more for left-wing and less for right-wing candidates between 1997 and 2022. On average, left-wing candidates had 2.8 additional percentage points in the treated municipalities, as right-wing candidates had 3.7 percentage points less. Results are both driven by moderate left and right-wing candidates.

Absolute-level results are however incomplete, as one additional percentage point can be much or negligible regarding the context of an election. Consequently, as in Ochsner and Roesel (2020), relative share results are presented in Tables A6 and A7 in the appendix. The latter is the ratio between the absolute vote in a given

³⁸Discontinuity graphs presenting non-linear correlations for the same electoral outcomes are in Figure A9.

³⁹Note that the coefficients between right and left are not symmetrical, indicating that the sum of votes between left and right does not reach 100%. This is explained by the fact that some candidates have been classified as neither right nor left, such as self-defined centrists like François Bayrou or the rather unclassifiable Jacques Cheminade.

⁴⁰Discontinuity graphs presenting non-linear correlations are in Figure A10.

Table 1.3: Treatment on 1st round legislative election results (1997-2022) - Absolute effect

Dep. Var.: $Vote_{m,t}$	Left			Right			Abstention
	All	Far	Moderate	All	Far	Moderate	
Treatment m	2.803 (1.311)**	0.001 (0.644)	2.802 (1.395)**	-3.725 (1.680)**	0.566 (0.721)	-4.291 (1.667)**	-0.602 (0.568)
N	3,366	3,366	3,366	3,366	3,366	3,366	3,366
R^2	0.56	0.51	0.66	0.40	0.70	0.52	0.56
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

commune over the average vote in the overall sample. One can interpret the relative shares as the fraction of voters that can be attributed to the absolute effect of the discontinuity. The latter accounts for 6% of the average vote share for moderate left-wing candidates in first-round presidential elections and for more than 10% of the average vote share for moderate left and right-wing candidates in first-round legislative elections.

Tables A8 and A9 report absolute and relative results for the second round results. The intuition is the same for legislative elections as treated communes are voting significantly more for left-wing candidates. Results are not significant for presidential elections.

In summary, it seems that different historical trajectories and early politicization during the 19th century had an impact on a relatively short-run horizon, and still have an impact on current electoral outcomes, since communes that were in France between 1815 and 1860 are significantly voting more for left-wing candidates in both presidential and legislative elections. Dubious readers might still question whether these results are influenced by the numerous events that took place between 1871 and 1995. It is then necessary to examine the historical continuity in this result.

Historic continuity: electoral district level results

Many events potentially explaining this result could have taken place between 1871 and 1995. Consequently, the historical continuity of these results must be demonstrated in order to attribute this effect to the 1815-1860 discontinuity. One must verify if this result holds over time between 1871 and 1995. Doing so is however difficult since communes-level election results are available only since the

1995 presidential election. Electoral district-level results were retrieved by Sciences Po⁴¹ for every election of the Fifth Republic (from 1958 to nowadays). Thanks to the online newspaper archives of the *Bibliothèque Nationale de France* (Gallica), election results at the electoral district level for legislative election from 1876 to 1958 were retrieved⁴². More precisely, election reports from the daily newspapers *L'Humanité*, *Le Figaro*, *La Croix*, *Le Petit Journal*, *Le Petit Parisien* and *L'Echo de Paris* were cross-checked and collected in order to build this first-hand original historic database. Since the geographic unit is not anymore at the commune level but at the electoral district one, the following model is performed:

$$Y_{c,t} = \alpha + \beta Treatment_c + \phi_t + \varepsilon_{c,t}$$

$Y_{m,t}$ is the electoral outcome for electoral district c during an election t . It can be the vote share in percent for left or right candidates during a given presidential or legislative election. Given the extensive time span of over a hundred years, the notion of political left or right was an evolving concept. For instance, the *Parti Radical* was the most left political offer in the 1870s and then allied with center-right parties in the 1930s. Therefore, I have based my left-right classification on the works of Julliard (2014) and Richard (2017). $Treatment_m$ is a dummy taking the value 1 if a given electoral district c was located in France between 1815 and 1860. An election fixed effect ϕ is included in order to account for one election's particularity. Standard errors are clustered at the electoral district level.

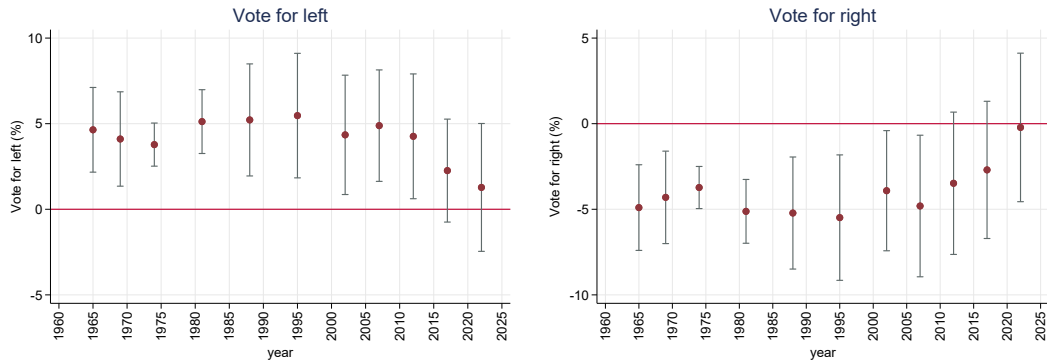
Even though this specification is imperfect, as it compares a wider population that is likely not homogeneous in terms of characteristics, it can provide insight into the historical continuity of the effect. Specifically, one must check whether older elections show similar results to contemporary elections (1995-2022). Figures 1.7 and 1.8 display the historical trend for respectively presidential and legislative elections. Tables A10, A11, A12, and A13 present the pooled presidential and legislative election results at the electoral-district level, notably by carrying-out two subsamples: ancient election (prior to 1995, *i.e.* elections for which there is no commune-level information) and contemporary elections (after 1995). It seems that both the 1871 and contemporary elections were not an exception, and the side that was in France between 1815 and 1860 experienced a higher vote share for left-wing candidates over the entire period. One can safely assume that such electoral behavior

⁴¹<https://www.data.gouv.fr/fr/pages/donnees-des-elections/>

⁴²Legislative election only, since all presidential elections were available in the Sciences Po database.

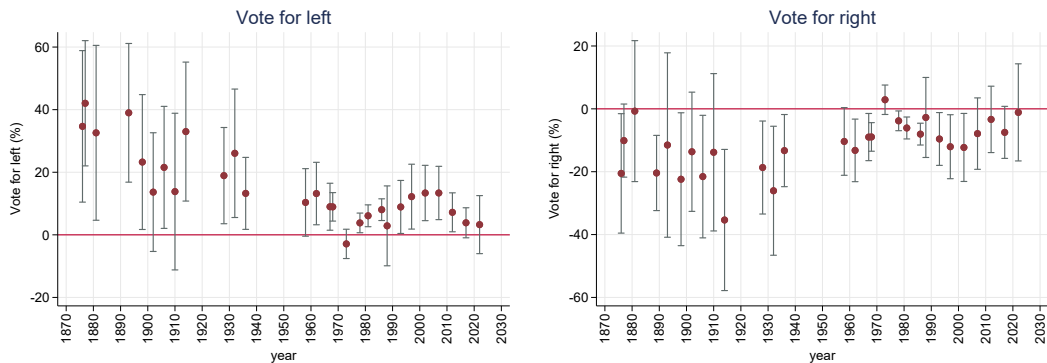
can also be observed at the commune level for older elections.

Figure 1.7: Historical continuity - Presidential elections at the electoral district level



Notes: Coefficients estimated with an election fixed effect, with spatial standard errors clustered at the electoral district level. Bars represent the 95% confidence interval. Observations: 26 from 1965 to 1981, 34 from 1988 to 2007, and 37 from 2012 to 2022

Figure 1.8: Historical continuity - Legislative elections at the electoral district level



Notes: Coefficients estimated with an election fixed effect, with spatial standard errors clustered at the electoral district level. Bars represent the 95% confidence interval. Observations: 34 from 1876 to 1936, 46 from 1958 to 1986, and 67 from 1988 to 2022. There are no coefficients during the Fourth Republic (1946-1958), because legislative election results were published at the department level.

1.4.2 External validity

As the 1815-1860 frontier partially follows administrative boundaries, dubious readers may think the previous results simply show the effect of different administrations or the consequences of close proximity to neighboring countries. Consequently, the commune-level model is used on other department borders that

are close to frontiers (Figure A11). As results in Tables A14 and A15 suggest, both north and south placebo frontiers do not translate discontinuities⁴³. It is unlikely that baseline results simply capture an administrative or proximity to neighboring countries' effect.

In order to verify whether this boundary is the one determining the discontinuity, other placebo frontiers were tested. More precisely, the 1815-1860 frontier has been moved 15km west and east. Results in Tables A16 and A17 are reassuring about the importance of the 1815-1860 frontier since the west and east placebos do not translate into significant differences in terms of voting.

There might also be concerns that either the northern or the southern part of the border drives entirely the results. Results in Tables A18, A19, A20, and A21 suggest the effect on presidential election results are driven by the northern part, whereas the southern part of the border explains the effect on legislative election outcomes.

One may also worry about discontinuities for the estimated share of cropland in 1800 and population density (*i.e.* the only two variables with a significant difference between the treatment and control groups), and about the role of income as a major determinant of voting behavior. These variables were consequently added to the model, and Tables A22 and A23 show that the results remain unchanged.

The confounding aspect of migration can also be of concern, especially if there was self-selected migration between treatment and control communes based on political criteria. In order to account for this issue, population growth per commune between 1876 and 1911 (INSEE) was added, notably in order to account for early migration. Results reported in Tables A24 and A25 indicate that controlling for population growth does not change the main findings.

Alternative dependent variables also depict a similar pattern for the treated side. Using Sciences Po's Inter-regional survey of political phenomena (CDSP) with self-positioning on the left-right scale for 14,637 individuals from the 9 departments considered between 1985 and 2004, and the data from Martelli (2010) on French Communist Party members by departments between 1913 and 2009, both sources validate that treated departments lean more toward the political left. They have significantly more people who self-declare as left-wing, as being close to a left-wing party, and more Communist Party members (Table A26).

Baseline results also remain robust to alternative definitions of left and right-wing

⁴³I didn't make a placebo border for the east side, as it would have captured various effects linked to Alsace's history, as depicted by Dehdari and Gehring (2022).

candidates⁴⁴ (Tables A27 and A28), alternative cluster at the canton level (Tables A29 and A30), alternative 5km and 15km spatial clusters (Tables A31, A32, A33, and A34), alternative 10, 20, 25, and 30km bandwidth (Tables A35, A36, A37, A38, A39, A40, A41, and A42), optimal bandwidth (Tables A43 and A44), and are not driven by one commune in particular (Figures A12 and A13) or by commune that merged between 1995 and 2017 ⁴⁵ (Tables A45 and A46).

1.4.3 Repression against Republicans in 1851

So far, the results presented above mostly display a long-run impact of different historic trajectories. This could be explained by several causes, such as the different politicization processes on each side of the border that occurred at the beginning of the 19th century. The political repression of 1851, which occurred only on the French side of the border, may have played a role in this outcome. Even if the discontinuity is probably not entirely due to the 1851 events, one can try to understand how this political repression inter-played with the politicization made earlier in the 19th century. Did it reinforce the preference for left-wing, as would suggest the findings of Nugent (2020); or did it mitigate it, as shown by Young (2019)?

“Administrative repression created martyrs to the republican cause and strengthened the solidarity of local populations against the state”. In order to test the intuition of Margadant (1979), I consider the role of repression on the preference for left-wing candidates in the treated side by adding the size of 1851 repression by communes in the model.

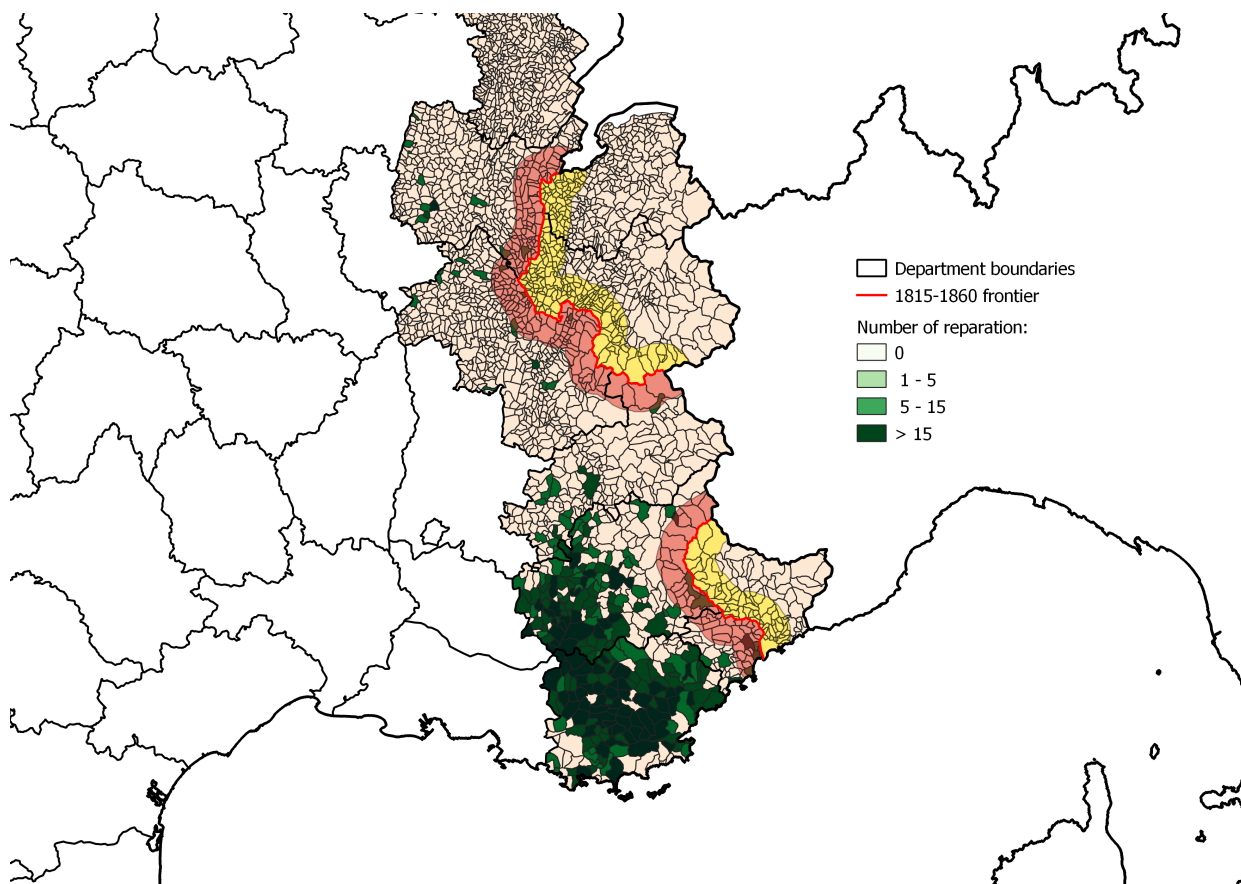
Based on the archive work of Devos (1992), who collected the pension requests of the 1851 repression’s victims following the law of reparation of 1881⁴⁶. The latter contains information about the name of the person who was repressed, the place and date of birth, as well as the place where the person was in 1851 and his or her occupation. The number of pension claims by communes is going to be the measure of political repression. This measure could be subject to selection bias since these data may not measure repression exhaustively. For example, if a young man was repressed in 1851, but is no longer alive or in France, and has no parents or descendants who made the claim at the prefecture in 1881, this person will not appear in the data. However, according to Margadant’s archive work, the

⁴⁴Jean-Luc Mélenchon as moderate left, centrists as moderate right

⁴⁵which concerns 2.5% of the communes in the sample

⁴⁶The national reparation law voted by Republicans when they came to power in 1881 gave the opportunity for 1851 political repression victims or their family to be financially compensated. The request had to be made to the prefecture, then a commission approved or rejected the request.

Figure 1.9: 1851 repression in southeastern France



insurgents were not particularly younger than the rest of the population. And even if a selection bias persists, it would only contribute to underestimating our effect. Regarding Figure 1.9, the political repression situation in 1851 was different between the Savoy Duchy frontier in the north and the Nice County in the south. Since the Republican insurgency mostly took place in the southern part, political repression was consequently tougher in this area. Some departments took advantage of this situation to repress locally known Republican notables, although there were no major insurrectionary events. Figure A14 shows the same information but with a binary equal to 1 if at least one individual was repressed in the commune. Table A47 shows political repression statistics for communes within the 15km bandwidth.

Results when adding the number of repressed per communes in the model are shown in Tables 1.4 and 1.5⁴⁷. On average, a repressed commune on the treated

⁴⁷One may wonder why the *Repressed* variable does not interact with the treatment. It is simply because there was no repressed on the control side (*i.e.* the Savoy Duchy and the Nice County) since they were not in France during 1851. Therefore, an interaction term is equivalent to the *Repressed* variable.

side would tend to have a significantly smaller preference for left-wing candidates, hence validating Young (2019) fear hypothesis. Communes subjected to repression on the treated side would also exhibit a significantly higher abstention rate. Targeted repression against Republicans would have diminished preference for the left, but not enough in order to reverse the initial preference for left-wing candidates. For instance, a commune in the Ain department named Belley counted 4 citizens repressed in 1851. On average, this commune would vote 2.357% more for left-wing candidates in the legislative elections, which is lower than their neighboring municipalities that have not experienced political repression, and vote on average 2.861% more for left-wing candidates. In other words, political repression could not change the initial preference brought by politicization.

Table 1.4: Treatment on 1st round presidential election results (1995-2022) - Number of repressed individuals by commune

Dep. Var.: $Vote_{m,t}$	Left			Right			Abstention
	All	Far	Moderate	All	Far	Moderate	
Treatment m	1.346 (0.596)**	0.393 (0.450)	0.923 (0.419)**	-0.400 (0.584)	-0.349 (0.552)	-0.051 (0.604)	0.459 (0.444)
Repressed m	-0.025 (0.035)	-0.019 (0.027)	-0.006 (0.028)	0.020 (0.037)	-0.032 (0.036)	0.052 (0.045)	0.057 (0.035)
N	3,335	3,335	3,335	3,335	3,335	3,335	3,335
R^2	0.54	0.56	0.83	0.58	0.69	0.54	0.55
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Since Margadant (1979) suggest that *Montagnard* clubs were often organized as peri-urban networks, with the main branch in an urban center (like the chief town of a canton) and smaller branches in villages nearby, one can safely assume that citizens repressed in a given commune had a network of Republican companion in nearby municipalities. Consequently, one can consider the effect of 5km buffers around repressed communes, since the reprimanded citizen is probably close to people living within this radius. Tables A48 and A49 present the results with a 5km buffer around repressed communes. The intuition remains the same: On average, communes close to a repressed municipality have a significantly smaller preference for left-wing candidates.

The intuition stays unchanged when considering a 10km radius around repressed

Table 1.5: Treatment on 1st round legislative election results (1997-2022) - Number of repressed individuals by commune

Dep. Var.: $Vote_{m,t}$	Left			Right			Abstention
	All	Far	Moderate	All	Far	Moderate	
Treatment $_m$	2.861 (1.311)**	0.018 (0.650)	2.843 (1.400)**	-3.839 (1.683)**	0.579 (0.722)	-4.418 (1.676)***	-0.650 (0.566)
Repressed $_m$	-0.126 (0.056)**	-0.037 (0.027)	-0.090 (0.052)*	0.246 (0.096)**	-0.027 (0.041)	0.273 (0.086)***	0.103 (0.048)**
N	3,366	3,366	3,366	3,366	3,366	3,366	3,366
R^2	0.56	0.51	0.66	0.40	0.70	0.52	0.56
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

communes (Tables A50 and A51), when using a dummy variable equal to one if at least one citizen was repressed in the commune (Tables A52 and A53), and when expressing the repression relative to the population (share of repressed citizen per thousand inhabitants, Tables A54 and A55). Despite the relatively different repression situation between North and South, results in Tables A56, A57, A58, and A59 suggest a similar effect between the northern and southern parts of the border.

These results illustrate the Young (2019) hypothesis since communes that have lived political repression against Republicans have a significantly lower preference for left-wing candidates in current elections, as compared to neighboring municipalities that did not suffer such repression. The targeted political repression against Republicans reduced the long-run preference for the left, but not to the point of reversing the initial preference brought by the different historic trajectories.

1.5 Channels

1.5.1 How? Political dynasties and memories of the repression

The previous section highlighted the long-run effects of politicization and political repression on current electoral results. How this memory has been passed on from the 19th century to the present day? Existing literature might suggest that memories of politicization and repression have been transmitted through generations via parental socialization, thus modifying political demand on the treated side. It is also possible that repression has affected individuals whose descendants have risen

to local elected positions (such as mayor), thus creating a political offer in which the personal memory of repression remains, hence influencing the electoral behavior of its administered citizens.

In order to test this hypothesis of a change in the political offer, I investigate repressed political dynasties in the treated communes with a definition similar to Lacroix et al. (2023): thanks to Devos (1992) archive works and the genealogy of French mayors⁴⁸, I can match the names of repressed citizens and mayors between 1851 to nowadays. Repressed political dynasties would be defined here as mayors who share a family name with individuals who experienced repression in 1851 within a radius of 20 kilometers around the commune where these mayors are in office. Communes in green in Figure 1.10 had at least one mayor with potentially repressed ancestors in the department. Note that some communes on the control side appear in green since it is possible that repressed citizens migrated to the control side after the events. From this information, I built the relative length of repressed political dynasties per commune, defined as the share of years with a mayor with potentially repressed ancestors divided by the total number of years between 1851 and 2022. This measure is the preferred one since it accounts for heterogeneity in terms of repressed political dynasties. The effect may be different whether the commune had a “repressed” mayor for 6 or 50 years. This measure also enables considering the length of the political dynasties, which can be a sign of political conservatism in and of itself. Table A60 presents descriptive statistics for different measures of repressed political dynasties.

Tables 1.6 and 1.7 present results with an interaction between the treatment and the share of years with a mayor likely to have repressed ancestors. The attenuated preference for the left due to repression is likely to be driven by the local political offer since communes that potentially had repressed political dynasties have a significantly lower preference for the left. Margin graphs in Figures A15 and A16 confirm that repressed political dynasties reduce the preference for the left.

Results are robust to alternative definitions of repressed-political dynasties, such as a binary variable if the commune had at least one mayor with potentially repressed ancestors (Tables A61 and A62), and with the total number of years with such mayor in office (Tables A63 and A64). In summary, the memory of the repression is likely to have been transmitted through repressed political dynasties, since communes where mayors have ancestors who experienced repression in 1851 have a significantly lower

⁴⁸Mayors names were cross-checked between Francegenweb, geneawiki, and Wikipedia.

Table 1.6: Presidential elections (1995-2022) - Interaction with relative number of years with mayor having the same name as repressed citizens 20km around

Dep. Var.: $Vote_{m,t}$	Left			Right			Abstention
	All	Far	Moderate	All	Far	Moderate	
Treatment m	1.388 (0.598)**	0.425 (0.456)	0.932 (0.416)**	-0.437 (0.583)	-0.319 (0.559)	-0.118 (0.604)	0.483 (0.445)
Rel. Length m	0.122 (0.051)**	0.080 (0.046)*	0.040 (0.020)**	-0.104 (0.052)**	0.060 (0.047)	-0.164 (0.054)***	-0.020 (.)
Treatment m x Rel. Length m	-0.239 (0.064)***	-0.110 (0.055)**	-0.127 (0.034)***	0.206 (0.069)***	0.030 (0.056)	0.177 (0.075)**	0.087 (0.033)***
N	3,335	3,335	3,335	3,335	3,335	3,335	3,335
R^2	0.54	0.56	0.83	0.58	0.69	0.54	0.55
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

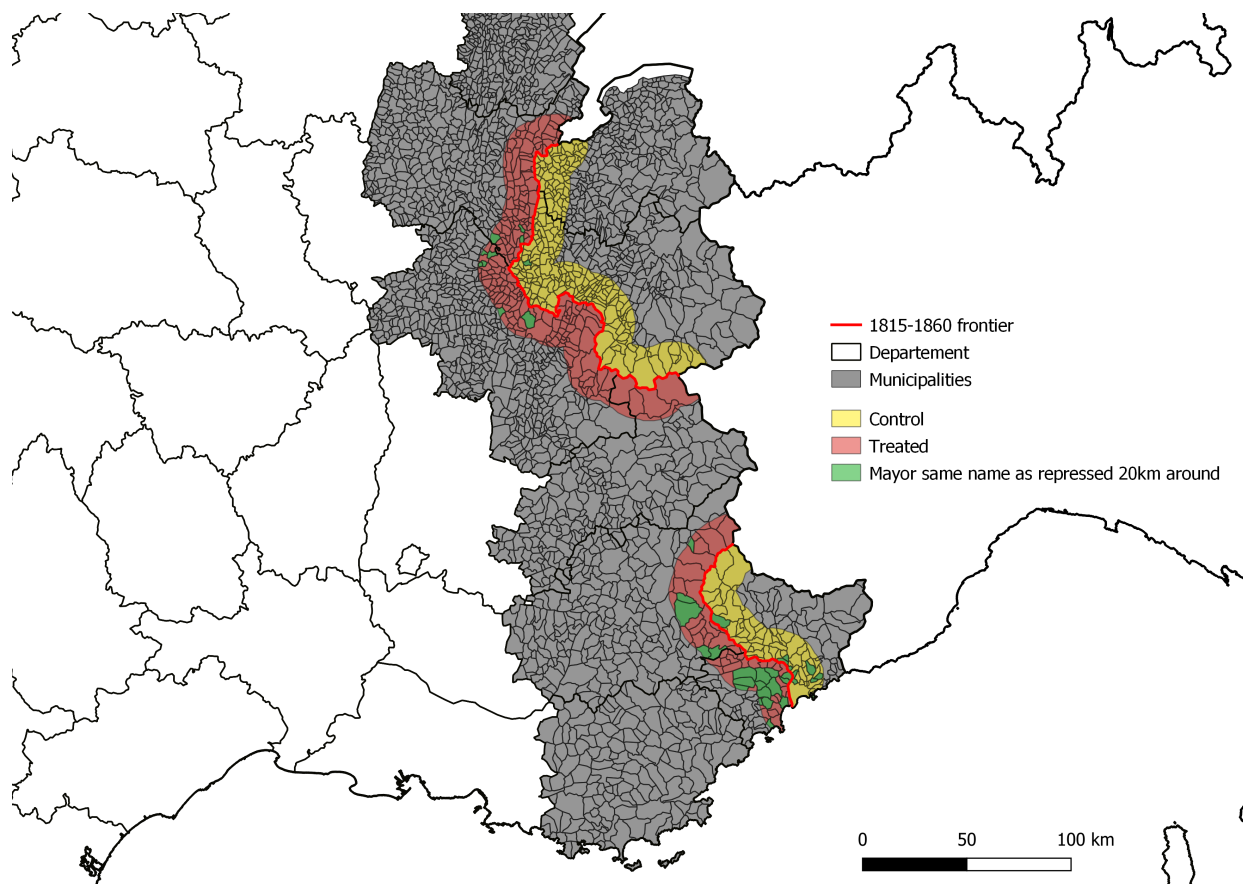
Table 1.7: Legislative elections (1997-2022) - Interaction with relative number of years with mayor having the same name as repressed citizens 20km around

Dep. Var.: $Vote_{m,t}$	Left			Right			Abstention
	All	Far	Moderate	All	Far	Moderate	
Treatment m	2.880 (1.318)**	0.097 (0.678)	2.784 (1.394)**	-3.736 (1.658)**	0.607 (0.720)	-4.343 (1.642)***	-0.558 (0.568)
Rel. Length m	0.163 (0.078)**	0.171 (0.093)*	-0.007 (0.051)	-0.030 (0.081)	0.052 (0.064)	-0.082 (0.091)	0.049 (0.010)***
Treatment m x Rel. Length m	-0.279 (0.092)***	-0.171 (0.090)*	-0.107 (0.076)	0.077 (0.110)	0.041 (0.081)	0.036 (0.130)	0.090 (0.038)**
N	3,366	3,366	3,366	3,366	3,366	3,366	3,366
R^2	0.56	0.51	0.66	0.40	0.70	0.52	0.56
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Figure 1.10: Communes with repressed-political dynasties



preference for the left.

1.5.2 How? Emigration of repressed citizens

As indicated in the introduction with the example of “Chacaille”, the effect of repression may have persisted over time with the migration of citizens subjected to it. According to [Margadant \(1979\)](#), approximately 34% of individuals arrested in 1851 were transported. This massive enforced emigration, coupled with the voluntary exile of citizens fearing repression, could have mechanically led to a decline in the number of Republicans, hence impacting the network of left-wing activists for decades. The archive work of [Devos \(1992\)](#) allows exploring this emigration mechanism since it provides detailed information on the legal sanction applied to repressed individuals. It also includes if they were sent to locations like Algeria or Guiana, or if they chose to go into exile. From this data, the number of repressed citizens leaving the commune for either forced transportation or exile is constructed. Table [A65](#) presents descriptive statistics on emigration variables. Tables [1.8](#) and [1.9](#)

present the model with the emigration variable included⁴⁹:

Table 1.8: Treatment on 1st round presidential election results (1995-2022) - Interaction with number of repressed who emigrated

Dep. Var.: $Vote_{m,t}$	Left			Right			Abstention
	All	Far	Moderate	All	Far	Moderate	
Treatment m	1.327 (0.595)**	0.382 (0.450)	0.915 (0.418)**	-0.388 (0.583)	-0.362 (0.552)	-0.026 (0.602)	0.481 (0.444)
Emigration m	0.117 (0.141)	0.026 (0.121)	0.094 (0.107)	-0.046 (0.142)	-0.039 (0.147)	-0.007 (0.190)	0.076 (0.153)
N	3,335	3,335	3,335	3,335	3,335	3,335	3,335
R^2	0.54	0.56	0.83	0.58	0.69	0.54	0.55
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table 1.9: Treatment on 1st round legislative election results (1997-2022) - Interaction with number of repressed who emigrated

Dep. Var.: $Vote_{m,t}$	Left			Right			Abstention
	All	Far	Moderate	All	Far	Moderate	
Treatment m	2.824 (1.304)**	0.002 (0.654)	2.822 (1.395)**	-3.748 (1.651)**	0.570 (0.716)	-4.318 (1.641)***	-0.639 (0.572)
Emigration m	-0.345 (0.195)*	-0.009 (0.066)	-0.336 (0.194)*	0.384 (0.175)**	-0.067 (0.145)	0.451 (0.151)***	0.614 (0.203)***
N	3,366	3,366	3,366	3,366	3,366	3,366	3,366
R^2	0.56	0.51	0.66	0.40	0.70	0.52	0.56
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

On average, communes with repressed citizens who emigrated have a significantly smaller preference for left-wing candidates. Once again, the effect is not large enough to reverse the initial preference for the left. For example, 1 citizen emigrated in 1851 and left the commune of Jausiers in the Alpes-de-Haute-Provence department. On average, this municipality would vote 2.479% more for left-wing candidates during the legislative elections, which is lower than neighboring communes that had no

⁴⁹Unlike analyses of repressed-political dynasties, the emigration variable does not interact since there were no repressed citizens on the control side.

documented emigration during 1851's repression. One may also note the larger coefficient for the emigration variable as compared to the repression one in Table 1.5, hence suggesting that mechanical reduction in the number of Republican activists played an important role in the repression's persistent effect on electoral outcomes.

1.5.3 Why? Lack of population mixing

Previous sections presented the long-run effects of politicization and political repression, and how the effect of the repression was transmitted. One may now ask what explains this persistent difference between treated and control communes, despite 150 years of shared history. The lack of population mixing can be a potential explanation for such lasting differences in political beliefs. Opinions can be expected to be firmly anchored if people have little contact with others from different regions. For instance, individuals from control communes will hardly change their opinions if they never encounter people from treated communes. In such cases, parental socialization will play a more important role in the construction of political beliefs, which ease the persistence of the previously established discontinuity across generations (Peisakhin, 2013). On the contrary, as suggested in Daudin et al. (2019) and Barsbai et al. (2017), contact with different populations can shape preferences, and individuals living in control communes can change their opinions if they meet people from the treated area. Using French academic division⁵⁰ and employment areas⁵¹ can be a way to verify the role of population mixing in persistence or convergence.

More precisely, the sample can be divided into two: treated and control communes that are located in the same academy or employment zone, versus communes that are located in different ones. People from the first sample are expected to mix more since they are very likely to study in the same universities and/or work in the same areas. Figures A17 and A18 respectively represent the academic division and employment zones in southeastern France.

Tables 1.10 and 1.11 present presidential and legislative election discontinuities for both samples according to academic division. Most of the time, differences in electoral results are driven by communes that are not in the same academy (except for the legislative first round). The same intuition is verified in the presidential and legislative second rounds (Tables A66 and A67). More precisely, treated and

⁵⁰An academy is an administrative district of the French Ministry of Education, responsible for supervising education within its geographic area

⁵¹As defined by the INSEE, an employment zone is a geographic area within which most people work.

Table 1.10: Treatment on 1st round presidential election results (1995-2022) - Subsamples with academic division

Dep. Var.:	Same academia		Different academia	
	Left	Right	Left	Right
$Vote_{m,t}$				
Treatment m	-0.326 (0.807)	1.274 (0.824)	2.083 (0.735)***	-1.434 (0.611)**
N	2,099	2,099	1,797	1,797
R^2	0.63	0.56	0.58	0.46
Commune controls	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table 1.11: Treatment on 1st round legislative election results (1997-2022) - Subsamples with academic division

Dep. Var.:	Same academia		Different academia	
	Left	Right	Left	Right
$Vote_{m,t}$				
Treatment m	4.197 (1.312)***	-0.858 (2.071)	0.701 (2.222)	-6.220 (2.585)**
N	1,812	1,812	1,554	1,554
R^2	0.58	0.37	0.58	0.49
Commune controls	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

control communes whose inhabitants are likely to mix during higher education do not significantly vote differently from one another. On the opposite, treated and control communes who are less likely to mix during higher education vote differently across the 1815-1860 border.

Table 1.12: Treatment on 1st round presidential election results (1995-2022) - Subsamples with employment zone

Dep. Var.:	Same zone		Different zone	
	Left	Right	Left	Right
$Vote_{m,t}$				
Treatment m	1.395 (0.615)**	-0.750 (0.546)	3.952 (1.354)***	-3.075 (1.420)**
N	1,936	1,936	1,960	1,960
R^2	0.62	0.49	0.62	0.54
Commune controls	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table 1.13: Treatment on 1st round legislative election results (1997-2022) - Subsamples with employment zone

Dep. Var.:	Same zone		Different zone	
	Left	Right	Left	Right
$Vote_{m,t}$				
Treatment m	0.739 (1.778)	-4.369 (1.992)**	7.925 (2.177)***	-3.634 (3.734)
N	1,674	1,674	1,692	1,692
R^2	0.51	0.52	0.61	0.37
Commune controls	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Tables 1.12 and 1.13 outline election discontinuities based on employment zone samples. Once again, most of the differences in electoral results stem from communes where individuals are working in different employment zones (except for the legislative first round). Similar results can be verified with second rounds (Tables

A68 and A69). In other words, treated and control communes whose inhabitants are working in the same zone do not vote differently from each other. And treated communes whose inhabitants do not work with control people are driving the discontinuity.

In summary, the persistent effects of this discontinuity on electoral results can be explained by the lack of population mixing during higher education and/or at work.

1.6 Conclusion

Based on the results of these spatial discontinuity design regressions, different historic trajectories during a century when contemporary political opinions were forged had long-run impacts on current electoral outcomes. While comparing homogeneous communes across the 1815-1860 border, the French side, politicized through Republican secret societies during the early 19th century, voted significantly more for left-wing candidates during the legislative election of 1871. The effect persisted for elections between 1995 and 2022. In other words, politicization had lasting effects, as the treated side still exhibits a preference for the ideological heirs of 19th-century Republicans more than 150 years after the events. The role of political repression in this discontinuity is then further discussed thanks to the number of repressed citizens by communes in 1851. Treated communes that were more repressed in 1851 tend to have a lower preference for left-wing candidates, but the repression does not entirely reverse the initial preference for the left. The repression's memory is likely to have been transmitted through repressed political dynasties and emigration following the repression. The discontinuity effects on electoral results seem to persist if populations from both sides are relatively unmixed.

Appendix

Figure A1: Map of Savoy Duchy and Nice County

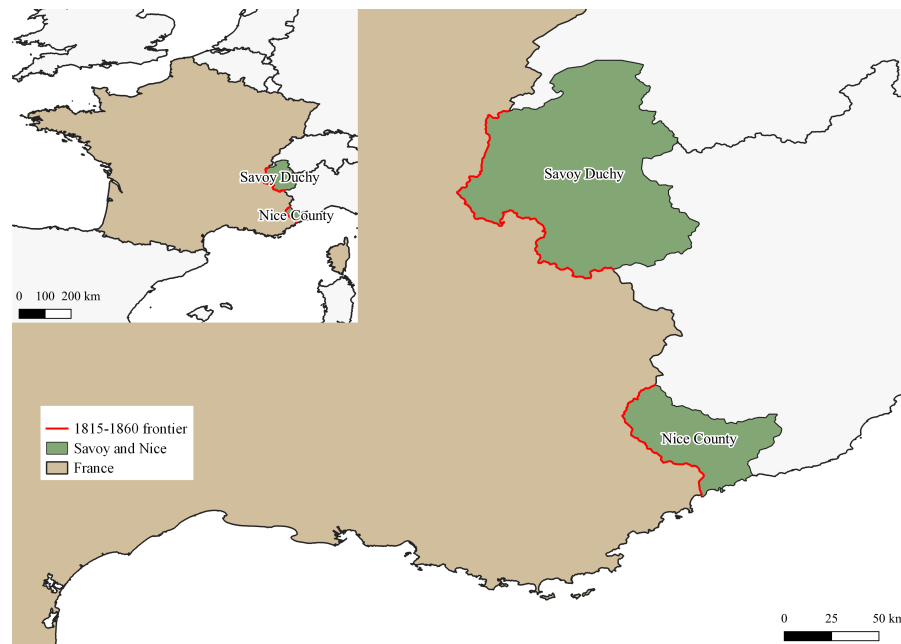


Figure A2: Historic timeline of the Duchy of Savoy and Nice County

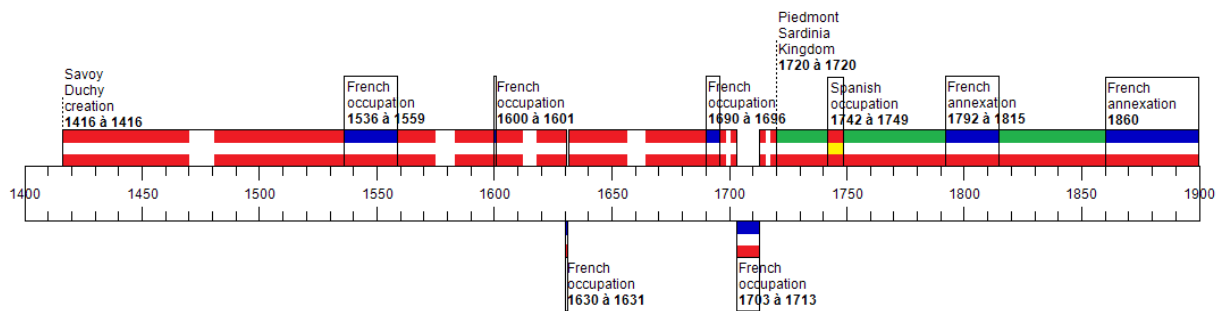


Table A1: History of occupations and annexations of Savoy Duchy and Nice County territories

Date	Reason	Direction
1536	Italian Wars	Duchy of Savoy to France
1559	Treaty of Cateau-Cambrésis	France to Duchy of Savoy
1660	Franco-Savoyard War	Duchy of Savoy to France
1601	Treaty of Lyon	France to Duchy of Savoy
1630	Thirty Years' war	Duchy of Savoy to France
1631	Treaty of Cherasco	France to Duchy of Savoy
1690	Nine Years' War	Duchy of Savoy to France
1696	Treaty of Turin	France to Duchy of Savoy
1703	War of the Spanish Succession	Duchy of Savoy to France
1713	Treaty of Utrecht	France to Duchy of Savoy
1742	War of the Austrian Succession	Piedmont Sardinia Kingdom to Spain
1749	Treaty of Aix-la-Chapelle	Spain to Piedmont Sardinia Kingdom
1792	Annexation by the French Revolutionary forces	Piedmont Sardinia Kingdom to France
1815	Congress of Vienna	France to Piedmont Sardinia Kingdom
1860	Risorgimento and Italian Unification	Piedmont Sardinia Kingdom to France

Table A2: Descriptive statistics - Social conflicts (15km bandwidth)

	Obs.	Mean	Std. Dev.	Min	Max
<i>Total riot: 1700-1789</i>	561	0.148	0.445	0	4
1750-1789	561	0.062	0.249	0	2
<i>Riot against:</i>					
Fiscality	561	0.05	0.240	0	2
State forces	561	0.018	0.132	0	1
Lord	561	0.021	0.157	0	2
Nobles	561	0.007	0.084	0	1
Church	561	0.005	0.094	0	2
Local dignitary	561	0.002	0.042	0	1
Local authorities	561	0.005	0.094	0	2
Religion	561	0.005	0.073	0	1
Other	561	0.012	0.126	0	2

Table A3: Pre-treatment electoral outcomes - Number of left-wing representatives per department

Dep. Var. <i>LeftRepresentatives_d</i>	Convention 1792-1795	Conseil 1795-1799	Corps Législatif 1800-1814
treated <i>d</i>	-0.004 (0.157)	-0.016 (0.125)	-0.045 (0.129)
<i>N</i>	55	84	55
<i>R</i> ²	0.00	0.00	0.00

Robust standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A4: Post-treatment discontinuities - Migration to Paris

	Number of migrant to Paris (TRA)	Share of local names in Paris (1890-2000)	Share of local names in Paris (1890-1910)
Treated <i>c</i>	-1473.688 (976.784)	-0.005 (0.029)**	-0.010 (0.301)
<i>N</i>	75	77	14
<i>R</i> ²	0.03	0.05	0.08

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A5: Treatment on Napoleonic plebiscite of 1870

Dep. Var.: <i>Vote_c</i>	% of votes expressed			% of registered voters		
	Yes	No	Invalid	Yes	No	Invalid
Treated <i>c</i>	-2.491 (5.303)	6.490 (3.957)	-0.813 (1.041)	-1.301 (4.291)	5.084 (2.991)*	-0.487 (0.712)
<i>N</i>	35	35	35	34	34	34
<i>R</i> ²	0.01	0.06	0.04	0.00	0.06	0.03

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A6: Treatment on 1st round presidential election results (1997-2022) - Relative effect

Dep. Var.:	Left			Right			Abstention
	All	Far	Moderate	All	Far	Moderate	
$Vote_{m,t}$							
Treatment m	0.043 (0.019)**	0.003 (0.029)	0.063 (0.024)***	-0.005 (0.010)	-0.016 (0.023)	-0.004 (0.021)	0.029 (0.025)
N	3,335	3,335	3,335	3,335	3,335	3,335	3,335
R^2	0.28	0.19	0.26	0.33	0.32	0.25	0.21
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A7: Treatment on 1st round legislative election results (1997-2022) - Relative effect

Dep. Var.:	Left			Right			Abstention
	All	Far	Moderate	All	Far	Moderate	
$Vote_{m,t}$							
Treatment m	0.096 (0.041)**	-0.002 (0.083)	0.151 (0.064)**	-0.078 (0.038)**	-0.080 (0.170)	-0.159 (0.068)**	-0.017 (0.014)
N	3,366	3,366	3,366	3,366	3,366	3,366	3,366
R^2	0.33	0.17	0.34	0.20	0.16	0.16	0.20
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A8: Treatment on 2nd round presidential election results (1995-2022)

Dep. Var.:	Absolute		Relative	
	Least right	Abstention	Least right	Abstention
$Vote_{m,t}$				
Treatment m	0.246 (0.627)	0.646 (0.479)	0.005 (0.013)	0.033 (0.026)
N	3,334	3,335	3,334	3,335
R^2	0.74	0.47	0.25	0.16
Commune controls	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A9: Treatment on 2nd round legislative election results (1997-2022)

Dep. Var.:	Absolute			Relative		
	Left	Right	Abstention	Left	Right	Abstention
$Vote_{m,t}$						
Treatment $_m$	5.779 (2.293)**	-5.319 (4.442)	-0.323 (0.743)	0.270 (0.152)*	-0.084 (0.127)	-0.011 (0.017)
N	3,083	3,083	3,083	3,083	3,083	3,083
R^2	0.69	0.43	0.67	0.16	0.24	0.22
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A10: Treatment on presidential election results (1965-2022) - Electoral district level, vote for left

Dep. Var.:	Absolute vote share			Relative vote share		
	All	1965-1995	1995-2022	All	1965-1995	1995-2022
$Vote_{c,t}$						
Treatment $_c$	4.061 (1.447)***	4.610 (1.489)***	3.704 (1.699)**	0.111 (0.041)**	0.116 (0.038)***	0.107 (0.051)**
N	351	138	213	351	138	213
R^2	0.66	0.73	0.50	0.10	0.17	0.08
Election FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at the legislative constituency level

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A11: Treatment on legislative election results (1876-2022) - Electoral district level, vote for left

Dep. Var.:	Absolute vote share			Relative vote share		
	All	1876-1997	1997-2022	All	1876-1997	1997-2022
$Vote_{c,t}$						
Treatment $_c$	15.519 (2.760)***	18.275 (3.251)***	6.321 (2.224)***	0.411 (0.068)***	0.480 (0.079)***	0.183 (0.065)***
N	940	727	213	940	727	213
R^2	0.27	0.25	0.50	0.10	0.11	0.12
Election FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at the legislative constituency level

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A12: Treatment on presidential election results (1876-2022) - Electoral district level, vote for right

Dep. Var.:	Absolute vote share			Relative vote share		
	All	1965-1995	1995-2022	All	1965-1995	1995-2022
$Vote_{c,t}$						
Treatment c	-3.897 (1.539)**	-4.687 (1.491)***	-3.383 (2.017)	-0.070 (0.029)**	-0.081 (0.026)***	-0.063 (0.040)
N	351	138	213	351	138	213
R^2	0.58	0.70	0.43	0.08	0.18	0.05
Election FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at the legislative constituency level

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A13: Treatment on legislative election results (1876-2022) - Electoral district level, vote for right

Dep. Var.:	Absolute vote share			Relative vote share		
	All	1876-1997	1997-2022	All	1876-1997	1997-2022
$Vote_{c,t}$						
Treatment c	-11.137 (2.280)***	-13.305 (2.626)***	-3.898 (3.405)	-0.274 (0.070)***	-0.332 (0.085)***	-0.080 (0.071)
N	940	727	213	940	727	213
R^2	0.36	0.38	0.23	0.04	0.04	0.02
Election FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at the legislative constituency level

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A14: Treatment on 1st round presidential election results (1995-2022)

Dep. Var.:	South placebo		North placebo	
	Left	Right	Left	Right
$Vote_{m,t}$				
Treatment m	-0.521 (0.734)	0.760 (0.751)	0.740 (0.395)*	-0.345 (0.474)
N	9,372	9,372	10,164	10,164
R^2	0.49	0.27	0.62	0.36
Commune controls	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A15: Treatment on 1st round legislative election results (1997-2022)

Dep. Var.:	South placebo		North placebo	
	Left	Right	Left	Right
$Vote_{m,t}$				
Treatment m	1.795 (1.246)	1.560 (1.818)	-0.497 (1.038)	3.592 (2.596)
N	9,372	9,372	10,230	10,230
R^2	0.38	0.17	0.56	0.30
Commune controls	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A16: Treatment on 1st round presidential election results (1995-2022) - Placebo frontiers

Dep. Var.:	15km West		15km East	
	Left	Right	Left	Right
$Vote_{m,t}$				
Treatment m	-0.558 (0.577)	0.684 (0.589)	-0.463 (0.751)	0.844 (0.841)
N	3,338	3,338	2,618	2,618
R^2	0.55	0.59	0.53	0.57
Commune controls	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A17: Treatment on 1st round legislative election results (1997-2022) - Placebo frontiers

Dep. Var.: $Vote_{m,t}$	15km West		15km East	
	Left	Right	Left	Right
Treatment m	-0.578 (0.709)	0.759 (0.914)	-0.257 (0.969)	2.260 (1.496)
N	3,360	3,360	2,627	2,627
R^2	0.57	0.45	0.55	0.35
Commune controls	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A18: Treatment on 1st round presidential election results (1995-2022) - North subsample

Dep. Var.: $Vote_{m,t}$	Left			Right			Turnout
	All	Far	Moderate	All	Far	Moderate	
Treatment m	1.669 (0.628)***	0.471 (0.483)	1.143 (0.436)***	-0.624 (0.580)	-1.253 (0.589)**	0.629 (0.601)	0.668 (0.548)
N	2,573	2,573	2,573	2,573	2,573	2,573	2,573
R^2	0.55	0.63	0.84	0.58	0.68	0.57	0.56
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A19: Treatment on 1st round presidential election results (1995-2022) - South subsample

Dep. Var.: $Vote_{m,t}$	Left			Right			Turnout
	All	Far	Moderate	All	Far	Moderate	
Treatment m	0.447 (1.341)	0.491 (1.078)	0.011 (1.000)	0.521 (1.461)	3.237 (1.117)***	-2.716 (1.416)*	0.120 (0.679)
N	762	762	762	762	762	762	762
R^2	0.53	0.45	0.80	0.48	0.75	0.60	0.54
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A20: Treatment on 1st round legislative election results (1997-2022) - North subsample

Dep. Var.:	Left			Right			Abstention
	All	Far	Moderate	All	Far	Moderate	
$Vote_{m,t}$							
Treatment m	1.449 (1.537)	-0.591 (0.589)	2.040 (1.628)	-2.884 (2.015)	-0.368 (0.872)	-2.516 (1.919)	-0.437 (0.665)
N	2,604	2,604	2,604	2,604	2,604	2,604	2,604
R^2	0.58	0.58	0.67	0.39	0.71	0.53	0.58
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A21: Treatment on 1st round legislative election results (1997-2022) - South subsample

Dep. Var.:	Left			Right			Turnout
	All	Far	Moderate	All	Far	Moderate	
$Vote_{m,t}$							
Treatment m	8.729 (1.780)***	2.855 (1.873)	5.874 (2.541)**	-8.009 (2.838)***	4.049 (0.955)***	-12.058 (3.072)***	-0.968 (1.015)
N	762	762	762	762	762	762	762
R^2	0.60	0.48	0.65	0.43	0.74	0.59	0.53
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A22: Treatment on 1st round presidential election results (1995-2022) - Characteristics with discontinuities added

Dep. Var.: $Vote_{m,t}$	Left			Right			Abstention
	All	Far	Moderate	All	Far	Moderate	
Treatment m	2.298 (0.602)***	1.378 (0.412)***	0.727 (0.464)	-1.753 (0.598)***	-0.069 (0.567)	-1.684 (0.457)***	0.021 (0.403)
Median Income m	-0.000 (0.000)***	-0.000 (0.000)	-0.000 (0.000)***	0.000 (0.000)	0.000 (0.000)***	-0.000 (0.000)***	-0.000 (0.000)***
Crop 1800 m	-0.060 (0.018)***	-0.041 (0.010)***	-0.041 (0.011)***	0.067 (0.019)***	-0.022 (0.020)	0.089 (0.020)***	-0.010 (0.013)
Pop. Density m	0.001 (0.000)***	0.000 (0.000)	0.000 (0.000)	-0.001 (0.000)***	0.000 (0.000)	-0.001 (0.000)***	0.002 (0.000)***
N	2,945	3,085	2,945	2,945	2,945	2,945	2,945
R^2	0.59	0.84	0.63	0.63	0.60	0.74	0.61
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A23: Treatment on 1st round legislative election results (1997-2022) - Characteristics with discontinuities added

Dep. Var.: $Vote_{m,t}$	Left			Right			Abstention
	All	Far	Moderate	All	Far	Moderate	
Treatment m	3.455 (1.285)***	0.151 (0.671)	3.304 (1.390)**	-3.772 (1.654)**	-0.152 (0.746)	-3.620 (1.650)**	0.132 (0.407)
Median Income m	-0.000 (0.000)***	-0.000 (0.000)***	0.000 (0.000)	0.000 (0.000)***	-0.000 (0.000)***	0.001 (0.000)***	-0.000 (0.000)***
Crop 1800 m	-0.062 (0.039)	-0.025 (0.014)*	-0.037 (0.038)	0.010 (0.073)	0.064 (0.030)**	-0.054 (0.078)	-0.007 (0.013)
Pop. Density m	0.000 (0.000)	-0.001 (0.000)***	0.001 (0.000)***	-0.001 (0.001)	-0.001 (0.000)*	0.000 (0.001)	0.001 (0.000)***
N	2,946	2,946	2,946	2,946	2,946	2,946	2,946
R^2	0.59	0.55	0.67	0.40	0.73	0.55	0.54
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A24: Treatment on 1st round presidential election results (1995-2022) - With population growth (1876-1911)

Dep. Var.: $Vote_{m,t}$	Left			Right			Abstention
	All	Far	Moderate	All	Far	Moderate	
Treatment m	1.291 (0.597)**	0.341 (0.453)	0.920 (0.412)**	-0.370 (0.594)	-0.369 (0.557)	-0.001 (0.606)	0.290 (0.428)
Pop. growth (1876-1911) m	0.005 (0.005)	0.005 (0.003)	0.000 (0.003)	-0.002 (0.004)	0.000 (0.003)	-0.003 (0.005)	0.022 (0.003)***
N	3,335	3,335	3,335	3,335	3,335	3,335	3,335
R^2	0.54	0.56	0.83	0.58	0.69	0.54	0.56
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A25: Treatment on 1st round legislative election results (1997-2022) - With population growth (1876-1911)

Dep. Var.: $Vote_{m,t}$	Left			Right			Abstention
	All	Far	Moderate	All	Far	Moderate	
Treatment m	2.853 (1.285)**	0.040 (0.647)	2.813 (1.383)**	-3.784 (1.658)**	0.547 (0.727)	-4.331 (1.648)***	0.290 (0.428)
Pop. growth (1876-1911) m	-0.005 (0.005)	-0.004 (0.004)	-0.001 (0.004)	0.006 (0.007)	0.002 (0.004)	0.004 (0.008)	0.022 (0.003)***
N	3,366	3,366	3,366	3,366	3,366	3,366	3,335
R^2	0.56	0.51	0.66	0.40	0.70	0.52	0.56
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius.

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A26: Treatment on alternative outcomes

Dep. Var. $Outcome_{i,d}$	CDSP (1985-2004)		PCF (1913-2009)
	Self-positioned Left	Close to Left party	PCF Members (%)
treated d	0.025 (0.008)***	0.041 (0.009)***	0.159 (0.026)***
N	13,809	14,637	451
R^2	0.01	0.01	0.83
Year FE	Yes	Yes	Yes

Robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A27: Presidential elections (1995-2022) - Alternative definition of right and left

Dep. Var.:	Left			Right		
	All	Far	Moderate	All	Far	Moderate
$Vote_{m,t}$						
Treatment m	1.208 (0.590)**	-0.109 (0.256)	1.317 (0.537)**	-1.209 (0.590)**	-0.364 (0.554)	-0.844 (0.669)
N	3,335	3,335	3,335	3,335	3,335	3,335
R^2	0.56	0.71	0.51	0.56	0.69	0.58
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A28: Legislative elections (1997-2022) - Alternative definition of right and left

Dep. Var.:	Left			Right		
	All	Far	Moderate	All	Far	Moderate
$Vote_{m,t}$						
Treatment m	2.803 (1.314)**	0.040 (0.610)	2.763 (1.410)*	-4.250 (1.060)***	0.560 (0.723)	-4.810 (1.170)***
N	3,366	3,366	3,366	3,366	3,366	3,366
R^2	0.56	0.45	0.53	0.62	0.70	0.59
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A29: 1st Round Presidential Election Results (1995-2022) - Alternative cluster at the canton level

Dep. Var.:	Left			Right			Abstention
	All	Far	Moderate	All	Far	Moderate	
$Vote_{m,t}$							
Treatment m	1.334 (1.272)	0.384 (0.763)	0.920 (0.753)	-0.391 (1.246)	-0.364 (0.901)	-0.027 (0.952)	0.485 (0.813)
N	3,335	3,335	3,335	3,335	3,335	3,335	3,335
R^2	0.54	0.56	0.83	0.58	0.69	0.54	0.55
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A30: 1st Round Legislative Election Results (1997-2022) - Alternative cluster at the canton level

Dep. Var.:	Left			Right			Abstention
	All	Far	Moderate	All	Far	Moderate	
$Vote_{m,t}$							
Treatment m	2.803 (1.582)*	0.001 (0.811)	2.802 (1.513)*	-3.725 (2.032)*	0.566 (0.761)	-4.291 (1.860)**	-0.602 (1.203)
N	3,366	3,366	3,366	3,366	3,366	3,366	3,366
R^2	0.56	0.51	0.66	0.40	0.70	0.52	0.56
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A31: Treatment on 1st round presidential election results (1995-2022) - 5km spatial clusters

Dep. Var.:	Left			Right			Abstention
	All	Far	Moderate	All	Far	Moderate	
$Vote_{m,t}$							
Treatment m	1.334 (0.643)**	0.384 (0.501)	0.920 (0.436)**	-0.391 (0.675)	0.384 (0.501)	0.920 (0.436)**	0.485 (0.447)
N	3,335	3,335	3,335	3,335	3,335	3,335	3,335
R^2	0.54	0.56	0.83	0.58	0.56	0.83	0.55
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 5km radius.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A32: Treatment on 1st round presidential election results (1995-2022) - 15km spatial clusters

Dep. Var.:	Left			Right			Abstention
	All	Far	Moderate	All	Far	Moderate	
$Vote_{m,t}$							
Treatment m	1.334 (0.642)**	0.384 (0.436)	0.920 (0.381)**	-0.391 (0.612)	0.384 (0.436)	0.920 (0.381)**	0.485 (0.429)
N	3,335	3,335	3,335	3,335	3,335	3,335	3,335
R^2	0.54	0.56	0.83	0.58	0.56	0.83	0.55
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 15km radius.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A33: Treatment on 1st round legislative election results (1997-2022) - 5km spatial clusters

Dep. Var.:	Left			Right			Abstention
	All	Far	Moderate	All	Far	Moderate	
$Vote_{m,t}$							
Treatment m	2.803 (1.179)**	0.001 (0.597)	2.802 (1.197)**	-3.725 (1.530)**	0.001 (0.597)	2.802 (1.197)**	-0.602 (0.657)
N	3,366	3,366	3,366	3,366	3,366	3,366	3,366
R^2	0.56	0.51	0.66	0.40	0.51	0.66	0.56
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 5km radius.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A34: Treatment on 1st round legislative election results (1997-2022) - 15km spatial clusters

Dep. Var.:	Left			Right			Abstention
	All	Far	Moderate	All	Far	Moderate	
$Vote_{m,t}$							
Treatment m	2.803 (1.376)**	0.001 (0.730)	2.802 (1.505)*	-3.725 (1.787)**	0.001 (0.730)	2.802 (1.505)*	-0.602 (0.517)
N	3,366	3,366	3,366	3,366	3,366	3,366	3,366
R^2	0.56	0.51	0.66	0.40	0.51	0.66	0.56
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 15km radius.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A35: Treatment on 1st round presidential election results (1995-2022) - 10km band

Dep. Var.:	Left			Right			Abstention
	All	Far	Moderate	All	Far	Moderate	
$Vote_{m,t}$							
Treatment m	1.466 (0.682)**	0.759 (0.485)	0.678 (0.497)	-0.078 (0.698)	-0.305 (0.631)	0.227 (0.753)	-0.007 (0.488)
N	2,216	2,216	2,216	2,216	2,216	2,216	2,216
R^2	0.54	0.56	0.84	0.58	0.73	0.55	0.57
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A36: Treatment on 1st round legislative election results (1997-2022) - 10km band

Dep. Var.:	Left			Right			Abstention
	All	Far	Moderate	All	Far	Moderate	
$Vote_{m,t}$							
Treatment m	2.843 (1.369)**	0.108 (0.571)	2.735 (1.397)*	-4.042 (1.653)**	0.368 (0.754)	-4.410 (1.703)***	-1.217 (0.644)*
N	2,238	2,238	2,238	2,238	2,238	2,238	2,238
R^2	0.56	0.53	0.66	0.41	0.72	0.55	0.59
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A37: Treatment on 1st round presidential election results (1995-2022) - 20km band

Dep. Var.:	Left			Right			Abstention
	All	Far	Moderate	All	Far	Moderate	
$Vote_{m,t}$							
Treatment m	1.141 (0.593)*	0.456 (0.439)	0.675 (0.412)	-0.255 (0.577)	-0.211 (0.519)	-0.044 (0.580)	0.192 (0.418)
N	3,769	3,769	3,769	3,769	3,769	3,769	3,769
R^2	0.54	0.55	0.83	0.58	0.69	0.53	0.55
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Figure A3: Topographic map

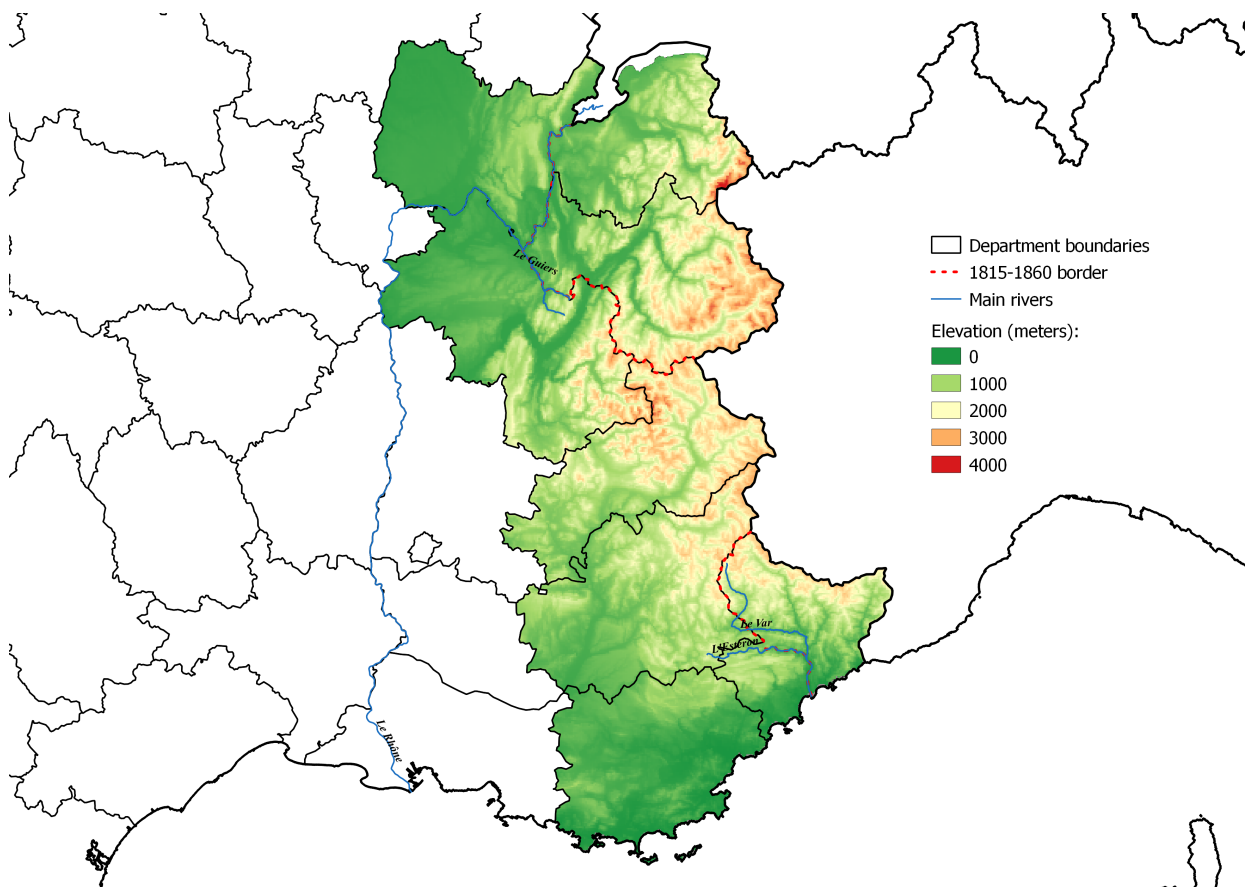


Figure A4: 1815-1860 border - Social conflicts between 1700 and 1789

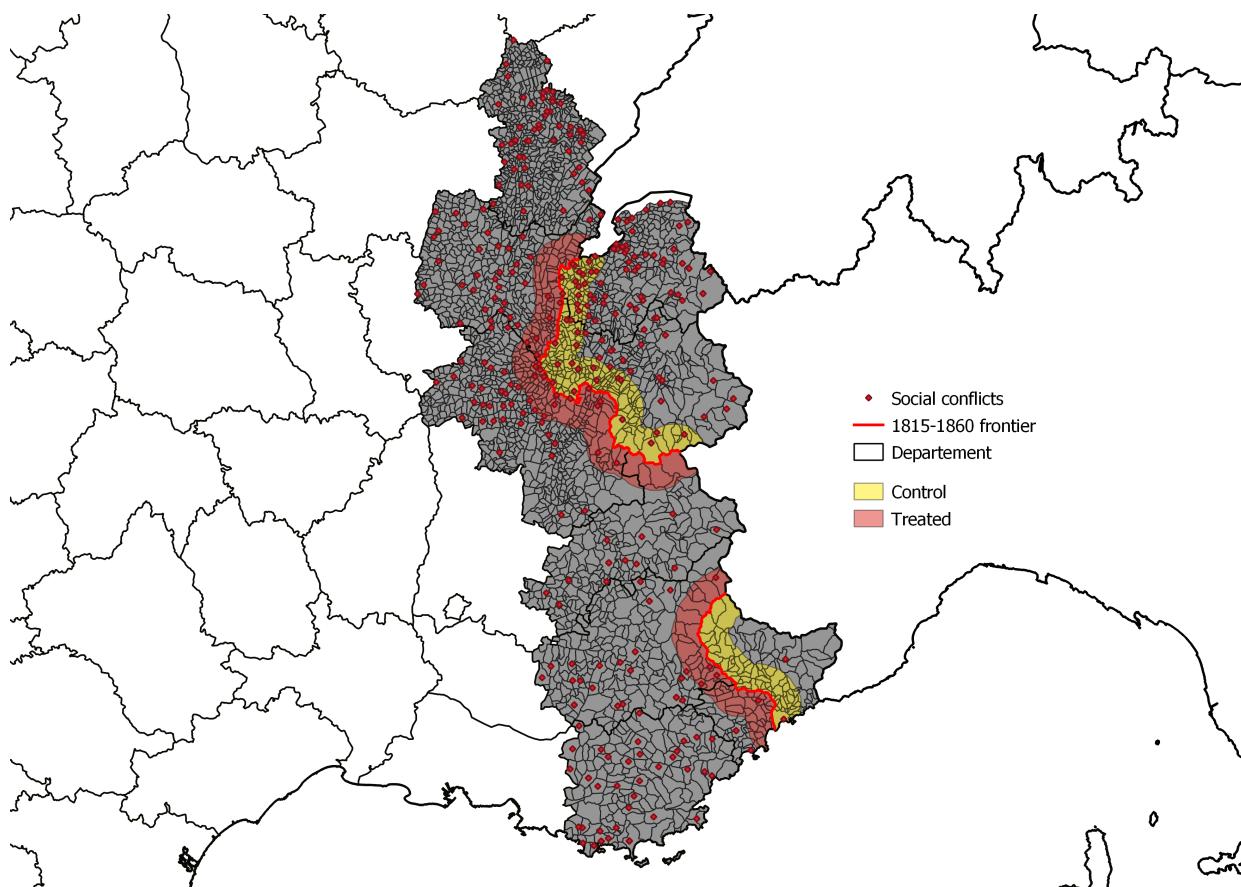


Figure A5: Share of *Jureur* priest by department (1791)

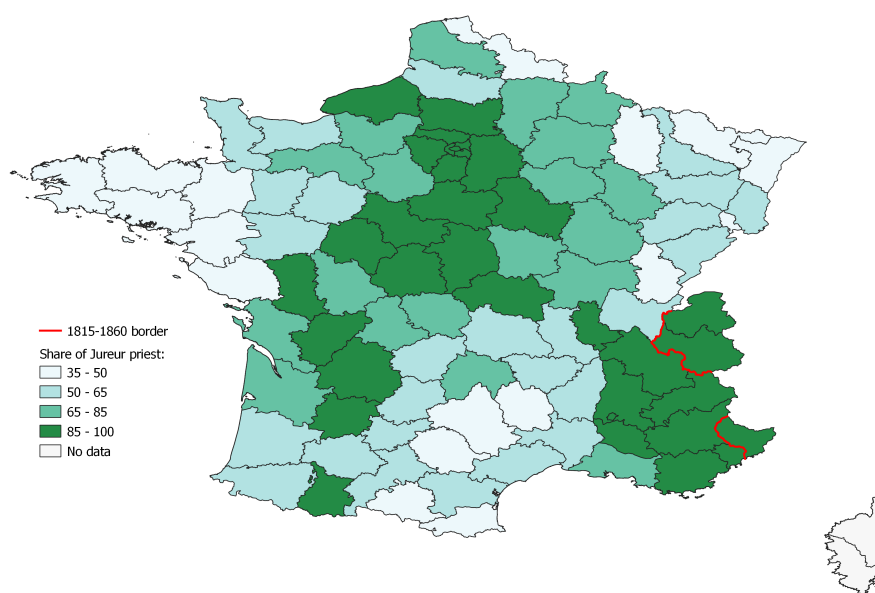
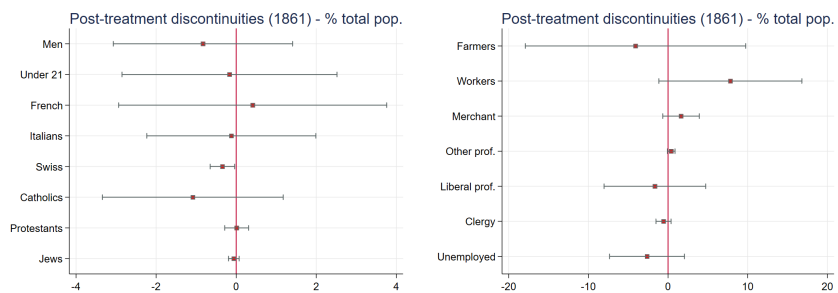


Figure A6: Post-treatment - Census 1861



Notes: Coefficients estimated at the arrondissement level. Bars represent the 95% confidence interval. Observations: 37.

Figure A7: Post-treatment - Population evolution

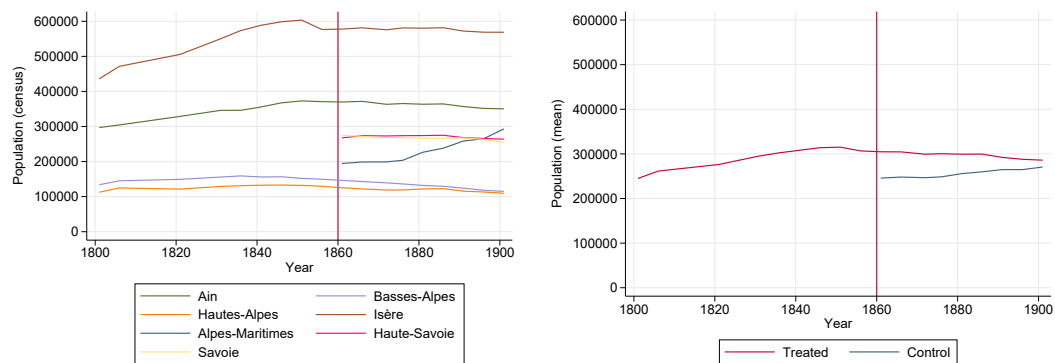
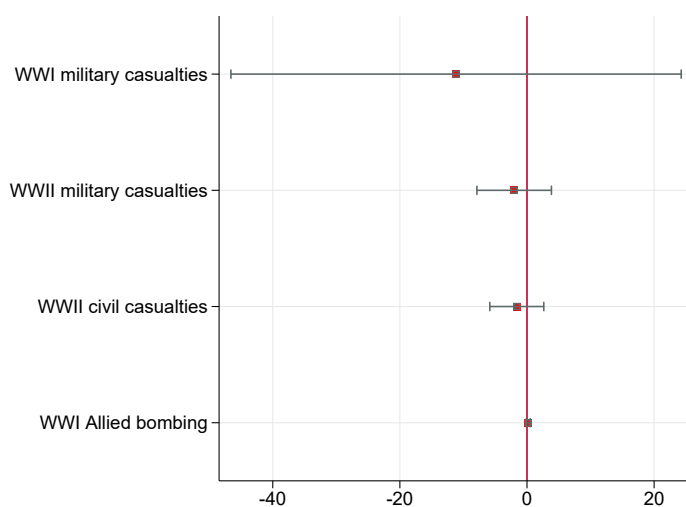


Figure A8: Post-treatment - World Wars



Notes: Coefficients estimated with a 15km bandwidth with polynomial interaction terms, commune-level controls, and border-segment fixed effects. Bars represent the 95% confidence interval, with spatial standard errors clustered at a 10km radius. Observations: 561.

Figure A9: 1st round presidential election results (1995-2022)

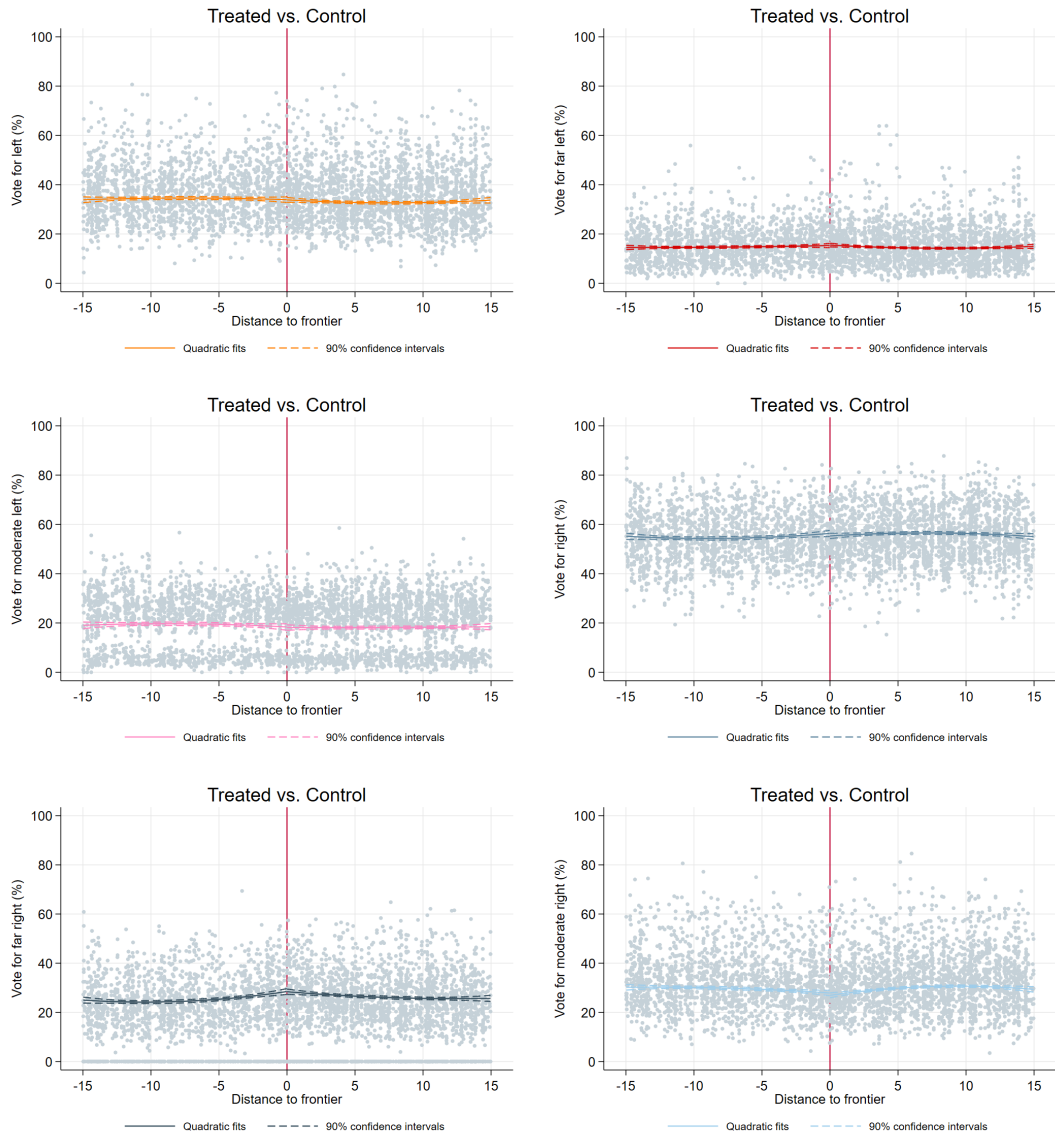


Figure A10: 1st round legislative election results (1997-2022)

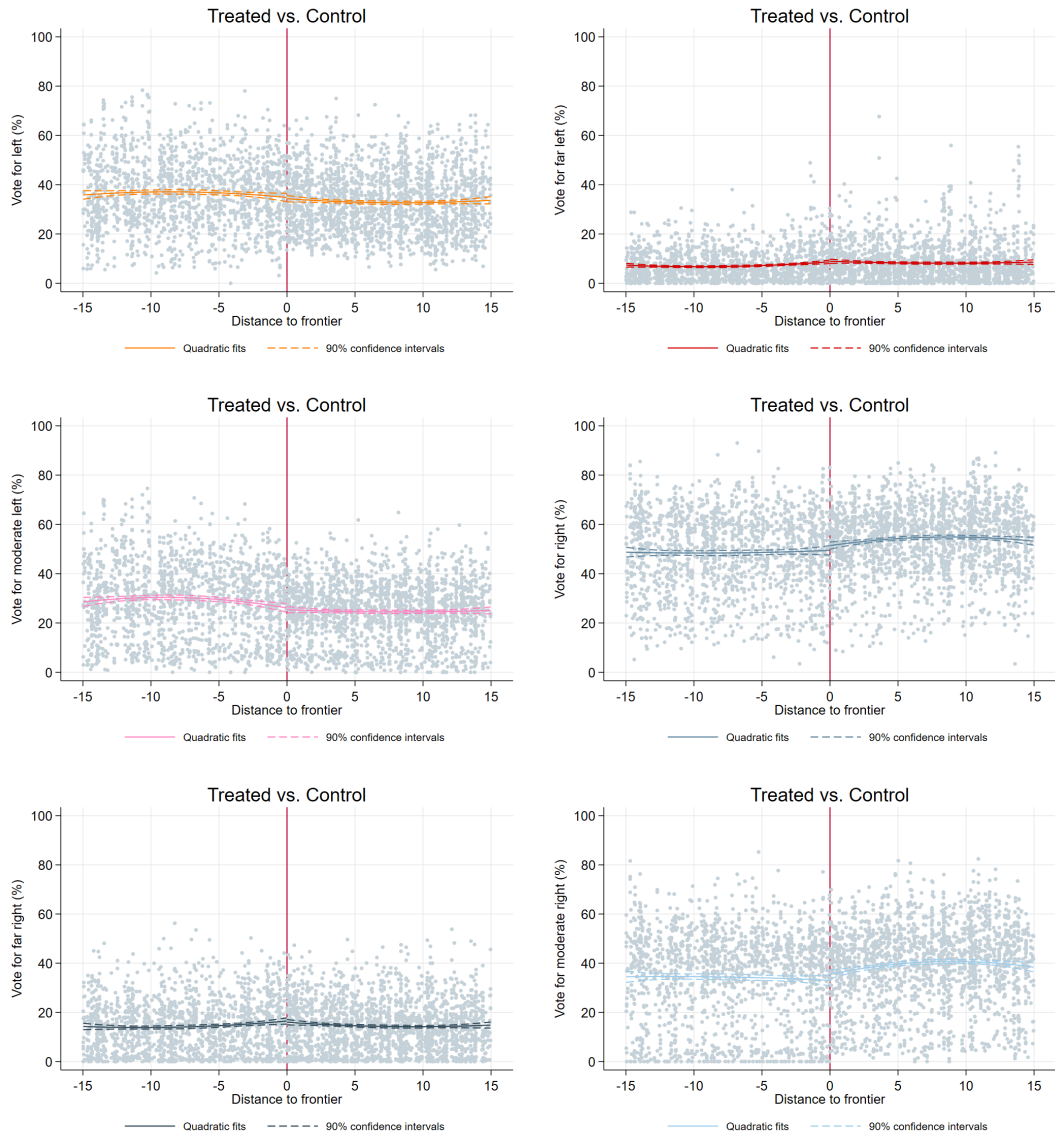


Figure A11: Placebo frontiers



Table A38: Treatment on 1st round legislative election results (1997-2022) - 20km band

Dep. Var.: $Vote_{m,t}$	Left			Right			Abstention
	All	Far	Moderate	All	Far	Moderate	
Treatment m	2.661 (1.316)**	-0.168 (0.660)	2.829 (1.397)**	-3.306 (1.717)*	0.838 (0.717)	-4.144 (1.679)**	-0.599 (0.540)
N	3,804	3,804	3,804	3,804	3,804	3,804	3,804
R^2	0.55	0.48	0.65	0.39	0.69	0.52	0.56
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A39: Treatment on 1st round presidential election results (1995-2022) - 25km band

Dep. Var.: $Vote_{m,t}$	Left			Right			Abstention
	All	Far	Moderate	All	Far	Moderate	
Treatment m	1.693 (0.552)***	0.312 (0.410)	1.343 (0.355)***	-1.595 (0.552)***	-2.112 (0.517)***	0.518 (0.549)	-0.887 (0.355)**
N	5,161	5,161	5,161	5,161	5,161	5,161	5,161
R^2	0.53	0.56	0.83	0.58	0.69	0.51	0.54
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A40: Treatment on 1st round legislative election results (1997-2022) - 25km band

Dep. Var.:	Left			Right			Abstention
	All	Far	Moderate	All	Far	Moderate	
$Vote_{m,t}$							
Treatment m	2.622 (1.141)**	-0.719 (0.573)	3.341 (1.228)***	-4.890 (1.609)***	-0.624 (0.686)	-4.265 (1.612)***	-1.438 (0.480)***
N	5,196	5,196	5,196	5,196	5,196	5,196	5,196
R^2	0.53	0.46	0.61	0.37	0.68	0.49	0.55
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A41: Treatment on 1st round presidential election results (1995-2022) - 30km band

Dep. Var.:	Left			Right			Abstention
	All	Far	Moderate	All	Far	Moderate	
$Vote_{m,t}$							
Treatment m	0.781 (0.524)	-0.276 (0.375)	1.018 (0.349)***	-0.939 (0.537)*	-2.935 (0.552)***	1.995 (0.587)***	-0.615 (0.333)*
N	6,185	6,185	6,185	6,185	6,185	6,185	6,185
R^2	0.53	0.56	0.82	0.57	0.68	0.51	0.53
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A42: Treatment on 1st round legislative election results (1997-2022) - 30km band

Dep. Var.:	Left			Right			Abstention
	All	Far	Moderate	All	Far	Moderate	
$Vote_{m,t}$							
Treatment m	1.539 (1.108)	-1.503 (0.609)**	3.042 (1.197)**	-4.697 (1.669)***	-1.190 (0.707)*	-3.508 (1.679)**	-1.113 (0.489)**
N	6,220	6,220	6,220	6,220	6,220	6,220	6,220
R^2	0.53	0.44	0.59	0.37	0.66	0.49	0.54
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A43: Treatment on 1st round presidential election results (1995-2022) - Optimal bands

Dep. Var.:	Left			Right			Turnout
	All	Far	Moderate	All	Far	Moderate	
$Vote_{m,t}$							
Treatment m	1.858 (0.579)***	0.516 (0.455)	1.320 (0.377)***	-1.185 (0.635)*	-0.171 (0.553)	-0.213 (0.610)	-0.894 (0.376)**
N	4,635	3,864	4,659	2,846	3,347	3,311	4,689
R^2	0.53	0.57	0.83	0.58	0.70	0.54	0.55
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Optimal Band (km)	22.616	14.876	15.135	12.783	22.292	17.817	22.157

Standard errors in parentheses, clustered at 10km radius.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A44: Treatment on 1st round legislative election results (1997-2022) - Optimal bands

Dep. Var.:	Left			Right			Turnout
	All	Far	Moderate	All	Far	Moderate	
$Vote_{m,t}$							
Treatment m	4.058 (1.325)***	0.170 (0.668)	2.517 (1.365)*	-4.649 (1.675)***	0.672 (0.707)	-5.702 (1.681)***	-1.618 (0.512)***
N	2,670	3,354	2,244	2,538	4,143	2,784	4,550
R^2	0.55	0.52	0.66	0.39	0.69	0.53	0.55
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Optimal Band (km)	11.7	14.953	10.138	11.269	19.433	12.412	21.633

Standard errors in parentheses, clustered at 10km radius.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A45: Treatment on 1st round presidential election results (1995-2022) - Without merged communes

Dep. Var.:	Left			Right			Abstention
	All	Far	Moderate	All	Far	Moderate	
$Vote_{m,t}$							
Treatment m	1.193 (0.600)**	0.356 (0.452)	0.809 (0.417)*	-0.226 (0.586)	-0.240 (0.551)	0.013 (0.600)	0.452 (0.434)
N	3,251	3,251	3,251	3,251	3,251	3,251	3,251
R^2	0.54	0.56	0.83	0.58	0.69	0.54	0.55
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Figure A12: Presidential 1st round, vote for left - drop commune

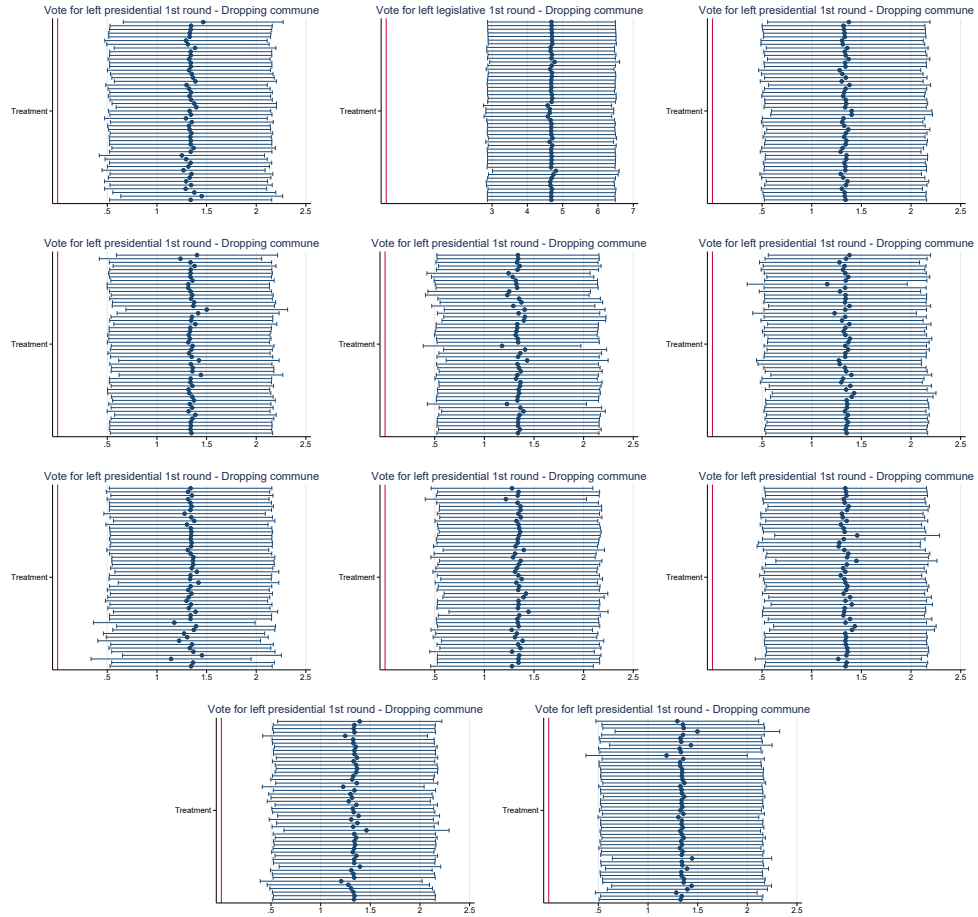


Table A46: Treatment on 1st round legislative election results (1997-2022) - Without merged communes

Dep. Var.:	Left			Right			
	All	Far	Moderate	All	Far	Moderate	Abstention
$Vote_{m,t}$							
Treatment m	2.655 (1.301)**	-0.004 (0.661)	2.659 (1.387)*	-3.507 (1.689)**	0.667 (0.736)	-4.174 (1.684)**	-0.516 (0.558)
N	3,282	3,282	3,282	3,282	3,282	3,282	3,282
R^2	0.56	0.51	0.66	0.40	0.70	0.52	0.56
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Figure A13: Legislative 1st round, vote for left - drop commune

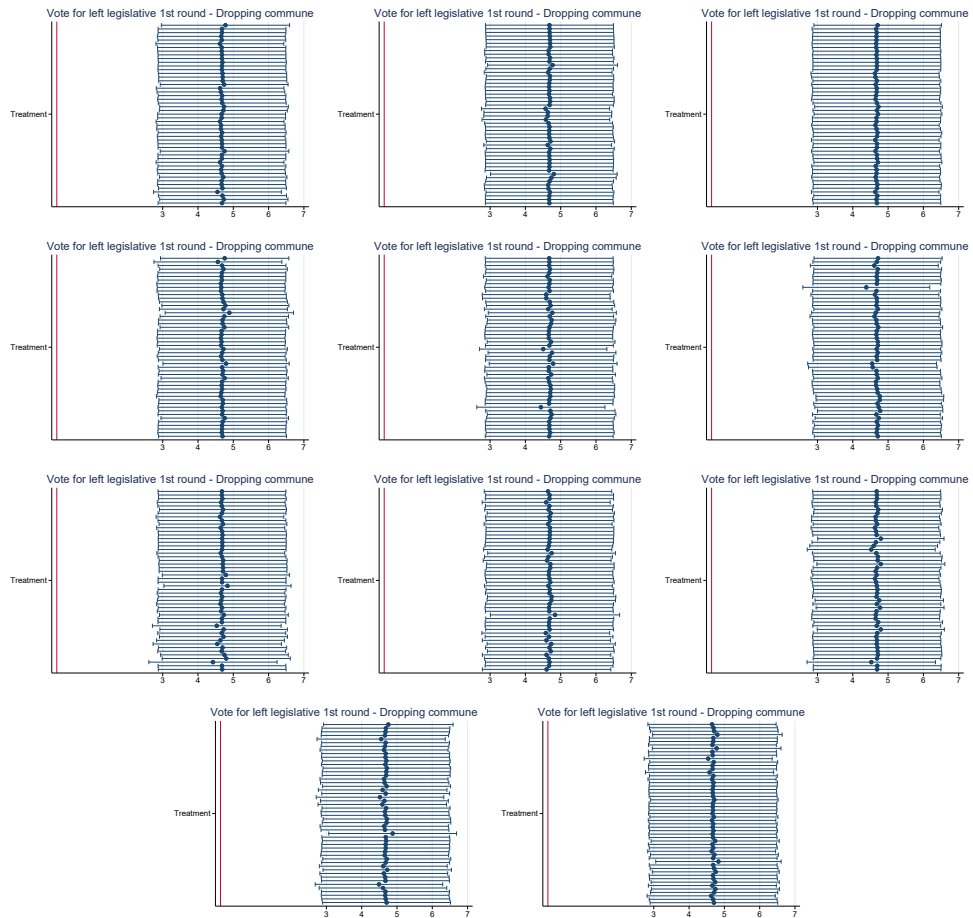


Table A47: Descriptive statistics - 1851 political repression (15km bandwidth)

	Obs.	Mean	Std. Dev.	Min	Max
Repressed	561	0.198	1.684	0	31
Repressed (per 1000)	561	0.136	0.915	0	13.827

Table A48: Treatment on 1st round presidential election results (1995-2022) - Interaction with 5km repression circles

Dep. Var.: $Vote_{m,t}$	Left			Right			Abstention
	All	Far	Moderate	All	Far	Moderate	
Treatment m	1.406 (0.662)**	0.245 (0.522)	1.136 (0.441)***	-0.495 (0.648)	-1.006 (0.563)*	0.511 (0.671)	0.676 (0.483)
5km Repression m	-0.003 (0.731)	-1.205 (0.516)**	1.206 (0.645)*	-1.724 (0.853)**	-4.638 (0.590)***	2.914 (0.793)***	0.014 (0.702)
Treatment m x 5km m	-0.194 (0.822)	1.130 (0.544)**	-1.338 (0.700)*	1.361 (0.925)	4.639 (0.684)***	-3.278 (0.833)***	-0.524 (0.742)
N	3,335	3,335	3,335	3,335	3,335	3,335	3,335
R^2	0.54	0.56	0.83	0.58	0.69	0.54	0.55
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A49: Treatment on 1st round legislative election results (1997-2022) - Interaction with 5km repression circles

Dep. Var.: $Vote_{m,t}$	Left			Right			Abstention
	All	Far	Moderate	All	Far	Moderate	
Treatment m	2.702 (1.429)*	-0.095 (0.701)	2.797 (1.511)*	-3.771 (1.711)**	0.202 (0.746)	-3.973 (1.694)**	-0.875 (0.661)
5km Repression m	0.381 (1.372)	-1.439 (0.480)***	1.819 (1.332)	0.530 (2.054)	-2.177 (1.045)**	2.707 (2.055)	-0.446 (0.922)
Treatment m x 5km m	0.034 (1.504)	1.165 (0.564)**	-1.131 (1.498)	-0.207 (2.241)	2.360 (0.989)**	-2.567 (2.201)	1.021 (0.995)
N	3,366	3,366	3,366	3,366	3,366	3,366	3,366
R^2	0.56	0.51	0.66	0.40	0.70	0.52	0.56
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A50: Treatment on 1st round presidential election results (1995-2022) - Interaction with 10km repression circles

Dep. Var.: <i>Vote_{m,t}</i>	Left			Right			Abstention
	All	Far	Moderate	All	Far	Moderate	
Treatment m	1.017 (0.901)	0.299 (0.562)	0.684 (0.612)	-0.511 (0.927)	-0.460 (0.744)	-0.050 (0.868)	0.683 (0.550)
10km Repression m	-2.401 (0.506)***	-0.967 (0.408)**	-1.413 (0.331)***	1.748 (0.498)***	0.101 (0.399)	1.647 (0.435)***	0.246 (0.307)
Treatment m x 10km m	0.311 (0.748)	0.064 (0.490)	0.253 (0.463)	0.273 (0.774)	0.143 (0.617)	0.131 (0.682)	-0.266 (0.378)
N	3,335	3,335	3,335	3,335	3,335	3,335	3,335
R^2	0.55	0.56	0.83	0.58	0.69	0.54	0.55
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A51: Treatment on 1st round legislative election results (1997-2022) - Interaction with 10km repression circles

Dep. Var.: <i>Vote_{m,t}</i>	Left			Right			Abstention
	All	Far	Moderate	All	Far	Moderate	
Treatment m	3.353 (1.746)*	-0.280 (0.778)	3.633 (1.897)*	-5.583 (2.084)***	0.608 (1.015)	-6.192 (2.051)***	-0.469 (0.776)
10km Repression m	-2.285 (0.758)***	-0.957 (0.431)**	-1.328 (0.773)*	1.465 (1.056)	-0.041 (0.540)	1.506 (1.131)	-0.516 (0.323)
Treatment m x 10km m	-0.926 (1.195)	0.340 (0.618)	-1.266 (1.293)	2.738 (1.704)	-0.062 (0.736)	2.800 (1.714)	-0.223 (0.538)
N	3,366	3,366	3,366	3,366	3,366	3,366	3,366
R^2	0.57	0.51	0.66	0.40	0.70	0.53	0.56
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A52: Treatment on 1st round presidential election results (1995-2022) -
Repression binary variable

Dep. Var.:	Left			Right			Abstention
	All	Far	Moderate	All	Far	Moderate	
$Vote_{m,t}$							
Treatment $_m$	1.433 (0.601)**	0.407 (0.455)	0.995 (0.421)**	-0.451 (0.589)	-0.419 (0.549)	-0.032 (0.607)	0.499 (0.446)
Repression (binary) $_m$	-0.956 (0.405)**	-0.222 (0.286)	-0.718 (0.296)**	0.581 (0.459)	0.525 (0.414)	0.056 (0.418)	-0.127 (0.357)
N	3,335	3,335	3,335	3,335	3,335	3,335	3,335
R^2	0.54	0.56	0.83	0.58	0.69	0.54	0.55
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A53: Treatment on 1st round legislative election results (1997-2022) -
Repression binary variable

Dep. Var.:	Left			Right			Abstention
	All	Far	Moderate	All	Far	Moderate	
$Vote_{m,t}$							
Treatment $_m$	2.895 (1.322)**	0.012 (0.653)	2.883 (1.411)**	-3.876 (1.693)**	0.523 (0.723)	-4.399 (1.681)**	-0.685 (0.569)
Repression (binary) $_m$	-0.898 (0.657)	-0.109 (0.306)	-0.789 (0.634)	1.474 (0.850)*	0.421 (0.401)	1.053 (0.844)	0.802 (0.374)**
N	3,366	3,366	3,366	3,366	3,366	3,366	3,366
R^2	0.56	0.51	0.66	0.40	0.70	0.52	0.56
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A54: Treatment on 1st round presidential election results (1995-2022) - Per thousand inhabitants repressed

Dep. Var.:	Left			Right			Abstention
	All	Far	Moderate	All	Far	Moderate	
$Vote_{m,t}$							
Treatment m	1.360 (0.598)**	0.381 (0.452)	0.946 (0.419)**	-0.411 (0.585)	-0.381 (0.550)	-0.029 (0.606)	0.524 (0.443)
Repressed (per thousand) m	-0.067 (0.097)	0.006 (0.062)	-0.067 (0.079)	0.051 (0.099)	0.044 (0.090)	0.008 (0.104)	-0.100 (0.095)
N	3,335	3,335	3,335	3,335	3,335	3,335	3,335
R^2	0.54	0.56	0.83	0.58	0.69	0.54	0.55
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A55: Treatment on 1st round legislative election results (1997-2022) - Per thousand inhabitants repressed

Dep. Var.:	Left			Right			Abstention
	All	Far	Moderate	All	Far	Moderate	
$Vote_{m,t}$							
Treatment m	2.808 (1.319)**	-0.008 (0.653)	2.816 (1.409)**	-3.828 (1.694)**	0.548 (0.724)	-4.376 (1.683)***	-0.609 (0.566)
Repressed (per thousand) m	-0.013 (0.145)	0.024 (0.075)	-0.036 (0.140)	0.269 (0.221)	0.046 (0.107)	0.223 (0.200)	0.019 (0.105)
N	3,366	3,366	3,366	3,366	3,366	3,366	3,366
R^2	0.56	0.51	0.66	0.40	0.70	0.52	0.56
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Figure A14: 1851 repression in southeastern France

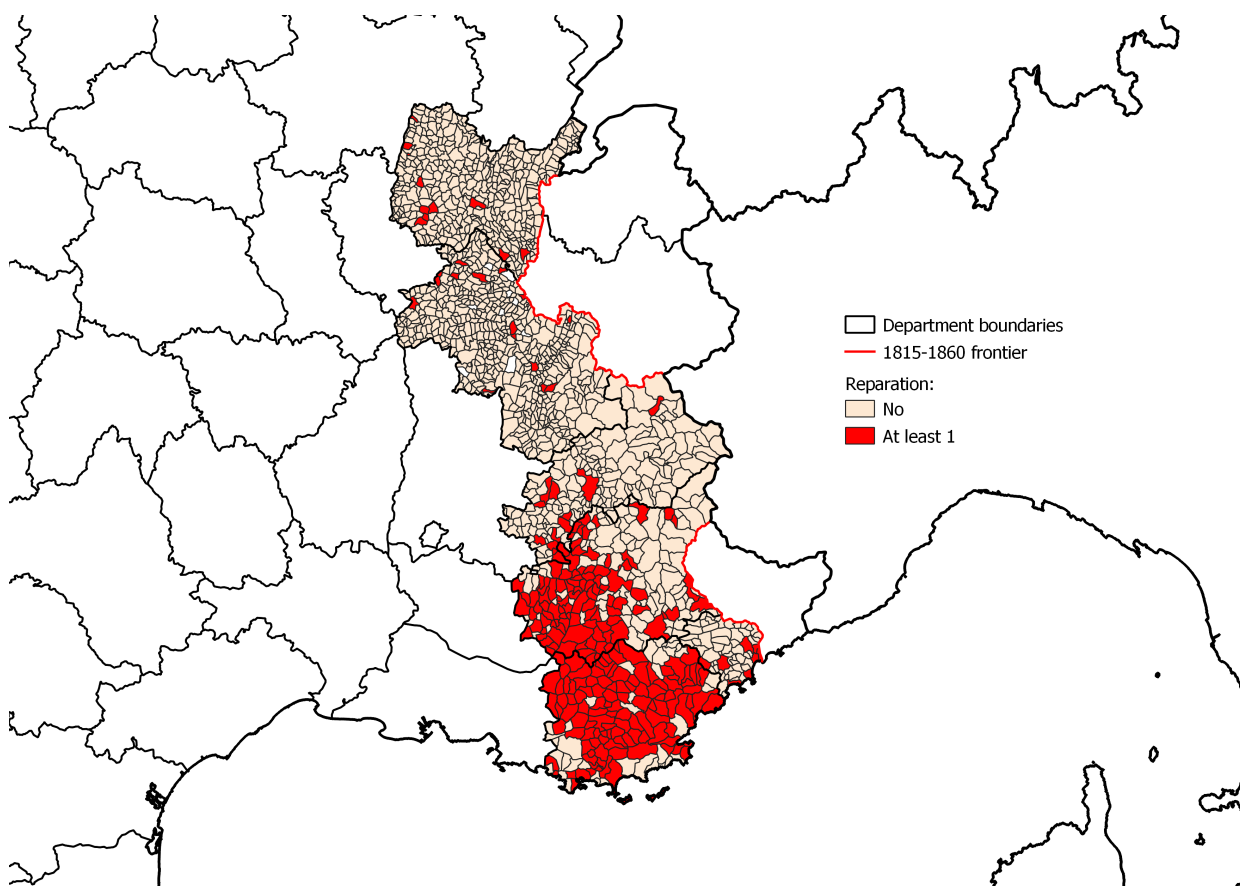


Table A56: Presidential, number of repressed individuals by commune - North subsample

Dep. Var.:	Left			Right			
	All	Far	Moderate	All	Far	Moderate	Abstention
$Vote_{m,t}$							
Treatment m	1.780 (0.631)***	0.518 (0.487)	1.207 (0.440)***	-0.720 (0.588)	-1.325 (0.584)**	0.604 (0.612)	0.605 (0.555)
Repressed m	-0.958 (0.253)***	-0.403 (0.193)**	-0.554 (0.202)***	0.830 (0.293)***	0.617 (0.274)**	0.214 (0.184)	0.549 (0.221)**
N	2,573	2,573	2,573	2,573	2,573	2,573	2,573
R^2	0.55	0.63	0.84	0.59	0.68	0.57	0.56
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A57: Presidential, number of repressed individuals by commune - South subsample

Dep. Var.: $Vote_{m,t}$	Left			Right			Abstention
	All	Far	Moderate	All	Far	Moderate	
Treatment m	0.430 (1.346)	0.493 (1.079)	-0.008 (1.001)	0.533 (1.462)	3.310 (1.118)***	-2.777 (1.423)*	-0.032 (0.693)
Repressed m	0.016 (0.029)	-0.003 (0.025)	0.017 (0.027)	-0.011 (0.030)	-0.066 (0.037)*	0.055 (0.041)	0.137 (0.033)***
N	762	762	762	762	762	762	762
R^2	0.53	0.45	0.80	0.48	0.75	0.60	0.55
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A58: Legislative, number of repressed individuals by commune - North subsample

Dep. Var.: $Vote_{m,t}$	Left			Right			Abstention
	All	Far	Moderate	All	Far	Moderate	
Treatment m	1.531 (1.537)	-0.554 (0.588)	2.085 (1.627)	-3.022 (2.015)	-0.415 (0.869)	-2.608 (1.920)	-0.545 (0.661)
Repressed m	-0.718 (0.374)*	-0.325 (0.191)*	-0.394 (0.348)	1.214 (0.503)**	0.408 (0.279)	0.807 (0.456)*	0.941 (0.257)***
N	2,604	2,604	2,604	2,604	2,604	2,604	2,604
R^2	0.58	0.58	0.67	0.39	0.71	0.53	0.58
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A59: Legislative, number of repressed individuals by commune - South subsample

Dep. Var.: $Vote_{m,t}$	Left			Right			Abstention
	All	Far	Moderate	All	Far	Moderate	
Treatment m	8.803 (1.784)***	2.884 (1.888)	5.919 (2.563)**	-8.218 (2.833)***	4.097 (0.950)***	-12.316 (3.064)***	-1.112 (1.000)
Repressed m	-0.067 (0.056)	-0.026 (0.034)	-0.040 (0.061)	0.189 (0.084)**	-0.044 (0.040)	0.233 (0.069)***	0.130 (0.046)***
N	762	762	762	762	762	762	762
R^2	0.60	0.48	0.65	0.43	0.74	0.60	0.53
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A60: Descriptive statistics - Repressed-political dynasties (15km bandwidth)

	Obs.	Mean	Std. Dev.	Min	Max
Mayor	561	0.069	0.254	0	1
Length mayor	561	1.057	5.031	0	56
Rel. length mayor	561	0.604	2.874	0	32

Table A61: Presidential elections (1995-2022) - Interaction with mayor having the same name as repressed citizens 20km around

Dep. Var.: $Vote_{m,t}$	Left			Right			Abstention
	All	Far	Moderate	All	Far	Moderate	
Treatment m	1.398 (0.613)**	0.366 (0.459)	0.994 (0.422)**	-0.450 (0.589)	-0.476 (0.573)	0.026 (0.609)	0.463 (0.448)
Mayor m	-0.104 (0.727)	-0.289 (0.643)	0.116 (0.334)	0.206 (0.777)	0.048 (0.674)	0.158 (0.709)	0.225 (0.507)
Treatment m x Mayor m	-1.184 (0.932)	0.105 (0.816)	-1.203 (0.456)***	1.164 (1.004)	1.966 (0.829)**	-0.803 (0.888)	0.545 (0.646)
N	3,335	3,335	3,335	3,335	3,335	3,335	3,335
R^2	0.54	0.56	0.83	0.58	0.69	0.54	0.55
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A62: Legislative elections (1997-2022) - Interaction with mayor having the same name as repressed citizens 20km around

Dep. Var.: $Vote_{m,t}$	Left			Right			Abstention
	All	Far	Moderate	All	Far	Moderate	
Treatment m	2.904 (1.331)**	0.049 (0.688)	2.854 (1.405)**	-3.696 (1.662)**	0.412 (0.735)	-4.108 (1.642)**	-0.779 (0.580)
Mayor m	0.258 (1.151)	0.485 (0.986)	-0.227 (0.843)	1.011 (1.361)	-0.515 (0.771)	1.526 (1.577)	-0.506 (0.548)
Treatment m x Mayor m	-1.577 (1.385)	-0.514 (1.016)	-1.063 (1.178)	0.163 (1.785)	2.336 (1.020)**	-2.173 (2.068)	2.728 (0.720)***
N	3,366	3,366	3,366	3,366	3,366	3,366	3,366
R^2	0.56	0.51	0.66	0.40	0.70	0.52	0.56
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A63: Presidential elections (1995-2022) - Interaction with number of years with mayor having the same name as repressed citizens 20km around

Dep. Var.:	Left			Right			Abstention
	All	Far	Moderate	All	Far	Moderate	
$Vote_{m,t}$							
Treatment $_m$	1.388 (0.598)**	0.425 (0.456)	0.932 (0.416)**	-0.437 (0.583)	-0.319 (0.559)	-0.118 (0.604)	0.483 (0.445)
Length $_m$	0.070 (0.029)**	0.046 (0.026)*	0.023 (0.011)**	-0.060 (0.030)**	0.034 (0.027)	-0.094 (0.031)**	-0.012 (.)
Treatment $_m$ x Length $_m$	-0.137 (0.037)**	-0.063 (0.031)**	-0.073 (0.019)**	0.118 (0.039)**	0.017 (0.032)	0.101 (0.043)**	0.050 (0.019)**
N	3,335	3,335	3,335	3,335	3,335	3,335	3,335
R^2	0.54	0.56	0.83	0.58	0.69	0.54	0.55
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A64: Legislative elections (1997-2022) - Interaction with number of years with mayor having the same name as repressed citizens 20km around

Dep. Var.:	Left			Right			Abstention
	All	Far	Moderate	All	Far	Moderate	
$Vote_{m,t}$							
Treatment $_m$	2.880 (1.318)**	0.097 (0.678)	2.784 (1.394)**	-3.736 (1.658)**	0.607 (0.720)	-4.343 (1.642)**	-0.558 (0.568)
Length $_m$	0.093 (0.044)**	0.097 (0.053)*	-0.004 (0.029)	-0.017 (0.046)	0.030 (0.037)	-0.047 (0.052)	0.028 (0.006)**
Treatment $_m$ x Length $_m$	-0.159 (0.053)**	-0.098 (0.051)*	-0.061 (0.043)	0.044 (0.063)	0.024 (0.047)	0.020 (0.075)	0.051 (0.022)**
N	3,366	3,366	3,366	3,366	3,366	3,366	3,366
R^2	0.56	0.51	0.66	0.40	0.70	0.52	0.56
Commune controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A65: Descriptive statistics - 1851 emigration (15km bandwidth)

	Obs.	Mean	Std. Dev.	Min	Max
Emigration	561	0.032	0.399	0	9
Exile	561	0.007	0.084	0	1
French Guiana	561	0.002	0.042	0	1
Algeria	561	0.005	0.094	0	2

Table A66: Treatment on 2nd round presidential election results (1995-2022) - Subsamples with academic division

Dep. Var.:	Same	Different
$VoteLeastRight_{m,t}$	academia	academia
Treatment $_m$	-1.108 (0.982)	1.269 (0.744)*
N	1,797	1,537
R^2	0.74	0.75
Commune controls	Yes	Yes
Border Segment FE	Yes	Yes
Election FE	Yes	Yes

Standard errors in parentheses, clustered at 10km radius
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A67: Treatment on 2nd round legislative election results (1997-2022) - Subsamples with academic division

Dep. Var.:	Same academia		Different academia	
	Left	Right	Left	Right
$Vote_{m,t}$				
Treatment $_m$	1.811 (3.708)	1.666 (5.626)	8.318 (2.385)***	-11.210 (6.782)*
N	1,651	1,651	1,432	1,432
R^2	0.70	0.49	0.72	0.45
Commune controls	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Figure A15: Presidential election - Margins repressed-political dynasties

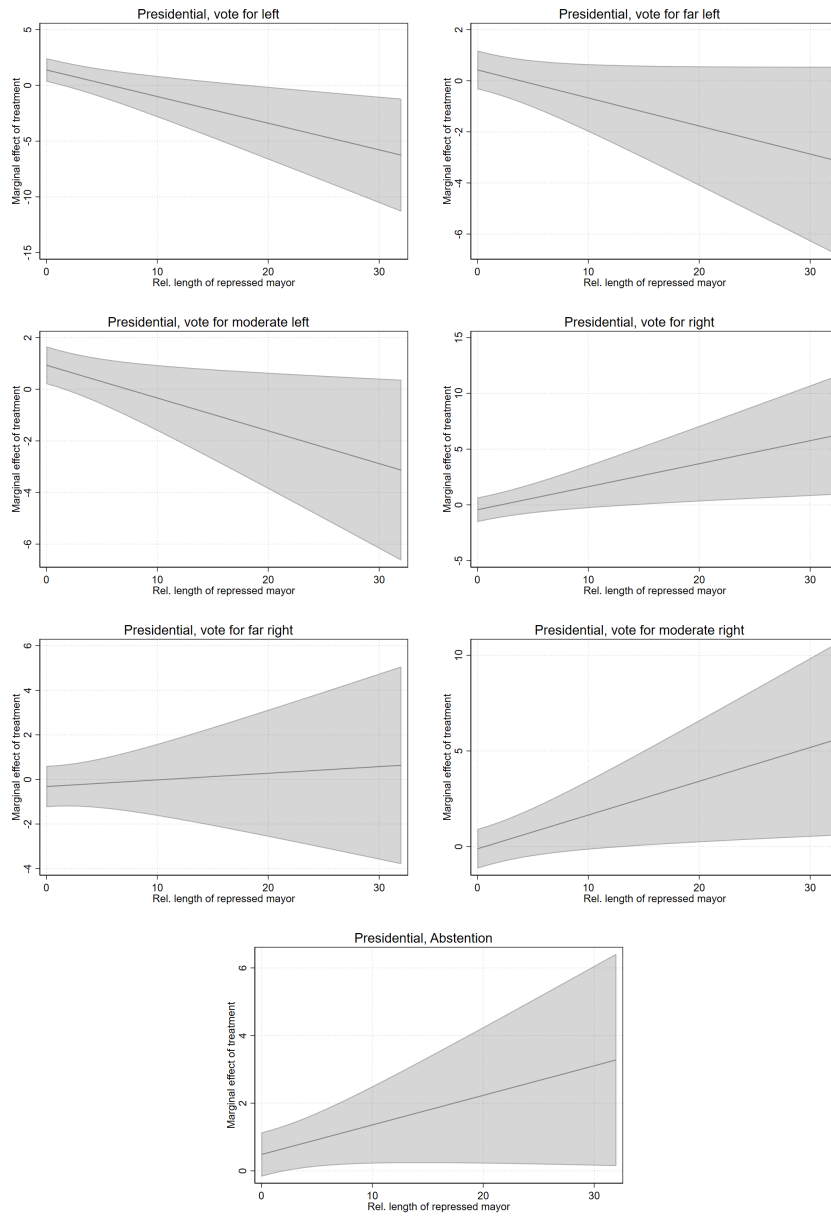


Figure A16: Legislative election - Margins repressed-political dynasties

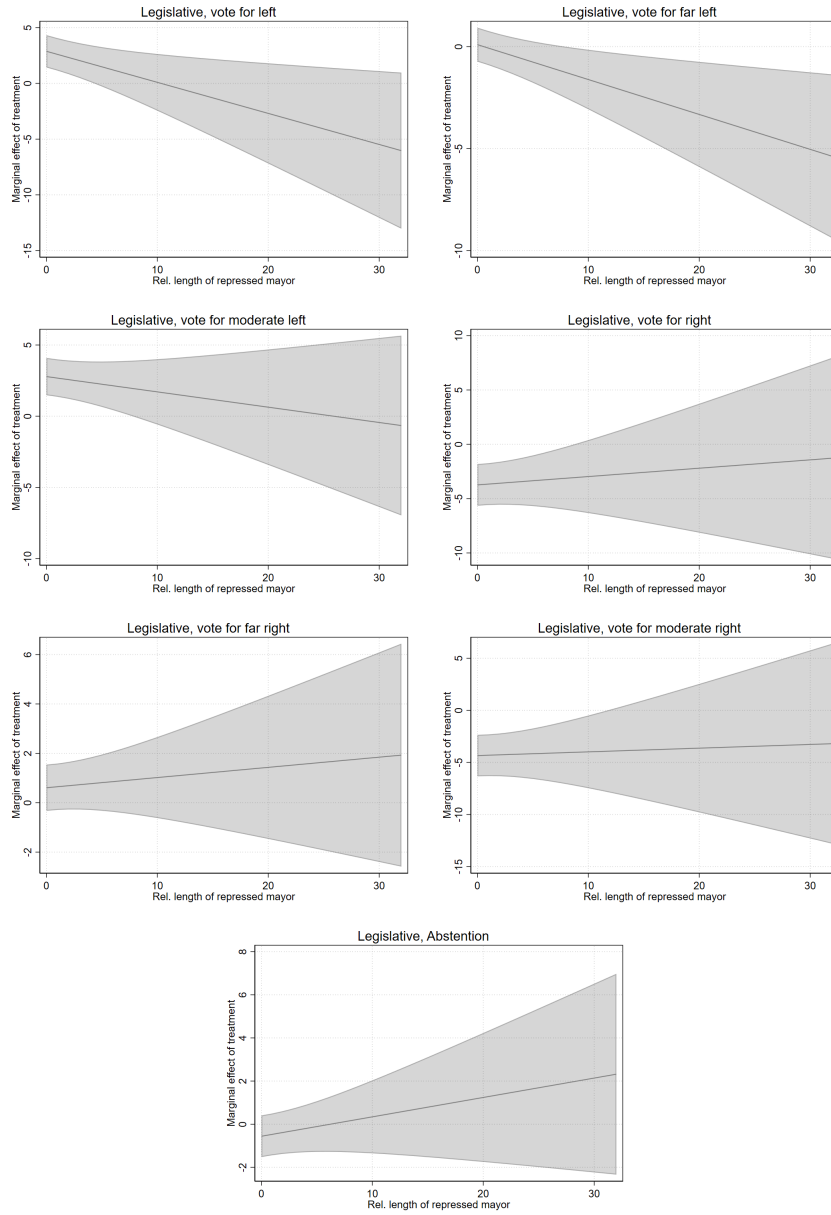


Figure A17: Academic divisions

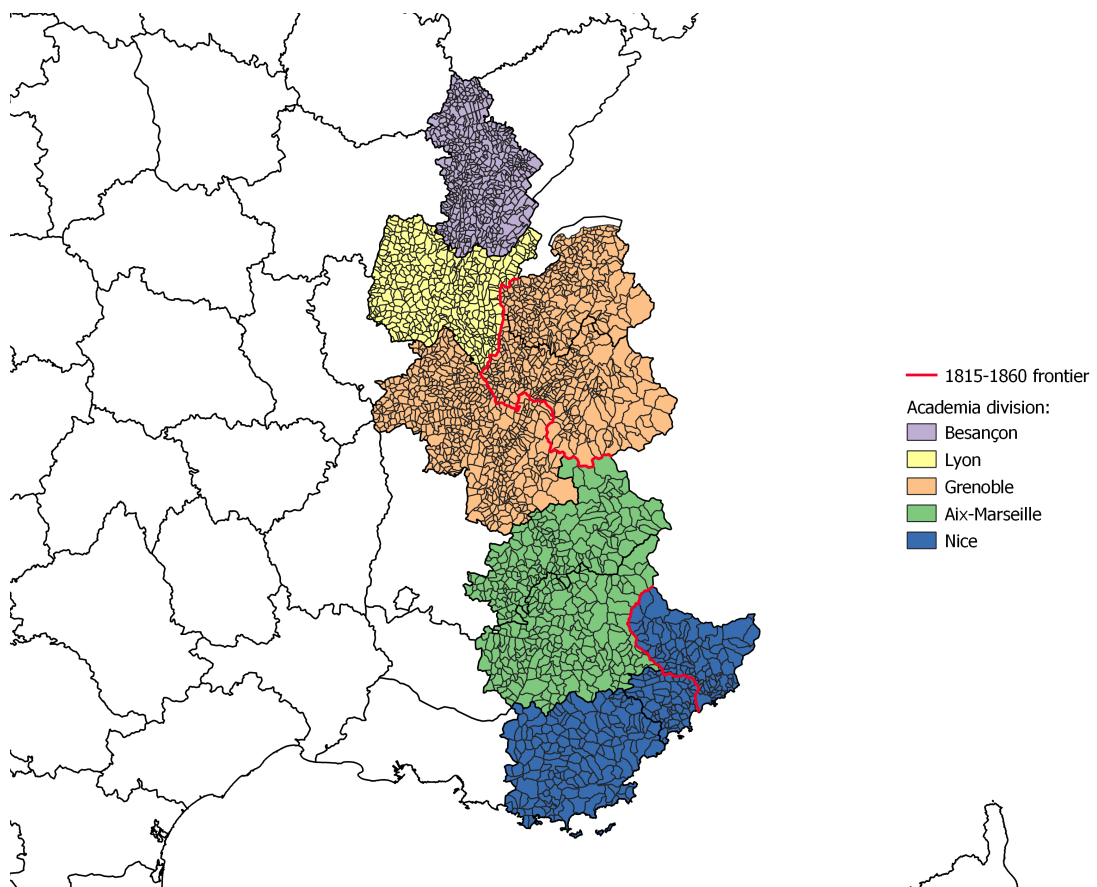


Figure A18: Employment areas

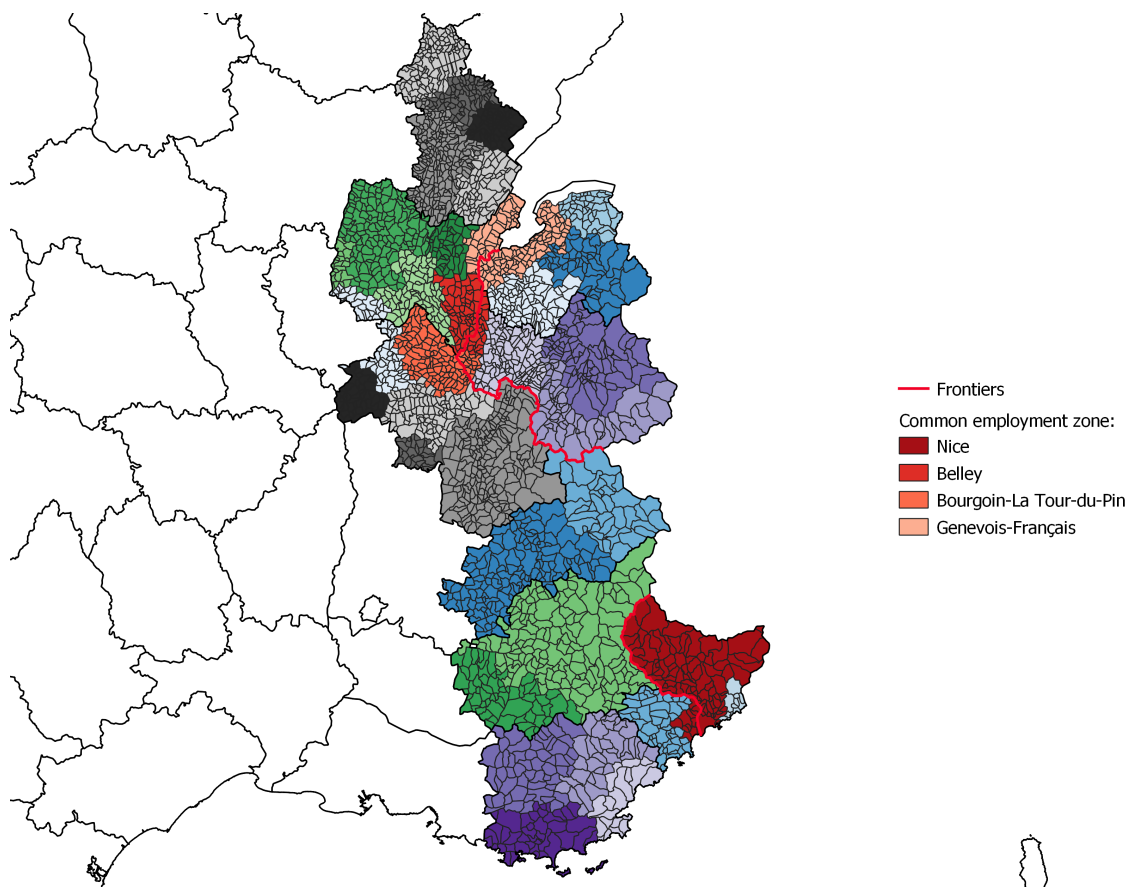


Table A68: Treatment on 2nd round presidential election results (1995-2022) - Subsamples with employment zone

Dep. Var.: <i>VoteLeastRight_{m,t}</i>	Same zone	Different zone
Treatment m	0.586 (0.642)	3.101 (1.302)**
N	1,657	1,677
R^2	0.75	0.74
Commune controls	Yes	Yes
Border Segment FE	Yes	Yes
Election FE	Yes	Yes

Standard errors in parentheses, clustered at 10km radius

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Table A69: Treatment on 2nd round legislative election results (1997-2022) - Subsamples with employment zone

Dep. Var.: <i>Vote_{m,t}</i>	Same zone		Different zone	
	Left	Right	Left	Right
Treatment m	1.312 (2.541)	-3.381 (5.420)	6.179 (2.870)**	-4.358 (8.113)
N	1501	1501	1582	1582
R^2	0.70	0.53	0.69	0.36
Commune controls	Yes	Yes	Yes	Yes
Border Segment FE	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered at 10km radius

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

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Résumé

Cette thèse d'économie politique présente les résultats de trois projets de recherche portant sur deux thématiques : l'aide au développement et les conséquences de la répression politique. Les trois chapitres présentés sont des articles de recherche indépendants les uns des autres et peuvent être lus séparément. Ces travaux s'inscrivent dans le champ de l'économie politique, car ils renvoient à la dynamique de pouvoir inhérente aux politiques publiques et son influence sur un large éventail de résultats, allant d'indicateurs économiques aux perceptions politiques des individus.

Les deux premiers chapitres analysent les déterminants politiques et les répercussions économiques de l'aide publique au développement (APD), à la fois bilatérale et multilatérale. L'APD correspond aux ressources financières fournies par les gouvernements et les organisations internationales pour promouvoir le développement économique et social des pays moins avancés. L'aide multilatérale est allouée par plusieurs pays via des organisations internationales afin de soutenir les initiatives de développement dans les pays bénéficiaires, tandis que l'aide bilatérale est une assistance directe d'un pays à un autre. Après la crise de légitimité de l'aide dans les années 1990, l'établissement des Objectifs du Millénaire pour le Développement a relancé l'engagement envers l'APD. Conjointement à cette montée de l'aide multilatérale et bilatérale, les années 2000 ont été témoins de l'émergence de donateurs non traditionnels ⁵², dont la Chine est un exemple marquant.

Bien que la Chine ait récemment intensifié ses efforts en matière d'APD, elle n'en est pas à ses débuts dans ce domaine. Ses premières initiatives d'aide ont commencé dès 1949, à l'issue de la guerre civile chinoise. Durant cette période, comme pour la majorité des autres donateurs bilatéraux, l'aide chinoise était principalement motivée par des considérations stratégiques. Elle visait notamment à soutenir les pays socialistes et non-alignés, ainsi qu'à contrer la reconnaissance diplomatique de Taïwan. Un exemple notable de ces premiers projets d'aide est la construction du

⁵²Donateurs ne suivant pas les règles du comité d'aide au développement de l'OCDE

chemin de fer TAZARA reliant la Tanzanie et la Zambie en 1975, une importante infrastructure de transport réalisée en coopération avec des gouvernements non-alignés et socialistes. Selon Brautigam (2011), l'accroissement substantiel de l'aide chinoise dans les années 2000 s'explique par plusieurs facteurs clés, incluant la demande croissante de la Chine pour les ressources primaires liée à son explosion économique, son désir de se positionner comme un acteur mondial d'envergure, la concurrence diplomatique avec Taïwan, et la stratégie de diversification de ses marchés industriels au-delà de son territoire national.

À mesure que l'APD chinoise gagnait en importance sur la scène internationale, elle devenait de plus en plus sujette à controverse. Des critiques l'ont notamment accusée de favoriser des régimes autoritaires, de plonger les pays bénéficiaires dans des crises d'endettement, de chercher uniquement à sécuriser l'accès aux ressources naturelles et d'être dépourvue de garanties environnementales et sociales. Par conséquent, une part croissante des recherches en développement économique s'est concentrée sur ce nouveau donateur, étudiant les déterminants de son aide et ses effets sur le développement des pays récipiendaires.

Sur la base des conclusions de Dreher and Fuchs (2015) et Dreher et al. (2018b), qui analysent les déterminants de l'aide chinoise, l'intervention économique de la Chine à l'étranger ne montre pas un degré de motivation politique significativement supérieur à celui des donateurs occidentaux. De plus, cette aide ne semble pas déterminée par les ressources naturelles des pays bénéficiaires. Cependant, étant donné que la Chine n'est pas membre du Comité d'aide aux donateurs, son aide ne respecte pas les normes internationales établies, nécessitant ainsi un examen plus minutieux pour identifier ses particularités. En distinguant les interventions économiques chinoises en deux catégories, les aides publiques au développement (selon la définition du comité d'aide au développement, les prêts comportant un élément de subvention d'au moins 25 %) et les autres flux financiers officiels, une image plus détaillée émerge. Dreher and Fuchs (2015) et Dreher et al. (2018b) montrent que l'allocation de l'APD chinoise est influencée par des objectifs de politique étrangère, comme la reconnaissance de Taïwan, tandis que les autres flux financiers officiels sont motivés par des considérations économiques, telles que la présence de ressources pétrolières, une moindre dette (indiquant une plus grande probabilité de remboursement), et l'existence de liens commerciaux avec la Chine. Ces résultats suggèrent que la réputation controversée de l'aide chinoise semble injustifiée, puisque ses critères d'allocation correspondent aux mêmes facteurs

politiques que ceux des autres principaux donateurs bilatéraux (Alesina and Dollar, 2000; Faye and Niehaus, 2012).

Au-delà des travaux analysant les déterminants de l'aide chinoise, plusieurs recherches indiquent que l'APD chinoise a globalement un effet bénéfique sur le développement des pays récipiendaires. Par exemple, cette aide générerait une croissance économique (Dreher et al., 2021b), malgré les préoccupations concernant la possible capture de ces fonds par les élites des pays bénéficiaires (Dreher et al., 2021a). Il a également été montré que l'APD chinoise contribue à des progrès dans les domaines de la santé et de l'éducation (Martorano et al., 2020; Cruzatti et al., 2023), qu'elle réduit les inégalités régionales en favorisant la répartition des activités économiques (Bluhm et al., 2018), augmente les chiffres d'affaires des entreprises locales (Marchesi et al., 2021), et cela sans nuire à la stabilité politique des pays récipiendaires (Gehring et al., 2022). Cependant, le bilan n'est pas entièrement positif, car l'aide chinoise a aussi entraîné une hausse de la corruption autour de ces projets (Isaksson and Kotsadam, 2018a), a découragé la syndicalisation (Isaksson and Kotsadam, 2018b), et a alimenté la répression gouvernementale dans les pays bénéficiaires (Gehring et al., 2022), reflétant ainsi une transmission des pratiques et normes chinoises vers les pays bénéficiaires.

Le premier chapitre de cette thèse contribue aux deux volets de la littérature sur l'APD chinoise présentés précédemment, analysant les déterminants et impacts de cette aide. Plus précisément, j'examine l'effet des projets d'infrastructure de transport financés par la Chine sur l'activité économique des entreprises dans les pays en développement. En m'appuyant sur les travaux de Mueller (2022), j'utilise des facteurs politiques internes à la Chine comme source de variation exogène afin d'expliquer la construction d'infrastructures de transport dans les pays en développement. Ce choix repose sur l'observation que la Chine favorise les activités de ses entreprises locales à l'étranger, notamment en période de troubles sociaux internes.

Je contribue à la littérature mentionnée précédemment en testant l'hypothèse proposée par Mueller (2022) dans un cadre plus agrégé, et en complétant les conclusions de Marchesi et al. (2021) en me focalisant exclusivement sur les projets de transport et sur le mécanisme d'allègement des contraintes d'infrastructure. L'accent mis sur les projets de transport, qui englobent la construction de routes, de chemins de fer, de ports et d'aéroports, correspond à la stratégie d'aide de la Chine, qui est encline à entreprendre des projets de grande ampleur, ce qui la distingue des

pratiques d'autres donateurs bilatéraux (Brautigam, 2011). De plus, les données récentes d'AidData révèlent que près d'un tiers de l'engagement économique chinois à l'étranger entre 2000 et 2014 a été investi dans la construction d'infrastructures de transport.

En même temps que la Chine se positionnait comme un acteur majeur de l'aide internationale, les années 2000 ont aussi vu les donateurs officiels multiplier les initiatives pour améliorer l'efficacité de l'aide. Ces efforts se sont manifestés par la promotion de meilleures pratiques d'aide lors de plusieurs conférences, dont la Déclaration de Paris en 2005 est un exemple éminent. Ces rencontres avaient pour but de passer d'un modèle d'aide centré sur les intérêts des donateurs vers un modèle axé sur les besoins du bénéficiaire. Le premier est correspond à aide motivée par des objectifs stratégiques des donateurs tels que la sécurisation des ressources naturelles, la formation d'alliances politiques, ou la promotion d'intérêts économiques; tandis que pour le second, l'aide est allouée en fonction des priorités de développement du pays receveur.

Conformément à ces principes, il est apparu nécessaire de privilégier l'aide multilatérale, plus à même de répondre aux besoins des bénéficiaires qu'aux intérêts des donateurs, comme le suggèrent Maizels and Nissanke (1984) et Neumayer (2003). Cependant, puisque le financement de l'aide multilatérale repose sur les contributions des nations développées — souvent les mêmes qui attribuent l'aide bilatérale selon leurs propres intérêts (Maizels and Nissanke, 1984; Alesina and Dollar, 2000) —, il devient crucial d'examiner les motivations politiques influençant l'allocation de l'aide multilatérale. Cette analyse est d'autant plus pertinente que l'aide multilatérale est de plus en plus distribuée via les marchés publics des pays bénéficiaires. Cela permet à leurs intérêts propres, y compris les considérations électorales des gouvernements receveurs, de jouer un rôle crucial.

De nombreuses recherches ont examiné les déterminants politiques de l'aide multilatérale, en se concentrant particulièrement sur l'influence des États-Unis et d'autres grands donateurs bilatéraux au sein des principales organisations distribuant cette aide. Par exemple, Kilby (2013) et Kersting and Kilby (2021) ont montré que la politique intérieure américaine influence l'attribution de l'aide de la Banque mondiale. Kersting and Kilby (2016) et Dreher et al. (2008) ont démontré que les intérêts étrangers des États-Unis affectent également l'aide multilatérale. Ils ont observé que les prêts de la Banque mondiale sont accordés plus rapidement et les prévisions d'inflation par le FMI sont plus basses lorsque

les gouvernements des pays bénéficiaires, alignés géopolitiquement avec les États-Unis, se rapprochent d'élections disputées. De plus, les pays récipiendaires qui occupent un siège au Conseil de sécurité de l'ONU ou qui sont représentés dans les conseils d'administration des institutions multilatérales reçoivent plus d'aide de diverses organisations internationales. (Kuziemko and Werker, 2006; Dreher et al., 2009b,a; Kersting and Kilby, 2019; Dreher et al., 2019; Kaja and Werker, 2010). Une extension naturelle de cette recherche pourrait être d'examiner l'aspect le plus substantiel de l'aide multilatérale au développement, à savoir les marchés publics attribués par la Banque mondiale.

Fondée en 1944 lors de la conférence de Bretton Woods, la Banque mondiale avait pour mission initiale de soutenir la reconstruction de l'Europe après la Seconde Guerre mondiale à travers des aides financières et techniques. Avec le temps, elle a étendu son champ d'action pour promouvoir le développement économique et la lutte contre la pauvreté à l'échelle mondiale, en octroyant prêts et subventions aux pays en développement. Devenue le principal bailleur de fonds multilatéral, elle représente 31% de l'aide totale dispensée par les organisations multilatérales depuis les années 2000, selon l'OCDE. En adéquation avec les principes de responsabilisation dans la gestion de l'aide (aid ownership), établis lors de sommets sur l'efficacité de l'aide tels que ceux de Paris en 2005, Accra en 2008, et Busan en 2011, une part croissante de l'aide de la Banque mondiale a été distribuée via les systèmes de marchés publics des pays bénéficiaires. Cette méthode consiste à transférer directement les fonds aux procédures locales de passation des marchés ou d'achat, plutôt que de passer par l'achat de biens ou de services par le pays donateur, qui les livre ensuite au bénéficiaire. Ce mécanisme de financement a gagné en importance dans le domaine de l'aide multilatérale, car il implique des entreprises du secteur privé et génère des ressources fiscales, contribuant ainsi à la croissance économique du bénéficiaire.

Par conséquent, le deuxième chapitre, co-écrit avec Lisa Chauvet et Marin Ferry, examine les cycles politiques affectant l'attribution des contrats publics financés par la Banque mondiale. Plus précisément, il analyse la manière dont les pays donateurs et bénéficiaires orientent l'attribution de ces contrats en faveur de leurs entreprises nationales afin de s'aligner sur leurs propres intérêts électoraux.

Le troisième chapitre se distingue des deux précédents en examinant l'impact de la politisation et de la répression sur les résultats électoraux dans une perspective historique. J'utilise le changement de pays de rattachement de la Savoie et de Nice entre 1815 et 1860 comme expérience naturelle pour explorer cet effet. L'analyse

se concentre sur les variations des résultats électoraux le long des frontières qui ont séparé ces régions du reste de la France. Plus précisément, je compare des groupes de population similaires qui ont été soumis à différents processus de politisation et de répression durant cette période.

La répression politique s'accompagne fréquemment d'expériences traumatisantes qui peuvent causer des troubles psychologiques persistants. Les effets dévastateurs de la répression politique en termes de santé mentale ont été largement documentés. [Munczek and Tuber \(1998\)](#), [Sales et al. \(2000\)](#) et [Stammel et al. \(2013\)](#) ont montré que les victimes de répression politique, ainsi que leurs familles, sont susceptibles de développer des troubles durables tels que la dépression, l'anxiété, les symptômes post-traumatiques et le deuil prolongé, qui peuvent perdurer des décennies après les événements. En outre, la répression politique peut également saboter les résultats économiques en érodant la confiance des individus. [Lichter et al. \(2021\)](#), [Booth et al. \(2022\)](#), [Nikolova et al. \(2022\)](#) et [Pronkina et al. \(2023\)](#) ont constaté que différentes formes de répression, comme la surveillance de la Stasi en Allemagne de l'Est, la Révolution culturelle en Chine, ou la connaissance de l'existence des camps de travail forcé en URSS, réduisent la confiance à la fois interpersonnelle et institutionnelle. Ces études mettent en évidence un impact profond et transgénérationnel de ces déficits de confiance, transmis d'une génération à l'autre à travers la socialisation familiale.

Et comme le souligne ([Nunn, 2009](#)), la confiance est essentielle pour le développement économique à long terme, en influençant la productivité (?), l'éducation, et la qualité des institutions (?). ([Algan and Cahuc, 2010](#)) a confirmé empiriquement cette relation à l'échelle mondiale, en se basant sur une mesure de la confiance héritée dérivée des niveaux de confiance des premiers immigrants américains envers leur pays d'origine. Leurs résultats montrent que la confiance joue un rôle prépondérant, expliquant 45% des variations du revenu par habitant grâce à la confiance héritée. Cette idée est confirmée par [Lichter et al. \(2021\)](#), qui observe que les zones de l'ancienne Allemagne de l'Est ayant subi une surveillance accrue de la Stasi affichent aujourd'hui des revenus plus bas et un taux de chômage plus élevé, en raison d'une confiance réduite.

Malgré ses effets délétères sur la santé mentale, la confiance et le développement économique, une vision héritée de Machiavel pourrait encore justifier le recours à la répression politique si elle atteint l'un de ses objectifs principaux, à savoir neutraliser la politisation initiée par l'opposition. De nombreuses recherches se sont

penchées sur l'impact de la répression politique sur les comportements électoraux. [Kapelko and Markevich \(2014\)](#), [Lupu and Peisakhin \(2017\)](#) et [Rozenas et al. \(2017\)](#) ont observé que les régions ou groupes ethniques qui ont été ciblés pendant l'ère soviétique ont aujourd'hui tendance à moins soutenir les partis pro-russes. Concernant la répression politique lors de la révolution culturelle en Chine, [Wang \(2021\)](#) suggère que les individus venant de localités fortement réprimées ont adopté des attitudes plus hostiles envers le régime. En Alsace-Lorraine, [Dehdari and Gehring \(2022\)](#) et [Gehring \(2021\)](#) indiquent qu'une répression relativement moins violente durant le processus de construction nationale a favorisé un penchant accru pour le régionalisme, la décentralisation, et le soutien à l'Union européenne. D'après cette littérature, la fin ne semble pas justifier les moyens, puisque la répression politique s'avère souvent inefficace, voire contre-productive, pour atteindre ses objectifs politiques.

La plupart des recherches examinant l'influence de la répression politique sur les résultats électoraux se focalisent sur les effets de répressions massives organisées sous les régimes communistes, ciblant souvent des régions entières ou des groupes ethniques spécifiques. Cependant, les répercussions peuvent-elles varier lorsque la répression est plus sélective et ciblée ? L'épisode de répression politique de 1851 dans le sud-est de la France, suite au coup d'État de Napoléon III et au soulèvement des Républicains, peut permettre de répondre à cette question. Lors de cet événement, 21 000 personnes ont été condamnées et 9 530 déportées, dont 239 aux tristement célèbres bagnes de Guyane française (selon les sources de [Margadant \(1979\)](#) et [Devos \(1992\)](#)). Cet exemple historique offre également un contexte idéal pour aborder le défi statistique majeur lié à l'estimation de l'impact de la répression politique, à savoir la nature non aléatoire des événements de répression. Les populations de la Savoie et de Nice, initialement similaires à celles de l'autre côté de la frontière avec la France, ont vécu des niveaux de répression politique différents de leurs voisins en raison de facteurs externes. L'analyse des variations le long de la frontière permet ainsi de mesurer l'effet de la répression sur les comportements électoraux.

La suite de ce résumé fournit un aperçu plus détaillé des trois articles qui composent cette thèse.

Projets d'infrastructure de transport chinois et probabilité d'exportation des entreprises

Le premier chapitre examine les effets des infrastructures de transport financées par la Chine sur les activités d'export des entreprises situées dans les pays en

développement. J'analyse comment les investissements chinois à l'étranger dans les routes, chemins de fer, ports et aéroports affectent la probabilité d'export des entreprises (*i.e.* la marge extensive). Pour ce faire, j'utilise une méthode instrumentale qui exploite les motivations politiques derrière l'attribution de l'aide chinoise. En effet, selon les travaux de Mueller [Mueller \(2022\)](#), le gouvernement chinois pourrait utiliser son aide internationale comme moyen d'apaiser les tensions sociales internes.

Les projets de transport financés par la Chine ont joué un rôle clé dans la construction récente d'infrastructures dans les pays en développement. D'après un rapport du Consortium pour les infrastructures en Afrique publié en 2018, les investissements chinois dans les infrastructures africaines au cours de la dernière décennie ont dépassé les contributions totales de tous les pays du G8⁵³. De plus, la construction d'infrastructures de transport constitue une part substantielle de l'aide chinoise allouée, représentant près d'un tiers des projets financés par la Chine.⁵⁴ L'approche chinoise en matière d'aide au développement se distingue nettement de celle des donateurs traditionnels et des institutions multilatérales, privilégiant des initiatives d'envergure axées sur des projets de grande ampleur, notamment dans le secteur du transport. Face à cette approche unique et à un niveau de conditionnalité généralement plus bas que les donateurs bilatéraux du Comité d'Aide au Développement, les dirigeants des pays en développement considèrent la Chine comme un partenaire de développement privilégié pour la construction d'infrastructures de transport ([Horigoshi et al., 2022](#)).

En s'appuyant sur la prévalence croissante de la Chine en tant que fournisseur d'infrastructures de transport dans les pays en développement, j'examine l'impact de ces projets sur le développement des entreprises implantées localement, notamment sur leurs probabilités d'exporter. Cette analyse est motivée par les intuitions des modèles de croissance tirée par les exportations. En effet, les recherches de [Minot and Goletti \(1998\)](#) et [Bas \(2012\)](#) ont indiqué que l'augmentation du nombre d'entreprises exportatrices peut stimuler le développement économique, notamment en facilitant l'adoption de nouvelles technologies et en augmentant les revenus des ménages. Ainsi, cette étude vise à déterminer si les projets de transport financés par la Chine peuvent effectivement encourager les entreprises des pays en développement à se lancer dans l'exportation, et soutenir ainsi le développement économique.

J'évalue l'impact des infrastructures de transport financées par la Chine sur

⁵³Source: <https://www.icafrica.org/en/topics-programmes/2018-overview/>

⁵⁴Source: AidData

la propension des entreprises à exporter en utilisant deux sources de données : l'Enquête sur les entreprises de la Banque mondiale pour les variables au niveau de l'entreprise et l'ensemble de données Geocoded Global Chinese Official Finance Dataset d'AidData pour les projets de transport financés par la Chine. La première source offre des données de panel et des données transversales comprenant des informations détaillées sur les entreprises dans les pays en développement, comme leur localisation régionale au niveau ADM1, leur secteur d'activité et leur activité exportatrice. La seconde source délivre des informations sur les projets financés par la Chine : leur classification sectorielle, leur emplacement exact et l'année de finalisation.

Après avoir combiné ces deux sources de données, j'évalue si la réalisation d'un projet d'infrastructure de transport financé par la Chine dans une région augmente la probabilité d'exportation des entreprises de cette région, par rapport à celles situées dans des régions sans projets. L'analyse de l'impact des infrastructures pose des défis empiriques, notamment en raison de possibles problèmes d'endogénéité entre les variables principales. Pour remédier à ces biais, j'utilise un instrument de shift-share proposé par [Mueller \(2022\)](#) basé sur les déterminants politiques internes de l'aide chinoise. Plus précisément, je multiplie la probabilité que des régions reçoivent des infrastructures de transport chinoises par le nombre de conflits sociaux en Chine, créant ainsi une variation exogène du nombre de projets de transport attribués par région. Cet instrument repose sur l'hypothèse que, en période de tensions sociales internes, la Chine est susceptible d'augmenter les projets d'infrastructure à l'étranger afin d'apaiser les tensions. Cette intuition se base sur l'observation que la construction de ces projets d'aide est confiée exclusivement à des entreprises chinoises. L'attribution de grands contrats de construction à ces entreprises peut donc améliorer les conditions de travail et augmenter l'emploi en Chine, contribuant ainsi à apaiser les tensions sociales.

Les résultats de la première étape confirment que les régions favorisées par la Chine ont reçu un nombre accru de projets de transport durant les périodes où le gouvernement chinois cherchait à calmer l'agitation sociale. La fiabilité de ce résultat est vérifiée par divers tests de robustesse, qui incluent la dépendance de l'échantillon et les interactions avec les facteurs confondants habituellement lié à l'aide. En continuant avec cette approche instrumentale, les analyses de la seconde étape révèlent que, de manière surprenante, les infrastructures de transport financées par la Chine ne semblent pas influencer de manière significative la probabilité

d'exportation des entreprises. Cette absence de résultats significatifs contraste avec l'impact positif général de l'aide chinoise sur le développement économique rapporté dans la littérature (Bluhm et al., 2018; Baniya et al., 2020; Martorano et al., 2020; Dreher et al., 2021b). Elle est également inattendue au regard des conclusions de Marchesi et al. (2021), qui ont trouvé que les projets d'aide chinois, tous secteurs confondus, exerçaient en moyenne une influence positive sur les ventes des entreprises, notamment quand les infrastructures de transport limitaient auparavant leur activité.

Une série de contrôles de robustesse confirme que l'absence de résultats statistiquement significatifs n'est pas due à la dépendance de l'échantillon ni à une mauvaise spécification du modèle. Des conclusions similaires sont obtenues en utilisant d'autres variables dépendantes, telles que le chiffre d'affaires et le montant des exportations. En analysant l'hétérogénéité des interventions chinoises en fonction des flux financiers et des différentes catégories de projets de transport, les analyses montrent que les résultats ne varient pas selon que le financement des projets est considéré comme de l'APD (i.e. si le prêt a un élément de subvention de moins de 25%) ou comme un autre financement officiel. La construction de routes, de chemins de fer, de ports ou d'aéroports n'a pas d'effet distinct sur la probabilité d'exportation des entreprises.

L'hétérogénéité au niveau de la région, du secteur et de l'entreprise est ensuite prise en compte. Il est surprenant de constater que les projets de transport n'ont pas d'impact significatif sur la probabilité d'exportation des entreprises opérant dans des secteurs dépendant structurellement du transport, et qui devraient normalement bénéficier le plus des améliorations dans ce domaine. En revanche, dans les régions à faible densité de population, les projets de transport financés par la Chine semblent augmenter la probabilité d'exportation des entreprises. Ce dernier constat semble intuitif, car les régions moins peuplées ont généralement un accès limité au marché et un manque d'infrastructures de transport. Par conséquent, les entreprises situées dans ces régions sont susceptibles de bénéficier davantage de ces projets de transport.

En résumé, en s'appuyant sur une stratégie d'instrumentation, les infrastructures de transport financées par la Chine ne semblent pas avoir d'impact significatif sur la probabilité d'exportation des entreprises. Cette absence d'effet pourrait être expliquée théoriquement par plusieurs facteurs, tels que des investissements insuffisants dans les infrastructures (Bougheas et al., 1999), l'absence de réformes commerciales accompagnant l'amélioration des infrastructures (Baniya et al., 2020),

ou un placement sous-optimal des réseaux de transport [Graff \(2019\)](#). Cependant, les projets de transport chinois semblent avoir un effet positif sur les marges extensives des entreprises lorsqu'ils sont réalisés dans des régions à faible densité de population.

Aide étrangère et jeux de pouvoir : Cycle politique dans l'attribution des marchés publics de la Banque mondiale

Le deuxième chapitre, co-écrit avec Lisa Chauvet et Marin Ferry, explore le cycle politique influençant l'attribution de marchés publics par la Banque mondiale. Nous examinons si les entreprises reçoivent des contrats Banque mondiale plus conséquents durant les périodes électorales, ce qui pourrait indiquer une forme de clientélisme.

Les études précédentes ont montré que l'attribution de l'aide multilatérale est généralement moins dictée par les intérêts des donateurs que les aides bilatérales ([Maizels and Nissanke, 1984](#); [Neumayer, 2003](#)). Cependant, des travaux récents s'intéressent aux distorsions politiques affectant les organisations internationales de développement. Par exemple, l'attribution de l'aide multilatérale pourrait être influencée par les intérêts des principaux donateurs bilatéraux, comme les États-Unis ([Kilby, 2013](#); [Kersting and Kilby, 2016, 2021](#)), et les pays bénéficiaires pourraient se voir octroyer plus d'aide lorsqu'ils détiennent un siège au Conseil de sécurité des Nations Unies ([Kuziemko and Werker, 2006](#); [Dreher et al., 2009b,a](#); [Kersting and Kilby, 2019](#)) ou lorsqu'ils sont représentés au conseil d'administration de l'institution ([Dreher et al., 2019](#); [Kaja and Werker, 2010](#)).

L'attribution de l'aide multilatérale peut aussi être influencée par les intérêts politiques des pays récipiendaires ([McLean, 2017](#)), et cette aide peut être partiellement captée par l'élite du pays bénéficiaire ([Andersen et al., 2022](#)). La Banque mondiale, un des principaux acteurs de l'aide multilatérale, distribue une quantité importante de son aide via les marchés publics des pays récipiendaires. Dans ce processus, le gouvernement du pays bénéficiaire choisit l'entreprise qui mettra en œuvre le projet. Plus précisément, après l'approbation du financement d'un projet par la Banque mondiale, le gouvernement du pays bénéficiaire doit sélectionner le prestataire selon une des méthodes d'attribution définies par la Banque : l'appel d'offres international, la sélection basée sur la qualité et le coût, le contrat de gré à gré et l'appel d'offres national. L'importance de l'attribution de l'aide via les marchés publics a augmenté au fil du temps, étant perçue comme un moyen de responsabiliser le pays bénéficiaire et de s'aligner sur les principes d'appropriation

discutés dans les forums dédiés à l'efficacité de l'aide.

Cependant, un corpus de recherches montre que les marchés publics peuvent être influencés par des considérations politiques. Il a été observé que les entreprises ayant soutenu financièrement des candidats gagnants voient la valeur de leurs contrats publics augmenter significativement, démontrant que ces contrats jouent un rôle central dans les rétrocommissions. (Daniele and Bennedsen, 2010; Goldman et al., 2013; Titl and Geys, 2019; Schoenherr, 2019; Baltrunaite, 2020).

Cet article cherche à lier la littérature sur les déterminants politiques de l'aide multilatérale et celle explorant les cycles électoraux des marchés publics. Étant donné le montant substantiel d'aide multilatérale distribuée par la Banque mondiale, son recours fréquent aux marchés publics pour répartir cette aide et le poids des pays développés au sein de cette institution (pays où les rétrocommissions dans les marchés publics sont courantes), il est pertinent de se demander si des schémas similaires existent pour les marchés publics de la Banque mondiale. En conséquence, nous examinons si la valeur moyenne des contrats fluctue en fonction des élections dans les pays fournisseurs et bénéficiaires. Comme le soulignent Dreher et al. (2018a) et Lehne et al. (2018), le cycle potentiel dans l'attribution des marchés publics représente une préoccupation majeure, car il pourrait compromettre l'efficacité et même l'efficacité des interventions de la Banque mondiale.

Nous identifions deux cycles politiques distincts en fonction du lieu où se déroulent les élections : soit dans le pays bénéficiaire, c'est-à-dire le pays qui reçoit un projet de la Banque mondiale, soit dans le pays fournisseur, à savoir le pays d'origine de l'entreprise étrangère qui fournit le service. Dans le cas d'une élection imminente dans le pays bénéficiaire, le gouvernement en place, désireux de se faire réélire, pourrait choisir d'accorder des contrats Banque mondiale plus importants à des entreprises locales. Cette stratégie vise à renforcer l'image d'un gouvernement qui stimule la croissance économique et crée des emplois, augmentant ainsi ses chances de réélection. Les entreprises locales peuvent également contribuer financièrement à la campagne électorale en contrepartie de l'obtention de contrats Banque mondiale. Nous appelons ce premier mécanisme le cycle politique domestique.

Dans le second cas, le gouvernement fournisseur, confronté à une élection proche, a des objectifs électoraux similaires. Étant donné que le pays bénéficiaire gère l'attribution des contrats Banque mondiale, le gouvernement fournisseur pourrait exercer une influence diplomatique ou économique sur le gouvernement récipiendaire pour orienter l'attribution des contrats vers une entreprise de son

propre pays. L'entreprise étrangère bénéficiant du contrat pourrait alors générer des emplois dans son pays d'origine ou soutenir financièrement la campagne électorale du gouvernement en place en retour de cette faveur. Nous nommons cette mécanique le cycle politique transfrontalier. La distinction principale entre les cycles politiques domestiques et transfrontaliers réside dans l'influence que le gouvernement fournisseur exerce sur le gouvernement bénéficiaire dans le second cas.

Afin d'examiner ces deux hypothèses, nous utilisons les données extraites de la base de données des contrats publics de la Banque mondiale, qui recense de manière détaillée les principaux contrats octroyés entre 1993 et 2019. Cette base de données inclut des informations telles que le nom de l'entreprise, son pays d'origine, la date de signature du contrat, son montant en dollars américains, le pays bénéficiaire, la catégorie du contrat et la méthode d'attribution. Nous croisons ces informations avec celles issues de la base de données National Elections across Democracy and Autocracy, qui fournit les dates précises des élections et des détails approfondis sur le contexte électoral.

Utilisant un modèle économétrique basé sur des estimateurs de Poisson Pseudo Maximum de Vraisemblance et une série d'effets fixes, nos résultats montrent que les entreprises locales obtiennent des contrats significativement plus conséquents autour des périodes électorales dans le pays bénéficiaire. Ces résultats corroborent non seulement les conclusions de [McLean \(2017\)](#), mais les élargissent également en indiquant que la préférence pour les entreprises domestiques suit un cycle électoral. Parallèlement, nous constatons aussi une confirmation du cycle politique transfrontalier, car les entreprises étrangères remportent des contrats significativement plus importants un semestre avant les élections dans leur pays d'origine.

La validité de ces deux résultats est vérifiée par une série de tests de robustesse, examinant un potentiel biais de sélection ainsi que l'endogénéité des calendriers électoraux. Afin d'évaluer si ces cycles politiques domestiques et transfrontaliers reflètent un favoritisme dans l'attribution des contrats Banque mondiale, nous explorons l'hétérogénéité des contextes électoraux. Ces cycles se manifestent seulement lorsque les entreprises peuvent légalement contribuer au financement des campagnes des candidats dans leurs pays, lorsque les élections sont compétitives, en période de hausse du chômage et quand les gouvernements en place cherchent à être réélus.

Nous examinons ensuite l'influence exercée par les pays fournisseurs sur les

récipiendaires dans le cadre du cycle politique transfrontalier. Les entreprises étrangères obtiennent des contrats significativement plus importants autour des élections si leur pays d'origine est un donateur majeur pour le pays bénéficiaire. Cela souligne l'importance des relations économiques dans l'influence exercée par le fournisseur sur le bénéficiaire. L'intensité de ce cycle transfrontalier s'accroît si le pays d'origine de l'entreprise réduit son aide liée⁵⁵. Cela suggère que ce mécanisme pourrait servir d'outil aux pays développés pour récupérer leurs contributions aux institutions de développement multilatéral. Le cycle transfrontalier est également plus marqué lorsqu'il existe une histoire coloniale entre les pays fournisseurs et bénéficiaires, montrant l'influence des connexions historiques.

En résumé, notre étude a mis en évidence l'existence de cycles politiques domestiques et transfrontaliers dans l'attribution des contrats Banque mondiale. En effet, nous observons que les entreprises domestiques et étrangères obtiennent des contrats Banque mondiale significativement plus importants lors des périodes électorales dans leurs pays d'origine respectifs. Les analyses d'hétérogénéité montrent des indices de favoritisme électoral, ces cycles survenant principalement quand les gouvernements au pouvoir cherchent à améliorer leur image et que les conditions légales facilitent les rétrocommissions via un financement des campagnes électorales.

L'héritage politique de la politisation et de la répression au XIXe siècle dans le sud-est de la France

Ce troisième chapitre examine l'impact de la politisation et de la répression politique sur les résultats électoraux, en se concentrant sur l'expérience naturelle offerte par la frontière de 1815-1860, séparant le Duché de Savoie et le Comté de Nice de la France.

La répression politique a une large gamme de conséquences néfastes, érodant le bien-être mental des individus ciblés (Munczek and Tuber, 1998; Sales et al., 2000; Stammel et al., 2013) et freinant le développement économique via une baisse de confiance (Nunn, 2009; Pronkina et al., 2023; Lichter et al., 2021; Booth et al., 2022; Nikolova et al., 2022). Malgré ses graves conséquences, la répression politique reste une stratégie couramment utilisée. Les indicateurs de sa prévalence suggèrent même qu'elle pourrait s'intensifier à l'avenir. Comme le montre l'Index

⁵⁵Aide qui limitent les achats aux entreprises du pays donateur.

de l'Échelle de la Terreur Politique, la situation s'est aggravée dans les pays à faible revenu lors de la dernière décennie. Cette tendance est exacerbée par l'influence des normes des donateurs autocratiques sur les pays bénéficiaires (Gehring et al., 2022; Isaksson and Kotsadam, 2018a), matérialisée par une augmentation de la répression gouvernementale et de la corruption autour des projets d'aide chinois. Cette inquiétude grandissante ne se limite pas aux pays en développement ; elle concerne également les pays développés, comme le démontre le déclin de la liberté de presse évaluée par l'indice de Freedom House.

Malgré ses effets néfastes, la répression politique est utilisée comme stratégie pour contrer l'opposition et conserver le pouvoir par tous les moyens nécessaires. Il est pertinent de se demander si cette tactique atteint son objectif principal. Une analyse à long terme basée sur une expérience naturelle semble appropriée pour une telle évaluation. En 1815, les régions de Savoie et de Nice, situées dans le sud-est de la France, ont été cédées au Royaume de Piémont-Sardaigne lors du Congrès de Vienne, qui visait à démanteler l'Empire français. En 1860, ces deux régions connaissent un nouveau changement exogène de leur affiliation territoriale, étant offertes à Napoléon III par le roi de Piémont-Sardaigne en remerciement de l'aide militaire française contre l'Autriche. De 1815 à 1860, à l'exception de la Savoie et de Nice, le sud-est de la France a connu une politisation marquée par l'émergence de sociétés secrètes Républicaines, précurseurs des mouvements de gauche modernes. La région a également été le théâtre d'une des répressions politiques les plus sévères de l'histoire française, suite au soulèvement contre le coup d'État de Napoléon III en 1851. Selon les travaux d'archives de Margadant (1979) et Devos (1992), 26 884 personnes ont été arrêtées, 21 000 ont été condamnées et 9 530 ont été déportées en Algérie ou en Guyane française. Durant cette même période, la Savoie et Nice ont connu une politisation différente, moins marqué par les idéologies républicaines et ont évité la répression politique.

Considérant le changement exogène de la Savoie et de Nice durant une époque cruciale pour l'émergence des idéologies politiques modernes, j'analyse les différences de résultats électoraux le long de la frontière séparant ces régions de la France entre 1815 et 1860. Je mets en œuvre un modèle de régression discontinue spatiale pour comparer les résultats électoraux des communes situées dans une zone de 15 km de part et d'autre de cette frontière. Les communes du groupe de traitement sont celles qui sont restées françaises entre 1815 et 1860, connaissant ainsi à la fois la politisation républicaine et la répression politique. Les communes dans cette

bande de 15 km partagent des caractéristiques géographiques et socio-économiques similaires. De plus, une analyse ex-ante des opinions politiques avant 1815, mesurés par des données de conflits sociaux recueillies par [Chambru and Maneuvrier-Hervieu \(2022\)](#), révèle que les populations des groupes de traitement et de contrôle avaient des opinions politiques comparables.

Dans un premier temps, seuls les effets de la politisation sont analysés. En exploitant les données issues des archives électorales des Archives Nationales Françaises, les résultats de l'élection législative de 1871 ⁵⁶ ont été recueillis au niveau communal. Du côté français de la frontière, les tendances de vote ont révélé une préférence significative pour les candidats Républicains radicaux, reflétant l'impact des efforts de politisation républicaine menés dans ces régions. Les conséquences à long terme de la politisation sont ensuite analysées avec les résultats électoraux au niveau communal pour les élections présidentielles et législatives de 1995 à 2022. Les candidats de gauche, considérés comme les descendants idéologiques des Républicains du XIXe siècle ([Julliard, 2014](#)), continuent d'obtenir des scores significativement plus élevés dans les municipalités du côté français de la frontière.

L'impact de la répression politique est étudié dans un second temps via une base de données sur la répression de 1851. Celle-ci a été constituée grâce à l'ouvrage de [Devos \(1992\)](#), présent aux Archives Nationales. Ces données inédites incluent des détails sur les personnes réprimées ayant demandé une pension en vertu de la Loi Nationale de Réparation de 1881 : leur nom, lieu et date de naissance, commune de résidence en 1851 et profession. Le nombre de personnes réprimées par commune est ajouté au modèle. Les résultats indiquent que la répression a réduit la préférence pour la gauche, mais pas suffisamment pour supprimer complètement la tendance initiale. Ce constat met en lumière l'efficacité limitée de la répression politique, qui n'a pas réussi à neutraliser pleinement les effets de la politisation initiale.

Les effets durables de la politisation et de la répression politique pourraient être expliqués par des dynasties politiques réprimées, l'émigration forcée et une population qui se mélange relativement peu. En effet, les municipalités où un maire porte le même nom qu'un citoyen réprimé situé dans un rayon de 20 km affichent une préférence significativement plus faible pour la gauche. Des observations similaires ont été trouvées en prenant en compte le nombre de citoyens réprimés qui ont été contraints de quitter leur commune par un départ forcé ou un exil. Il apparaît aussi que les résultats obtenus avec les élections récentes sont influencés par les communes

⁵⁶ première élection libre après le rattachement de la Savoie et de Nice à la France

de traitement et de contrôle se situant dans des zones d'emploi et d'académies distinctes. Cette observation suggère que les effets de longue durée de la politisation et de la répression se maintiennent lorsque les populations ne se mélangent pas dans leurs environnements de travail et durant l'enseignement supérieur.

En résumé, une offre politique avec des souvenirs familiaux de la répression, la diminution mécanique du nombre d'activistes Républicains suite à l'émigration et une population relativement peu mélangée peuvent expliquer la persistance des effets de la politisation et de la répression politique sur les résultats électoraux. Cette étude complète la littérature sur les conséquences de la répression politique sur les résultats électoraux, en examinant un contexte différent des régimes soviétiques et de la Chine communiste (Kapelko and Markevich, 2014; Lupu and Peisakhin, 2017; Rozenas et al., 2017; Zhukov and Talibova, 2018; Wang, 2021). Elle apporte également une dimension temporelle plus étendue, car elle se penche sur des événements survenus il y a plus d'un siècle, tandis que la plupart des recherches dans ce domaine se concentrent sur des événements datant du milieu du XXe siècle. De plus, cette recherche introduit une nouvelle base de données individuelle, riche et détaillée, qui affine l'analyse des impacts étudiés.

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RÉSUMÉ

Ma thèse de doctorat comporte trois chapitres traitant de problématiques d'économie politique abordées par le prisme de l'économie du développement et de l'économie historique. Les deux premiers portent sur l'influence politique et économique de deux bailleurs de fonds majeurs intervenant dans les pays en développement. Le troisième chapitre se concentre sur les effets de la politisation et de la répression politique sur les résultats électoraux. Le premier chapitre s'intéresse aux déterminants politiques et aux impacts économiques de l'intervention d'un bailleur devenu incontournable dans le secteur de l'aide au développement, à savoir la Chine. Plus précisément, ce chapitre évalue l'effet des projets d'infrastructure de transport financés par l'aide chinoise sur la capacité des entreprises des pays récipiendaires à exporter. Pour quantifier cet impact, j'ai recours à une stratégie reposant sur l'utilisation de variables instrumentales. L'intuition de cet instrument repose sur l'hypothèse que la Chine réagit aux troubles sociaux internes en investissant dans des projets d'aide gérés par des entreprises chinoises. Cette stratégie viserait à stimuler l'emploi et à améliorer les conditions de travail au sein de ces entreprises, contribuant ainsi à apaiser les tensions sociales. Les résultats soulignent l'absence d'effet significatif moyen, mais suggèrent un impact positif de ces projets d'aide chinoise sur la probabilité d'exporter des entreprises situées dans les régions peu densément peuplées. Le deuxième chapitre analyse quant à lui l'influence des élections sur l'attribution des marchés publics financés par la Banque mondiale dans les pays en développement. Les résultats empiriques soulignent que les entreprises des pays récipiendaires gagneraient des contrats en moyenne plus lucratifs autour des semestres d'élections du pays receveur, et ce particulièrement si la législation de ce dernier autorise le financement privé des campagnes électorales. Les entreprises étrangères gagneraient également des contrats plus importants autour des élections dans leur pays d'origine, notamment s'il existe une coopération solide en matière d'aide ou une histoire coloniale commune entre leur pays d'origine et le pays récipiendaire. Ces résultats suggèrent ainsi l'existence d'une forme de clientélisme à des fins électorales dans le processus d'allocation des marchés publics de la Banque mondiale, et mettent en évidence l'influence persistante des bailleurs bilatéraux sur les pays en développement à des fins tant politiques qu'économiques. Le dernier chapitre étudie l'effet de la politisation et de la répression politique sur les résultats électoraux, en exploitant l'expérience naturelle de la séparation puis réintégration du duché de Savoie et du comté de Nice à la France entre 1815 et 1860. À l'aide de données collectées auprès des Archives nationales, les résultats de ce chapitre montrent que les différentes trajectoires historiques de part et d'autre de la frontière séparant la France de ces deux régions, marquées par des processus de politisation distincts, se sont traduites par une préférence pour les républicains radicaux en 1871 et pour les candidats de gauche lors des élections récentes (1995-2022). Les résultats suggèrent également que la répression politique de 1851 a été inefficace car elle a été incapable d'inverser les effets initiaux de la politisation. Les analyses supplémentaires suggèrent que les effets durables de ces événements sur les résultats électoraux peuvent être attribués aux dynasties politiques des victimes, à l'émigration causée par la répression, ainsi qu'à la sédentarité de la population.

MOTS CLÉS

Economie Politique, Economie du développement, Economie Historique, Aide Publique au Développement

ABSTRACT

My Ph.D. dissertation consists of three chapters exploring political economy topics approached through the lens of development economics and historical economics. The first two chapters focus on the political and economic influence of two major donors operating in developing countries. The third chapter examines the combined effects of politicization and political repression on electoral outcomes. The first chapter evaluates the political determinants and the economic impacts of a donor's intervention that has become a key player in the field of official development assistance, namely China. More specifically, it investigates the effect of transport infrastructure projects financed by Chinese aid on the capacity of companies in recipient countries to export. To quantify this impact, I employ an instrumental variable strategy. The intuition behind this instrument is based on the assumption that China uses its aid in response to internal labor unrest by investing in major transport projects carried out by Chinese companies. This strategy would aim to stimulate employment and improve working conditions within these companies, thus helping to ease social tensions. The results underline the absence of any significant average effect, but suggest a positive impact of these Chinese aid projects on the export probability of firms located in sparsely populated regions. The second chapter analyzes the influence of elections on the award of World Bank-financed public contracts in developing countries. Empirical results show that, on average, firms from recipient countries would win more lucrative contracts around the election semesters of the receiving country, especially if the latter's legislation allows private funding of election campaigns. Foreign firms would also secure larger contracts around elections in their home country, particularly if the home and recipient countries enjoy strong aid cooperation or share a common colonial history. These findings thus suggest the existence of a form of clientelism for electoral purposes in the World Bank's procurement allocation process and highlight the persistence of bilateral donors' influence on developing countries for political and economic purposes. The final chapter delves into the effect of politicization and political repression on electoral outcomes, by exploiting the natural experiment of the separation and reintegration of the Duchy of Savoy and the County of Nice into France between 1815 and 1860. Using first-hand data collected from the National Archives, this chapter's findings show that the different historical trajectories, marked by distinct politicization processes, translated into a preference for radical Republicans in 1871 and left-wing candidates in recent elections (1995-2022). Results also suggest that the political repression of 1851 was ineffective as it failed to reverse the initial effects of politicization. Further analyses propose that enduring effects of these events on electoral outcomes can be attributed to the political dynasties of victims, emigration caused by the repression, and the sedentary nature of the population.

KEYWORDS

Political Economy, Development Economics, Economic History, Official development assistance