Year : 2023

THREE ESSAYS ON POLITICAL ECONOMY

Lax Martinez Gema

UNIL | Université de Lausanne

FACULTÉ DES HAUTES ÉTUDES COMMERCIALES

DÉPARTEMENT D'ÉCONOMIE

THREE ESSAYS ON POLITICAL ECONOMY

THÈSE DE DOCTORAT

présentée à la

Faculté des Hautes Études Commerciales
de l'Université de Lausanne

pour l'obtention du grade de
Docteure ès Sciences Économiques,
mention « Économie politique »

par

Gema LAX MARTINEZ

Directeur de thèse
Prof. Dominic Rohner

Jury

Prof. Christian Zehnder, président
Prof. Rafael Lalive, expert interne
Prof. Giovanni Prarolo, expert externe

LAUSANNE
2023

UNIL | Université de Lausanne

FACULTÉ DES HAUTES ÉTUDES COMMERCIALES

DÉPARTEMENT D'ÉCONOMIE

THREE ESSAYS ON POLITICAL ECONOMY

THÈSE DE DOCTORAT

présentée à la

Faculté des Hautes Études Commerciales
de l'Université de Lausanne

pour l'obtention du grade de
Docteure ès Sciences Économiques,
mention « Économie politique »

par

Gema LAX MARTINEZ

Directeur de thèse
Prof. Dominic Rohner

Jury

Prof. Christian Zehnder, président
Prof. Rafael Lalive, expert interne
Prof. Giovanni Prarolo, expert externe

LAUSANNE
2023

UNIL | Université de Lausanne
HEC Lausanne
Le Décanat
Bâtiment Internef
CH-1015 Lausanne

I M P R I M A T U R

Sans se prononcer sur les opinions de l'autrice, la Faculté des Hautes Etudes Commerciales de l'Université de Lausanne autorise l'impression de la thèse de Madame Gema LAX MARTINEZ, titulaire d'un bachelor en Economie de l'Université de Murcie et d'un master en Analyse économique de l'Université Autonome de Barcelone, en vue de l'obtention du grade de docteure ès Sciences économiques, mention « économie politique ».

La thèse est intitulée :

THREE ESSAYS ON POLITICAL ECONOMY

Lausanne, le 16 juin 2023

La Doyenne

Marianne SCHMID MAST

Members of the doctoral committee

Professor Dominic Rohner

Professor of Economics, HEC, University of Lausanne

Thesis supervisor

Professor Rafael Lalive

Professor of Economics, HEC, University of Lausanne

Internal member of the doctoral committee

Professor Giovanni Prarolo

Professor of Economics, University of Bologna

External member of the doctoral committee

University of Lausanne
Faculty of Business and Economics

PhD in Economics
Subject area Political Economy

I hereby certify that I have examined the doctoral thesis of

Gema LAX MARTINEZ

and have found it to meet the requirements for a doctoral thesis.
All revisions that I or committee members
made during the doctoral colloquium
have been addressed to my entire satisfaction.

Signature: _____ Date: 20/5/2023

Prof. Dominic ROHNER
Thesis supervisor

University of Lausanne
Faculty of Business and Economics

PhD in Economics
Subject area Political Economy

I hereby certify that I have examined the doctoral thesis of

Gema LAX MARTINEZ

and have found it to meet the requirements for a doctoral thesis.
All revisions that I or committee members
made during the doctoral colloquium
have been addressed to my entire satisfaction.

Signature:  Date: May 21, 2023

Prof. Rafael LALIVE
Internal member of the doctoral committee

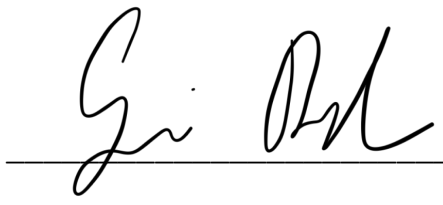
University of Lausanne
Faculty of Business and Economics

PhD in Economics
Subject area Political Economy

I hereby certify that I have examined the doctoral thesis of

Gema LAX MARTINEZ

and have found it to meet the requirements for a doctoral thesis.
All revisions that I or committee members
made during the doctoral colloquium
have been addressed to my entire satisfaction.

Signature:  Date: May 22, 2023

Prof. Giovanni PRAROLO
External member of the doctoral committee

UNIL | Université de Lausanne
HEC Lausanne

Doctoral Thesis

Three Essays on Political Economy

Université de Lausanne

Author: Gema Lax Martinez

Supervisor: Dominic Rohner

Acknowledgements

I would like to express my sincere gratitude to my supervisor Dominic Rohner for his guidance, precious support and generosity during the whole PhD journey. I would choose him as a supervisor again if I had the opportunity to do so. A special thanks goes to Alessandro Saia, from which I have learnt a lot and to whom I will always be extremely indebted.

I am thankful to my thesis committee Rafael Lalive and Giovanni Prarolo for their brilliant comments and suggestions that allowed me to improve my thesis.

I wish to thank the faculty, postdoctoral fellows and my PhD colleagues that have made this whole experience much more rewarding. A heartful thanks goes to the Microeconomics team: Bettina Klaus for being an incredible example of excellent teaching, coordination and kindness. My colleagues and friends Annabelle, for always being there whenever I needed it and Wenxia, for cheering me up in difficult times.

I am also grateful to the CLEANers Paolo Pinotti, Gianmarco Daniele and Marco Le Moglie for their warm welcome in an excellent environment at both Bocconi University and Catholic University in Milan.

My most warm thank goes to my mum, who has always believed in me, supported and followed me wherever I went. To my sister Angela, who has hold my hand everyday since we were kids. To my sister Mari Carmen, whom I miss every single day. She was the one suggesting me to study Economics but unfortunately never saw me graduating. I would give up everything to have her here.

Finally, there are not enough words to thank my husband Gabriele for his continuous love, support and encouragement that provided me the confidence to complete the PhD. I cannot be happier for the life we are building together and for the best gift ever you gave me: our beautiful daughters Alessandra and Martina. This thesis is for you from the bottom of my heart.

Abstract

This thesis contributes to the comprehension of how certain strategies carried out by those in power- such as fiscal policies or the implementation of public spending- usually have side effects, not a priori contemplated in the approval of those policies and finally affect individuals' voting decisions or their propensity to get involved in riots.

Chapter one analyzes the context of the military dictatorship in Spain, when dictator Franco ordered the construction of different types of infrastructure, especially reservoirs and dams. That served the far-right public to defend the figure of Franco over the years. This chapter finds that places with dams built by the regime exhibit a lower political support for right-wing parties during bad times, i.e. when municipalities are hit by unemployment shocks. We believe that, even though it has been emphasized the positive effects of those constructions, the expected positive impact on political parties has vanished due to the use of forced labor to build them. Interestingly, we demonstrate that the effects vanish when we examine the support for left-wing political parties or dams built after the Franco era, so our results are only due to constructions made by the autocratic regime.

Chapter two, coauthored with Dominic Rohner and Alessandro Saia, investigates the effect of fiscal duties on revolts. We know that taxation may trigger social unrest, as highlighted by historical examples. At the same time, tax income could boost state capacity which may, in turn, foster political stability. Understanding the a priori ambiguous taxation-turmoil nexus is particularly relevant for low-income countries today – yet causal evidence on the topic is very scarce. Using a regression discontinuity design, we exploit a unique policy experiment in 19th century Sicily to identify the effect of taxation on social unrest. It turns out that it is mostly the threat of taxation that may distort economic investment and ultimately result in greater political turmoil.

Chapter three, coauthored with Dominic Rohner and Alessandro Saia, analyzes how different dimensions of democracy might impact the propensity to engage on violent conflict or protests. Democracy is composed of a variety of institutional rules, some of which may be key to promote peace and others neutral at best. Hence, it is of foremost salience to understand what types of democratic arrangements promote peace in contexts of political tensions. In this chapter we

start off from a simple game-theoretic model of proportional versus majoritarian representation and then test the key implications of the model, exploiting a unique natural experiment in Cameroon. We find that moving to proportional representation dampens the risk of "violence" (armed conflict) and promotes instead "voice" (peaceful protests).

Contents

List of Figures

List of Tables

Introduction

Individual choices are frequently driven by political decisions taken by the government and ruling parties as they might affect, among other things, income inequality, the quality of public goods provision or simply because they are sometimes perceived as arbitrary choices. For instance, our propensity to engage in political dissidence is surely driven by political resolutions. A recent example is the Chilean protests carried out mainly by students after the government decision of increasing the price of public transport in 2019. Another example of that same year is the Hong Kong protests motivated by the amendment bill that would authorize the extradition of criminal suspects to mainland China.

In this thesis, I examine different scenarios in which people's decisions- such as electoral voting or taking part in riots and/or protests- are altered by policy changes. The former is explored in the first chapter of the thesis: I examine how public spending can interfere with voting outcomes in the long run. More precisely, I analyze the construction of dams during the dictatorship that Spain lived right after the Civil War. During these 36 years (1939-1975), the regime of the dictator Franco centered the attention of public works on dams. They were meant to be a lifesaver of a country that was desolated. As it is well known in the pork-barrel spending literature, these policy interventions also serve as a political advantage for the administration that applied them, which see their effort compensated in the ballot box later on. However, I demonstrate that this is not always the case, but it depends on how local public spending is applied. If the regime implements brutal methods in its construction plans, that will backfire future support for that administration. I examine a panel of more than 7,000 Spanish municipalities using novel data on the universe of dams throughout the Spanish territory and a proxy for forced works, given that they were essential for the construction of public works during the regime. I then analyze whether during an unemployment shock, towns that have Francoist traces (dams) see their right-wing vote share decreased. I confirm this hypothesis by examining the vote share from political parties that currently hold a substantial vote share in the Spanish Parliament and were founded by Francoist leaders. Importantly, I show that these effects are only driven by dams built during that Franco period of the recent history of Spain and not after, and that the voting for different left-wing political parties is not affected by the construction of dams. The correlation shown between dams and the proxy for forced labor reinforces the role that

victimization plays on the future penalization suffered by political parties that are somehow linked to that period in history.

Other political decisions, like fiscal policies, might also influence individuals' propensity to participate in revolts when their economic conditions have deteriorated. One example is seen in recent times in Colombia, where a tax reform was followed by protests on the streets, as they were perceived as a threat to low and middle-income classes. The government was forced to withdraw the reform after the revolts turned deadly. However, examining a causal relationship between fiscal duties and riots is an arduous task, as finding an exogenous change in taxation is extremely complicated. This is what I explore in the second chapter of my thesis, coauthored with Dominic Rohner and Alessandro Saia. We analyze an exogenous increase in taxation, applied by the Bourbon administration in the Kingdom of the Two Sicilies in 1810. Given that fiscal duties were only in effect in the Sicilian towns with more than 2,000 inhabitants, we are able to analyze the effect of such increase on the rise of riots using a Regression Discontinuity Design strategy. Indeed, we are capable of comparing towns in the neighborhood of the aforementioned population cutoff. We find that municipalities that were exempted from fiscal duties were revolting more against the Bourbon administration in 1860 (when the Kingdom of the Two Sicilies was overthrown, followed by the Unification of Italy). We also analyze, as a robustness check, the Sicilian Revolution of 1848. We then explore potential channels of transmission through which this property tax might have affected the propensity of taking part on uprisings. First, we analyze some proxies for state capacity and public spending. We do not find an effect of tax exposure on any of these state capacity proxies, which agrees with historians' claims about the ineffectiveness of the Bourbon regime. Second, we investigate whether the exempted towns might have held back in their population numbers to avoid costly taxation. We find evidence for this, showing that more houses remained empty in the exempted municipalities while the taxed counterparts attracted new arrivers in order to boost agricultural production, partly compensating for the increase in taxes.

In other instances, groups are moved in their decisions followed by changes in the electoral system. In particular, when such modifications provoke arbitrary and different political representation of the population in the legislature. In the last chapter of my thesis, I investigate, together with Dominic Rohner and Alessandro Saia, this phenomenon. First, we develop a simple game-theoretic model of conflict and protest. Second, we use empirical evidence from Cameroon to demonstrate the main findings of the model. The electoral system in Cameroon provides the perfect scenario to test the main hypothesis, as it contains both elements of proportional representation (PR) and majoritarianism. Notably, in multi-member constituencies, the list that obtains a majority of votes wins all the seats allocated to that constituency in the National Assembly. We find that proportional representation lowers the probability of violent conflict while results in higher levels of peaceful protests.

Chapter 1

When Pork-Barrel Fails to Work: Evidence from the construction of dams and the use of Forced Labor

1.1 Introduction

Local public spending, such as infrastructure construction, can interfere on the citizens' voting decisions. During democratic periods, public spending tends to benefit the incumbent politician, the so-called pork-barrel phenomenon.[1] During autocratic times, public spending might target areas so as to increase the popularity of the regime and/or consolidate its power (see Carillo (2022) and Voigtländer and Voth (nd)). However, especially in the latter case, the political effect of public spending is an open question where countervailing forces coexist. On the one hand, the construction of certain infrastructures tends to increase the income of those who live nearby. Thus, one would expect citizens to reciprocate by showing a greater support for the administration that built them. On the other hand, how public spending is implemented and the methods used by the regime matter, which is what we analyze in the present paper. We believe this is a crucial question as voters consider many angles when it comes to political accountability. No doubt one being the present situation of the local economy, but also past aspects, especially when they are often brought up in the public debate. For instance, Oto-Peralías (2015) and Tur-Prats and Valencia Caicedo (2020) argue that the Spanish Civil War affects political attitudes and collective memory in the present day. Ochsner and Roesel (2017) show that political campaigns that make explicit references to past events impact the vote share of political parties in areas affected by those past episodes. Rozenas and Vlasenko (2022) also

[1] Grossman and Helpman (2005) define pork-barrel spending as projects that are financed by broad-based taxation but provide benefits that are geographically limited in scope.

show that historical symbols can shape current political attitudes when they are brought up to the debate and removed, using empirical evidence from Ukraine.

This paper examines the political effects during current bad times of infrastructure built throughout the time of the Francoist regime in Spain (in particular, dams).[2] It is important to note that the method used to construct during the regime was brutal and a current increase in unemployment may reactivate latent drama from the events that even if occurred at least 50 years ago, were traumatizing for the Spanish population. We believe that concealed tensions become more manifest when citizens experience a critical situation. Contrary to the expectations in the pork-barrel literature, we find that the support for right-wing political parties is weakened in municipalities that have Francoist traces when they are currently hit by unemployment shocks.[3] Once we control for the use of forced labor, proxied by the number of victims in the dictatorship, we find that the results barely change. However, this control is never significant. We attribute this aspect to the fact that it is not probably a good proxy for the use of forced labor. Importantly, the main effects found in the paper dissipate when analyzing the support for left-wing political parties and dams built after the end of the dictatorship so we can assure that the impact is only due to dams built by the oppressive regime that lasted nearly 40 years. We presume that there are different causes behind these results. First, the role of propaganda, the numerous street names or symbols referring to Francoist leaders that, honorary nameplates that show gratitude for the construction plans, as well as the political debates that are often brought up to eliminate the former might contribute to shape political support.[4] They are, however, difficult to measure and, in many cases, suffer from endogeneity concerns. We analyze possible channels of transmission through which dams might have affected political support during difficult times. For instance, due to the utilization of forced labor for the construction of the infrastructures by the regime, we analyze whether the use of political prisoners might be the cause of the weakening of the right-wing support. For that, we use a proxy for political prisoners, using historical data from different National archives as access to archival data on convicts from that period is quite limited. We demonstrate that there is a positive correlation between our proxy for forced works and the construction of dams. That is why we believe that the construction of dams resulted in a drama for the population that translated in a punishment

[2]Note that, contrary to the study of Duflo and Pande (2007), we do not analyze the impact of upstream dams on the vote share for the cities located downstream from them. On the one hand, since dams are highly concentrated in certain areas, analyzing the effects of upsteam dams might confound the effects of infrastructures built at different points in time. On the other hand, this paper focuses on the idea that the construction of such infrastructures created a long-lasting drama to the citizens due to the methods applied by the Francoist regime.

[3]Water spending are typically thought as the worst type of political pork barrel (see Del Rossi (1995), page 299) as are distributive policies that focus on a specific area while being paid by the country as a whole through taxes. This thesis is also reinforced by the fact that the regime put emphasis on advertising public works carried out by its administration (for instance through propagande, documentaries or nameplates in each dam).

[4]The Historical Memory Law approved during the socialist legislature in 2007 banned street names, monuments, symbols, emblems and mentions to the Francoist regime. Villamil and Balcells (2021) show that this law caused an increase of the support for a new far-right political party.

for the political parties that are somehow related to that era of the Spanish history. We also analyze heterogeneous effects of the labor market. In particular, whether some economic sectors or gender differences might be behind the main results of the paper.

One is aware of possible concerns which must be taken care of, in the sense that pro-Francoist municipalities might be the ones receiving greater allocation of resources, or the opposite, i.e. that pro-republican areas were attracting more attention by the regime with the idea of reverting the situation or simply to have a higher availability of cheap labor force. The latter conjecture can be understood as the dictator enacted different laws that "legalized" the use of forced labor by political prisoners in his construction plans, as we will explain in detail below. However, extensive evidence tells us that many groups of engineers and scientists actively participated in the Francoist plans by carefully selecting where to construct dams and reservoirs according to specifics of the territory, such as the gradient, geological composition and its impermeability (see Radio Televisión Española (1952)). Indeed, in the words of Camprubí (2017), the engineers of Franco were considered as active participants of the configuration of the regime (page 21). According to these testimonies, it seems that we can rule out the fact that the Francoist regime chose strategically the places to construct the infrastructures either because these areas already supported the regime but wanted to gain more support, or the contrary. Nonetheless, these endogeneity issues will be taking into account by employing an Instrumental Variable strategy in the spirit of Duflo and Pande (2007).

Our unit of analysis is a Spanish municipality. Throughout all the specifications, we control for differences between rural and urban areas.[5] As a way of avoiding confounding the results with the construction of other dams before the surge of the Francoist dictatorship, we also control for the number of dams built before this period. Lastly, we also control for some relevant time-invariant geographical characteristics. Our identification strategy relies on within-province differences in unemployment shock and dams' construction during the Franco era across municipalities in a given province. Among a bunch of robustness checks, we show that places where Franco built dams did not exhibit a specific support for any political party ex-ante. For this, we use a novel database on historical elections celebrated right before the Civil War and the dictatorial period for a small sample of Spanish municipalities.[6]

The task of reconstructing a country devastated by the Civil War (1936-39) has been compared to the New Deal spending of the American President Roosevelt, who carried out large investments to activate the American economy after the 1929 crisis (Franco (2018)). In the words of Pearce (page 96) "under the Fascist leadership of General Franco, Spain built more dams than any

[5]For that, we define an indicator variable equal to 1 if a municipality has more than 50,000 inhabitants. Following of Transports Mobility Urban Agenda (2021), big urban areas must have at least 50,000 inhabitants.

[6]We only have access to historical electoral data for 5 out of the 47 Spanish provinces that are part of this study (Huesca, Navarra, Álava, Gipuzkoa, Bizkaia), accounting for more than 8% of all the municipalities in the sample.

other nation of comparable size on Earth". Dams constructed by the Francoist regime were highly publicized among the society thanks to the use of propaganda (Brendel (2020), page 397, Radio Televisión Española (1952), Radio Televisión Española (1954) and Radio Televisión Española (1970)).

Our paper is related to different strands of literature. First, on the effects of local public spending on political support (see North (1990) and Dixit (1996) for an analysis of transaction-cost politics, where politicians promise to comply for a given policy in exchange of votes, and Grossman and Helpman (2001)). These effects have been mostly analyzed in democratic periods (see for instance Limosani and Navarra (2001), Cadot et al. (2006), Roberson (2008), Curto-Grau et al. (2012) and Maskin and Tirole (2019)), but also recently examined in non-democratic regimes as well (see for instance the study of the Italian case of Mussolini's New Towns (Carillo (2022)) or the motorway network construction in Nazi Germany (Voigtländer and Voth (nd))).

Second, a growing literature examines past factors that alter the political effects of crises. For instance, Sanz et al. (2021) analyze how corruption intensifies the impact that local unemployment shocks have on political fragmentation. They find that past corruption scandals tend to punish traditional parties during crises, for a sample of around 3,000 municipalities of Spain. Caprettini and Voth (2022) look at the impact of the New Deal on patriotism during the World War II and find that individuals tend to reciprocate towards the state if it has previously helped in difficult times. Fouka and Voth (2022) analyze how the Greek debt crisis affected the vote share for anti-German parties, especially in areas more affected by German reprisals.

Finally, several papers investigate the impact of dams and other infrastructures. Duflo and Pande (2007) evaluate the distributional effects of large dams in India. They find that agricultural production tends to increase in districts located downstream from the dam, at the same time that rural poverty decreases in those same areas. Instead, the impact of dams on agricultural production is insignificant, while rural poverty increases in places where dams are located. They propose an instrumental variable strategy based on the nonmonotonic relationship between river gradient and dam construction. Strobl and Strobl (2011) also analyze the distributional effects of large dams in Africa at drainage basin level. They find that cropland productivity raises in regions located downstream from the dam, while vicinity areas do not show advantageous effects. Eberle (2020) analyzes the impact of dams on conflict, finding that the construction of such infrastructures augments the incidence of intrastate conflict.

The remainder of the paper is organized as follows. Next section provides the historical background of the Spanish dictatorial regime and politics. Section 3.3 describes the data sources that form the dataset used in the present study, as well as the descriptive statistics. Section 3.5 explains the identification strategy used in the paper. Section 3.6 presents the baseline results, robustness checks and channels of transmission. Finally, section 3.7 concludes.

1.2 Historical background

After the resignation of dictator Primo de Rivera in 1930, Spain celebrated local elections, leading to the proclamation of the Republic. At the National elections of 1933 the right-wing parties won, followed by a victory of an alliance of left-wing parties (the Popular Front) in 1936. However, before the second round was celebrated, the person in charge of managing the elections resigns and a government was immediately formed. The atmosphere was very tense on the streets and after the murder of some politicians by opponent groups, a military uprising occurred in Morocco, which translated later on to the Spanish peninsula. The failure of the coup d'etat gave rise to the Civil War.[7]

The rebel bloc of Franco counted with the support of Nazi Germany and Fascist Italy, while the Republican side had the support of the Soviet Union and around 40,000 volunteers recruited by the Communist International from more than 50 countries, forming the International Brigades (García de Cortázar and Ruiz de Aguirre (2002), page 27 and Sánchez Sánchez and Selva Iniesta, page 17). Franco declared the victory of the conflict by the National band on April 1^{st} of 1939 and imposed a military dictatorship (García de Cortázar and Ruiz de Aguirre (2002), page 21). Around 500,000 people died in the Civil War and another half million of compatriots migrated, mainly to France and Latin America (Castro Oury (1993), page 48). Among them, around 115,000 bodies still remain unidentified, making Spain one of the countries with the highest number of unrecognized bodies.[8]

The dictator Francisco Franco ruled as Head of State from October 1936 until his death and as a President of the government from 1938 until 1973.[9] During the dictatorship, autarky was the economic model introduced and the priority was to reactivate the economy and the reconstruction of the country, that was devastated after the Civil War. Public works played an important role on this: railways, hydrological works, roads, airports and, to a lesser extent, ports.

Franco's Hydrological Plans stood out among all the infrastructure plans. Fernández de la Mora (1992), Minister of Public Works during the last years of the dictatorship (from 1970 to 1974), pointed out the necessity of hydrological infrastructures due to the *hyper-drought* suffered in Spain. In his own words: "the duty of governments is to afront [the drought] with caution and to overtake the needs. This is what has been done during the Franco era and not after". It is well known that during the Franco era, the country saw their number of dams raised.[10] Even though the first dams built in Spain date from the Roman era and it is the world-leading country with

[7]The information contained in this paragraph comes from Congreso de los Diputados (2022).

[8]Juzgado de Instrucción Penal n. 5 de la Audiencia Nacional, November 2008.

[9]See Spanish Official Bulletin *(Boletín Oficial del Estado)* (September 30, 1936)

[10]In the words of Fernández de la Mora (1992): "In 1940 the capacity of dam reached a volume of 4,133 hm^3; in 1972 it reached 38,819 hm^3, so it almost increase tenfold in 32 years."

more dams per capita (Gil Delgado), dams built during the Franco period have received more attention by the general public, mostly due to the effort of the propaganda of the regime.[11] In absolute numbers, Spain is the fifth country in the world, only after China, United States, India and Japan, with the highest number of large dams (Parasuraman and Sengupta (2001)). However, the construction of such large infrastructures came at a cost of forced displacements that were particularly easy to accomplish in a context were press freedom and rights in general were strictly controlled by the regime.[12]

Fundamental for the construction of public works during the dictatorship was the labor force provided by prisoners. The prison system after the Spanish Civil War was distinguished from the previous ones by the Redemption System of Penalties for labor approved during the Francoist dictatorship. The *Order June 1, 1937* provided the right of work out of prison for war prisoners that had not received a sentence. The regime justified this order by the need to reduce the number of prisoners, that did not cease to increase due to *"the victorious and continuous advance of the national forces in the conquest of the patriotic territory"*.[13]

The death of the dictator in 1975 marked a milestone and as the Fundamental Laws of the Francoist regime stated, the now emeritus King Juan Carlos I would be his successor as Head of the State. The next political system was an authoritarian monarchy with non-democratic political institutions. The first government pretended a continuation of the previous regime, which failed to work only few months after.[14] The first general elections were celebrated in 1977. The political transition was then formalized with the approval of the Constitution of 1978. Spain has seen 14 legislatures, two presided by a center-right political party, eight with socialist presidents and five with right-wing presidents from the Popular Party. Of particular importance is the latter party, that has its origins in the party People's Alliance, founded by Francoist leaders.[15] It was initially created as a federation of parties and then converted into

[11] See for example the Roman origins of the Proserpina dam in Badajoz, https://sig.mapama.gob.es/WebServices/clientews/snczi/default.aspx?origen=8&nombre=EGISPE_PRESA&claves=ID_INFRAESTRUCTURA&valores=987

[12] There does not exist a comprehensive list of municipalities that total or partially suffered forced displacements due to the construction of dams. In certain cases (such as in Peñarrubia in Málaga), entire municipalities ceased to exist and were annexed to other cities by even bringing stones from historical buildings to the new location. Those were in general small cities and have occurred not only during the Franco era but also in other periods.

[13] There were successive modifications to this Order in the following years. First, the *Order October 11, 1938* gave the possibility of redemption of (part of) the prison sentence for war prisoners. It was in force from January 1^{st} 1939. Prisoners could reduce their sentence by one day for each day worked. Second, the *Law July 19 1944* extended the application of the previous law to all prisoners, i.e. war and common prisoners. Prisoners could redeem their sentence by one day for every 2 days worked. Prisoners received as salary of 2 *pesetas* per day on average and also private firms could benefit from the cheap workforce. Working and living conditions of prisoners were deplorable and extreme. Apart from citizens incarcerated in prisons, there were around half a million people in concentration camps that were not judged or sentenced and around another 150,000 in battalions. There were 294 concentration camps in Spain, created by Franco only 2 days after the military uprising started. Hernández de Miguel (2019).

[14] This paragraph has been elaborated following del Campo and Félix Tezanos (2008), chapter 5.

[15] In the words of Penella: "AP [People's Alliance] was born as a coalition with electoral objectives. An alliance

a party by the Francoist Minister Manuel Fraga (and president of the party), among others, in 1977.[16] It was transformed to Popular Party in 1989 with Manuel Fraga as its president. After the approval of the so-called Historical Memory Law by the Socialist President Zapatero in 2007, that was meant to recognize and increase the rights of those who suffered during the Civil War and the dictatorship, debates about the dictator and his legacy have been frequent among both the political sphere and a broad part of the population.[17]

1.3 Data

In order to analyze the effect of dams' construction on political outcomes, we construct a municipality-year level dataset using different sources that we describe in detail below. The sample includes most of the peninsular municipalities from Spain. After deducting the islands and 2 autonomous cities, Ceuta and Melilla, the resulting panel data sample contains around 7,300 municipalities with its 47 provinces.

1.3.1 Dams

Information about dams constructed in Spain comes from the inventory of dams and reservoirs created by the Spanish Ministry for the Ecological Transition and the Demographic Challenge. When the ending date of construction was not available for a dam, we use data from the Spanish Society of dams and reservoirs (SEPREM in its Spanish acronym). We have information on the exact location, date of construction, property (public or private) and some other specific characteristics, from 1900 until now. Figure 1.1 depicts the cummulative number of dams in Spain during that period. Overall, there are 1,453 dams in the country but only 1,170 of them count with information on the completion year (i.e. 283 dams have missing data of construction). Within the municipalities that form our sample we have 1,389 dams (261 with missing date). Around 60% of the dams in our sample (853 dams) are used for irrigation, while 386 are used for hydropower solely.

What is relevant for our analysis is that approximately 500 dams were built during the dictatorship (i.e. 34% out of the total dams that exist in the country) and 295 of them are state-owned (more than 60% of the dams built during the dictatorship period). Map in Figure 1.2 depicts the distribution of dams constructed during the Franco era *(red dots)*, as well as the fraction of each

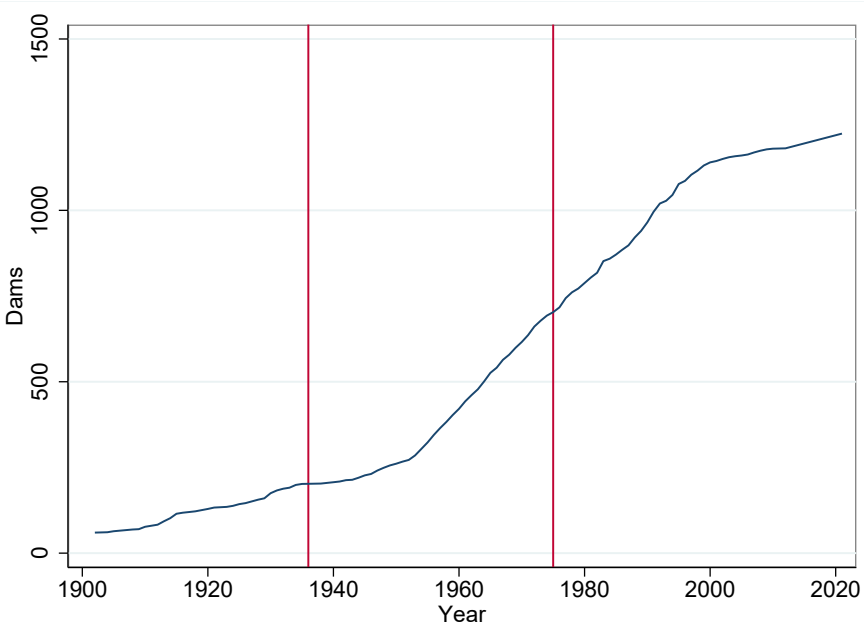
among representatives of the different families of the regime [...]"

[16]"AP [People's Alliance], that was transformed into the current Popular Party in 1989, was inscribed in October 9 1976 as a federation of seven organizations and, among all, of seven ex ministers of Francisco Franco, dead one year before (Marcos (2006)).

[17]In many cities, right-wing mayors have infringed that law by for example, keeping Francoists' street names. In addition to the Historical Memory Law, the Socialist Party is processing the Democratic Memory Law by which, among other things, will illegalize the Francisco Franco Foundation, which is said to incite Francoist values.

municipality area with a suitable river gradient *(in blue)*.[18] Panel A in Table 1.1 reports that around 12% of the Spanish municipalities in the sample contains at least one dam. This number reduces when we only consider the constructions built during the Francoist era, approximately 5% (see Panel B in Table 1.1).

Figure 1.1: Total dams built in Spain



NOTES: This figure shows the stock of dams built in Spain from 1900 to 2020. Vertical red lines indicate the dictatorial period 1936-1975.

1.3.2 Electoral Data

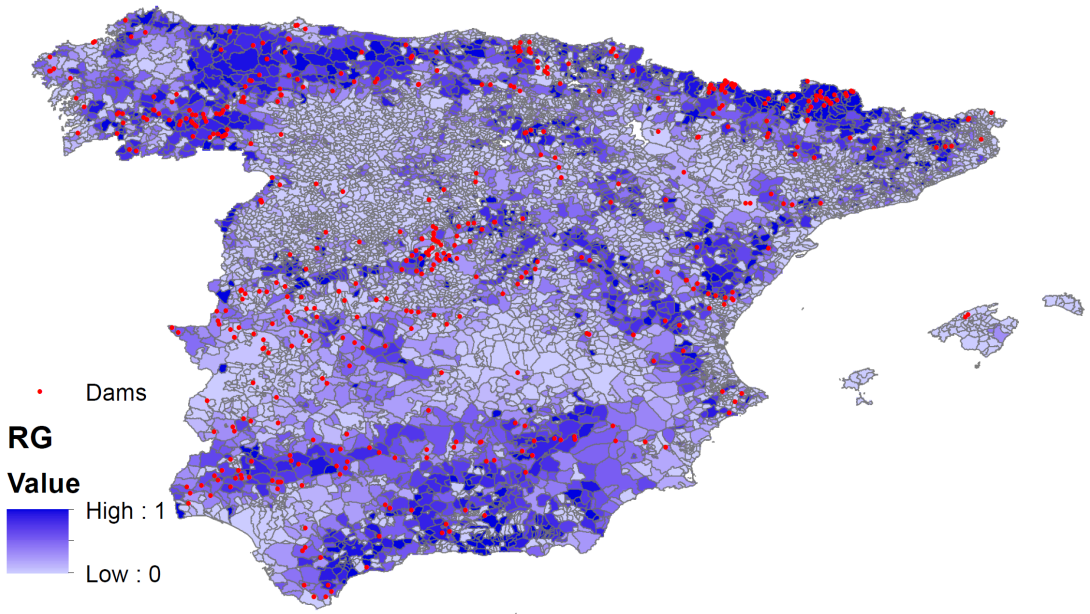
To construct our dependent variable of interest we use data from the Spanish Ministry of Interior. National Congress elections have been celebrated fourteen times after the end of the Francoist dictatorship. Due to data limitations on local unemployment, the elections to National Congress that we analyze were celebrated in the years 2008, 2011, 2015, 2016 and 2019.[19] The dependent variable vote share for right-wing parties sums up the number of votes the Popular Party and the fascist party *Falange Española de las Juntas de Ofensiva Nacional Sindicalista* (FE de las Jons) get at the electoral processes celebrated during our period of interest over the number of voters.[20] For the falsification exercises, we construct the measure of left-wing vote share as the

[18] Appendix Map 1.F1 shows the corresponding map with all the dams built in Spain from 1900 until now *(black dots)* and the fraction of each municipality area with a suitable river gradient *(in blue)*.

[19] From the beginning of democracy, national elections were held in 1977, 1979, 1982, 1986, 1989, 1993, 1996, 2000, 2004, 2008, 2011, 2015, 2016 and 2019 while local elections were held in 1987, 1991, 1995, 1999, 2003, 2007, 2011, 2015 and 2019.

[20] After some failed attempts, Francisco Franco managed to unify the political movements Falange Española and Carlism (García de Cortázar and Ruiz de Aguirre (2002), page 23). The Spanish Bulletin of April 20 1937 declared the creation of a unique political entity called *Falange Española Tradicionalista de las JONS*. (Boletín Oficial del Estado, April 20^{th}1937 https://www.boe.es/gazeta/dias/1937/04/20/pdfs/BOE-1937-182.pdf)

Figure 1.2: River gradient suitability and dams built during the Francoist regime



NOTES: This map shows the location of dams constructed during the Franco era *(red dots)* and the fraction of each municipality area with a suitable river gradient *(in blue), as defined in Section 3.5.*

ratio of the votes received by left-wing political parties and the turnout. In our left-wing vote share count we include the Socialist Party (*PSOE* in the Spanish acronym), the Communist Party, *Izquierda Unida*, Podemos and Unidas Podemos.[21]

1.3.3 Unemployment

Monthly data on unemployed people at municipality level comes from the Spanish Ministry of Labor and Social Economy. This data is available from 2005 to 2021. Data on the working age population (from 16 to 64 years old) for each city comes from the Spanish National Statistics Institute. The unemployment rate for each municipality is then computed by first averaging the number of unemployed people over the 12 months of the year and second by taking the ratio between the unemployed people over the working age population.[22] We compute logarithmic unemployment growth rate of every year with respect to the previous election year.

1.3.4 Forced Labor

As outlined in Section 3.2, most of the people that were forced to work in the construction of the hydrological plans were political prisoners that were opposed to the regime. Brendel

[21]Note that the political party Podemos was founded in 2015 after the inspiration of the 15*M* movement in 2011 and run for the first elections only in 2015. Unidas Podemos is an electoral coalition founded in 2016 and formed by Podemos and Izquierda Unida, among other political parties.

[22]This ratio is smaller than the official unemployment rate as *working age population* includes (as the labor force) the unemployed people, working people but also -and as opposed to the labor force measure- people who do not actively look for a job.

(2020) (page 398) points out that written records were strictly controlled during the dictatorship era and victims were outright silenced. That makes the task of compiling lists of political prisoners extremely difficult. To overcome this issue, we use a proxy for forced labor, which is the number of victims of the Civil War and reprisals of Franco. This data comes from the Ministry of Culture and Sports: Historical Memory- Spanish Archives Portal (*PARES* in the Spanish acronym). A person is considered a victim if it complies with the following definition: "people who suffered personal violence, repression, purging, injustice and affront for political, ideological, religious motives or other reasons". We have information about the location of each victim (there are around 1 million of victims but only half of them with the location where they come from). We construct our proxy for forced labor by summing up all the victims of the dictatorship in each municipality and creating a dummy variable for the existence of at least one victim in each town.[23]

1.3.5 Other variables

Geographical data such as river length (in kilometers), municipality area (in square kilometers), average elevation (in meters) and average gradient are calculated from the Digital Elevation Model GTOPO30 produced by the US Geological Survey. [24] This file contains grids of 30 arc seconds (approximately 1 kilometer). See Appendix Table 1.A2 for descriptive statistics.

Table 1.1: Summary Statistics

PANEL A: All dams					
	No dam		Dam		Mean-Diff
All dams	0.00	(0.00)	1.66	(1.47)	-1.66***
Vote share for right-wing parties	0.38	(0.18)	0.36	(0.13)	0.02***
Vote share for left-wing parties	0.36	(0.15)	0.44	(0.13)	-0.08***
N	6,459		836		7,295
PANEL B: Dams built during dictatorship					
	No dam		Dam		Mean-Diff
All dams	0.00	(0.00)	1.42	(1.03)	-1.42***
Public dams	0.00	(0.00)	0.87	(0.94)	-0.87***
Vote share for right-wing parties	0.38	(0.18)	0.35	(0.14)	0.03***
Vote share for left-wing parties	0.37	(0.15)	0.43	(0.14)	-0.07***
N	6,955		340		7,295

NOTES: The sample consists of 7,295 Spanish municipalities. Panel A differentiates between municipalities with no dams (6,459 observations) and those with a dam built at any point in time (836 obs.). Panel B distinguishes between towns that do not have a dam built during the Franco era (6,955 obs.) and those that do have at least one (340 obs.). Standard deviations shown in parentheses.

[23] Note that we exclude teachers that were purged by the regime in our victims count as the Francoism regime punished all the teachers, independently of their political beliefs (from private or public schools, and even religious schools were affected). Moreover, most of the teachers were women, and only men were forced to work in the Francoist constructions. Those in power justified it on the fact that all the instructors received university education after the Constitution of the Republic, based on a public, free, laic, mixed and compulsory education (around 70% of the municipalities of our sample have at least one of such teachers).

[24] Available at https://www.usgs.gov/centers/eros/science/usgs-eros-archive-digital-elevation-global-30-arc-second-elevation-gtopo30

1.4 Identification Strategy

One can argue that the placement of a dam is correlated with political outcomes. On the one hand, an incumbent politician might see her dam placement decisions affected by past electoral outcomes. You can think of a politician that tries to maintain her political power over a region, or to try to gain it when she is not far away from the majority threshold. On the other hand, a government might decide to construct a dam in a certain area right before the elections in order to influence the future outcome of the poll. As pointed out in Section 3.1, a simple OLS regression on the effect of dams on the vote share obtained by a political party does not take into consideration potential endogeneity concerns.

In order to take care of reverse causality issues, we instrument dams' construction (and its interaction with unemployment) with river gradient, as suggested by Duflo and Pande (2007). This instrument is inspired by the findings in the engineering literature, namely, that a certain river gradient favors dams construction. Duflo and Pande (2007) show for the case of India that regions with either flat ($< 1.5\%$) or steep river gradient ($3 - 6\%$) are not suitable for dams construction. Instead, regions with a moderate ($1.5 - 3\%$) or very steep river gradient ($> 6\%$) tend to facilitate the construction of dams. In the case of Spain, we show that a river gradient above 3% is positively correlated with the presence of dams in a given territory.[25] Consequently, we define river gradient suitability as the share of a municipality area with a river gradient above 3% and use it as an instrument for dams construction. To have a measure of river gradient for each municipality, we compute the fraction of each municipality area with a suitable gradient, focusing only on cells with a river. We ensure that all the regressions satisfy the exclusion restriction by controlling for municipality fixed effects, year fixed effects, province-year fixed effects and for different time-invariant municipality characteristics (interacted with unemployment).

Our baseline 2SLS system of equations is

$$D_{ip} \times UR_{ipt} = \phi_1 + \phi_2 RG_{ip} \times UR_{ipt} + \phi_3 Z_{ip} \times UR_{ipt} + \tau_i + \nu_t + \mu_{pt} + \omega_{ipt} \tag{1.1}$$

$$Vote\ share_{ipt} = \beta_0 UR_{ipt} + \beta_1 UR_{ipt} \times D_{ip} + Z_{ip} \times UR_{ipt}\beta_2 + \tau_i + \nu_t + \mu_{pt} + \omega_{ipt} \tag{1.2}$$

where i, p and t denotes municipality, province and time, respectively. D_{ip} is a dummy variable that indicates whether municipality i of province p has a dam built during the dictatorship

[25] According to Spanish engineers' testimonies, the higher the river gradient the greater the probability of encountering a rocky foundation, slightly deformable, resistant and the easier and the cheaper it is to design and build a durable dam. Moreover, Spain is one of the countries that counts with more dams per capita due to the scarce natural regularity of rivers. As a consequence, dams have been built also to mitigate this issue and not only following river gradient characteristics.

period. UR_{ipt} denotes a dummy variable that takes value of 1 if municipality i of province p has an unemployment growth rate above the median at time t. RG_{ip} denotes the share of area with suitable river gradient in municipality i of province p, i.e. with a river gradient above 3%. *Vote share*$_{ipt}$ is the vote share obtained by the right-wing political parties in municipality i of province p in the elections celebrated at time t, as described in Section 3.3.

We control all the specifications by geographical (time-invariant) characteristics that might affect dam placement, denoted by the vector Z_{ip}, interacted with unemployment UR_{ipt}. They are river length (in kilometers), municipality area (in square kilometers), average elevation (in meters), average gradient, indicator variables for the presence of a dam built in municipality i before the onset of the Francoist dictatorship and a dummy variable that indicates whether municipality i is urban, i.e. if population is above 50,000. Both equations control for municipality fixed effects τ_i that account for any unobserved heterogeneity between the different units, province-year fixed effects that controls for trends that are province specific (μ_{pt}) and year fixed effects (ν_t). Note that all the time-invariant variables are interacted with the unemployment growth rate variable UR_{it} as otherwise would be absorbed by the municipality fixed effects. We therefore exploit variation in the unemployment shock and dams construction given by differences in river gradient suitability across municipalities within provinces.

Equation (1.1) is the first-stage where we regress the endogenous variable dams (D_{ip}) interacted with unemployment on the excluded instrument $RG_{ip} \times UR_{ipt}$. To ensure the validity of our instrument, we proceed in two steps. First, we argue that the exclusion restriction is satisfied in our model. Given that our instrumental variable strategy makes use of an interaction, we discuss the validity of all its components. We believe it is very unlikely that river gradient (RG_{ip}) could directly affect the support for a certain political party. Nevertheless, our regressions control for municipality fixed effects that address potential omitted variable bias. The second term in the instrument (UR_{ipt}) might raise concerns about probable impacts on our dependent variable of interest, the vote share for right-wing parties. To deal with this consideration we add province-year fixed effects. It is important to note that the electoral circumscription in the general elections in Spain is a province. Therefore, in case candidates devote different efforts in the provinces for instance, the fixed effects can account for that. We also add a dummy for urban cities since we believe that urban areas usually differ from their rural counterparts in their political activism as well as in their exposure to certain economic shocks such as the one suffered by the construction sector in 2007. One could also argue that the interaction $RG_{ip} \times UR_{ipt}$ could correlate with the error term. In order to address this, we control for a set of municipality controls that do not vary over time but we do interact with the unemployment variable. Second, next section discusses the relevance of the instrument by providing the first-stage results. Lastly, equation (1.2) is the second-stage where the dependent variable is the vote share for right-wing parties in the national elections celebrated at time t as defined in section 3.3. Coefficient β_0

measures the average effect of being exposed to an unemployment shock on the vote share for right-wing parties in areas that do not have a Francoist construction. Coefficients $\beta_0 + \beta_1$ captures the effect of the unemployment shock in places with dams built during the Franco era.

1.5 Results

1.5.1 Main results

Table 1.2 provides the first-stage results. Columns 1-4 show the results for the cross-sectional analysis while Columns 5-8 show the corresponding results for the panel data inspection. Recall that in the latter specification we interact the (time invariant) river gradient variables with the unemployment growth rate. We first examine whether river gradient has any impact on the probability of having a dam (State or privately-owned) in municipality i of province p (Columns 1-2 and 5-6). Second, Columns 3-4 and 7-8 analyze the effect of river gradient on the existence of a public dam in municipality i built during the Francoist regime. Note that odd columns make use of the river gradient categories as independent variables (with the flat river gradient category omitted). The most relevant thing we retrieve from this analysis is that the steep and very steep river gradient categories correlate with the construction of dams. These two river gradient categories form the base of the river gradient suitability variable, as detailed in section 3.5. Even columns use the instrument river gradient suitability as predictor for dams construction. The baseline specification, depicted in Column 6, reports a strong positive relationship between river gradient suitability (interacted with unemployment) and the presence of dams in a municipality, after controlling for all the relevant variables mentioned in Section 3.3. The corresponding Kleibergen-Paap F statistic of the instrument is 16.8, suggesting that our regression does not suffer from weak instrument. Next section examines more in detail the robustness of the instrument.

Table 1.3 presents the baseline results for the OLS, the reduced form and the 2SLS regressions. All the regressions add municipality fixed effects, year fixed effects, as well as province-year fixed effects. Column 1 shows the OLS results for the correlation between the support for right-wing parties and the unemployment shock variable interacted with dams built by the Francoist regime. The dependent variable is the vote share for right-wing parties as defined in Section 3.3. The estimated coefficient of the latter is zero but not significant. Column 2 presents the estimated coefficients from the reduced form of the direct causal effect from the instrument to the dependent variable. It shows a negative relationship between river gradient suitability and the political support for right-wing parties, which is what we expected. Column 3 shows the 2SLS results on the relationship between unemployment shock, its interaction with dams built during the Francoist era and the right-wing vote share. The coefficient β_0 unveils that municipalities that are hit by unemployment shocks and do not have a dam built during the turbulent times

Table 1.2: First-Stage

	Cross-Section				Panel Data			
	(1) All Dams	(2) All Dams	(3) Public Dams	(4) Public Dams	(5) All Dams	(6) All Dams	(7) Public Dams	(8) Public Dams
Moderate River Gradient	0.007		0.001					
	(0.010)		(0.008)					
Steep River Gradient	0.022*		0.013					
	(0.011)		(0.008)					
Very Steep River gradient	0.067***		0.028**					
	(0.015)		(0.012)					
RG		0.044***		0.020***				
		(0.010)		(0.007)				
Moderate River Gradient × High Unemp.					0.008		0.001	
					(0.010)		(0.008)	
Steep River Gradient × High Unemp.					0.018		0.012	
					(0.011)		(0.008)	
Very Steep River Gradient × High Unemp.					0.067***		0.027**	
					(0.015)		(0.012)	
High Unemp. × RG						0.042***		0.019***
						(0.010)		(0.007)
Mun. FE	No	No	No	No	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes	No	No	No	No
Year FE	No	No	No	No	Yes	Yes	Yes	Yes
Province-Year FE	No	No	No	No	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,295	7,295	7,295	7,295	36,534	36,534	36,534	36,534
N. Clusters	47	47	47	47	47	47	47	47
F Statistic	7.9	20.7	3.3	8.8	6.7	16.8	2.8	7.6

NOTES: This table shows the first-stage estimates. The OLS estimations in Columns 1-4 show the cross-sectional results while Columns 5-8 show the panel data estimates. The dependent variable in Columns 1-2 (and Columns 5-6) is a dummy for the existence of at least one dam built during the Francoist regime (interacted with the unemployment shock variable). The dependent variable in Columns 3-4 (and Columns 7-8) is a dummy for the existence of at least one dam built during the Francoist regime that are owned by the State (interacted with the unemployment shock variable). All the specifications control for municipality area (in square kilometers), average elevation (in meters), average gradient, river length (in kilometers), a dummy variable indicating whether the municipality is urban (population is above 50,000 inhabitants) or not, and the number of dams built before the surge of the dictatorship. The time-invariant control variables enter in the panel data analysis interacted with the unemployment shock. Clustered standard errors in parentheses at province level. * significant at 10% , ** significant at 5%, *** significant at 1%.

of Franco tend to augment their vote for right-wing political parties. In particular, areas that are particularly hit by the unemployment shock (note that the unemployment variable is an above-median dummy) tend to increase their vote share by 0.013 p.p. The main coefficients of interest ($\beta_0 + \beta_1$) manifest that having Francoist traces negatively affects the previous result. In other words, places that have a Francoist trace see their right-wing vote share reduced by 0.145 p.p. $(0.013 - 0.158)$ when they suffer unemployment shocks in the present. At the sample mean, having a Francoist construction decreases the vote share for the right-wing political parties from 0.38 to 0.24 or equivalently, a reduction of 1.2%. This coefficient is of larger magnitude than the reduced form one, due to a larger coefficient of the actual treatment or first stage estimation that shows a strong a positive correlation between river gradient suitability and the existence of a dam built in the dictatorship.

1.5.2 Robustness Checks

This section demonstrates that the baseline results are robust to different specifications. First, we examine the robustness of our findings to the inclusion of an additional instrument that, according to the literature, might be relevant to dams construction. In particular, Díez-Cascón and Bueno Fernández (2003) (page 15) emphasize that, in addition to river gradient and other

Table 1.3: Baseline results

	Dep. Variable: Right-Wing Vote Share		
	(1) OLS	(2) R. Form	(3) 2SLS
High Unempl.	0.014***	0.014***	0.013**
	(0.004)	(0.004)	(0.005)
High Unempl. × Dams	-0.000		-0.158***
	(0.002)		(0.056)
High Unempl. × RG		-0.007***	
		(0.002)	
Mun. FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Province-Year FE	Yes	Yes	Yes
Observations	36,534	36,534	36,534
N. Clusters	47	47	47
F Statistic			16.3
Mean of Dep. Variable	0.38	0.38	0.38
Controls	Yes	Yes	Yes

NOTES: Column 1 shows the OLS results on the effect of the unemployment shock and its interaction with the presence of at least one dam on the vote share for right-wing political parties in the national elections celebrated in the years 2008, 2011, 2015, 2016 and 2019. Column 2 shows the reduced form. Column 3 shows the 2SLS results on the effect of the unemployment shock, interacted with dams built in the municipality on the vote share. The instrument variable is the river gradient suitability for each municipality. All the specifications control for municipality area (in square kilometers), average elevation (in meters), average gradient, river length (in kilometers), a dummy variable indicating whether the municipality is urban (population is above 50,000 inhabitants) or not, and the number of dams built before the surge of the dictatorship. Clustered standard errors at province level in parentheses. * significant at 10% , ** significant at 5%, *** significant at 1%.

topographic characteristics of a territory, the climatic aspects of the country are one of the factors that influence the occupation of the territory and the construction of dams. That is why we also include average rainfall from 1921-1930 as instrument, as it is considered a factor that impacts dams' construction. Columns 1-2 in Appendix Table 1.B1 displays the cross-sectional results, while Columns 3-4 the corresponding results for the panel data analysis. In both cases, we do not find that rainfall predicts dams construction while river gradient suitability continues to be positive and significant in all cases. Appendix Table 1.B2 shows the second-stage coefficients of this alternative specification.[26] The results continue to be significant although the magnitudes slightly change with respect to the baseline regression. Given that the estimated coefficient of rainfall is zero in the first-stage and the Kleibergen-Paap F-Statistic is smaller than in our baseline specification (12.4 versus 16.3), we relegate this alternative strategy to the robustness checks section.

Second, in order to discard the idea that the Francoist regime strategically chose the areas

[26]Note that the number of observations in Appendix Table 1.B2 is reduced with respect to the baseline analysis without rainfall. This is due to the lack of data on rainfall for certain municipalities.

where dams would be built following political reasons, we analyze national elections celebrated right before the Spanish Civil War, that preceded the onset of the Francoist rule. We only have access to historical elections data for 5 out of the 47 Spanish provinces that are part of this study (Huesca, Navarra, Álava, Gipuzkoa, Bizkaia), accounting for more than 8% of all the municipalities in the sample. This data, that comes from the archives of each of these mentioned provinces, was manually digitized. Appendix Table 1.C1 shows the corresponding results using this cross-section data of historical elections of 1936. The dependent variable in Column 1 analyzes the right-wing political parties that run for the national elections in that year.[27] The dependent variable in Column 2 is the vote share for left-wing oriented political parties.[28] Lastly, the dependent variable in the last column is the vote share for the republican political parties. As we can see, the estimated coefficient on the correlation between dams and vote share show that these municipalities do not exhibit a particular support for any political party ex-ante.

Third, even though the fiscal capacity of the Spanish regions is very limited (almost 2% of all the revenues), around 80% of the regional tax collection comes from water-related objects (see Lago-Peñas (2019), page 2). In Appendix Table 1.C2 we add regional-year fixed effects in order to examine if there could be significant differences over time among the Spanish regions that might alter the results. We can see that the estimated coefficients continue to be significant throughout all specifications while the magnitude of the coefficients remain virtually unchanged.

Fourth, we analyze the robustness of our results when excluding the Spanish regions that experienced a special treatment by the regime or the central administration. On the one hand, Franco abrogated the Catalonian Statute (see the Spanish Official Bulletin April 8^{th} 1938, page 6,674) and prohibited the use of the Catalonian language in the public services (see the Provincial Official Bulletin from July 31^{st} de 1940). Columns 1-3 in Appendix Table 1.C3 replicates the baseline estimates when omitting the four provinces of Catalunya. On the other hand, considering that two Spanish regions have a fiscal model in which the central administration does not have any significant fiscal power, we exclude Navarra and the Basque Country in Columns 4-6. Estimations continue to be significant in both cases, although the coefficients are smaller in magnitude. The F-Statistic is well above the threshold that allows us to claim for the relevance of the instrument as well.

Fifth, Columns 1-3 in Appendix Table 1.C4 controls for national (log) unemployment growth rate in order to address potenial endogeneity concerns of unemployment. We add the same control variables as in the baseline results. Column 3 also controls for province-specific time trends.

[27]The right-wing political parties are CEDA, Partido Agrario Falange Española, Independientes de Derechas, Bloque de Derechas, Comunión Tradicionalista and Renovación Española.

[28]The left-wing vote share includes the Socialist Party, Izquierda Republicana, Unión Republicana and Frente Popular.

Given that the nature of the local unemployment variable does not allow for much identifying variation when one controls for municipality time trend, we provide an additional exercise defining the local unemployment variable as a continuous measure instead of an above-median one as in the baseline specification. The later results are presented in Columns 4-6, with the last column adding municipality-specific time trends as well. The results continue to be significant in all cases.

Sixth, we replicate the baseline results using a balanced panel of municipalities. Corresponding results are shown in Appendix Table 1.C5. Estimates are still significant although slightly smaller in magnitude. Our baseline estimates, which relies on the extensive margin of dams, i.e. whether a municipality has or not a dam built during the Francoist era, are also robust to its intensive margin form. Appendix Table 1.C6 reports the results for this alternative version. The 2SLS estimate reported in Column 3 for the effect of the unemployment shock is not significant anymore while its interaction with the number of dams is significant at a 5%. However, the magnitude of the β_1 coefficient decreased with respect to the baseline one.

Throughout all the specifications, we have analyzed all the dams, public or privately-owned, that were constructed during the period the dictator Francisco Franco ruled. During that time, many operating firms in the country, especially the successful ones had strong ties with the regime. As Toboso Sánchez (2007) (page 146) points out, "during the Francoist dictatorship [...] the nepotism, corruption and the arbitrarily that dominated the political life potentiated that the relationships were based on contacts, friends or relatives and that the success of the business depended mainly on the relationships with the authorities." Also, Díaz Linde et al. (2007) (page 11) detail that the system of redemption was very broad and thanks to it, many infrastructures were built and benefited firms that helped the coup d'etat previously. These arguments support the idea of analyzing both State and private-owned dams together, as both types of infrastructure should have resulted in a similar drama to the Spanish population after the use of forced labor. In order to be more conservative, Appendix Table 1.C7 replicates the baseline results focusing on State-owned dams only, i.e. the ones that were designed and constructed by the Francoist regime only and not by private firms. The 2SLS results in Columns 3 are stronger in magnitude than the baseline ones, while the Kleibergen-Paap F-Statistic reduces considerably, from 16.3 to 7.1 for the baseline specification.

Finally, in order to rule out the possibility that our results are driven by the most affected municipalities in terms of unemployment, we replicate our baseline estimates dropping these towns. Appendix Table 1.C8 shows the robustness of this version, excluding the cities with an unemployment growth rate above the 75 percentile (Columns 1-3) and 90 percentile (Columns 4-6). The Kleibergen-Paap F-Statistic ensures the relevance of the instrument even after eliminating some cities.

1.5.3 Channels

This section explores the mechanisms through which dams construction during the Francoist era might deteriorate the support for the right-wing political parties when experiencing unemployment shocks. First, we analyze the use of forced labor for the elaboration of the hydrological plans by the Francoist regime. Second, we investigate whether the final use of the dam influences the results. Then we move to analyze the heterogeneous effects with respect to the unemployment shock.

Table 1.4 presents the results of the first test where we show the correlation between dams and the presence of at least one victim of the dictatorship, our proxy for forced labor as explained in Section 1.3.4. The independent variable is an indicator variable equal to 1 if there is at least one dam in a municipality (Columns 1 and 3) or the number of dams in a municipality, the intensive margin form (Columns 2 and 4). Columns 1 and 2 analyze all dams, state and private-owned, while Columns 3 and 4 examine public dams only. Overall, we find that the presence of dams in a certain municipality is positively correlated with the existence of a victim of the Francoist regime in the same municipality. This effect, as expected, is even stronger in the case of State-owned dams as the use of forced labor was mainly employed by the regime itself.

Table 1.4: Channels: Forced Labor

	Dep. Variable: Victims			
	(1) Ext. Margin	(2) Int. Margin	(3) Ext. Margin	(4) Int. Margin
All dams	0.689*			
	(0.371)			
All dams		0.516*		
		(0.290)		
Public dams			1.498*	
			(0.899)	
Public dams				1.150
				(0.742)
Observations	7,295	7,295	7,295	7,295
Mean of Dep. Variable	0.7	0.7	0.7	0.7

NOTES: Cross-section estimates on the effect of dams on an indicator variable equal to 1 if there is at least one victim of the Francoist dictatorship in the municipality, as defined in Section 1.3.4 . All the specifications control for municipality area (in square kms), average elevation (in meters), average gradient, river length (in kms.), and a dummy variable indicating whether the municipality is urban (population is above 50,000 inhabitants) or not. Columns 1 and 3 analyzes the extensive margin of a dam while columns 2 and 4 the intensive margin. Standard errors in parentheses. * significant at 10% , ** significant at 5%, *** significant at 1%.

Table 1.5 replicates the baseline results adding Column 4, that controls for the presence of at least one victim of the Francoist regime in each municipality, our proxy for forced labor, that enters interacted with the unemployment shock variable in the equation. Surprisingly, forced labor seems to have no effect on the vote share for right-wing parties while the effect of dams decreases. We believe this is due to a lack of a good and reliable measure for forced labor as we

would expect that this practice carried out by the regime had a long-lasting effect on political outcomes.

Table 1.5: Channels: Forced Labor

	Dep. Variable: Right-Wing Vote Share			
	(1) 2SLS	(2) 2SLS	(3) 2SLS	(4) 2SLS
High Unempl.	0.013**	0.011**	0.012**	0.011*
	(0.005)	(0.005)	(0.005)	(0.006)
High Unempl. × Dams	-0.158***	-0.158***		
	(0.056)	(0.056)		
High Unempl. × Dams			-0.359**	-0.361**
			(0.148)	(0.148)
High Unempl. × Victims		0.002		0.002
		(0.002)		(0.002)
Mun. FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Province-Year FE	Yes	Yes	Yes	Yes
Observations	36,534	36,534	36,534	36,534
N. Clusters	47	47	47	47
F Statistic	16.3	16.3	7.1	7.1
Mean of Dep. Variable	0.38	0.38	0.38	0.38
Controls	Yes	Yes	Yes	Yes

NOTES: Column 1 shows the 2SLS results on the effect of the unemployment shock and its interaction with the presence of at least one dam on the vote share for right-wing political parties in the national elections celebrated in the years 2008, 2011, 2015, 2016 and 2019 (as Table 1.3) . Column 2 controls for the presence of at least on victim of the Francoist regime. Columns 3 and 4 replicate the previous columns, focusing on public dams only. All the specifications control for municipality area (in square kilometers), average elevation (in meters), average gradient, river length (in kilometers), a dummy variable indicating whether the municipality is urban (population is above 50,000 inhabitants) or not, and the number of dams built before the surge of the dictatorship. Clustered standard errors at province level in parentheses. Standard errors in parentheses. * significant at 10% , ** significant at 5%, *** significant at 1%.

Second, as indicated in Section 3.3, most of the dams built in Spain are used for irrigation only (61%), while around 28% of them are hydroelectric dams. Appendix Table 1.D1 analyzes whether the type of dam influences the main results obtained in the present paper. From the results it seems that irrigation dams have a stronger negative effect on political outcomes compared to the hydroelectric dams. However, the F statistic in the former case is much lower so conclusions about the comparisons of the two should be taken with a grain of salt.

Third, we examine heterogeneous effects with respect to the unemployment shock. Appendix Table 1.D2 shows the results when we analyze female (Column 1) and male (Column 2) unemployment shocks for separate. The main coefficients of interest are only significant for the male analysis. The F statistic in the female analysis is much lower the male unemployment study (2 versus 15). From these results we can conclude that the main results obtained in the

paper are due to the unemployment shock suffered by the male population, which corresponds to the gender that suffered the most the financial crisis of 2007-2008.[29]

Fourth, we analyze differences in the unemployment shock by some economic sectors: agriculture, industry and construction. Appendix Table 1.D3 shows that the presence of a dam built under the rule of Franco negatively influences the vote share when municipalities suffer an unemployment shock, no matter which sector we analyze. However, the construction sector seems to be the most affected one, which could be in line with the outcome obtained in the previous Appendix Table 1.D2, as most of the people employed in that sector are men and during the period analyzed the number of male employed substantially decreased in the construction sector, while the number of female remained essentially unchanged.

1.5.4 Falsification

In this section we carry out two falsification exercises. First, in order to rule out the idea that the results might also hold when analyzing different political parties, Appendix Table 1.E1 shows the results when we examine the vote share for left-wing parties. First, Columns 1 and 4 analyze the vote share for many left-wing parties, as detailed in section 3.3. Columns 2 and 5 show the result when we only examine the Socialist Party, the political group with the largest representation among the left-wing ones. Lastly, Columns 3 and 6 explore the vote share for the Communist party. The first three columns analyze all dams (public and private) while the last three columns includes private dams only. As expected, we do not find any significant coefficient on the relationship between dams built under the Francoism regime and its interaction with the unemployment shock and the vote share for different left-wing political parties.

The last exercise is provided in Appendix Table 1.E2, where we analyze only the dams built after the dictatorship of Franco, during democratic legislatures. If our results were driven by any dam built in the country, and not only by the ones that created so much controversy and suffering during the dictatorship, we should see this in the present table. In Columns 1 - 3 we analyze all the dams (public and private) that were constructed during democracy while Columns 4-6 investigate public dams only. Even though the main coefficients remain negative, they are not significant anymore. These results confirm, even though the F-statistic does not allow us to establish a causal relationship, that our baseline results are only due to dams built by the regime of the dictator Franco, and not otherwise.

[29] According to the Spanish National Statistics Institute, the male unemployment rate more than doubled during the first years analyzed in this paper.

1.6 Conclusion

This paper analyzes how parts of our past -in particular, the construction of dams- can undermine the support for political parties that are somewhat associated with that part of the history. Precisely, we show that Francoists traces negatively influences the vote share for right-wing parties when municipalities experience an unemployment shock. Interestingly, the effects dissipate when analyzing the support for left-wing political parties and dams built during democratic times, i.e. after the end of the dictatorship. In order to correct for endogeneity issues concerning the construction of dams and its political effects, we employ an Instrumental Variable strategy that takes into account the demonstrated positive relationship between dams construction and river gradient.

We examine different channels of transmission through which dams might have affected the political support during bad times. Precisely, we analyze the methods used by the Francoist regime to construct the dams. The positive correlation between dams built during the Franco era and the existence of victims of the dictatorship (our proxy for forced works) might be behind the main outcomes and resulted in a punishment for political parties that have their origins in the Franco era. It is important to analyze this issue as current political attitudes might be shaped by actions carried out by autocratic regimes even if occurred long time ago. One action that could prevent this to happen could be to provide more spaces of memory in order to reconcile past trauma.

Bibliography

Brendel, B. (2020). Dam construction in Francoist Spain in the 1950s and 1960s: Negotiating the future and the past. Sustainable Development, 28:396–404.

Cadot, O., Röller, L.-H., and Stephan, A. (2006). Contribution to productivity or pork barrel? the two faces of infrastructure investment. Journal of Public Economics, 90:1133–1153.

Camprubí, L. (2017). Los ingenieros de Franco. Ciencia, Catolicismo y Guerra Fría. Crítica.

Caprettini, B. and Voth, H.-J. (2022). New deal, new patriots: How 1930s government spending boosted patriotism during WWII. The Quarterly Journal of Economics.

Carillo, M. F. (2022). *Fascistville*: Mussolini's New Towns and the persistence of Neo-Fascism. Journal of Economic Growth, 26:527–567.

Castro Oury, E. (1993). La II República y la Guerra Civil Española. Ediciones AKAL.

Congreso de los Diputados (2022). Ii república española 1931-1939. https://www.congreso.es/cem/iirepesp. Accessed: 2022-03-09.

Curto-Grau, M., Herranz-Loncán, A., and Solé-Ollé, A. (2012). Pork-barrel politics in semi-democracies: The Spanish "parliamentary roads," 1880—1914. The Journal of Economic History, 72(3):771–796.

del Campo, S. and Félix Tezanos, J. (2008). España Siglo XXI. La política., volume 2. Biblioteca Nueva.

Del Rossi, A. F. (1995). The politics and economics of pork barrel spending: The case of federal financing of water resources development. Public Choice, 85:285–305.

Díaz Linde, E., Galiano Barajas, J., and Mendiola Gonzalo, F. (2007). Batallones disciplinarios. Esclavos del Franquismo, volume 1. Memoria Antifranquista del Baix LLobregat.

Díez-Cascón, J. and Bueno Fernández, F. (2003). Las presas y embalses en España. Historia de una necesidad, volume 1. Ministerio de Medio Ambiente.

Dixit, A. K. (1996). The Making of Economic Policy: A Transaction-Cost Politics Perspective. MIT press.

Duflo, E. and Pande, R. (2007). Dams. The Quarterly Journal of Economics, 122(2):601–646.

Eberle, U. (2020). Damned by dams? infrastructure and conflict. CEP Discussion Paper, (1694).

Fernández de la Mora, G. (1992). El agua en la era de franco. El País.

Fouka, V. and Voth, H.-J. (2022). Collective remembrance and private choice: German-Greek conflict and behavior in times of crisis.

Franco, F. N. F. (2018). Obra señera de franco: embalses, regadíos, colonización y repoblación forestal. Hydrological Processes, 1.

García de Cortázar, F. and Ruiz de Aguirre, A. (2002). Nueva Historia de España. La historia en su lugar. De la Guerra civil al siglo XXI, volume 10. Planeta.

Gil Delgado, E. La naturaleza nos protege y nosotros la despreciamos: Desmontando la posverdad. Kolima Books.

Grossman, G. M. and Helpman, E. (2001). Special Interest Politics. MIT Press.

Grossman, G. M. and Helpman, E. (2005). Party discipline and pork barrel politics. Working Paper.

Hernández de Miguel, C. (2019). Los campos de concentración de Franco. S.A. EDICIONES B.

Lago-Peñas, S. (2019). La descentralización tributaria en España: avances significativos, retos pendientes. Cuadernos de Información Económica, (272).

Limosani, M. and Navarra, P. (2001). Local pork-barrel politics in national pre-election dates: The case of Italy. Public Choice, 106:317–326.

Marcos, P. (2006). De cuando alianza popular nació con siete padrinos. El País.

Maskin, E. and Tirole, J. (2019). Pandering and pork-barrel politics. Journal of Public Economics, (176):79–93.

North, D. C. (1990). A transaction cost theory of politics. Journal of Theoretical Politics, 2(4):355–367.

Ochsner, C. and Roesel, F. (2017). Activated history – the case of the Turkish sieges of Vienna. CESifo Working Papers, (6586).

of Transports Mobility Urban Agenda, M. (2021). Áreas urbanas en España. Centro Virtual de publicaciones del Ministerio de Fomento.

Oto-Peralías, D. (2015). The long-term effects of political violence on political attitudes: Evidence from the Spanish Civil War. Kyklos, 68(3):412–442.

Parasuraman, S. and Sengupta, S. (2001). World commission on dams: Democratic means for sustainable ends. Economic and Political Weekly, 36(21):1881–1891.

Pearce, F. When the rivers run dry: Water - the defining crisis of the twenty-first century. Beacon Press.

Penella, M. Los orígenes y la evolución del Partido Popular: una historia de AP (1973-1989). Caja Duero.

Radio Televisión Española (1952). NO-DO la presa y el embalse. available here: https://www.rtve.es/play/videos/revista-imagenes/presa-embalse/2870095/.

Radio Televisión Española (1954). NO-DO vida de un río. available here: https://www.rtve.es/play/videos/documentales-b-n/vida-rio/2847575/.

Radio Televisión Española (1970). NO-DO franco inaugura el embalse de alcántara. available here: https://www.rtve.es/filmoteca/no-do/not-1436/1486616/.

Roberson, B. (2008). Pork-barrel politics, targetable policies, and fiscal federalism. Journal of the European Economic Association, 6(4):819–844.

Rozenas, A. and Vlasenko, A. (2022). The real consequences of symbolic politics: Breaking the Soviet past in Ukraine. The Journal of Politics, 84(3):1263–1277.

Sánchez Sánchez, I. and Selva Iniesta, A. Las Brigadas Internacionales, 80 años después. Instituto de Estudios Albacetenses "Don Juan Manuel".

Sanz, C., Solé-Ollé, A., and Sorribas-Navarro, P. (2021). Betrayed by the elites: How corruption amplifies the political effects of recessions. Comparative Political Studies, 55(7):1095–1129.

Strobl, E. and Strobl, R. O. (2011). The distributional impact of large dams: Evidence from cropland productivity in Africa. Journal of Development Economics, 96(2):432–450.

Toboso Sánchez, P. (2007). Empresarios y política en la dictadura de Franco. Ayer, 66(2):143–173.

Tur-Prats, A. and Valencia Caicedo, F. (2020). The long shadow of the Spanish Civil War. Tur-Prats, Ana and Valencia Caicedo, Felipe, The Long Shadow of the Spanish Civil War (July 15, 2020). Available at SSRN: https://ssrn.com/abstract=3659549.

Villamil, F. and Balcells, L. (2021). Do TJ policies cause backlash? evidence from street name changes in Spain. Research & Politics, 8(4):1–7.

Voigtländer, N. and Voth, H.-J. (n.d.). Highway to Hitler. American Economic Journal: Applied Economics.

Appendix

A Data

Table 1.A1: Data Description

Variable	Description	Source
Electoral Variables		
Vote share for Right-Wing Parties	Total number of votes received at the National elections by Popular Party and *FE de las JONS* over the number of voters for the elections celebrated in years 1977, 1979, 1982, 1986, 1989, 1993, 1996, 2000, 2004, 2008, 2011, 2015, 2016 and 2019.	Ministry of Interior
Vote share for Left-Wing Parties	Total number of votes received at the National elections by Socialist Party (Partido Socialista Obrero Español), *Podemos*, Communist Party and *Izquierda Unida* over the number of voters for the elections velebrated in years 1977, 1979, 1982, 1986, 1989, 1993, 1996, 2000, 2004, 2008, 2011, 2015, 2016 and 2019.	Ministry of Interior
Huesca	Total number of votes received at the National elections celebrated in 1936 by the following political parties:	Official Bulletin of the province of Huesca *(n. 44, 22/2/1936)*. Archive of the provincial government of Huesca
Navarra	Total number of votes received at the National elections celebrated in 1936 by the following political parties:	Official Bulletin of the province of Navarra *(n. 22, 19/2/1936)*. Royal and General Archive of Navarra

Continued on next page

Table 1.A1 – continued from previous page

Variable	Description	Source
Basque Country	Total number of votes received celebrated in 1936 by the following political parties:	Official Bulletin of the provinces of Álava *(n. 29, 7/3/1936)*, Guipúzcoa *(n. 27, 3/3/1936 and n.29, 6/3/1936)* and Vizcaya *(n. 53, 4/3/1936)*
Dams		
Dams	All dams and reservoirs built in Spain. The dataset contains public and private property dams	Ministry for the ecological transition and demographic challenge
Dams	All dams and reservoirs built in Spain. The dataset contains public and private property dams	Spanish Society for Dams and Reservoirs (SEPREM from its Spanish acronym)
Geographical Variables		
Rainfall	Average rainfall at municipality level for the period 1921-1930.	Climatic Research Unit at the University of East Anglia
Other Variables		
Unemployment	Monthly unemployed people registered in each Spanish municipality from 2006 to 2019	Public Service of National Employment (SEPE from its Spanish acronym), Ministry of Labor and Social Economy
Population	Historical Population Censuses	National Statistics Institute (INE from its Spanish acronym)

Continued on next page

Table 1.A1 – continued from previous page

Variable	Description	Source
Victims of the Civil War and Francoism Reprisals	People who suffered personal violence, repression, purging, injustice and affront for political, ideological, religious motives or other reasons.	Portal of Victims of the Civil War and Reprisals of the Francoist regime from Ministry of Culture. This portal compiles original data coming from the Archive of the Real Chancery of Valladolid, Archive of the General Administration, National Administration Archive, Historical Archive of the Province of Huesca, Association of Studies of the Repression in Leon, Documental Center of the Historical Memory.

Table 1.A2: Summary Statistics

	No dam		Dam		Mean-Diff
Panel A: Cross-Section					
All dams	0.00	(0.00)	1.66	(1.47)	-1.66***
Dams built during dictatorship	0.00	(0.00)	0.58	(0.96)	-0.58***
Public dams built during dictatorship	0.00	(0.00)	0.35	(0.73)	-0.35***
River gradient suitability (RG)	0.23	(0.33)	0.40	(0.35)	-0.17***
Victims	0.65	(0.48)	0.75	(0.44)	-0.10***
Municipality area (km^2)	51.73	(68.06)	151.65	(181.96)	-99.92***
River Length (km)	4.27	(7.14)	14.01	(18.67)	-9.73***
Avg gradient	5.81	(5.17)	7.77	(6.16)	-1.96***
Avg elevation	679.83	(374.62)	674.37	(411.15)	5.46
Urban	0.01	(0.12)	0.04	(0.19)	-0.02**
N	6,459		836		7,295
Panel B: Panel-Data					
Right-Wing Vote Share	0.38	(0.21)	0.36	(0.16)	0.02***
Left-Wing Vote Share	0.36	(0.16)	0.44	(0.15)	-0.08***
Unemployment rate	8.41	(5.21)	11.03	(5.94)	-2.62***
Unemployment growth rate	0.19	(1.20)	0.12	(0.95)	0.07***
Unempl.	0.50	(0.50)	0.51	(0.50)	-0.01
N	32,367		4,190		36,557

NOTES: The sample consists of 7,295 Spanish municipalities. Panels A and B differentiates between municipalities with no dams (6,459 observations) and those with a dam built at any point in time (836 obs.). Panel A uses the cross-sectional sample while Panel B the panel data structure. Standard deviations shown in parentheses.

B Robustness Checks for the Instrument

Table 1.B1: First-Stage with Rainfall as an additional Instrument

	Cross-Section		Panel Data	
	(1)	(2)	(3)	(4)
	All Dams	Public Dams	All Dams	Public Dams
RG	0.044***	0.020***		
	(0.011)	(0.007)		
Rainfall	0.000**	0.000**		
	(0.000)	(0.000)		
High Unempl. × RG			0.046***	0.021**
			(0.012)	(0.008)
High Unempl. × Rainfall			0.000**	0.000
			(0.000)	(0.000)
Mun. FE	No	No	Yes	Yes
Province FE	Yes	Yes	No	No
Year FE	No	No	Yes	Yes
Province-Year FE	No	No	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	6487	6487	32494	32494
N. Clusters	46	46	46	46
F Statistic				

NOTES: This table shows the first-stage estimates. Columns 1-2 show the cross-sectional results while Columns 3-4 show the panel data estimates. The dependent variable in Columns 1 and 3 are all the dams built during the Francoist regime, while the dependent variable in the rest of Columns is the number of dams that are owned by the State. Instruments include river gradient and average rainfall in the period 1921-30. All the specifications control for municipality area (in square kms), average elevation (in meters), average gradient, river length (in kms.), a dummy variable indicating whether the municipality is urban (population is above 50,000 inhabitants) or not, and the number of dams built before the surge of the dictatorship. The time-invariant control variables enter in the panel data analysis interacted with the unemployment shock. Clustered standard errors in parentheses. * significant at 10% , ** significant at 5%, *** significant at 1%.

Table 1.B2: Rainfall as an additional Instrument

	Dep. Variable: Right-Wing Vote Share		
	(1)	(2)	(3)
	OLS	R. Form	2SLS
High Unempl.	0.014***	0.023***	0.012**
	(0.004)	(0.007)	(0.005)
High Unempl. × Dams	-0.000		-0.189**
	(0.002)		(0.075)
High Unempl. × RG		-0.007***	
		(0.002)	
High Unempl. × Rainfall		-0.000*	
		(0.000)	
Mun. FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Province-Year FE	Yes	Yes	Yes
Observations	36,534	32,494	32,494
N. Clusters	47	46	46
F Statistic			12.4
Mean of Dep. Variable	0.38	0.36	0.36
Controls	Yes	Yes	Yes

NOTES: Column 1 shows the OLS results on the effect of the unemployment shock with the interaction of dams on the vote share for the right-wing vote share in the national elections celebrated in the years 2008, 2011, 2015, 2016 and 2019. Column 2 shows the reduced form. Column 3 shows the 2SLS results on the effect of the unemployment shock, interacted with dams built in the municipality on the vote share, with as instrument variable river gradient suitability. All the specifications control for municipality area (in square kms), average elevation (in meters), average gradient, river length (in kms.), a dummy variable indicating whether the municipality is urban (population is above 50,000 inhabitants) or not, and the number of dams built before the surge of the dictatorship. Clustered standard errors in parentheses. * significant at 10% , ** significant at 5%, *** significant at 1%.

C Robustness Checks for Main Results

Table 1.C1: Historical elections

	Right-wing	Left-wing	Republican
	(1)	(2)	(3)
Dams	5.619	201.100	85.997
	(209.823)	(1311.402)	(562.972)
Observations	674	674	674
Mean of Dep. Variable	3.6	1.0	0.3

NOTES: Cross-sectional regression of the effect of dams built during the Francoist regime on the vote share for different political parties in the national elections celebrated in 1936. Robust standard errors are in parentheses. * significant at 10% , ** significant at 5%, *** significant at 1%.

Table 1.C2: Adding Region-year FE

	Dep. Variable: Right-Wing Vote Share		
	(1) OLS	(2) R. Form	(3) 2SLS
High Unempl.	0.012***	0.013***	0.011***
	(0.003)	(0.003)	(0.004)
High Unempl. × Dams	-0.001		-0.164***
	(0.002)		(0.056)
High Unempl. × RG		-0.007***	
		(0.002)	
Mun. FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Region-Year FE	Yes	Yes	Yes
Observations	36,534	36,534	36,534
N. Clusters	47	47	47
F Statistic			15.9
Mean of Dep. Variable	0.38	0.38	0.38
Controls	Yes	Yes	Yes

NOTES: Column 1 shows the OLS results on the effect of the unemployment shock and its interaction with the presence of at least one dam on the vote share for right-wing political parties in the national elections celebrated in the years 2008, 2011, 2015, 2016 and 2019. Column 2 shows the reduced form. Column 3 shows the 2SLS results on the effect of the unemployment shock, interacted with dams built in the municipality on the vote share. The instrument variable is the river gradient suitability for each municipality. . All the specifications control for municipality area (in square kilometers), average elevation (in meters), average gradient, river length (in kilometers), a dummy variable indicating whether the municipality is urban (population is above 50,000 inhabitants) or not, and the number of dams built before the surge of the dictatorship. Clustered standard errors at province level in parentheses. * significant at 10% , ** significant at 5%, *** significant at 1%.

Table 1.C3: Excluding Some Regions

	Exclude Catalonia			Exclude Basque Country & Navarra		
	(1) OLS	(2) R. Form	(3) 2SLS	(4) OLS	(5) R. Form	(6) 2SLS
High Unempl.	0.012***	0.012***	0.012**	0.016***	0.016***	0.015***
	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)	(0.004)
High Unempl. × Dams	0.000		-0.139**	0.000		-0.128***
	(0.002)		(0.057)	(0.002)		(0.042)
High Unempl. × RG		-0.006***			-0.006***	
		(0.002)			(0.002)	
Mun. FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Province-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	31,811	31,811	31,811	33,919	33,919	33,919
N. Clusters	43	43	43	43	43	43
F Statistic			14.1			17.6
Mean of Dep. Variable	0.42	0.42	0.42	0.39	0.39	0.39
Controls	Yes	Yes	Yes	Yes	Yes	Yes

NOTES: Columns 1-3 (Columns 4-6) replicates the baseline specification in Table 1.3 excluding Catalonia (the Basque Country and Navarra). Columns 1 and 4 show the OLS results on the effect of the unemployment shock and its interaction with the presence of at least one dam on the vote share for right-wing political parties in the national elections celebrated in the years 2008, 2011, 2015, 2016 and 2019. Columns 2 and 5 show the reduced form. Columns 3 and 6 show the 2SLS results on the effect of the unemployment shock, interacted with dams built in the municipality on the vote share. The instrument variable is the river gradient suitability for each municipality. All the specifications control for municipality area (in square kilometers), average elevation (in meters), average gradient, river length (in kilometers), a dummy variable indicating whether the municipality is urban (population is above 50,000 inhabitants) or not, and the number of dams built before the surge of the dictatorship. Clustered standard errors at province level in parentheses. * significant at 10% , ** significant at 5%, *** significant at 1%.

Table 1.C4: Controlling for national unemployment

	Above-median Local Unemployment			(continuous) Local Unemployment		
	(1)	(2)	(3)	(4)	(5)	(6)
	R. Form	2SLS	2SLS	R. Form	2SLS	2SLS
High Unempl.	-0.001	-0.003	0.007			
	(0.009)	(0.009)	(0.005)			
High Unempl. × RG	-0.003					
	(0.003)					
Nat. Unemploym. × RG	-0.022***					
	(0.008)					
Nat. Unemploym.	0.289***	0.295***	1.920***	0.219***	0.221***	0.138***
	(0.011)	(0.012)	(0.267)	(0.011)	(0.011)	(0.010)
Unemploym. × Dams		-0.252**	-0.116			
		(0.100)	(0.095)			
High Unempl. × Dams		-0.131*	-0.098*			
		(0.073)	(0.050)			
Unemploym.				0.012**	0.014**	0.006
				(0.006)	(0.006)	(0.007)
Unemploym. × RG				-0.014***		
				(0.005)		
Unemploym. × Dams					-0.187***	-0.123**
					(0.065)	(0.057)
Mun. FE	Yes	Yes	Yes	Yes	Yes	Yes
Prov. specific time trends	No	No	Yes	No	No	No
Mun. specific time trends	No	No	No	No	No	Yes
Observations	36,534	36,534	36,534	36,534	36,534	36,534
N. Clusters	47	47	47	47	47	47
F Statistic		6.8	7.5		11.9	7.9
Mean of Dep. Variable	0.38	0.38	0.38	0.38	0.38	0.38
Controls	Yes	Yes	Yes	Yes	Yes	Yes

NOTES: Column 1 replicates the baseline reduced form controlling for the effect of the national (log) unemployment growth rate (and its interaction with the presence of at least one dam) on the vote share for right-wing political parties in the national elections celebrated in the years 2008, 2011, 2015, 2016 and 2019. Column 2 also replicates the 2SLS baseline results adding the overall national (log) unemployment growth rate. Columns 3 adds province time trends. Columns 4-6 replicate the previous columns with the continuous version for the local unemployment growth rate. All the specifications control for municipality area (in square kilometers), average elevation (in meters), average gradient, river length (in kilometers), a dummy variable indicating whether the municipality is urban (population is above 50,000 inhabitants) or not, and the number of dams built before the surge of the dictatorship. Clustered standard errors at province level in parentheses. * significant at 10% , ** significant at 5%, *** significant at 1%.

Table 1.C5: Balanced Panel Data

	All Dams			Public Dams		
	(1) OLS	(2) R. Form	(3) 2SLS	(4) OLS	(5) R. Form	(6) 2SLS
High Unempl.	0.014***	0.014***	0.013**	0.014***	0.014***	0.012**
	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)	(0.005)
High Unempl. × Dams	-0.000		-0.157***			
	(0.002)		(0.055)			
High Unempl. × RG		-0.007***			-0.007***	
		(0.002)			(0.002)	
High Unempl. × Dams				0.001		-0.357**
				(0.003)		(0.148)
Observations	36,475	36,475	36,475	36,475	36,475	36,475
N. Clusters	47	47	47	47	47	47
F Statistic			16			7
Mean of Dep. Variable	0.38	0.38	0.38	0.38	0.38	0.38
Controls	Yes	Yes	Yes	Yes	Yes	Yes

NOTES: This table makes use of a balanced panel of Spanish municipalities. Column 1 shows the OLS results on the effect of the unemployment shock and its interaction with the presence of at least one dam on the vote share for right-wing political parties in the national elections celebrated in the years 2008, 2011, 2015, 2016 and 2019. Column 2 shows the reduced form. Column 3 shows the 2SLS results on the effect of the unemployment shock, interacted with dams built in the municipality on the vote share. The instrument variable is the river gradient suitability for each municipality. Column 4 controls for the number of victims of the dictatorship. All the specifications control for municipality area (in square kilometers), average elevation (in meters), average gradient, river length (in kilometers), a dummy variable indicating whether the municipality is urban (population is above 50,000 inhabitants) or not, and the number of dams built before the surge of the dictatorship. Clustered standard errors at province level in parentheses. * significant at 10% , ** significant at 5%, *** significant at 1%.

Table 1.C6: Intensive Form

	Dep. Variable: Right-Wing Vote Share		
	(1) OLS	(2) R. Form	(3) 2SLS
High Unempl.	0.014***	0.014***	0.006
	(0.004)	(0.004)	(0.007)
High Unempl. × Dams	0.001		-0.117**
	(0.001)		(0.049)
High Unempl. × RG		-0.007***	
		(0.002)	
Mun. FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Province-Year FE	Yes	Yes	Yes
Observations	36,534	36,534	36,534
N. Clusters	47	47	47
F Statistic			8.5
Mean of Dep. Variable	0.38	0.38	0.38
Controls	Yes	Yes	Yes

NOTES: Column 1 shows the OLS results on the effect of the unemployment shock and its interaction with the number dams on the vote share for the right-wing vote share in the national elections celebrated in the years 2008, 2011, 2015, 2016 and 2019. Column 2 shows the reduced form. Column 3 shows the 2SLS results on the effect of the unemployment shock, interacted with dams built in the municipality. The instrument variable is the river gradient suitability for each municipality. All the specifications control for municipality area (in square kms), average elevation (in meters), average gradient, river length (in kms.), a dummy variable indicating whether the municipality is urban (population is above 50,000 inhabitants) or not, and the number of dams built before the surge of the dictatorship. Clustered standard errors in parentheses. * significant at 10% , ** significant at 5%, *** significant at 1%.

Table 1.C7: Public Dams

	Dep. Variable: Right-Wing Vote Share		
	(1)	(2)	(3)
	OLS	Reduced Form	2SLS
High Unempl.	0.014***	0.014***	0.012**
	(0.004)	(0.004)	(0.005)
High Unempl. × Dams	0.001		-0.359**
	(0.003)		(0.148)
High Unempl. × RG		-0.007***	
		(0.002)	
Mun. FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Province-Year FE	Yes	Yes	Yes
Observations	36,534	36,534	36,534
N. Clusters	47	47	47
F Statistic			7.1
Mean of Dep. Variable	0.38	0.38	0.38
Controls	Yes	Yes	Yes

NOTES: Column 1 shows the OLS results on the effect of the unemployment shock and its interaction with the presence of at least one public dam on the vote share for right-wing political parties in the national elections celebrated in the years 2008, 2011, 2015, 2016 and 2019. Column 2 shows the reduced form. Column 3 shows the 2SLS results on the effect of the unemployment shock, interacted with the existence of public dams in the municipality on the vote share. The instrument variable is the river gradient suitability for each municipality. All the specifications control for municipality area (in square kilometers), average elevation (in meters), average gradient, river length (in kilometers), a dummy variable indicating whether the municipality is urban (population is above 50,000 inhabitants) or not, and the number of dams built before the surge of the dictatorship. Clustered standard errors at province level in parentheses. * significant at 10% , ** significant at 5%, *** significant at 1%.

Table 1.C8: Excluding Top Percentiles

	Exclude 75 Perc.			Exclude 90 Perc.		
	(1) OLS	(2) R. Form	(3) 2SLS	(4) OLS	(5) R. Form	(6) 2SLS
High Unempl.	0.009**	0.009***	0.008*	0.015***	0.015***	0.014***
	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.005)
High Unempl. × Dams	-0.001		-0.139***	0.000		-0.155***
	(0.002)		(0.048)	(0.002)		(0.048)
High Unempl. × RG		-0.008***			-0.008***	
		(0.002)			(0.002)	
Mun. FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Province-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	27,390	27,390	27,390	32,880	32,880	32,880
N. Clusters	47	47	47	47	47	47
F Statistic			16.0			19.2
Mean of Dep. Variable	0.36	0.36	0.36	0.37	0.37	0.37
Controls	Yes	Yes	Yes	Yes	Yes	Yes

NOTES: Columns 1-3 (Columns 4-6) replicates the baseline specification in Table 1.3 excluding the towns with an unemployment growth rate above the 75 (95) percentile. Columns 1 and 4 show the OLS results on the effect of the unemployment shock and its interaction with the presence of at least one dam on the vote share for right-wing political parties in the national elections celebrated in the years 2008, 2011, 2015, 2016 and 2019. Columns 2 and 5 show the reduced form. Columns 3 and 6 show the 2SLS results on the effect of the unemployment shock, interacted with dams built in the municipality on the vote share. The instrument variable is the river gradient suitability for each municipality. All the specifications control for municipality area (in square kilometers), average elevation (in meters), average gradient, river length (in kilometers), a dummy variable indicating whether the municipality is urban (population is above 50,000 inhabitants) or not, and the number of dams built before the surge of the dictatorship. Clustered standard errors at province level in parentheses. * significant at 10% , ** significant at 5%, *** significant at 1%.

D Channels

Table 1.D1: Channels: Type of Dams

	Dep. Var.: Right-Wing Vote Sh.	
	(1)	(2)
High Unempl.	0.020***	0.006
	(0.005)	(0.006)
High Unempl. × Irrigation Dams	-0.411**	
	(0.184)	
High Unempl. × Hydroelectric Dams		-0.327**
		(0.131)
Mun. FE	Yes	Yes
Year FE	Yes	Yes
Province-Year FE	Yes	Yes
Observations	36,534	36,534
N. Clusters	47	47
F Statistic	7	13
Mean of Dep. Variable	0.4	0.4
Controls	Yes	Yes

Notes: Column 1 (2) shows the 2SLS results on the effect of the unemployment shock, interacted with irrigation (hydroelectric) dams built in the municipality on the vote share for right-wing political parties in the national elections celebrated in the years 2008, 2011, 2015, 2016 and 2019. The instrument variable is the river gradient suitability for each municipality. All the specifications control for municipality area (in square kilometers), average elevation (in meters), average gradient, river length (in kilometers), a dummy variable indicating whether the municipality is urban (population is above 50,000 inhabitants) or not, and the number of dams built before the surge of the dictatorship. Clustered standard errors at province level in parentheses. * significant at 10% , ** significant at 5%, *** significant at 1%.

Table 1.D2: Channels: Unemployment by Gender

	Dep. Var.: Right-Wing Vote Share	
	(1)	(2)
High Female Unempl.	0.013**	
	(0.006)	
High Female Unempl. × Dams	-1.435	
	(2.059)	
High Male Unempl.		0.017***
		(0.005)
High Male Unempl. × Dams		-0.174***
		(0.055)
Mun. FE	Yes	Yes
Year FE	Yes	Yes
Province-Year FE	Yes	Yes
Observations	36,534	36,534
N. Clusters	47	47
F Statistic	1	18
Mean of Dep. Variable	0.4	0.4

Notes: Column 1 (2) shows the 2SLS results on the effect of the female (male) unemployment shock, interacted with dams built in the municipality on the vote share for right-wing political parties in the national elections celebrated in the years 2008, 2011, 2015, 2016 and 2019. The instrument variable is the river gradient suitability for each municipality. All the specifications control for municipality area (in square kilometers), average elevation (in meters), average gradient, river length (in kilometers), a dummy variable indicating whether the municipality is urban (population is above 50,000 inhabitants) or not, and the number of dams built before the surge of the dictatorship. Clustered standard errors at province level in parentheses. * significant at 10% , ** significant at 5%, *** significant at 1%.

Table 1.D3: Channels: Unemployment by Economic Sector

	Agriculture	Industry	Construction
	(1)	(2)	(3)
High Unempl., agric	-0.000		
	(0.006)		
High Unempl. × Dams	-0.154**		
	(0.058)		
High Unempl., ind		0.017*	
		(0.009)	
High Unempl. × Dams		-0.129**	
		(0.057)	
High Unempl., const			0.009
			(0.013)
High Unempl. × Dams			-0.516**
			(0.252)
Mun. FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	36,534	36,534	36,534
N. Clusters	47	47	47
F Statistic	14	15	7
Mean of Dep. Variable	0.4	0.4	0.4

NOTES: Columns 1, 2 and 3 shows the 2SLS results on the effect of the agriculture, industry and construction unemployment shock, respectively interacted with dams built in the municipality on the vote share for right-wing political parties in the national elections celebrated in the years 2008, 2011, 2015, 2016 and 2019. The instrument variable is the river gradient suitability for each municipality. All the specifications control for municipality area (in square kilometers), average elevation (in meters), average gradient, river length (in kilometers), a dummy variable indicating whether the municipality is urban (population is above 50,000 inhabitants) or not, and the number of dams built before the surge of the dictatorship. Clustered standard errors at province level in parentheses. * significant at 10% , ** significant at 5%, *** significant at 1%.

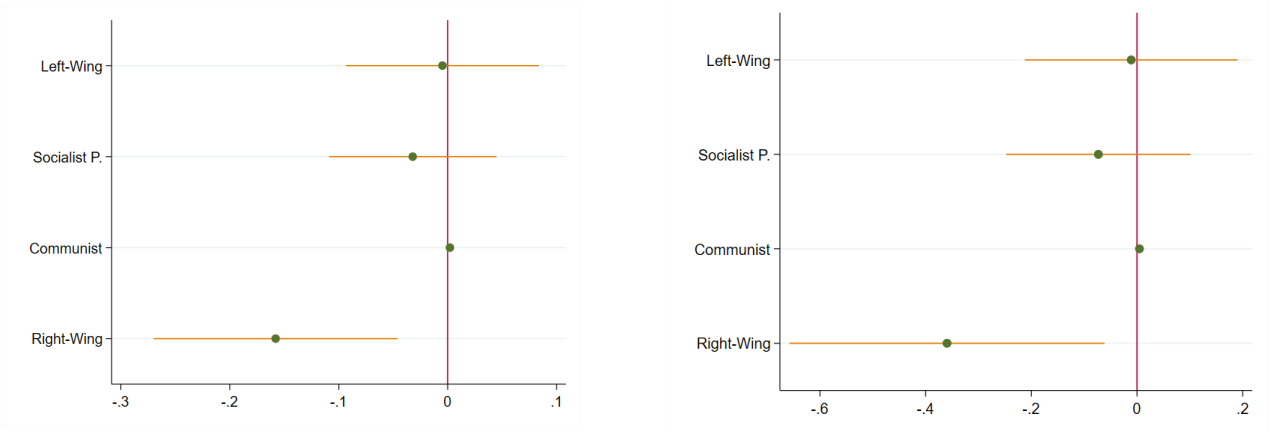
E Falsification Exercises

Table 1.E1: Falsification. Other political parties

	All Dams			Public Dams		
	(1) Left Wing	(2) Socialist P.	(3) Communist	(4) Far-right Wing	(5) Left Wing	(6) Socialist P.
High Unempl.	0.002	0.007**	0.000	0.002	0.007***	0.000
	(0.003)	(0.003)	(0.000)	(0.003)	(0.002)	(0.000)
High Unempl. × Dams	-0.005	-0.032	0.002			
	(0.044)	(0.038)	(0.001)			
High Unempl. × Dams				-0.011	-0.073	0.005
				(0.100)	(0.087)	(0.003)
Mun. FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Province-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	36534	36534	36534	36534	36534	36534
N. Clusters	47	47	47	47	47	47
F Statistic	16	16	16	7	7	7
Mean of Dep. Variable	0.37	0.29	0.00	0.37	0.29	0.00
Controls	Yes	Yes	Yes	Yes	Yes	Yes

NOTES: This table shows the 2SLS results on the effect of the unemployment shock, interacted with dams built in the municipality on the vote share for different political parties in the national elections celebrated in the years 2008, 2011, 2015, 2016 and 2019. Columns 1 and 2 examine the left-wing vote share, Columns 2 and 5 the support for the Socialist party, while the remaining columns the Communist support. The first three columns examine all the dams built during the Franco era while the rest of the columns focuses on the dams owned by the State only. The instrument variable is the river gradient suitability for each municipality. All the specifications control for municipality area (in square kilometers), average elevation (in meters), average gradient, river length (in kilometers), a dummy variable indicating whether the municipality is urban (population is above 50,000 inhabitants) or not, and the number of dams built before the surge of the dictatorship. Clustered standard errors at province level in parentheses. * significant at 10% , ** significant at 5%, *** significant at 1%.

Figure 1.E1: Falsification



A) All dams

B) Public dams

Panel A (B) displays the estimated coefficients on the effect of all (public) dams built during the Francoist dictatorship, interacted with unemployment, on the vote share for different left-wing political parties. Both panels also shows the baseline coefficient for right-wing political parties for comparison and the 95% confidence intervals.

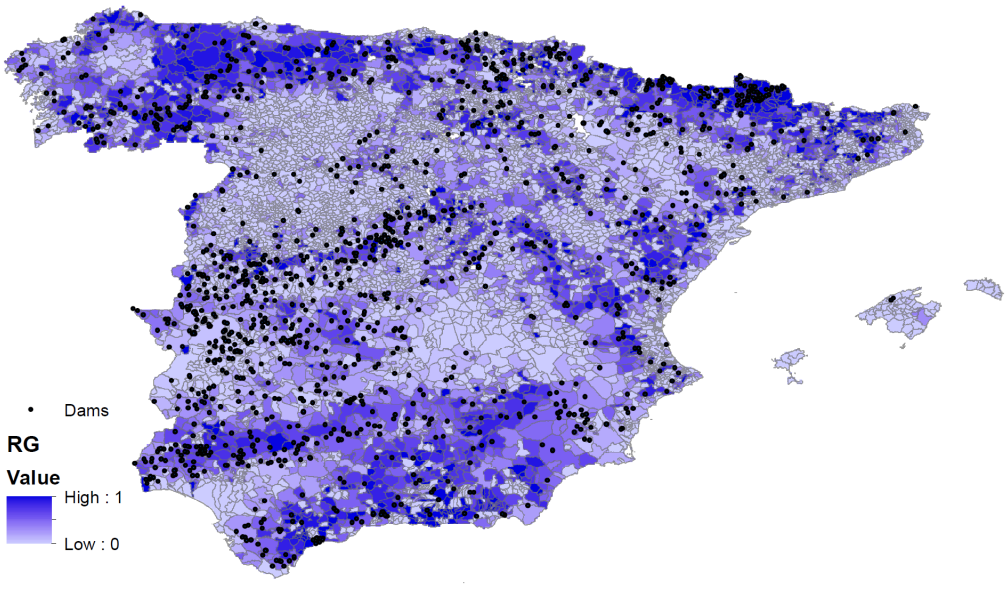
Table 1.E2: Falsification. Dams built during democracy

	All Dams			Public Dams		
	(1) OLS	(2) R. Form	(3) 2SLS	(4) OLS	(5) R. Form	(6) 2SLS
High Unempl.	0.014***	0.014***	0.024	0.014***	0.014***	0.023
	(0.004)	(0.004)	(0.019)	(0.004)	(0.004)	(0.025)
High Unempl. × Dams	-0.001		-0.846			
	(0.002)		(1.073)			
High Unempl. × RG		-0.007***			-0.007***	
		(0.002)			(0.002)	
High Unempl. × Dams				0.002		-1.357
				(0.002)		(2.396)
Mun. FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Province-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	36534	36534	36534	36534	36534	36534
N. Clusters	47	47	47	47	47	47
F Statistic			1			0
Mean of Dep. Variable	0.4	0.4	0.4	0.4	0.4	0.4
Controls	Yes	Yes	Yes	Yes	Yes	Yes

NOTES: Column 1 shows the OLS results on the effect of the unemployment shock and its interaction with the presence of at least one dam on the vote share for right-wing political parties in the national elections celebrated in the years 2008, 2011, 2015, 2016 and 2019. Column 2 shows the reduced form. Column 3 shows the 2SLS results on the effect of the unemployment shock, interacted with dams built in the municipality on the vote share. The instrument variable is the river gradient suitability for each municipality. The rest of the columns replicate the previous results focusing on public dams only. All the specifications control for municipality area (in square kilometers), average elevation (in meters), average gradient, river length (in kilometers), a dummy variable indicating whether the municipality is urban (population is above 50,000 inhabitants) or not, and the number of dams built before the surge of the dictatorship. Clustered standard errors at province level in parentheses. * significant at 10% , ** significant at 5%, *** significant at 1%.

F Figures

Figure 1.F1: River gradient suitability and dams



NOTES: This map shows the location of dams built from 1900 until now *(black dots)* and the fraction of each municipality area with a suitable river gradient *(in blue), as defined in Section 3.5.*

Chapter 2

Threat of Taxation, Stagnation and Social Unrest: Evidence from 19*th* Century Sicily

[1] **with Dominic Rohner and Alessandro Saia**.

Published at the *Journal of Economic Behavior & Organization (2022) Volume 202, pages 361-371.*

2.1 Introduction

Taxation and fiscal policy are some of the state's key responsibilities; their design may have wide-ranging implications on political stability. Strikingly enough, not much is known about how tax policies affect civil unrest. Conceptually, the effects of increased taxation are ambiguous. On one hand, taxation perceived as extractive or unfair may trigger revolt, as was the case during the Boston Tea Party of 1773 or the subsequent American Revolution, the Rebecca Riots against tolls in Wales between 1839 and 1843, the so-called "dog tax conflict" in New Zealand in 1898, the Tax Resistance at Bayt Sahur (West Bank) in 1989, the Poll Tax Riots in the United Kingdom in 1990, or more recently during the Yellow Vest Movement in France since 2018, among many other examples. On the other hand, greater fiscal revenue could enhance state capacity, making it thereby more difficult for would-be rebels to challenge the state (as argued e.g. by Collier et al. (2009) and Besley and Persson (2011)). As famously stressed

[1]We are grateful for helpful comments from the editor Erich Kirchler, two anonymous referees and Elena Esposito, Jürgen Maurer, Camille Terrier, Mathias Thoenig, and seminar participants at the University of Lausanne, the University of Bologna and the University of Bergamo. Gema Lax-Martinez, Dominic Rohner and Alessandro Saia gratefully acknowledge financial support from the ERC Starting Grant POLICIES FOR PEACE-677595. We thank Charlotte Goriup for excellent research assistance.

by Max Weber (Weber (2013)), strong state capacity allows for a "monopoly of power" which is efficient in preventing what Thomas Hobbes called "war of every man against every man" (Hobbes (2016)).

Fiscal expansion may be linked to greater accountability, as an increased state budget could foster a demand for increased transparency and formalized *bookkeeping* of expenses (Collier and Rohner (2008)). In essence, a better funded state has more means for building up a powerful regulatory compliance infrastructure. At the same time, a larger state may also be more redistributive, which in turn can help reduce inequality. Overall, both a surge in accountability and a drop in inequality may increase the likelihood of pacification, by addressing popular grievances (Gurr (2015)).

Finally, increased taxation may also have indirect effects or effects on places and people that have escaped the taxman. For instance, tax systems containing discontinuities and loopholes may enhance tax avoidance. In this regard, the threat of taxation distorts incentives, which can result in slowing economic growth (think e.g. of the stylized fact that informality is associated with lower productivity – La Porta and Shleifer (2014)). In a faltering economy, the opportunity cost of rebellion is low which in turn increases the probability of civil unrest (see e.g. Miguel et al. (2004); Dell et al. (2014)).

Empirical evidence on these issues is scarce due to the fact that it is very difficult to find exogenous variation in taxation, limiting most possible analyses to pure correlations. In this paper, we aim to address this shortcoming by exploiting a unique policy experiment in 19th century Sicily. In particular, we exploit the implementation of a property tax *(imposta fondiaria)* introduced in Sicily in 1810. Municipalities of over 2,000 inhabitants were subject to the tax, while those under that threshold remained untaxed. This discontinuity allows us to perform a regression discontinuity design (RDD). To the best of our knowledge, the current paper is the first to study the effects of an arguably exogenous variation in taxation on the prevalence of conflict, while investigating potential channels of transmission.

In particular, we examine for municipalities closely below and above the 2,000 inhabitant threshold to what extent a municipality's exogenous exposure to the property tax impacted social unrest in 1860 (when the Kingdom of the Two Sicilies was overthrown and integrated into Italy), and during the revolution of 1848 (as a major robustness check). For this purpose, we have assembled a novel dataset on tax assignment, population and social unrest, drawing on a variety of original sources from historical archives. We detect a strong and robust effect of tax exposure around the threshold lowering the propensity for engaging in political violence. Next, we investigate potential channels of transmission, using a series of proxies for state capacity and public spending, some of which we have freshly collected from archival records. Our results suggest that tax exposure does not measurably affect public spending or state capacity building.

These findings are in line with historians' unequivocal assessment of King Ferdinand II's regime being repressive and public spending being dismal and outright ineffective.

We then evaluate an alternative mechanism, namely that places right below the 2,000 inhabitants' threshold may be held back by the desire to avoid costly taxation, while villages already subject to taxation have bigger incentive to welcome new arrivers. We indeed find substantial evidence for this, with the population (and typically also the economy) of municipalities right above the tax threshold thriving much more than their counterparts right below. Importantly, this divergent growth path is only detected for the period of the differential tax treatment, while before and afterwards we do not find any differences in population growth above and below the threshold. Drawing on fine-grained data on housing stocks, we show that significantly more houses remained empty in untaxed villages and that differences in population growth between taxed and untaxed villages were larger for villages close in geographical proximity (i.e. providing an easy outside option).

Taken together, our results are consistent with the notion that it is not taxes, per se, but rather the threat of taxation that stirs up social unrest, since distorted incentives hamper growth and development. In terms of external validity, the current results may extend to past and current contexts where fiscal rules create major distortions in incentives, threatening growth and development. In contrast, we expect fewer –or possibly even no– adverse effects of taxation on violence in settings where the tax collection is designed in a non-distortionary way.

The remainder of the paper is organized as follows. First, Section 2.2 reviews the most related literature. Next, Section 2.3 contains a discussion of the historical context, and Section 2.4 presents the data. Section 2.5 outlines our identification strategy, and displays our baseline results, as well as a battery of robustness checks. The mechanisms at work are investigated in Section 2.6, and Section 2.7 concludes. The Appendix contains robustness checks and additional information on the data construction.

2.2 Literature Review

In terms of the existing literature, we are unaware of any other research that explores the effects of exogenous variation in tax rates on conflict. Nonetheless, a series of related literature is relevant for the current analysis. First of all, this paper draws on the general literature of the main drivers of armed conflict (see e.g. the recent literature surveys of Rohner and Thoenig (2021); Anderton and Brauer (2021); Rohner (2022)). This literature highlights the factors that may motivate groups to engage in armed conflict, such as e.g. the availability of natural resource rents that can be appropriated (see Dube and Vargas (2013); Caselli et al. (2015); Berman et al. (2017)), as well as a multitude of potential weather or commodity price shocks that affect the

opportunity cost of fighting compared to working (Miguel et al. (2004); Bazzi and Blattman (2014); Harari and Ferrara (2018); McGuirk and Burke (2020); Eberle et al. (2020)). In addition, ethnic diversity has received much attention as an important factor affecting war and peace (Montalvo and Reynal-Querol (2005); Esteban et al. (2012); Amodio and Chiovelli (2018)).

Surprisingly, the links between fiscal policy and conflict have been severely under-studied. While we are unaware of any existing study that exploits exogenous variation in taxation to investigate the impact on conflict, there is nevertheless related literature relevant to investigate the mechanisms at work. A first and obvious effect of taxation is that it triggers resistance against tax collection in some instances, which may lead to riots and unrest.[2] A series of papers have studied e.g. protests linked to the fiscal burden of the welfare state (Burg (2004), Martin and Gabay (2012)) or to austerity measures (Ponticelli and Voth (2019), Passarelli and Tabellini (2017)). Moreover, the implementation of fiscal reforms matters: Bansak et al. (2021) have shown that relative support or resistance regarding austerity programs very much depends on the mix of tax rises and spending cuts, while Garfias and Sellars (2021) have found that an increase in centralized tax collection is associated with enhanced social unrest in 17-19th century Mexico.

Second, taxes may contribute to increased state capacity which has been found to reduce conflict. In fact, a substantial amount of literature has shown weak state capacity to be correlated with political instability (Fearon and Laitin (2003), Collier et al. (2009), Besley and Persson (2010), Besley and Persson (2011), Gennaioli and Voth (2015), and Acemoglu et al. (2020)).

Third, taxation could also imply greater accountability, hence better governance. Many researchers have argued that the state's accountability and good governance are major factors for preventing popular discontent and political unrest (see e.g. Collier and Rohner (2008), Hegre and Nygård (2015)). Fourth, to the extent that tax revenues are partially used for redistribution, lower inequality can then reduce the prevalence of fighting (see Esteban et al. (2012); Gurr (2015)).

Beyond these straightforward direct mechanisms, taxation may also affect those who remain untaxed. The threat of a fiscal burden may distort economic choices, giving way to costly tax avoidance (see e.g. the discussion in Feldstein (1999)). In the context of arbitrary population thresholds for the tax base, as was the case for the income taxation in early 19th century Sicily, villages below the 2,000 citizen threshold had –at least in the short run– strategic incentives to hold back on demographic (and economic) growth to avoid being taxed. This may lead to economic slowdown and lack of growth, which are both powerful drivers of political turmoil (see e.g. Miguel et al. (2004); Dell et al. (2014); König et al. (2017)).

[2]Note that such protests against taxation have sometimes been successful and led to a tax reduction (see e.g. Steele et al. (2017) for Early Modern Japan).

Lastly, this paper is also related to the nascent literature that studies the effects of particular public policies on conflict prevention: see e.g. Cilliers et al. (2016) on reconciliation ceremonies, Mousa (2020) on multi-religious sports teams, Fetzer (2020) on workfare programs, Rohner and Saia (2019) on school construction, or Berlanda et al. (2022) on health interventions.

In a nutshell, our research is novel in various dimensions. It is –to the best of our knowledge– the first one to exploit an exogenous variation in fiscal duties to assess the impact of taxation on the risk of conflict. Furthermore, using a newly constructed dataset of civil unrest in 19th century Sicily, we carry out an analysis of the possible channels of transmission linking taxation to political stability.

2.3 Historical Background

Before the Unification of Italy, the southern part of the country, the so-called "Kingdom of the Two Sicilies" was governed by the Bourbon dynasty (1816-1861).[3] The land was formed after the unification of the Kingdom of Naples and the Kingdom of Sicily and governed by Spanish Kings. This period of Sicilian history has been characterized by popular grievances against the oppressive, unjust and arbitrary domination of outside forces. Initially acclaimed by liberals, King Ferdinand II quickly transformed into an authoritarian monarch. As pointed out by De Lorenzo (2013), the despotism of the regime, widespread corruption, incompetent administration, and –crucially– a difficult economic-financial situation fuelled popular discontent, both among the nobles, the economic bourgeois elite and the population at large. Population discontent and unrest grew stronger from unjust repression, the new kingdom's capital being relocated to Naples, but also from the abolition of the Sicilian Parliament and Constitution. The regime reacted with crackdowns, severely repressing many liberal and national revolts (including the Bandiera brothers' uprising in 1844). While populations' anger built up over several decades (and lower-scale armed contestation was frequent in the first three decades of the Kingdom of the Two Sicilies), civil unrest reached one of its highest peaks during the well-documented "Sicilian Revolution" of 1848. As documented by Venosta (1863) and De Lorenzo (2013), support for revolt stretched over various socio-economic classes of society. Widespread turmoil continued until general Giuseppe Garibaldi and his one thousand volunteers (i.e. in the "Expedition of the Thousand") conquered "The Kingdom of the Two Sicilies", incorporating this territory into the newly proclaimed Kingdom of Italy in 1861.

Our study exploits the adoption of a property tax implemented by King Ferdinand's General Parliament in 1810, six years before the unification of the two southern kingdoms.[4] The tax

[3] The discussion in this section draws on a series of historical sources, notably several entries in the Encyclopedia Britannica (www.britannica.com), as well as Horner (1860).

[4] See pages 58-65 of the Law of September 28^{th}, 1810, by the General Parliament of the Kingdom of Sicily.

was levied in Sicilian municipalities of more than 2,000 inhabitants and consisted of 5 percent of the last cadaster's urban rent.[5] The population data of 1806 was used as reference to determine which municipalities were tax exempt.[6] The property tax was widely viewed as extractive, with only a very modest part going into funding public spending and infrastructure. Indeed, as stressed in Balletta et al. (2010), during the first half of the 19th century Sicily suffered from a lack of public investment, and, as deplored by Carano-Donvito (1910), "very little was spent [for the citizens], and this was spent badly" (Carano-Donvito (1910), p.73) and there was "negligence in the expenses necessary to promote increases in wealth" (Carano-Donvito (1910), p.73). Historical accounts paint a grim picture of galloping corruption, overcrowded prisons, overpaid civil servants recruited based on cronyism and not merit, no elementary education or basic healthcare, almost inexistent media, unsafe roads, lack of street lighting and cemeteries, disenfranchised municipalities and neglected provinces (see e.g. Conte di Cavour and Nigra (1926) and Armino (2015)).

Interestingly, while turmoil and violent political protests regularly took place after the 1810 tax reform, regime opposition culminated to its worst about thirty years after. As discussed in more detail below, this is consistent with a key mechanism pinpointed by our analysis, namely that extractive and distortive taxation constrains and slows down the economic development of municipalities, thus increasing distress and discontent over time.

2.4 Data

We start from the comprehensive list of municipalities provided by "Parlamenti Generali del Regno di Sicilia" which allows us to disentangle the municipalities that did and did not pay the property tax in 1810. We also collect population data for 1748, 1806, 1861, 1871 from a variety of sources. Data from 1806, the most important for our study, was retrieved from Pagano (1952), whereas 1748 data come from Maggiore-Perni (1892), 1861 from Ministero d'Agricoltura (1864) and 1871 from Ministero d'Agricoltura (1874). We observe almost perfect compliance to the treatment (tax obligation) with respect to the 1806 census population, with only very few exceptions.[7]

[5]The property tax that we examine was levied on building owners. The tax base corresponded to the net rents of the funds after deducing the expenses for maintenance (see Dias (1856) for more details). Rural houses and other rural units intended for land cultivation, pastoralism or warehouses were exempted. Tax evasion was punished harshly: when detected, an offender would pay three times the tax on the hidden rent. The collected money would be equally distributed to the Council of the Kingdom and the accuser. Thus, citizens had incentives to inculpate their compatriots in case of evasion. Note that there also existed already a tax on feudal and allodial land of 5 percent of the land rents, which did not contain any exemption and which is not exploited in our study.

[6]See pages 61-65 from the Law of September 28^{th}, 1810 by the General Parliament of The Kingdom of Sicily.

[7]Within our data-driven bandwidth, the only municipalities (population of 1806 in brackets) with more than 2,000 inhabitants that should be paying the tax but are exempted are Saponara (2183), Savoca (2196), Santo Stefano di Camastra (2218) and Cianciana (2614). We shall in the whole baseline analysis remove these four exceptions and perform a sharp RDD. However, in Appendix B we show that our results are virtually unchanged

As discussed above, several rebellions took place on the island of Sicily against the House of Bourbon (a cadet branch of the Spanish royal family) during our period of interest. We measure social unrest using the Medal of Honor recipients for riots in 1848 and 1860, recorded in the National Archives in Palermo. Our data contains very detailed information on these recipients, which crucially includes their municipality of origin. These measures are good indicators of the extent of social unrest in various locations of Sicily, as the awarding of medals was carried out by an external commission (created by two laws in December 1860 and February 1861), reporting directly to the Italian State. This procedure was designed to exhaustively reward all citizens that proved their loyalty to the Italian Kingdom (having displayed great bravery, being injured or killed in combat) by participating in uprisings against the Bourbon's rule. The process of examining each file was extremely stringent. Our measures unlikely suffer from large measurement errors, as the aim was to exhaustively locate all citizens meeting the criteria, and –if anything– under-reporting of combat involvement would downward bias our estimates. Concerning non-classical measurement errors, there is no reason to believe that the commission would systematically accept more applications from citizens originating from control (or treated) municipalities. Still, we will carry out a series of robustness checks aimed at addressing concerns about these measurement errors.

Using the aforementioned data we code as dependent variable a binary measure of social unrest taking a value of one for municipalities having received at least one Medal of Honor for rebellion against the House of Bourbon in 1860, and zero otherwise.[8]

The standard control variables include a dummy variable indicating whether the municipality had access to a postal road that connected the largest towns of Sicily at the beginning of the 1800s (Cary (1799)), as well as a battery of controls taken from Buonanno et al. (2015): The distance of each municipality to the nearest non-seasonal river and commercial port, average terrain ruggedness in the municipality, a dummy for water scarcity, a dummy variable that indicates whether the municipality is within 10 kilometers from one of Sicily's five largest cities, land suitability for cereals, citrus fruits and olives. A further control variable used is the type of municipality, i.e. *demaniale* (royal, cities that are part of the State property) and *baronale* (baronial, subordinated to a feudatory) from Pagano (1952), p. 124-136. For additional (placebo) analyses, we also draw on data on the number of sulfur mines in the municipality in 1886 (from Buonanno et al. (2015)).

In Appendix Table 3.A1 we display descriptive summary statistics. We see that in the 1860 episode of social unrest (our main variable), exactly half of the municipalities had citizens

if we include them.

[8]We follow the approach in the conflict literature that often uses as main dependent variable a dummy of conflict incidence (see e.g. Fearon and Laitin (2003), Collier et al. (2009), Berman et al. (2017), Harari and Ferrara (2018)). Further, moving beyond a municipality-level dummy would have been extremely complicated, due to the poor condition of the archival records for individuals.

involved in the fighting, while in the social unrest period of 1848 (used in a robustness check) only 14 percent of municipalities had at least one citizen participating. As far as the tax is concerned, it was levied in 58 percent of municipalities. The table also contains summary statistics about the more than a dozen controls and additional variables.

2.5 Empirical Analysis

2.5.1 Identification Strategy

As previously mentioned, in 1810, a property tax consisting of 5% of urban rents was introduced and levied exclusively in municipalities of more than 2,000 inhabitants. We exploit this discontinuity in treatment to investigate whether the introduction of the property tax affected the likelihood of observing social unrest in the municipality, performing a Regression Discontinuity Design (RDD). Our sample consists of 338 Sicilian municipalities with population numbers ranging from 108 to 155,740. Out of these settlements, 196 (i.e. 58%) are subject to the tax.

The identification assumption required to hold for the validity of an RDD is that there is no sorting around the threshold, i.e. that there is a "smooth" distribution of municipalities in the neighborhood of the known threshold (2,000) of the forcing variable population. Figure 2.1, Panel A illustrates (using bins of 150 inhabitants) that the density in numbers of municipalities of a given size is "smooth" in the neighborhood of the threshold.[9] This is confirmed by a formal test, following Cattaneo et al. (2019).[10]

We perform a sharp RDD analysis to estimate the casual impact of taxation around the threshold of 2,000 inhabitants. The treatment effect, identified at the threshold of 2,000 inhabitants, is given by

$$E[y_{i1} - y_{i0}|X_i = 2,000] = \lim_{X_i \to 2,000^-} E[y_i|X_i = x] - \lim_{X_i \to 2,000^+} E[y_i|X_i = x] \tag{2.1}$$

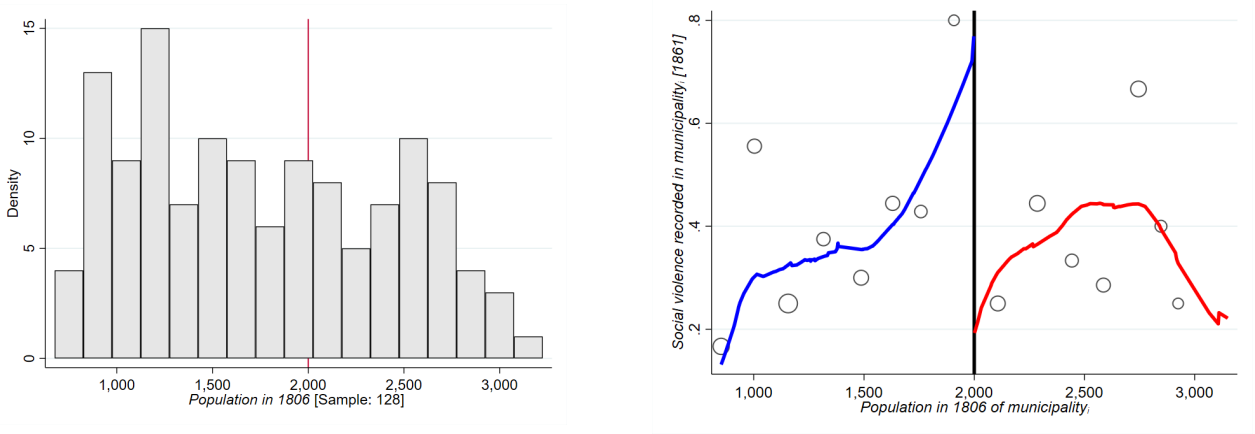
where the outcome variable y_i is social unrest in municipality i defined in Section 2.4. It can take two potential values: y_{i1} for municipality i subject to the treatment, and y_{i0} otherwise. X_i is the forcing variable, i.e. the number of inhabitants in municipality i.

We estimate non-parametrically the regression function $E[y_i|X_i = x]$ at each side of the threshold,

[9] The sample of municipalities displayed in Figure 2.1 corresponds to the sample selected by the data-driven bandwidth of our unconstrained baseline estimates. For the sake of completeness, we report the analogous figure with the density of all Sicilian municipalities in the Appendix A.

[10] According to their test, which does not require pre-binning the data, we cannot reject the null hypothesis of no difference of the population data in 1806 around the 2,000 inhabitants' cutoff (the t-stat is 0.86 for the sample of our baseline Table 2.1).

Figure 2.1: Density plot and relation between social unrest and population



A) Density of municipalities

B) Social Unrest and Population

Panel A displays the density in numbers of Sicilian municipalities in 1806 with population around the cut-off of 2,000 inhabitants. Panel B shows the lowess (Locally Weighted Scatterplot Smoothing) smoothed relationship between Medals of Honor and population in municipalities below and above 2,000 inhabitants in 1806, represented by the blue and red lines. The scatter-plot averages over intervals of 150 inhabitants and the bin size (represented by the size of the circles) is a function of the number of municipalities in the corresponding interval.

separately for control and treated municipalities.[11] In practice it is like estimating by OLS the following regression equation including only observations ranging from $2,000-h$ to $2,000+h$ inhabitants, where $h>0$ is a data-driven bandwidth, as suggested by Cattaneo et al. (2017)[12]:

$$y_i = \beta_0+\beta_1\mathbb{1}\{X_i \geq 2,000\}+\beta_2(X_i-2,000)+\beta_3\mathbb{1}\{X_i \geq 2,000\}\cdot(X_i-2,000)+\beta_3\, Q_i+\epsilon_i \quad (2.2)$$

where $\mathbb{1}\{X_i \geq 2,000\}$ is the threshold indicator function, $(X_i-2,000)$ is the continuous, normalized population measure and Q_i represents the quadratic version of the former, i.e. $Q_i=(X_i-2,000)^2+\mathbb{1}\{X_i \geq 2,000\}\cdot(X_i-2,000)^2$. The coefficient of $\mathbb{1}\{X_i \geq 2,000\}$ is the treatment effect at the cutoff.[13]

2.5.2 Main Results

In the current subsection we shall report the main baseline results of the RDD analysis. Figure 2.1, Panel B depicts graphically the main finding. On the horizontal axis, all Sicilian municipalities are ordered by size around the tax threshold, where the ones above 2,000 inhabitants have to

[11] Our baseline estimations use a second-order polynomial. The use of a quadratic regression follows Gelman and Imbens (2019). The Appendix B displays the results for a local linear regression, leading to very similar findings.

[12] This approach selects the bandwidth, taking into consideration the bias-variance trade-off, so that the mean square error of the point estimator is minimized. Section B in the Appendix presents the results for different manually-chosen bandwidths of 500, 750, 1,000 and 1,500 inhabitants.

[13] The baseline regression is weighted by a triangular kernel function $K\left(\frac{x_i-2,000}{h}\right)$. The triangular kernel function $K(x)=(1-|x|)1(|x|\leq 1)$ assigns a positive weight to the municipalities inside the interval $(2,000-h, 2,000+h)$ that declines, as their distance from the cutoff increases. Section B in the Appendix depicts the results for the unweighted regression (rectangular kernel), which are consistent with our baseline estimates.

pay the tax, while the others are exempted. On the vertical axis we display the incidence of social unrest in a given municipality (as measured by our variable on Medals of Honor from the National Archives in Palermo). In particular, it displays the lowess (Locally Weighted Scatterplot Smoothing) smoothed relationship between Medals of Honor and population around the 2,000 inhabitants' threshold. The blue and red lines display the incidence of social unrest observed in municipalities below and above 2,000 inhabitants in 1806, respectively. The bins indicate the average level of social unrest of municipalities in the corresponding population intervals (for better readability, we considered municipalities over intervals of 150 inhabitants) and the bin size (represented by the size of the circles) is a function of the number of municipalities in the corresponding interval. We find that political violence is much more prevalent in municipalities exempt from taxation compared to the more peaceful group of treated municipalities.

In what follows, we investigate in further detail this relationship, starting with baseline Table 2.1. We perform sharp RDD estimations with a data-driven bandwidth (following Cattaneo et al. (2017), as discussed above), and display the coefficient of the impact of the property tax exemption (according to 1806 population data) on social unrest in 1860, measured using a dummy variable indicating whether someone from the municipality has received a Medal of Honor in that year. This captures whether there has been at least one incident of anti-government unrest in a given municipality in the year 1860.

Column 1 displays the results of the unconstrained regression. It confirms the negative, statistically significant effect of being subject to taxation on unrest previously found above in Figure 2.1, Panel B. Quantitatively, municipalities just below the threshold of 2,000 inhabitants tend to have on average four times more episodes of social unrest compared to the municipalities just above the threshold.

Column 2 includes a battery of controls, namely a dummy variable indicating whether the municipality had access to a postal road that connected the largest towns of Sicily at the beginning of the 1800s, the distance of each municipality to the nearest non-seasonal river and commercial port, average terrain ruggedness in the municipality, a dummy for water scarcity, a dummy variable that indicates whether the municipality is within 10 kilometers from one of Sicily's five largest cities, land suitability for cereals, citrus fruits and olives and the type of municipality, i.e. *demaniale* (royal, cities that are part of the State property) and *baronale* (baronial, subordinated to a feudatory).

In the third column we add fixed effects for historical provinces (from 1806, called *valli*, there are 3), while, similarly, in column four we include fixed effects for more recent province boundaries (from 1861, there are 7). Column five includes at the same time controls and valli fixed effects, while the sixth column includes both controls and province fixed effects. In all columns we find a negative, significant effect of tax exposure on social unrest. Reassuringly, the coefficient of

interest is extremely stable across specifications.

Table 2.1: Baseline results of the effect of taxation on social unrest

Dep. Variable: Social Unrest$_i$	(1)	(2)	(3)	(4)	(5)	(6)
Tax in 1810$_i$	-0.534**	-0.656***	-0.546**	-0.563***	-0.633***	-0.684***
	(0.239)	(0.207)	(0.239)	(0.214)	(0.206)	(0.198)
Observations	128	145	137	129	153	149
Bandwidth	[846-3,154]	[685-3,315]	[774-3,226]	[839-3,161]	[622-3,378]	[657-3,343]
Controls	No	Yes	No	No	Yes	Yes
Valli FEs	No	No	Yes	No	Yes	No
Province FEs	No	No	No	Yes	No	Yes
Sample Mean	0.375	0.373	0.370	0.369	0.370	0.369

NOTES: Sharp RD Estimates of the impact of the property tax exemption (given by population data in 1806) on social unrest in 1860, measured with a dummy variable indicating whether someone from the municipality has received a Medal of Honor in that year. A data-driven bandwidth is implemented. Column 1 is the unconstrained regression. Column 2 adds the following controls: a dummy variable indicating whether the municipality had access to a postal road that connected the largest towns of Sicily at the beginning of the 1800s, the distance of each municipality to the nearest non-seasonal river and commercial port, average terrain ruggedness in the municipality, a dummy for water scarcity, a dummy variable that indicates whether the municipality is within 10 kilometers from one of Sicily's five largest cities (urban), land suitability for cereals, citrus fruits and olives and the type of municipality, i.e. *demaniale* or *baronale*. The third column adds province (from 1806, called *valli*, there are 3) fixed effects. The fourth column adds province FE (from 1861, there are 7). Robust standard errors are reported in parenthesis. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

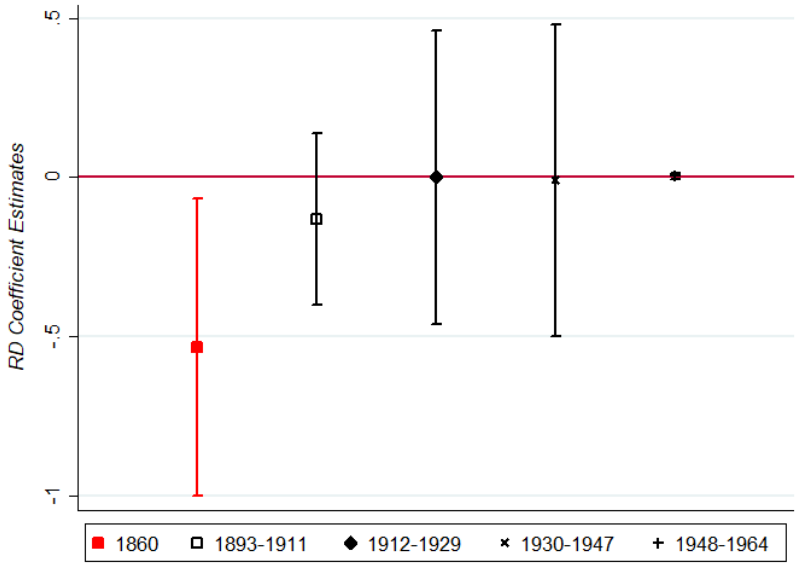
2.5.3 Robustness checks

The first robustness check that we present is a placebo analysis of our main baseline results. In particular, we report in Figure 2.2 the coefficient of our baseline estimate (in red) together with a placebo exercise (in black) where we rely on the same explanatory variables as in the baseline Table 2.1 but include a proxy of social unrest for other years, i.e. the presence of a political opponent to the government (anarchists, republicans, socialists, etc.). Every point estimate represented in the figure corresponds to a period of equal length. We see that –as expected– social unrest was only impacted during the period when the differential tax policy was in place.[14] Overall, the results displayed in Figure 2.2 provide support to the idea that the choice of the tax threshold was not driven by the notion that municipalities below 2,000 inhabitants were intrinsically more prone to violence. In the Appendix B, we provide additional variants of this placebo analysis, and we find, as above, only an effect for the period when this differentially applied tax was active, but not for any other time period.[15]

[14]Note that we have also studied whether Peasant Fasci at the end of the 19th century and mafia presence during the 20th century were affected by our unequal tax treatment in the early 19th century. In line with the results of Figure 2.2, we find no effect on these later phenomena. This is consistent with the notion that the differential tax policy's impact was limited to the period when it was in place, and that it did not generate persistent effects carrying over until today.

[15]Note that we have also collected additional data on military presence for the period before the introduction of the tax. We have replicated the baseline estimates, but using on the left hand side data on infantry and cavalry soldiers observed in the municipality in 1714 (results available upon request). We do not observe any discontinuity in military presence around the 2,000 inhabitants threshold, which is in line with the notion that municipalities below the threshold were not intrinsically more conflict-prone (and hence that the Bourbon

Figure 2.2: Placebo - Effect of taxation on social unrest after 1860 - Different time-windows



NOTES: The figure shows the coefficient estimates (and the 95% confidence intervals) from a set of unconstrained RD regressions of the impact of the property tax exemption (given by population data in 1806) on social unrest in 1860 (in red), and the presence of "subversive individuals" for different periods of equal length (in black). The coefficient estimate shown in red corresponds to that in Column 1 in Table 2.1. Social unrest data from 1860 comes from the National Archives in Palermo (Sicily), population data on 1806 comes from Pagano (1952) and data on "subversive individuals" from 1893 onwards is retrieved from the National Archives *(Archivio Centrale dello Stato)*.

A variety of further robustness checks are relegated to the Appendix. We shall briefly describe here the tests performed and results found. In particular, we implement a series of sensitivity tests linked to alternative ways of computing RDD coefficients and standard errors (Section B) and alternative definitions of the dependent variable (Section B), and we allow for other functional forms of the RD function (Section B) and manual bandwidths (Section B). In Section B we use an uniform kernel, and in Section B a fuzzy RDD is applied. Finally, in Section B we include the battery of control variables used in Acemoglu et al. (2020), while in Section B we perform sensitivity tests with respect to the geographical controls included. Our results remain statistically significant and of similar magnitude in all these sensitivity specifications.

2.6 Channels

Below we study through which channels of transmission the Sicilian property tax may have affected social unrest. We first explore different proxies for state capacity such as whether the municipality has its own municipal administration, district judge and the presence of police stations. Second, we investigate different measures of population growth as a proxy for economic progress.

Kingdom is unlikely to have strategically selected the 2,000 inhabitants threshold to minimize dissent).

2.6.1 State capacity

An obvious potential mechanism explaining why being above the tax threshold diminishes conflict could be that tax collection is associated with building up state capacity (see Tilly (2017) and Besley and Persson (2011)) which in turn helps prevent social unrest (Fearon and Laitin (2003), Collier et al. (2009), Besley and Persson (2010), Besley and Persson (2011), Gennaioli and Voth (2015), and Acemoglu et al. (2020)). While this nexus has been uncovered in several contexts, it does not seem very plausible for the Kingdom of the Two Sicilies in the early and mid 19th century, given Ferdinand II's infamously poor and wasteful public spending policies (as discussed above in Section 2.3).

In the additional material, Appendix Table 2.C1 performs an RDD analysis of the impact of being above the tax threshold on a series of indicators and proxies of state capacity. Columns 1-2 focus on a municipality having an own administration, Col. 3-4 on the (log) value of public buildings, Col. 5-6 on the existence of municipal level judiciary, Col. 7-8 on the municipality being mentioned in legal texts, and, finally Col. 9-10 on a principal component analysis of the aforementioned indicators.[16] While the coefficients found have in general a positive sign, they are in almost all cases very far from statistical significance, with the exception of the legal mention where the coefficient of tax assignment is positive and significant at the 10 percent level. Overall, this table does not provide strong support for the notion that taxation reduces social unrest trough enhanced state capacity, as was hypothesized. However, as always, the absence of evidence of an effect is not to be mistaken for evidence of absence of an effect. We cannot rule out that these non-results are partly due to noisy measures of state capacity leading to attenuation bias.

2.6.2 Population growth

An alternative mechanism explaining why occurrences of social unrest are more common in non-taxed municipalities is that these places have hampered their development (by staying below the 2000 inhabitant threshold) to escape taxation. Economic growth indicators for 19th century Sicily are sparse at the municipal level. However, this missing data on economic and demographic development can reasonably be proxied by population growth rates. In the case of stagnant municipalities – held back by the treat of taxation – population should remain approximately the same. Hence, in this subsection we investigate how the discontinuous tax

[16]Information on which municipalities have an own administration is retrieved from a chorographic dictionary with detailed information on any Sicilian municipality (De Luca (1852)); value of public buildings comes from Mortillaro (1854), that quantifies the value of buildings such as mills, industries like foundries, silk, cotton or wool manufactures for every municipality. Records on the so-called *district judges* are taken from Serristori (1842) (apart from the magistrates listed in this source, every district counted as having a single investigating judge). Using automatic text extraction algorithms, we retrieve the number of times a municipality is mentioned in over 12,000 pages of legal texts related to the period 1806-1860. The full list of legal texts used in this exercise is available in Appendix Section D.

duties have affected population growth in the municipalities subject to the tax with respect to the ones exempted from this fiscal levy.

Table 2.2 provides the main estimates with as dependent variable population growth between 1806 and 1833 (and 1806 and 1861). The same control variables are included as in the baseline Table 2.1. In all four specifications we find a strong and statistically significant positive effect of taxation on population growth. Differently put, we find that –in the neighborhood of the population threshold—tax exempt municipalities have experienced much smaller demographic development than their taxed counterparts.

Table 2.2: Channel - Effect of taxation on population growth

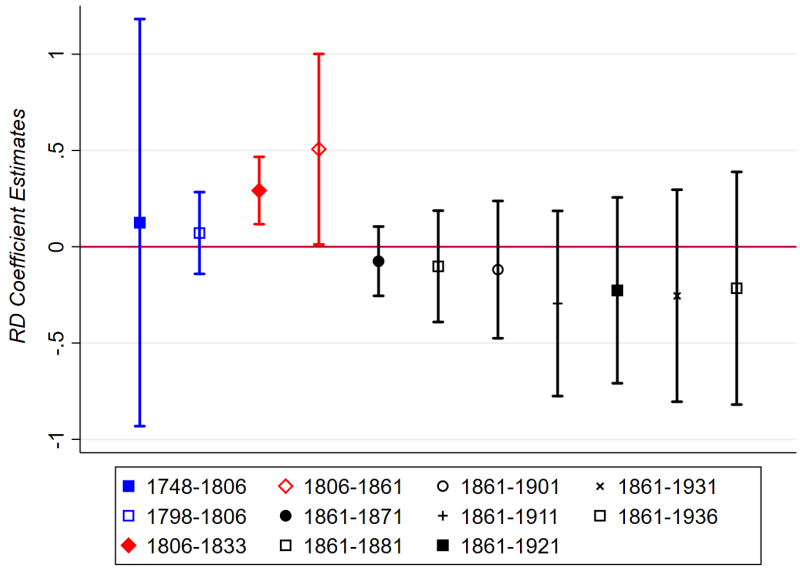
	(1)	(2)	(3)	(4)
Dep. Variable:	*Pop. Growth 1806-1833$_i$*		*Pop. Growth 1806-1861$_i$*	
Tax in 1810$_i$	0.292***	0.259***	0.507**	0.394**
	(0.0893)	(0.0680)	(0.252)	(0.193)
Observations	142	138	136	136
Bandwidth	[730-3,270]	[717-3,283]	[781-3,219]	[726-3,274]
Controls	No	Yes	No	Yes
Province FEs	No	Yes	No	Yes
Sample Mean	0.255	0.262	0.591	0.585

NOTES: Sharp RD Estimates of the impact of the property tax exemption (given by population data in 1806) on population growth 1806-1833 and 1806-1861. A data-driven bandwidth is implemented. Columns 1 and 3 show results from the unconstrained regression. Columns 2 and 4 adds province FE (from 1861, there are 7) and the following controls: a dummy variable indicating whether the municipality had access to a postal road that connected the largest towns of Sicily at the beginning of the 1800s, the distance of each municipality to the nearest non-seasonal river and commercial port, average terrain ruggedness in the municipality, a dummy for water scarcity, a dummy variable that indicates whether the municipality is within 10 kilometers from one of Sicily's five largest cities (urban), land suitability for cereals, citrus fruits and olives and the type of municipality, i.e. *demaniale* or *baronale*. Robust standard errors are reported in parenthesis. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A major robustness check for the aforementioned results is performed in Figure 2.3 where we present placebo estimation results using population growth before the introduction of the tax and after 1861 (when they removed exemption because the Kingdom of the Two Sicilies was absorbed by Italy). Again, as expected, no effect is found for these "before" and "after" periods where our differential fiscal treatment does not apply. Note that in Appendix C we carry out a further placebo analysis, finding again only an effect for the period when this differentially used tax was active, but not for any other time period. In Appendix Section C we also show that the results of Table 2.C4 are robust for using predicted instead of actual population growth.

Finally, we explore the extent to which these differential growth rates could be explained by

Figure 2.3: Placebo - Effect of taxation on population growth after 1860 - Different time-windows



NOTES: The figure shows the coefficient estimates (and the 95% confidence intervals) from a set of unconstrained RD regressions of the effect of the property tax exemption (given by population data in 1806) on population growth over different periods. Estimated coefficients shown in red corresponds to those presented in columns 1 and 3 in Table 2.2. Sources are Pagano (1952), Maggiore-Perni (1892), Ministero d'Agricoltura (1864), Ministero d'Agricoltura (1874) and Italian National Institute of Statistics (ISTAT).

internal migration patterns. It is important to keep in mind that the economic structure of Sicily in the 19th Century was overwhelmingly skewed toward the agricultural sector where the (de facto) abolition of feudalism in 1812 helped "to further strengthen the dominance of *latifundia*" (Lucchini and Fiore (1902), p.37). In such a context, the owners of vast land holdings could influence the internal movements of inhabitants and prevent the arrival of new peasants to avoid reaching the population threshold for taxation. Given that people are more likely to migrate to municipalities where they could find work, new arrivals were more likely in cities already above the threshold. There, landowners had incentive to employ more labor to boost production (among others, to compensate for the existence of the tax), while the opposite was true for municipalities below the threshold.

To provide evidence of internal migration / depopulation, we implement two separate empirical exercises. First, we draw on fine-grained data retrieved from the 1861 Census on the presence of uninhabited houses (*case vuote*) in given municipalities. The results presented in the Appendix C show a negative effect of being taxed on the number (and incidence) of uninhabited houses, meaning that taxed municipalities have in general a lower amount of uninhabited houses.[17] Second, we link the distance between municipalities subject versus not subject to taxation to

[17]This finding is consistent with the notion that landowners from taxed municipalities may be keen to increase production in order to compensate for the fiscal duties on their own houses. For this purpose, they may want to hire more workers, which may trigger migration from exempted municipalities to non-exempted ones. In contrast, the incentives differ for landowners from exempted municipalities, who may want to contain population numbers for the sake of escaping additional tax duties. These internal migration incentives may be part of the explanation of why more houses remained empty in the exempted municipalities, compared to the taxed ones.

differences in their population growth rates. A shorter distance implies that an easy outside option is available, which may increase incentive and ease to channel migration flows away from municipalities below the taxation threshold. The results are presented in Appendix C. We document a negative effect of distance on growth rate differentials: more precisely, the smaller the distance between paying and non-tax paying municipalities, the larger is the differential in population growth observed after the introduction of the tax. Overall, both empirical exercises suggest that the tax treatment may impact internal migration patterns and, ultimately, economic growth.[18]

2.7 Conclusion

In this paper we have exploited the 1810 Sicilian municipality property tax *(imposta fondiaria)* to provide novel evidence about the impact of taxation on social unrest. To do so we collected novel historical data on population and Medal of Honor recipients for riots in 1848 and 1860, among others.

We find that taxation has a statistically significant, quantitatively sizeable impact on reducing the prevalence of social unrest (as paying the tax reduces the baseline social unrest risk by more than half). In terms of channels of transmission, we fail to detect an impact of taxation on increasing state capacity and fostering public investments but discover that municipalities right below the threshold experience much reduced demographic (and typically economic) growth compared to municipalities right above the tax threshold. These findings are consistent with the notion that it is not fiscal levies, per se, but rather the threat of taxation that leads to distorted incentives slowing down growth and development – which in turn can fuel unrest.

While the current study is arguably the first one to exploit an exogenous variation in tax exposure to investigate drivers of social violence, the generalizability of results is limited to contexts of ill-designed, distortionary taxation. Hence, various fruitful avenues for future research remain under-explored. First of all, it would be very worthwhile to set up a formal contest model that incorporates several countervailing forces, such as the benefits of fostered state capacity and accountability on the one hand, versus a potentially greater scope for tax-related grievances and unrest on the other hand. The relative magnitude of these forces typically depends on the exact design of the tax system and scope for incentive distortions. A natural, related research question to explore empirically is the causal impact of taxation on unrest in a setting of a well-crafted

[18] A further potential channel that could explain differential patterns of population growth could be related to fertility patterns. The tax treatment may also have had an impact on household fertility decisions. Unfortunately, we were not able to find precise fertility data for villages in Sicily in the early 19th century. We have computed, however, two proxies for fertility behaviour in a given municipality: (i) average family size (calculated as the ratio of the overall population divided by the number of families in 1861) and (ii) the ratio of unmarried men over the population of 1861. Overall, we fail to detect conclusive evidence in support of the hypothesis that the introduction of the tax treatment had an effect on household fertility decisions (see Appendix Table 2.C9).

fiscal system where tax revenues are used for building up infrastructure and state capacity. This may be able to highlight a potentially multifaceted picture of the impact of levying higher taxes. Further, it would be useful to investigate the relative importance of communicating effectively how taxes are used. If the population is exposed to a credible signal that tax incomes are well-spent and not wasted, the scope for unrest is attenuated. Putting in place randomized control trials related to tax communication and accountability appear particularly promising. Last but not least, another very relevant avenue for future work is the study of the trade-offs between different means of opposing taxation, such as e.g. revolting versus tax evasion.

Bibliography

Acemoglu, D., De Feo, G., and De Luca, G. (2020). Weak states: Causes and consequences of the Sicilian mafia. Review of Economic Studies, 87(2):537–581.

Amodio, F. and Chiovelli, G. (2018). Ethnicity and violence during democratic transitions: Evidence from south africa. Journal of the European Economic Association, 16(4):1234–1280.

Anderton, C. H. and Brauer, J. (2021). Mass atrocities and their prevention. Journal of Economic Literature, 59(4):1240–92.

Armino, P. (2015). Brigantaggio politico nelle Due Sicilie. Condizioni socio-economiche del regno di Napoli e storia dei movimenti reazionari contro l'unità italiana. Città del Sole Edizioni.

Balletta, F. et al. (2010). Il pensiero e l'opera di Domenico Demarco. FrancoAngeli.

Bansak, K., Bechtel, M. M., and Margalit, Y. (2021). Why austerity? the mass politics of a contested policy. American Political Science Review, 115(2):486–505.

Bazzi, S. and Blattman, C. (2014). Economic shocks and conflict: Evidence from commodity prices. American Economic Journal: Macroeconomics, 6(4):1–38.

Berlanda, A., Cervellati, M., Esposito, E., Rohner, D., and Sunde, U. (2022). Medication against conflict. medRxiv.

Berman, N., Couttenier, M., Rohner, D., and Thoenig, M. (2017). This mine is mine! how minerals fuel conflicts in africa. American Economic Review, 107(6):1564–1610.

Besley, T. and Persson, T. (2010). State capacity, conflict, and development. Econometrica, 78(1):1–34.

Besley, T. and Persson, T. (2011). Pillars of prosperity: The political economics of development clusters. Princeton University Press.

Buonanno, P., Durante, R., Prarolo, G., and Vanin, P. (2015). Poor institutions, rich mines: Resource curse in the origins of the sicilian mafia. The Economic Journal, 125(586):F175–F202.

Burg, D. F. (2004). A world history of tax rebellions: An encyclopedia of tax rebels, revolts, and riots from antiquity to the present. Routledge.

Carano-Donvito, G. (1910). Il tributo fondiario nell'ex Regno delle due Sicilie nei rapporti con le condizioni delle classi rurali. Rivista popolare di politica, lettere e scienze sociali.

Cary, J. (1799). A new map of Italy, including the islands of Sicily, Sardinia and Corsica with the post roads from the latest authorities. Cary Engraver and Map-seller.

Caselli, F., Morelli, M., and Rohner, D. (2015). The geography of interstate resource wars. The Quarterly Journal of Economics, 130(1):267–315.

Cattaneo, M. D., Idrobo, N., and Titiunik, R. (2017). A practical introduction to regression discontinuity designs. Cambridge Elements: Quantitative and Computational Methods for Social Science-Cambridge University Press I.

Cattaneo, M. D., Jansson, M., and Ma, X. (2019). Simple local polynomial density estimators. Journal of the American Statistical Association, pages 1–7.

Cilliers, J., Dube, O., and Siddiqi, B. (2016). Reconciling after civil conflict increases social capital but decreases individual well-being. Science, 352(6287):787–794.

Collier, P., Hoeffler, A., and Rohner, D. (2009). Beyond greed and grievance: feasibility and civil war. Oxford Economic Papers, 61(1):1–27.

Collier, P. and Rohner, D. (2008). Democracy, development, and conflict. Journal of the European Economic Association, 6(2-3):531–540.

Conte di Cavour, C. B. and Nigra, C. (1926). Il carteggio dal 1858 al 1861.

De Lorenzo, R. (2013). Borbonia felix: il regno delle Due Sicilie alla vigilia del crollo. Salerno Rome.

De Luca, F. (1852). Dizionario corografico-universale dell'Italia. Stabilimento di Civelli Giuseppe e comp., 4(2).

Dell, M., Jones, B. F., and Olken, B. A. (2014). What do we learn from the weather? the new climate-economy literature. Journal of Economic Literature, 52(3):740–98.

Dias, F. (1856). Amministrazione finanziera del regno delle due sicilie: Esposta secondo i principi fondamentali di pubblica economia e secondo le leggi, i decreti ed i regolamenti in vigore. presso G. Pellizzone, 1(6):383–388.

Dube, O. and Vargas, J. F. (2013). Commodity price shocks and civil conflict: Evidence from colombia. The review of economic studies, 80(4):1384–1421.

Eberle, U. J., Rohner, D., and Thoenig, M. (2020). Heat and hate: Climate security and farmer-herder conflicts in africa. CEPR Discussion Paper 15542.

Esteban, J., Mayoral, L., and Ray, D. (2012). Ethnicity and conflict: An empirical study. American Economic Review, 102(4):1310–42.

Fearon, J. D. and Laitin, D. D. (2003). Ethnicity, insurgency, and civil war. American Political Science Review, 97(1):75–90.

Feldstein, M. (1999). Tax avoidance and the deadweight loss of the income tax. Review of Economics and Statistics, 81(4):674–680.

Fetzer, T. (2020). Can workfare programs moderate conflict? evidence from india. Journal of the European Economic Association, 18(6):3337–3375.

Garfias, F. and Sellars, E. A. (2021). When state building backfires: Elite coordination and popular grievance in rebellion. American Journal of Political Science.

Gelman, A. and Imbens, G. (2019). Why high-order polynomials should not be used in regression discontinuity designs. Journal of Business & Economic Statistics, 37(3):447–456.

Gennaioli, N. and Voth, H.-J. (2015). State capacity and military conflict. The Review of Economic Studies, 82(4):1409–1448.

Gurr, T. R. (2015). Why men rebel. Routledge.

Harari, M. and Ferrara, E. L. (2018). Conflict, climate, and cells: a disaggregated analysis. Review of Economics and Statistics, 100(4):594–608.

Hegre, H. and Nygård, H. M. (2015). Governance and conflict relapse. Journal of Conflict Resolution, 59(6):984–1016.

Hobbes, T. (2016). Leviathan. Routledge.

Horner, S. (1860). A Century of Despotism in Naples and Sicily. Edmonston and Douglas.

König, M. D., Rohner, D., Thoenig, M., and Zilibotti, F. (2017). Networks in conflict: Theory and evidence from the great war of Africa. Econometrica, 85(4):1093–1132.

La Porta, R. and Shleifer, A. (2014). Informality and development. Journal of Economic Perspectives, 28(3):109–26.

Lucchini, L. and Fiore, P. (1902). Il Digesto italiano, volume 14. Unione Tipografico-Editrice.

Maggiore-Perni, F. (1892). La popolazione di Sicilia e di Palermo dal X al XVIII secolo. Stab. tip. Virzi.

Martin, I. W. and Gabay, N. (2012). Fiscal protest in thirteen welfare states. Socio-Economic Review, 11(1):107–130.

McGuirk, E. and Burke, M. (2020). The economic origins of conflict in africa. Journal of Political Economy, 128(10):3940–3997.

Miguel, E., Satyanath, S., and Sergenti, E. (2004). Economic shocks and civil conflict: An instrumental variables approach. Journal of political Economy, 112(4):725–753.

Ministero d'Agricoltura, I. e. C. (1864). Statistica del Regno d'Italia. Censimento Generale. Popolazione.

Ministero d'Agricoltura, I. e. C. (1874). Statistica del Regno d'Italia. Movimento dello Stato civile nell'anno 1871.

Montalvo, J. G. and Reynal-Querol, M. (2005). Ethnic polarization, potential conflict, and civil wars. American economic review, 95(3):796–816.

Mortillaro, V. (1854). Notizie economico-statistiche ricavate sui catasti di Sicilia.

Mousa, S. (2020). Building social cohesion between christians and muslims through soccer in post-isis iraq. Science, 369(6505):866–870.

Pagano, L. A. (1952). La popolazione del Regno di Sicilia nell'anno 1806 secondo un censimento inedito. Rivista italiana di economia, demografia e statistica : organo della Società Italiana di Economia, Demografia e Statistica, 6:107–137.

Passarelli, F. and Tabellini, G. (2017). Emotions and political unrest. Journal of Political Economy, 125(3):903–946.

Ponticelli, J. and Voth, H.-J. (2019). Austerity and anarchy: Budget cuts and social unrest in Europe, 1919–2008. Journal of Comparative Economics.

Rohner, D. (2022). Mediation, military and money: The promises and pitfalls of outside interventions to end armed conflicts. Journal of Economic Literature.

Rohner, D. and Saia, A. (2019). Education and conflict: Evidence from a policy experiment in Indonesia. Working Paper.

Rohner, D. and Thoenig, M. (2021). The elusive peace dividend of development policy: From war traps to macro complementarities. Annual Review of Economics, 13(1):111–131.

Serristori, L. (1842). Statistica dell'Italia. Stamperia granducale.

Steele, A., Paik, C., and Tanaka, S. (2017). Constraining the samurai: Rebellion and taxation in early modern japan. International Studies Quarterly, 61(2):352–370.

Tilly, C. (2017). Coercion, capital, and European states, ad 990–1990. In Collective Violence, Contentious Politics, and Social Change, pages 140–154. Routledge.

Venosta, F. (1863). Rosolino Pilo e la rivoluzione siciliana 1863. Barbini, Carlo.

Weber, M. (2013). From Max Weber: essays in sociology. Routledge.

Appendix

In the Appendices below we provide additional information and further results for the various sections of the paper. We shall always start the title of a given Appendix section with the same wording as the corresponding section in the main text.

In particular, we will start off with focusing in Appendix A on descriptive summary statistics, then in Appendix B on investigating the sensitivity of the main baseline results before assessing below in Appendix C the robustness of our results on channels of transmission. Finally, in Appendix D we provide a list of legal texts that serve as basis for coding the state capacity measures.

A Data

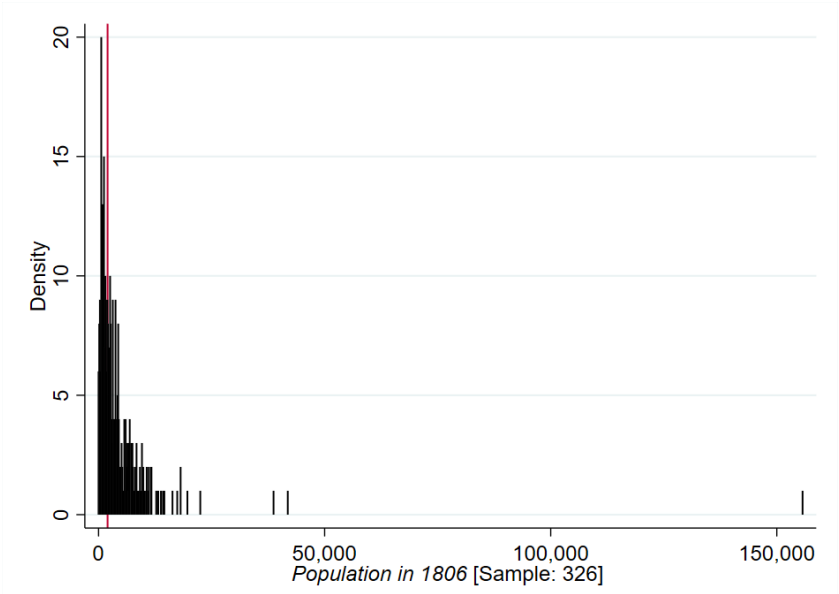
Descriptive summary statistics

Below in Table 3.A1 we display descriptive summary statistics of all main variables (see discussion in the main text).

Density plot using all Sicilian municipalities

Figure 2.A1 displays the density plot obtained using all Sicilian municipalities in our sample.

Figure 2.A1: Density plot



Notes: The figure displays the density in numbers of all Sicilian municipalities in 1806.

Table 2.A1: Summary Statistics

Variable	Mean	Std. Dev.	Min.	Max.	N
Social Unrest Variables					
Social Unrest in 1860	0.497	0.501	0	1	338
Social Unrest in 1848	0.139	0.347	0	1	338
Social Unrest [1894-1945]	0.669	0.471	0	1	338
Tax Variable					
Tax in 1810	0.58	0.494	0	1	338
Population Variables					
Population in 1748	4,772.841	15,348.585	81	200,096	296
Population in 1798	5,207.429	12,328.246	179	201,741	310
Population in 1806	4,537.601	9,465.844	108	155,740	338
Population in 1833	5,537.804	11,340.95	76	172,835	336
Population in 1861	6,756.086	13,266.985	125	194,463	336
Population in 1871	7,620.66	15,555.46	387	223,689	309
Population in 1881	8,606.48	17,413.17	413	244,898	309
Population in 1901	10,435.12	22,246.23	463	309,566	309
Population in 1911	11,174.71	24,840.06	501	339,465	309
Population in 1921	12,407.25	29,820.29	2,606	397,486	309
Population in 1931	11,365.58	27,893.77	5,752	379,905	309
Population in 1936	11,632.02	3,0001.3	470	411,879	309
State Capacity Variables					
Own administration	0.614	0.488	0	1	308
(log) Value Public Building	6.431	1.776	0	10.587	288
Judge seat	0.389	0.488	0	1	319
(log) Munic. in legal texts	1.291	1.01	0	3.219	338
Main Control Variables					
Postal road in 1799	0.524	0.5	0	1	319
Distance to closest river in km	10.177	7.754	0.552	42.075	319
Distance to closest port in km	37.936	20.123	0.132	83.919	319
Water scarcity	0.624	0.485	0	1	319
Urban Municipality	0.147	0.355	0	1	319
Munic. type: baronale (1) or demaniale (2)	1.143	0.350	1	2	336
Average land suitability for cereals	16.496	10.728	1.49	62.585	318
Average land suitability for citrus	14.812	7.418	0	42.533	318
Average land suitability for olive	29.244	12.827	0	69.273	318
Average terrain ruggedness	227.096	112.334	27.619	578.288	319

Notes: For data sources see main text.

B Robustness Checks For Main Results

Placebo for baseline results

Below we present in Table 2.B1 placebo estimations using data on subversive activities (social unrest) for the period after 1894. This alternative measure, which started to be collected after the passing of *circolare* 5116 on 25 May 1894 related to public security, is a similar measure of social unrest as our baseline measure of medals of honor. It quantifies the number of political opponents (anarchists, republicans, socialists, etc.) that might be considered dangerous to public security. The National Archives (Archivio Centrale dello Stato) contain more than 150,000 personal files with informative notes, interrogatories, police files, among others. While information starts being collected from 1878 onwards, the biggest part of the data spans over the 1894 to 1945 period. In the following Table 2.B2 we carry out the same exercise, but we divide the sample in four periods of equal length. Both tables taken together, as expected, no effect is found for periods for which there is no more differential fiscal treatment.

Table 2.B1: Placebo - Effect of taxation on social unrest after 1860

Dep. Variable: Social Unrest [1894-1945]$_i$	(1)	(2)	(3)	(4)	(5)	(6)
Tax in 1810$_i$	0.00193	-0.135	-0.120	-0.0733	-0.162	-0.0318
	(0.247)	(0.226)	(0.234)	(0.224)	(0.229)	(0.228)
Observations	139	136	133	142	130	141
Bandwidth	[767-3,233]	[745-3,255]	[819-3,181]	[727-3,273]	[793-3,207]	[711-3,289]
Controls	No	Yes	No	No	Yes	Yes
Valli FEs	No	No	Yes	No	Yes	No
Province FEs	No	No	No	Yes	No	Yes
Sample Mean	0.597	0.596	0.604	0.594	0.607	0.591

NOTES: Sharp RD Estimates of the impact of the property tax exemption (given by population data in 1806) on a dummy variable indicating whether there are individuals living in that municipality considered to be dangerous for the order and public security over the period 1894-1945. A data-driven bandwidth is implemented. Column 1 is the unconstrained regression. Column 2 adds the following controls: a dummy variable indicating whether the municipality had access to a postal road that connected the largest towns of Sicily at the beginning of the 1800s, the distance of each municipality to the nearest non-seasonal river and commercial port, average terrain ruggedness in the municipality, a dummy for water scarcity, a dummy variable that indicates whether the municipality is within 10 kilometers from one of Sicily's five largest cities (urban), land suitability for cereals, citrus fruits and olives, the type of municipality, i.e. *demaniale* or *baronale* and the number of sulfur mines in the municipality (1886). The third column adds province (from 1806, called *valli*, there are 3) fixed effects. The fourth column adds province FE (from 1861, there are 7). Robust standard errors in parentheses. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.B2: Placebo - Effect of taxation on social unrest after 1860 - Different time-windows

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Variable: Social Unrest$_i$	*1894-1911$_i$*		*1912-1929$_i$*		*1930-1947$_i$*		*1948-1964$_i$*	
Tax in 1810$_i$	-0.131	-0.145*	0.000124	-0.123	-0.00998	-0.0406	0.00597	-0.00205
	(0.137)	(0.0779)	(0.236)	(0.194)	(0.249)	(0.230)	(0.00502)	(0.00879)
Observations	134	156	120	114	135	143	270	231
Bandwidth	[808-3,192]	[592-3,408]	[909-3,091]	[919-3,081]	[788-3,212]	[703-3,297]	[2,979-6,979]	[1,952-5,952]
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Province FEs	No	Yes	No	Yes	No	Yes	No	Yes
Sample Mean	0.045	0.048	0.108	0.102	0.600	0.583	0.007	0.004

Notes: Sharp RD Estimates of the impact of the property tax exemption (given by population data in 1806) on a dummy variable indicating whether there are individuals living in that municipality considered to be dangerous for the order and public security over different periods. A data-driven bandwidth is implemented. Odd columns display results from unconstrained regressions. Even columns display results obtained with the inclusion of province FE (from 1861, there are 7) and the following controls: a dummy variable indicating whether the municipality had access to a postal road that connected the largest towns of Sicily at the beginning of the 1800s, the distance of each municipality to the nearest non-seasonal river and commercial port, average terrain ruggedness in the municipality, a dummy for water scarcity, a dummy variable that indicates whether the municipality is within 10 kilometers from one of Sicily's five largest cities (urban), land suitability for cereals, citrus fruits and olives and the type of municipality, i.e. *demaniale* or *baronale*. Robust standard errors are reported in parenthesis. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Alternative RDD methods for computing coefficients and standard errors

Appendix Table 2.B3 below displays for the baseline results alternative methods for computing the RDD coefficients and standard errors. The first set of coefficients correspond to our baseline estimate (conventional RDD estimates with a conventional variance estimator). The second and third sets of coefficients correspond to bias-corrected RDD estimates with a conventional variance estimator and bias-corrected estimates with a robust variance estimator, respectively. It turns out that our results are very similar, no matter which of these methods we apply.

Table 2.B3: Robustness coefficients/s.e. - Effect of taxation on social unrest

Dep. Variable: Social Unrest$_i$	(1)	(2)	(3)	(4)	(5)	(6)
Conventional	-0.534**	-0.656***	-0.546**	-0.563***	-0.633***	-0.684***
	(0.239)	(0.207)	(0.239)	(0.214)	(0.206)	(0.198)
Bias-corrected	-0.602**	-0.792***	-0.629***	-0.616***	-0.742***	-0.816***
	(0.239)	(0.207)	(0.239)	(0.214)	(0.206)	(0.198)
Robust	-0.602**	-0.792***	-0.629**	-0.616***	-0.742***	-0.816***
	(0.263)	(0.222)	(0.267)	(0.239)	(0.227)	(0.221)
Observations	128	145	137	129	153	149
Bandwidth	[846-3,154]	[685-3,315]	[774-3,226]	[839-3,161]	[622-3,378]	[657-3,343]
Controls	No	Yes	No	No	Yes	Yes
Valli FEs	No	No	Yes	No	Yes	No
Province FEs	No	No	No	Yes	No	Yes
Sample Mean	0.375	0.373	0.370	0.369	0.370	0.369

NOTES: Sharp RD Estimates of the impact of the property tax exemption (given by population data in 1806) on social unrest in 1860, measured with a dummy variable indicating whether someone from the municipality has received a Medal of Honor in that year. A data-driven bandwidth is implemented. Column 1 is the unconstrained regression. Column 2 adds the following controls: a dummy variable indicating whether the municipality had access to a postal road that connected the largest towns of Sicily at the beginning of the 1800s, the distance of each municipality to the nearest non-seasonal river and commercial port, average terrain ruggedness in the municipality, a dummy for water scarcity, a dummy variable that indicates whether the municipality is within 10 kilometers from one of Sicily's five largest cities (urban), land suitability for cereals, citrus fruits and olives and the type of municipality, i.e. *demaniale* or *baronale*. The third column adds province (from 1806, called *valli*, there are 3) fixed effects. The fourth column adds province FE (from 1861, there are 7). Robust standard errors are reported in parenthesis. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Alternative outcomes

Table 2.B4 follows the structure of Table 2.1, but replaces the dependent variable by a proxy of social unrest in 1848. We have relegated this alternative dependent variable to the appendix, as the insurrection that took place in 1848 was less widespread than the one occurring in 1860. In fact, around 13% of the Sicilian municipalities saw someone taking part of the 1848 insurrections, compared to almost half of the municipalities in 1860. The results are similar and continue to be statistically significant in most columns. We then carry on in Tables 2.B5 and 2.B6 to use as dependent variable a combination of the two instances of social unrest, both as count variable (Table 2.B5) and as dummy (Table 2.B6). Again, the results are very similar, and we always detect a statistically significant negative effect.

Table 2.B4: Robustness - Effect of taxation on social unrest in 1848

Dep. Variable: Social Unrest [1848]$_i$	(1)	(2)	(3)	(4)	(5)	(6)
Tax in 1810$_i$	-0.228	-0.231*	-0.266	-0.295**	-0.232*	-0.222**
	(0.191)	(0.124)	(0.171)	(0.135)	(0.123)	(0.0987)
Observations	109	133	110	113	134	136
Bandwidth	[1,041-2,959]	[771-3,229]	[1,006-2,994]	[939-3,061]	[763-3,237]	[727-3,273]
Controls	No	Yes	No	No	Yes	Yes
Valli FEs	No	No	Yes	No	Yes	No
Province FEs	No	No	No	Yes	No	Yes
Sample Mean	0.055	0.043	0.054	0.053	0.043	0.042

NOTES: Sharp RD Estimates of the impact of the property tax exemption (given by population data in 1806) on social unrest in 1848, measured with a dummy variable indicating whether someone from the municipality has received a Medal of Honor in that year. A data-driven bandwidth is implemented. Column 1 is the unconstrained regression. Column 2 adds the following controls: a dummy variable indicating whether the municipality had access to a postal road that connected the largest towns of Sicily at the beginning of the 1800s, the distance of each municipality to the nearest non-seasonal river and commercial port, average terrain ruggedness in the municipality, a dummy for water scarcity, a dummy variable that indicates whether the municipality is within 10 kilometers from one of Sicily's five largest cities (urban), land suitability for cereals, citrus fruits and olives and the type of municipality, i.e. *demaniale* or *baronale*. The third column adds province (from 1806, called *valli*, there are 3) fixed effects. The fourth column adds province FE (from 1861, there are 7). Robust standard errors are reported in parenthesis. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.B5: Robustness - Effect of taxation on social unrest in 1848 and 1860 - Intensive margin

Dep. Variable: Tot. Social Unrest [1848-1860]$_i$	(1)	(2)	(3)	(4)	(5)	(6)
Tax in 1810$_i$	-0.787**	-0.878***	-0.810***	-0.839***	-0.868***	-0.875***
	(0.327)	(0.253)	(0.310)	(0.238)	(0.259)	(0.232)
Observations	114	143	123	133	145	151
Bandwidth	[953-3,047]	[695-3,305]	[875-3,125]	[809-3,191]	[683-3,317]	[633-3,367]
Controls	No	Yes	No	No	Yes	Yes
Valli FEs	No	No	Yes	No	Yes	No
Province FEs	No	No	No	Yes	No	Yes
Sample Mean	0.447	0.424	0.427	0.418	0.418	0.419

NOTES: Sharp RD Estimates of the impact of the property tax exemption (given by population data in 1806) on social unrest in 1848 and 1860, measured with a 0/1/2 variable indicating whether someone from the municipality has received a Medal of Honor in none, one or two of the revolutions, respectively. A data-driven bandwidth is implemented. Column 1 is the unconstrained regression. Column 2 adds the following controls: a dummy variable indicating whether the municipality had access to a postal road that connected the largest towns of Sicily at the beginning of the 1800s, the distance of each municipality to the nearest non-seasonal river and commercial port, average terrain ruggedness in the municipality, a dummy for water scarcity, a dummy variable that indicates whether the municipality is within 10 kilometers from one of Sicily's five largest cities (urban), land suitability for cereals, citrus fruits and olives and the type of municipality, i.e. *demaniale* or *baronale*. The third column adds province (from 1806, called *valli*, there are 3) fixed effects. The fourth column adds province FE (from 1861, there are 7). Robust standard errors are reported in parenthesis. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.B6: Robustness - Effect of taxation on social unrest in 1848 and 1860 - Extensive margin

Dep. Variable: Social Unrest$_i$	(1)	(2)	(3)	(4)	(5)	(6)
Tax in 1810$_i$	-0.548**	-0.699***	-0.572**	-0.591***	-0.677***	-0.743***
	(0.239)	(0.209)	(0.242)	(0.212)	(0.206)	(0.201)
Observations	129	136	135	132	148	144
Bandwidth	[841-3,159]	[731-3,269]	[784-3,216]	[825-3,175]	[660-3,340]	[693-3,307]
Controls	No	Yes	No	No	Yes	Yes
Valli FEs	No	No	Yes	No	Yes	No
Province FEs	No	No	No	Yes	No	Yes
Sample Mean	0.380	0.378	0.382	0.376	0.378	0.382

NOTES: Sharp RD Estimates of the impact of the property tax exemption (given by population data in 1806) on social unrest in 1848 and 1860, measured with dummy variable indicating whether someone from the municipality has received a Medal of Honor for at least one of the two revolutions. A data-driven bandwidth is implemented. Column 1 is the unconstrained regression. Column 2 adds the following controls: a dummy variable indicating whether the municipality had access to a postal road that connected the largest towns of Sicily at the beginning of the 1800s, the distance of each municipality to the nearest non-seasonal river and commercial port, average terrain ruggedness in the municipality, a dummy for water scarcity, a dummy variable that indicates whether the municipality is within 10 kilometers from one of Sicily's five largest cities (urban), land suitability for cereals, citrus fruits and olives and the type of municipality, i.e. *demaniale* or *baronale*. The third column adds province (from 1806, called *valli*, there are 3) fixed effects. The fourth column adds province FE (from 1861, there are 7). Robust standard errors are reported in parenthesis. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Alternative Polynomials

Below in Table 2.B7 we replicate our main results of Table 2.1, but focus on a polynomial of order 1. The results are virtually unchanged.

Table 2.B7: Robustness polynomials - Effect of taxation on social unrest

Dep. Variable: Social Unrest$_i$	(1)	(2)	(3)	(4)	(5)	(6)
Tax in 1810$_i$	-0.434**	-0.464***	-0.432**	-0.400**	-0.483***	-0.456***
	(0.215)	(0.179)	(0.211)	(0.172)	(0.173)	(0.162)
Observations	82	107	85	100	107	114
Bandwidth	[1,270- 2,730]	[1,038- 2,962]	[1,244- 2,756]	[1,114- 2,886]	[1,034- 2,966]	[916- 3,084]
Controls	No	Yes	No	No	Yes	Yes
Valli FEs	No	No	Yes	No	Yes	No
Province FEs	No	No	No	Yes	No	Yes
Sample Mean	0.415	0.404	0.395	0.406	0.404	0.378

NOTES: Sharp RD Estimates of the impact of the property tax exemption (given by population data in 1806) on social unrest in 1860, measured with a dummy variable indicating whether someone from the municipality has received a Medal of Honor in that year. A data-driven bandwidth is implemented. Column 1 is the unconstrained regression. Column 2 adds the following controls: a dummy variable indicating whether the municipality had access to a postal road that connected the largest towns of Sicily at the beginning of the 1800s, the distance of each municipality to the nearest non-seasonal river and commercial port, average terrain ruggedness in the municipality, a dummy for water scarcity, a dummy variable that indicates whether the municipality is within 10 kilometers from one of Sicily's five largest cities (urban), land suitability for cereals, citrus fruits and olives and the type of municipality, i.e. *demaniale* or *baronale*. The third column adds province (from 1806, called *valli*, there are 3) fixed effects. The fourth column adds province FE (from 1861, there are 7). Robust standard errors in parentheses. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Manual bandwidths

In Table 2.B8 we replicate our results of columns 4 and 6 of Table 2.1 when using a series of manual bandwidths, yielding again very similar findings.

Table 2.B8: Robustness bandwidths - Effect of taxation on social unrest

Dep. Variable: Social Unrest$_i$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Tax in 1810$_i$	-0.712**	-1.293***	-0.581**	-0.693***	-0.571**	-0.739***	-0.442**	-0.628***
	(0.284)	(0.203)	(0.265)	(0.220)	(0.247)	(0.210)	(0.221)	(0.194)
Observations	52	50	86	84	112	109	169	158
Bandwidth	[1,500-2,500]	[1,500-2,500]	[1,250-2,750]	[1,250-2,750]	[1,000-3,000]	[1,000-3,000]	[500-3,500]	[500-3,500]
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Valli FEs	No	No	No	No	No	No	No	No
Province FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample Mean	0.423	0.423	0.395	0.395	0.402	0.402	0.379	0.379

NOTES: Sharp RD Estimates of the impact of the property tax exemption (given by population data in 1806) on social unrest in 1860, measured with a dummy variable indicating whether someone from the municipality has received a Medal of Honor in that year. Manual bandwidths are implemented. Even columns display the unconstrained regression. Odd columns adds province FE (from 1861, there are 7) and the following controls: a dummy variable indicating whether the municipality had access to a postal road that connected the largest towns of Sicily at the beginning of the 1800s, the distance of each municipality to the nearest non-seasonal river and commercial port, average terrain ruggedness in the municipality, a dummy for water scarcity, a dummy variable that indicates whether the municipality is within 10 kilometers from one of Sicily's five largest cities (urban), land suitability for cereals, citrus fruits and olives and the type of municipality, i.e. *demaniale* or *baronale*. Robust standard errors in parentheses. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Uniform Kernel

The next robustness check that we perform below is to make use of an uniform Kernel. Table 2.B9 displays the results. It turns out that our findings are very similar as in the baseline Table 2.1 and we continue to find a statistically significant negative coefficient.

Table 2.B9: Robustness kernel - Effect of taxation on social unrest

Dep. Variable: Social Unrest$_i$	(1)	(2)	(3)	(4)	(5)	(6)
Tax in 1810$_i$	-0.605**	-0.742***	-0.576**	-0.480*	-0.670***	-0.690***
	(0.253)	(0.241)	(0.265)	(0.252)	(0.241)	(0.250)
Observations	101	98	99	76	99	90
Bandwidth	[1,106-2,894]	[1,124-2,876]	[1,130-2,870]	[1,288-2,712]	[1,115-2,885]	[1,190-2,810]
Controls	No	Yes	No	No	Yes	Yes
Valli FEs	No	No	Yes	No	Yes	No
Province FEs	No	No	No	Yes	No	Yes
Sample Mean	0.406	0.400	0.400	0.416	0.406	0.391

NOTES: Sharp RD Estimates of the impact of the property tax exemption (given by population data in 1806) on social unrest in 1860, measured with a dummy variable indicating whether someone from the municipality has received a Medal of Honor in that year. A data-driven bandwidth is implemented. Column 1 is the unconstrained regression. Column 2 adds the following controls: a dummy variable indicating whether the municipality had access to a postal road that connected the largest towns of Sicily at the beginning of the 1800s, the distance of each municipality to the nearest non-seasonal river and commercial port, average terrain ruggedness in the municipality, a dummy for water scarcity, a dummy variable that indicates whether the municipality is within 10 kilometers from one of Sicily's five largest cities (urban), land suitability for cereals, citrus fruits and olives and the type of municipality, i.e. *demaniale* or *baronale*. The third column adds province (from 1806, called *valli*, there are 3) fixed effects. The fourth column adds province FE (from 1861, there are 7). Robust tandard errors in parentheses. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Fuzzy Regression Discontinuity Design

As in a very small number of cases there is non-compliance to the treatment (see discussion above), one option is to run a fuzzy RDD, which is what we do below in Table 2.B10. All our results remain very similar.

Table 2.B10: Robustness Fuzzy RDD - Effect of taxation on social unrest

Dep. Variable: Social Unrest$_i$	(1)	(2)	(3)	(4)	(5)	(6)
Tax in 1810$_i$	-0.553**	-0.678***	-0.552**	-0.607**	-0.650***	-0.743***
	(0.258)	(0.232)	(0.271)	(0.259)	(0.237)	(0.242)
Observations	128	136	138	127	149	140
Bandwidth	[866-3,134]	[781-3,219]	[797-3,203]	[869-3,131]	[680-3,320]	[723-3,277]
Controls	No	Yes	No	No	Yes	Yes
Valli FEs	No	No	Yes	No	Yes	No
Province FEs	No	No	No	Yes	No	Yes
Sample Mean	0.383	0.376	0.381	0.383	0.376	0.374

NOTES: Fuzzy RD Estimates of the impact of the property tax exemption (given by population data in 1806) on social unrest in 1860, measured with a dummy variable indicating whether someone from the municipality has received a Medal of Honor in that year. The received treatment is defined as for the list of municipalities that paid the tax in 1810. A data-driven bandwidth is implemented. Column 1 is the unconstrained regression. Column 2 adds the following controls: a dummy variable indicating whether the municipality had access to a postal road that connected the largest towns of Sicily at the beginning of the 1800s, the distance of each municipality to the nearest non-seasonal river and commercial port, average terrain ruggedness in the municipality, a dummy for water scarcity, a dummy variable that indicates whether the municipality is within 10 kilometers from one of Sicily's five largest cities (urban), land suitability for cereals, citrus fruits and olives and the type of municipality, i.e. *demaniale* or *baronale*. The third column adds province (from 1806, called *valli*, there are 3) fixed effects. The fourth column adds province FE (from 1861, there are 7). Robust tandard errors in parentheses. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Using district controls from Acemoglu et al. (2020)

In Table 2.B11 we replicate our baseline Table 2.1 when controlling for a battery of district controls included in Acemoglu et al. (2020). This specification yields very similar findings.

Table 2.B11: Robustness district level controls - Effect of taxation on social unrest

Dep. Variable: Social Unrest$_i$	(1)	(2)	(3)	(4)	(5)	(6)
Tax in 1810$_i$	-0.534**	-0.523***	-0.546**	-0.563***	-0.514***	-0.643***
	(0.239)	(0.188)	(0.239)	(0.214)	(0.193)	(0.202)
Observations	128	131	137	129	133	137
Bandwidth	[846-3,154]	[826-3,174]	[774-3,226]	[839-3,161]	[809-3,191]	[771-3,229]
Controls	No	Yes	No	No	Yes	Yes
Valli FEs	No	No	Yes	No	Yes	No
Province FEs	No	No	No	Yes	No	Yes
Sample Mean	0.375	0.371	0.370	0.369	0.373	0.370

NOTES: Sharp RD Estimates of the impact of the property tax exemption (given by population data in 1806) on social unrest in 1860, measured with a dummy variable indicating whether someone from the municipality has received a Medal of Honor in that year. A data-driven bandwidth is implemented. Column 1 is the unconstrained regression. Column 2 adds the following controls: a dummy variable indicating whether the municipality is an agro-town, share of cultivated land in 1853, share of cultivated land devoted to grains, citrus, vineyards, olives in 1853, altitude of the town center, maximum altitude, average altitude, a dummy variable indicating whether the municipality had access to a postal road that connected the largest towns of Sicily at the beginning of the 1800s, and the type of municipality, i.e. *demaniale* or *baronale*. The third column adds province (from 1806, called *valli*, there are 3) fixed effects. The fourth column adds province FE (from 1861, there are 7). Robust standard errors in parentheses. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

One geographic control at the time

In this Section we replicate in Table 2.B12 our main findings of Table 2.1 when adding geographical controls one at a time. We again find that the results are stable over specifications.

Table 2.B12: Robustness controls - Effect of taxation on social unrest

Dep. Variable: Social Unrest$_i$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Tax in 1810$_i$	-0.534**	-0.647***	-0.628***	-0.610***	-0.596***	-0.636***	-0.632***	-0.622***	-0.641***	-0.629***	-0.656***	-0.684***
	(0.239)	(0.205)	(0.202)	(0.199)	(0.202)	(0.208)	(0.208)	(0.209)	(0.211)	(0.208)	(0.207)	(0.198)
Observations	128	137	136	136	147	153	153	153	149	147	145	149
Bandwidth	[846-3,154]	[740-3,260]	[751-3,249]	[755-3,245]	[666-3,334]	[636-3,364]	[629-3,371]	[625-3,375]	[661-3,339]	[673-3,327]	[685-3,315]	[657-3,343]
Roads in 1799	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dist. rivers	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Avg ruggedness	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Water scarcity	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dist. ports	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Urban	No	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Avg. Cereals Suitability	No	No	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Avg. Citrus Suitability	No	No	No	No	No	No	No	No	Yes	Yes	Yes	Yes
Avg. Olive Suitability	No	No	No	No	No	No	No	No	No	Yes	Yes	Yes
Mun. type	No	No	No	No	No	No	No	No	No	No	Yes	Yes
Province FEs	No	No	No	No	No	No	No	No	No	No	No	Yes

NOTES: Sharp RD Estimates of the impact of the property tax exemption (given by population data in 1806) on social unrest in 1860, measured with a dummy variable indicating whether someone from the municipality has received a Medal of Honor. A data-driven bandwidth is implemented. Robust standard errors in parentheses. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C Robustness Checks And Further Results For Channels

Impact of Tax Treatment on State Capacity

We display below in Table 2.C1 the regression results of the impact of tax treatment on state capacity. As discussed in more detail in the main text, overall we do not detect any strong capacity building effects from being subject to the taxation treatment.

Table 2.C1: Effect of taxation on state capacity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dep. Variable:	*Own Administration*$_i$		*(log) Value Public Build.*$_i$		*Judge Seat*$_i$		*(log) Munic. in Legal Texts*$_i$		*PCA*$_i$	
Tax in 1810$_i$	-0.106	0.0575	1.074	0.613	0.0340	0.120	0.819*	0.734*	0.450	0.465
	(0.271)	(0.217)	(0.794)	(0.596)	(0.160)	(0.162)	(0.432)	(0.420)	(0.594)	(0.572)
Observations	132	154	100	98	119	126	114	133	110	111
Bandwidth	[728-3,272]	[393-3,607]	[1013-2,987]	[1013-2,987]	[870-3,130]	[792-3,208]	[945-3,055]	[777-3,223]	[784-3,216]	[782-3,218]
Controls	No	Yes	No	Yes	No	Yes	No	Yes	Yes	Yes
Province FEs	No	Yes	No	Yes	No	Yes	No	Yes	Yes	Yes
Sample Mean	0.470	0.464	5.860	5.860	0.134	0.138	1.165	1.183	-0.637	-0.631

NOTES: Sharp RD Estimates of the impact of the property tax exemption (given by population data in 1806) on different proxies for state capacity. Columns 1-2 focus on a municipality having an own administration, Col. 3-4 on the (log) value of public buildings, Col. 5-6 on the existence of municipal level judiciary, Col. 7-8 on the municipality being mentioned in legal texts, and, finally Col. 9-10 on a principal component analysis of the aforementioned indicators. A data-driven bandwidth is implemented. Odd columns shows the unconstrained regressions. Even columns display results obtained with the inclusion of province FE (from 1861, there are 7) and the following controls: a dummy variable indicating whether the municipality had access to a postal road that connected the largest towns of Sicily at the beginning of the 1800s, the distance of each municipality to the nearest non-seasonal river and commercial port, average terrain ruggedness in the municipality, a dummy for water scarcity, a dummy variable that indicates whether the municipality is within 10 kilometers from one of Sicily's five largest cities (urban), land suitability for cereals, citrus fruits and olives and the type of municipality, i.e. *demaniale* or *baronale*. Robust standard errors are reported in parenthesis. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Placebos on population growth over different periods

Below in Tables 2.C2 and 2.C3 we present placebo estimation results using population growth before the introduction of the tax and after 1861 (when they removed the exemption because the kingdom of the Two Sicilies was absorbed by Italy). Again, as expected, no effect is found for these periods where the differential fiscal treatment does not apply.

Table 2.C2: Placebo - Effect of taxation on population growth - Different time-windows - Unconstrained estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Dep. Var.: Pop. Growth$_i$	*1748-1806*	*1798-1806*	*1806-33*	*1806-61*	*1806-71*	*1806-81*	*1861-1901*	*1861-1911*	*1861-1921*	*1861-1931*	*1861-1936*
Tax in 1810$_i$	0.125	0.0714	0.292***	0.507**	-0.0751	-0.102	-0.119	-0.295	-0.226	-0.254	-0.215
	(0.539)	(0.108)	(0.0893)	(0.252)	(0.0918)	(0.148)	(0.182)	(0.245)	(0.246)	(0.281)	(0.308)
Observations	98	111	142	136	114	107	117	110	133	111	105
Bandwidth	[976-3,024]	[868-3,132]	[730-3,270]	[781-3,219]	[895-3,105]	[970-3,030]	[864-3,136]	[930-3,070]	[758-3,242]	[928-3,072]	[1006-2,994]
Controls	No	No	No	No	No	No	No	No	No	No	No
Province FEs	No	No	No	No	No	No	No	No	No	No	No
Sample Mean	-0.040	-0.046	-0.049	-0.050	-0.045	-0.040	-0.046	-0.039	-0.050	-0.039	-0.037

NOTES: Sharp RD Estimates of the impact of the property tax exemption (given by population data in 1806) on population growth over different periods. A data-driven bandwidth is implemented. Columns display unconstrained regressions. Robust standard errors in parentheses. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.C3: Placebo - Effect of taxation on population growth - Different time-windows - Constrained estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Dep. Var.: Pop. Growth$_i$	*1748-1806*	*1798-1806*	*1806-33*	*1806-61*	*1806-71*	*1806-81*	*1861-1901*	*1861-1911*	*1861-1921*	*1861-1931*	*1861-1936*
Tax in 1810$_i$	-0.265	0.0832	0.259***	0.394**	-0.0243	-0.0659	-0.107	-0.300	-0.237	-0.167	-0.172
	(0.666)	(0.0891)	(0.0680)	(0.193)	(0.116)	(0.149)	(0.160)	(0.205)	(0.298)	(0.276)	(0.283)
Observations	97	117	138	136	107	107	111	108	113	109	107
Bandwidth	[977- 3,023]	[816- 3,184]	[717- 3,283]	[726- 3,274]	[978- 3,022]	[983- 3,017]	[919- 3,081]	[938- 3,062]	[898- 3,102]	[937- 3,063]	[971- 3,029]
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample Mean	-0.040	-0.047	-0.052	-0.049	-0.040	-0.040	-0.039	-0.041	-0.045	-0.040	-0.040

NOTES: Sharp RD Estimates of the impact of the property tax exemption (given by population data in 1806) on population growth over different periods. A data-driven bandwidth is implemented. All columns display results obtained with the inclusion of province FE (from 1861, there are 7) and the following controls: a dummy variable indicating whether the municipality had access to a postal road that connected the largest towns of Sicily at the beginning of the 1800s, the distance of each municipality to the nearest non-seasonal river and commercial port, average terrain ruggedness in the municipality, a dummy for water scarcity, a dummy variable that indicates whether the municipality is within 10 kilometers from one of Sicily's five largest cities (urban), land suitability for cereals, citrus fruits and olives and the type of municipality, i.e. *demaniale* or *baronale*. Robust standard errors in parentheses. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Robustness with respect to predicted population growth versus actual population growth

In Table 2.C4 we confront actual population growth with the predicted one. The dependent variable is the difference between actual population in 1833 (columns 1-2) and 1861 (columns 3-4) and the predicted population.[19] As expected, we find that municipalities with a heavier tax burden do grow more (with respect to this counterfactual).

Table 2.C4: Robustness - Effect of taxation on population growth

	(1)	(2)	(3)	(4)
	Diff. Between		*Diff. Between*	
Dep. Variable:$_i$	*Actual Pop. - Pred. Pop. 1833*$_i$		*Actual Pop. - Pred. Pop. 1861*$_i$	
Tax in 1810:$_i$	506.2**	506.2**	1,110.6*	1,110.9*
	(247.5)	(247.5)	(578.3)	(578.4)
Observations	116	115	108	108
Bandwidth	[829- 3,171]	[829- 3,171]	[883- 3,117]	[883- 3,117]
Controls	No	Yes	No	Yes
Province FEs	No	Yes	No	Yes
Sample Mean	151.522	151.522	427.387	427.387

NOTES: Sharp RD Estimates of the impact of the property tax exemption (given by population data in 1806) on predicted population growth. A data-driven bandwidth is implemented. Odd columns shows the unconstrained regressions. Even columns display results obtained with the inclusion of province FE (from 1861, there are 7) and the following controls: a dummy variable indicating whether the municipality had access to a postal road that connected the largest towns of Sicily at the beginning of the 1800s, the distance of each municipality to the nearest non-seasonal river and commercial port, average terrain ruggedness in the municipality, a dummy for water scarcity, a dummy variable that indicates whether the municipality is within 10 kilometers from one of Sicily's five largest cities (urban), land suitability for cereals, citrus fruits and olives and the type of municipality, i.e. *demaniale* or *baronale*. Robust standard errors in parentheses. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[19] The predicted population in 1833 (and similarly for 1861) is computed as follows: Predicted population$_{1833}$ = $population_{1806} \cdot (1 + \text{yearly growth rate}_{1748-1806} \cdot (1833 - 1806)$

Impact of tax treatment on uninhabited houses

In Tables 2.C5 and 2.C6 below we follow the structure of the baseline Table 2.1, but using as dependent variables the (log) number of uninhabited houses (*Case Vuote*) and the ratio of non-empty houses (*Case Abitate*) over uninhabited houses, respectively. The underlying, fine-grained data stems from the 1861 Census. As discussed in the main text, we detect an effect of being subject to taxation on the extent of empty housing at the municipal level. As discussed in the main text, these results are consistent with the notion that taxation treatment may impact on internal migration patterns.

Table 2.C5: Effect of taxation on uninhabited houses in 1860

Dep. Variable: (log) Uninhabited Houses$_i$	(1)	(2)	(3)	(4)	(5)	(6)
Tax in 1810$_i$	-0.931*	-1.190**	-0.993**	-0.931**	-1.232**	-0.688
	(0.492)	(0.542)	(0.490)	(0.436)	(0.528)	(0.452)
Observations	112	136	111	112	135	151
Bandwidth	[983- 3,017]	[724- 3,276]	[995- 3,005]	[986- 3,014]	[753- 3,247]	[646- 3,354]
Controls	No	Yes	No	No	Yes	Yes
Valli FEs	No	No	Yes	No	Yes	No
Province FEs	No	No	No	Yes	No	Yes
Sample Mean	5.071	4.948	5.073	5.071	4.968	4.887

Notes: Sharp RD Estimates of the impact of the property tax exemption (given by population data in 1806) on the log number of uninhabited houses in the municipality in 1860 (data from the 1861 Census). A data-driven bandwidth is implemented. Column 1 is the unconstrained regression. Column 2 adds the following controls: a dummy variable indicating whether the municipality is an agro-town, share of cultivated land in 1853, share of cultivated land devoted to grains, citrus, vineyards, olives in 1853, altitude of the town center, maximum altitude, average altitude, a dummy variable indicating whether the municipality had access to a postal road that connected the largest towns of Sicily at the beginning of the 1800s, and the type of municipality, i.e. *demaniale* or *baronale*. The third column adds province (from 1806, called *valli*, there are 3) fixed effects. The fourth column adds province FE (from 1861, there are 7). Robust standard errors in parentheses. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.C6: Effect of taxation on ratio of non-empty houses over uninhabited houses in 1860

Dep. Variable: $\frac{Non-Empty\ Houses}{Uninhabited\ Houses}_i$	(1)	(2)	(3)	(4)	(5)	(6)
Tax in 1810$_i$	24.93	38.45**	26.27*	34.05**	38.74***	36.39***
	(15.32)	(15.17)	(15.08)	(16.03)	(14.92)	(13.93)
Observations	105	126	105	106	126	129
Bandwidth	[1,056- 2,944]	[827- 3,173]	[1,070- 2,930]	[1,045- 2,955]	[828- 3,172]	[804- 3,196]
Controls	No	Yes	No	No	Yes	Yes
Valli FEs	No	No	Yes	No	Yes	No
Province FEs	No	No	No	Yes	No	Yes
Sample Mean	15.114	14.346	15.114	15.038	14.346	14.627

Notes: Sharp RD Estimates of the impact of the property tax exemption (given by population data in 1806) on the ratio of the number of non-empty houses over the number of uninhabited houses in the municipality in 1860 (data from the 1861 Census). A data-driven bandwidth is implemented. Column 1 is the unconstrained regression. Column 2 adds the following controls: a dummy variable indicating whether the municipality is an agro-town, share of cultivated land in 1853, share of cultivated land devoted to grains, citrus, vineyards, olives in 1853, altitude of the town center, maximum altitude, average altitude, a dummy variable indicating whether the municipality had access to a postal road that connected the largest towns of Sicily at the beginning of the 1800s, and the type of municipality, i.e. *demaniale* or *baronale*. The third column adds province (from 1806, called *valli*, there are 3) fixed effects. The fourth column adds province FE (from 1861, there are 7). Robust standard errors in parentheses. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Distance between municipalities and divergence in population growth

As discussed in the main text in Section 2.6.2, there were greater incentives for large landowners –who could typically influence mobility patterns– to hire workers in municipalities above the taxation threshold. Naturally, directing migration is easier when a nearby outside option exists. In the current section we provide additional evidence on this, by studying how the population growth differential between a given treated versus untreated municipality relates to the physical distance between them. Our sample for the current exercise consists of municipality pairs consisting of one treated and one untreated municipality being located within 20 km from each other (which makes them credible alternatives for migration). We rely on the following specification:

$$Pop.Growth_{06-33,iT} - Pop.Growth_{06-33,j\not T} = \beta_0 + \beta_1 Below\ 10 KM_{iT-j\not T} + \epsilon_{ij} \qquad (2.3)$$

where iT and $j\not T$ indicates municipalities that were paying or not paying the tax in 1806, respectively. The outcome of interest is the difference in the population growth between 1833 and 1806 between municipality iT and municipality $j\not T$ and the variable $Below\ 10\ KM_{iT-j\not T}$ is equal to 1 when the distance between municipality iT and municipality $j\not T$ is below 10 kilometres.[20] This specification boils down to comparing growth differentials for city pairs located less than 10 km from each other versus city pairs separated by a distance between 10 and 20 km.

Table 2.C7: Distance between treated and untreated municipalities and difference in Population Growth - Extensive Margin

	(1)	(2)	(3)	(4)
Dep. Variable: Diff. Between Pop. Growth$_{X,iT}$- Pop. Growth$_{X,j\not T}$	*X=1806-1833*		*X=1806-1861*	
Below 10 KMs$_{iT-j\not T}$	0.130**	0.185***	0.411***	0.373***
	(0.0663)	(0.0529)	(0.120)	(0.0981)
Observations	985	985	985	985
Controls iT and j$\not T$	No	Yes	No	Yes

NOTES: Table displays OLS estimates. The unit of observation is the municipality dyad $iT - j\not T$. In the first two columns, the dependent variable is the difference between the population growth between 1833 and 1806 between municipality iT and municipality $j\not T$, where iT and $j\not T$ indicates municipalities that were paying or not paying the tax in 1806, respectively. The last two columns display results obtained using the difference in population growth between 1861 and 1806. The variable *Below 10 KMs* takes value equals to 1 if the distance between municipality iT and municipality $j\not T$ is below 10 KMs. Odd columns show the unconstrained regressions. Even columns adds the controls (for both municipalities) used in Column 2 of Table 2.1. Robust standard errors in parentheses. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The results are displayed in Table 2.C7. The coefficient of interest is positive, which is consistent

[20] Distances are computed using coordinates obtained through the OPENCAGE GEOCODING API and the current name of each municipality.

with the notion that the closer treated and untreated municipalities are, the larger is their differential in population growth observed after the introduction of the tax. Similar results are obtained in Table 2.C8 when defining the main explanatory variable as the intensive margin (i.e., using the (inverse) distance between the two municipalities). Taken together, these findings are in line with the view that close proximity of treated versus untreated municipalities exacerbates incentives for channeling migration towards municipalities already subject to the tax (which have no reason to fear adverse fiscal consequences of growing).

Table 2.C8: Distance between treated and untreated municipalities and difference in Population Growth - Intensive Margin

	(1)	(2)	(3)	(4)
Dep. Variable: Diff. Between Pop. Growth$_{X,iT}$- Pop. Growth$_{X,j\not T}$	*X=1806-1833*		*X=1806-1861*	
(Inverse) Distance$_{iT-j\not T}$	0.537***	0.371***	1.056***	0.723***
	(0.116)	(0.0951)	(0.214)	(0.192)
Observations	985	985	985	985
Controls iT and j$\not T$	No	Yes	No	Yes

NOTES: Table displays OLS estimates. The unit of observation is the municipality dyad $iT - j\not T$. In the first two columns, the difference between the population growth between 1833 and 1806 between municipality iT and municipality $j\not T$, where iT and $j\not T$ indicates municipalities that were paying or not paying the tax in 1806, respectively. The last two columns display results obtained using the difference in population growth between 1861 and 1806. The variable *Inverse Distance* is computed as $1/(distance_{iT-j\not T} + 1)$ where $distance_{iT-j\not T}$ is the distance between municipality iT and municipality $j\not T$. Odd columns show the unconstrained regressions. Even columns adds the controls (for both municipalities) used in Column 2 of Table 2.1. Robust standard errors in parentheses. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Alternative potential mechanism: fertility behavior

Finally, below in Table 2.C9 we investigate another potential mechanism affecting population growth, namely fertility decisions. As discussed in the main text, as we lack reliable fertility data, we need to rely on two proxies for fertility behaviour in a given municipality: (i) average family size (calculated as the ratio of the overall population divided by the number of families in 1861) and (ii) the ratio of unmarried men over the population of 1861.[21] As becomes clear from the table, we do not detect any conclusive evidence in support of the hypothesis that the introduction of the tax treatment had an effect on household fertility decisions.[22]

Table 2.C9: Baseline results of the effect of taxation on fertility behavior

	(1)	(2)	(3)	(4)
Dep. Variable:	*Avg. Family Size 1861$_i$*		*Ratio Unmarried Men 1861$_i$*	
Tax in 1810$_i$	0.145	-42.89	-0.0651	-0.00208
	(0.194)	(33.95)	(0.0646)	(0.0339)
Observations	106	221	202	165
Controls	No	Yes	No	Yes
Province FEs	No	Yes	No	Yes
Bandwidth	[1,049 - 2,951]	[1,183 - 5,183]	[144 - 3,856]	[394 - 3,606]
Sample Mean	0.415	0.477	0.465	0.455

NOTE: Sharp RD Estimates of the impact of the property tax exemption (given by population data in 1806) on average family size, calculated as the ratio between the population over the number of families in 1861 and on the number of unmarried men over the population in 1861. These variables are computed using data from *Ministry of Agriculture, Industry and Trade (Torino, 1864). Statistica del Regno d'Italia. Popolazione. Censimento Generale. Vol. I.* A data-driven bandwidth is implemented. Columns 1 and 3 show results from the unconstrained regression. Columns 2 and 4 add province FE (from 1861, there are 7) and the following controls: a dummy variable indicating whether the municipality had access to a postal road that connected the largest towns of Sicily at the beginning of the 1800s, the distance of each municipality to the nearest non-seasonal river and commercial port, average terrain ruggedness in the municipality, a dummy for water scarcity, a dummy variable that indicates whether the municipality is within 10 kilometers from one of Sicily's five largest cities (urban), land suitability for cereals, citrus fruits and olives and the type of municipality, i.e. *demaniale* or *baronale*. Robust standard errors are reported in parenthesis. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[21] Both variables are obtained using data from the *Ministry of Agriculture, Industry and Trade (Torino, 1864). Statistica del Regno d'Italia. Popolazione. Censimento Generale. Vol. I.*

[22] A word of explanation on why the number of observations increases from col. 1 to col. 2. While adding controls does –as expected– reduce the total number of observations in the sample, the number of observations that are close to the (data-driven) bandwidth (and hence included in the regression) may increase, which is case here.

D Appendix: Lists of Legal Texts

Below we display the list of books used, as well as information on the number of times a municipality is mentioned in legal texts.

#	Title	Refer. Period	Tot. Pages
1	*Annali civili del regno delle Due Sicilie*	1834	536
2	*Annali civili del regno delle Due Sicilie*	1835	550
3	*Annali civili del regno delle Due Sicilie*	1836	548
4	*Annali civili del regno delle Due Sicilie*	1838	480
5	*Annali civili del regno delle Due Sicilie*	1839	490
6	*Annali civili del regno delle Due Sicilie*	1840	570
7	*Annali civili del regno delle Due Sicilie*	1841	456
8	*Annali civili del regno delle Due Sicilie*	1845	552
9	*Annali civili del regno delle Due Sicilie*	1846	552
10	*Collezione delle leggi e de' decreti reali del Regno delle Due Sicilie - Indice generale*	1806-1840	957
11	*Collezione delle leggi e de' decreti reali del Regno delle Due Sicilie - Supplemento*	1837-1840	150
12	*Codice per lo Regno Delle Due Sicilie*	1829	500
13	*Codice per lo Regno delle Due Sicilie - Leggi civili*	1830	498
14	*Codice per lo Regno delle Due Sicilie - Leggi di eccezione per gli affari di commercio*	1838	194
15	*Indice General delle Leggi e dei Decreti dall'anno 1806 a tutto il 1836*	1806-1836	800
16	*Collezione delle leggi e de' decreti reali del Regno delle Due Sicilie*	1855	826
17	*Bollettino delle Leggi del Regno di Napoli*	1810	1,060
18	*Le Leggi Amministrative del Regno delle Due Sicilie*	1845	684
19	*Raccolta di tutti i Sovrani Decreti ed Atti Governativi che trovansi attualmente in vigore Emanati per la Rettifica dei Catasti in Sicilia*	1846	486
20	*Repertorio sull'Amministrazione Civile del Regno delle due Sicilie*	1836	865
21	*Repertorio Amministrativo del Regno delle due Sicilie*	1851	745

Chapter 3

Voice Versus Violence: The Impact of Proportional versus Majoritarian Representation in Cameroon

3.1 Introduction

Democracies come in all shapes and forms, and while recent research suggests that democratic institutions may overall foster peace (see discussion below), the heterogeneity is so large, that positive effects of democracy may be driven by some features, while other specific institutional features may not promote peace and could even backfire. To illustrate crucial specific institutions, consider the example of Switzerland, which was in Medieval times a hotspot of religious wars and a regular provider of mercenaries throughout Europe. Then in 1847 the to-date last civil war took place between Catholics and Protestants, and after the swift victory of the Protestant troops, a new Constitution was penned in 1848. While aleady pre-1848 democratic votes took place, the reform led to a major overhaul of *specific institutional features*. Pillars of the modern political system in Switzerland include proportional representation, bicameralism, federalism and direct democratic votes. In hindsight, the new Constitution of 1848 proved to be a crucial structural break, turning a poor and violence-prone region into a prosperous oasis of peace.

Another example is Northern Ireland which has been typically classified as democracy for over a century, yet has still been notoriously conflicted until the Good Friday Agreement of 1998. Since the institutional reform of 1998 –championing proportional group representation in government, policy and administration– the incidence of violence has spectacularly dropped.

The key question here is of course whether these two examples are special cases or whether there is a general pattern of specific democratic institutions promoting peace. Unfortunately,

investigating the effects of particular institutional features is a hard nut to crack for every scholar, as institutional traditions and timing of reforms are typically endogenous to a multitude of potential confounders. One important dimension to distinguish between two types of democracies is proportional versus majoritarian representation. This is key, as majoritarianism features built-in advantages for large parties and groups, leading typically to a smaller number of parties, and often a two-party system (according to the famous Duverger's Law – see Duverger (1959)). While this has the advantage of facilitating government formation and stability, it comes at the price of lacunary representation of smaller groups and stackholders in society. It is not by chance that, say, the green party had an easier time to enter the political arena in the German system with (a particular form of) proportional representation (PR) than in Westminster's majoritarian system. Similarly, a minority ethnic group may be well represented in parliament under PR while possibly excluded from political decisions under majoritarianism.

Fortunately, Cameroon's transition to democracy offers a unique natural experiment that allows to study exogenous variation in proportional versus majoritarian representation. After the independence from France and Britain and the union of French and British Cameroon, the country has experienced major changes in its political system. The introduction of a multi-party system took place in 1990 and two years later the first multi-party elections were celebrated. The electoral system contains both elements of PR and majoritarianism, which results in very sizeable differences across districts in group representation in the Cameroonian National Assembly. In particular, in multi member constituencies the list that obtains an absolute majority wins all the seats allocated to that constituency. If nobody achieves an absolute majority, the list obtaining most votes wins half of the seats, while the remaining seats will be distributed proportionally to the rest of the lists. These *sharp changes around thresholds* allow us to examine how majoritarian and proportional representation systems differently affect the propensity of engaging in conflict and protests. As discussed in more depth below, later political changes blurred the electoral features after the first elections, and hence we focus in the current paper on Cameroon's first democratic elections only.

We find that exogenous exposure to proportional representation leads to lower levels of political violence (military conflict) and instead to more voice (peaceful protests), which is in line with the predictions of a simple game-theoretic model of conflict (between groups-those in government versus opposition) and protest (within groups) that we present in the next section of this paper. The intuition is as follows: The cost of demonstrating in the streets is higher in situations of high violence, such as during military fighting, which implies that peaceful protests occur rather under peace than under conflict. In turn, engaging in conflict is relatively more attractive when a group is deprived of a fair political representation, which is more often the case for minority ethnicities in a majoritarian system. Taken together, this accounts for the fact that proportional representation is associated with voice (peaceful protests) while majoritarianism can more easily

give rise to violence (military conflict).

Our paper relates to several strands of literature. First and foremost, the literature on democracy and conflict is particularly relevant.[1] In terms of theoretical models of democracy and conflict, reasons why one may expect democracy to promote peaceful bargaining instead of conflict include that democracy acts as a credible commitment device lessening commitment problems (Acemoglu and Robinson (2005), Fearon (2011), and Bidner et al. (2014)) and that democratic elections reduce asymmetric information (Laurent-Lucchetti et al. (2023)). In terms of empirical evidence, the pioneering cross-country regression literature has not found a clear-cut linear association between democracy and peace (Fearon and Laitin (2003)), but rather a more complex relationship. In particular, the seminal article of Hegre et al. (2001) has found that heightened conflict is documented in intermediate regimes (anocracies) while full democracies or full autocracies prove more stable. Related to this, Collier and Rohner (2008) show that the peace-promoting features of democracy are more powerful in rich countries (while having no effect in poor countries). A conditional relationship is also uncovered by Cervellati and Sunde (2013) who show that "third wave" democratization promotes peace mostly in the presence of non-violent transitions. Finally, Esteban et al. (2015) stress that while full democracy reduces the scope for mass killings, spells of initial democratization bear significant risks.[2] One weakness of the above studies is that they typically exploit variation at the (cross-)country-level and are not able to draw on exogenous changes in democracy. This raises worries about unobserved heterogeneity and potential confounders. One exception is the article by Marcucci et al. (2023) which exploits exogenous variation in enfranchisement intensity across cities in Victorian England's Second Reform Act, finding that democracy strongly reduces conflict and boosts prosperity.

A particularly relevant strand of the literature are the few studies focusing on particular institutional features. The pioneering contributions of Reynal-Querol (2002); Saideman et al. (2002) present cross-country associations between proportional representation and peace, and Cederman et al. (2013a); Mueller and Rohner (2018); Mueller and Rauh (2022) study the impact of power-sharing coalitions in the executive. Finally, Fujiwara (2015); Facchini et al. (2020); Lacroix (2020) investigate particular laws that improved the legal status and protection by the rule of law for minority groups.[3]

In a nutshell, to the best of our knowledge our current paper is the first one that studies the impact on conflict and protest of an exogenous variation in proportional versus majoritarian

[1] For recent surveys of the literature on conflict, see Anderton and Brauer (2021); Rohner and Thoenig (2021); Rohner (2022).

[2] Related strands of this literature also study post-election violence (Dercon and Gutiérrez-Romero (2012); Cederman et al. (2013b); Collier and Vicente (2014); Fergusson et al. (2020)) and show that cohesive institutions manage to deal better with shocks (Besley and Persson (2011); Fetzer and Kyburz (2018)).

[3] Also somewhat related are the literatures on the electoral system in Cameroon, e.g. Albaugh (2001); Boone and Wahman (2015); Enonchong (2021) and on protests (see e.g. Passarelli and Tabellini (2017); Enikolopov et al. (2020); Bursztyn et al. (2021)).

representation. In line with our simple game-theoretic model we show that proportional representation reduces the scope for military conflict ("violence") and fosters peaceful protests ("voice").

The remainder of the paper is organized as follows: Section 3.2 describes the historical background of Cameroon's transition to multi-party democracy. Section 3.3 is devoted to a description of the data used. Section 3.4 presents a simple game-theoretic model of conflict and protests, while in Section 3.5 the identification strategy is presented. The main results and robustness checks are displayed in Section 3.6, and Section 3.7 concludes. Additional results and explanations are relegated to the Online Appendix.

3.2 Historical and institutional background

The Republic of Cameroon is a 475,440 sq km large country, situated in Central Africa, with a tropical climate in the South and semi-arid in the North.[4] It is very ethnically, religiously and linguistically diverse, and has as official languages English and French. Historically, after a history of chiefdoms and German colonization, Cameroon was divided after World War I between France and the United Kingdom. It gained independence from France in 1960 and from the United Kingdom in 1961, and adopted a new constitution in 1972, replacing the previous federation by a unitary state. The first president of independent Cameroon was Ahmadou Ahidjo, who ruled the country from 1960 to 1982, and was succeeded by Paul Biya who is still in office today.

In 1989 and 1990 political demands for democratization became ever louder and were met by repression, such as the killing of six people ("the Bamenda Six") at a rally, which led to widespread international condemnation and pressure. In December 1990 a multi-party system was introduced, leading to the first multi-party elections for the National Assembly in 1992. The 180 members of parliament were elected for a 5-year mandate by universal suffrage, representing the electoral constituencies of the country. The electoral system was mixed: Namely, there were single member constituencies (with only one seat in the National Assembly), where plurality rules applied and hence where the candidate with most votes won the seat. In the multi-member constituencies, in contrast, there was only majoritarian representation if a given party won the absolute majority of votes in a constituency. If no party won an absolute majority of votes in a constituency, then the list (party) with most votes received half of the available seats and the rest of seats were distributed proportionally to the rest of lists. There was also a quorum of 5 percent, meaning that a list with a less than 5 percent vote share did not gain any seats. The electoral system was overhauled in 1996, with the creation of a bicameral parliament (that materialized

[4]This Section draws on Dike DeLancey et al., Group (2016), DeLancey et al. (2019), Enonchong (2021), the CIA Factbook and Encyclopedia Britannica.

in 2013). However, the government has been accused of engaging in heavy gerrymandering, fraudulent vote counting, limits to voting rights and ethnic favoritism (see Albaugh (2001) and Enonchong (2021)).[5] Importantly, it has been argued that the 1992 parliamentary and presidential elections were relatively "clean" – which is also consistent with the very slim margins of victory – and that from then onwards the political system turned increasingly authoritarian. While the president won the disputed 1992 presidential election with 40 percent of the votes, in the following (widely boycotted) election he was announced to have won the re-election with 93 percent of votes. Similarly, in the elections of the National Assembly in 1992 Biya's party Cameroon People's Democratic Movement (RDPC) won 49 percent of the seats, while 1997 it won (after partial result cancelling and replacement polls) 64 percent, and from 2002 onwards always at least 82 percent of all seats. Albaugh (2001) studies in depth the re-districting of electoral constituencies and argues that gerrymandering has played a key role in the heightened electoral fortunes of the president and his party. She refers to "the electoral opening provided by the 1992 elections and the gradual closing of that window with each subsequent election". (2001: 390). Hence, for the purpose of having a methodologically "clean" natural experiment, we limit our analysis to the first elections of 1992.

In terms of social tensions and potential for conflict, a first important cleavage is French vs English language areas. According to Albaugh (2001), the Francophone were politically dominant from the start, creating increasing resentment among the Anglophone, and fueling an Anglophone secessionist movement. There have also been mounting ethnic tensions, among others related to allegations of ethnic favoritism. According to Albaugh (2001) in 1991 among the 47 senior prefects 37 were of the president's own ethnic group, as well as 22 out of 38 high-ranking bureaucrats. These proportions are much larger than the population share of the president's Beti ethnic group, which is estimated to 21.6 percent (see CIA Factbook). Last but not least, there are inter-religious tensions, with the Christian president Biya facing opposition from Muslim-majority areas of the country. One source of tension was the power struggle in the 1980s with Biya's predecessor Ahmadou Ahidjo –a Muslim– who was accused of being involved in the 1984 coup attempt and had to flee the country. In the 1980s and 1990s there were also various pro-democracy rallies and strikes (among which the "Ghost Town" civil disobedience movement) that at least in part were met with violence – such as in the case of the aforementioned killing of the "Bamenda Six" in 1990 and the violence at the 2 October 1991 rally in Bamenda. There has also been large-scale communal violence such as e.g. in the North of the country between the Kotoko and "Choa Arab" groups.

[5] See also Voice of America, 6 November 2022, https://www.voanews.com/a/cameroonian-president-paul-biya-marks-40-years-in-power/6823157.html.

3.3 Data

To examine the impact of the political system (PR versus majoritarianism) on the incidence of conflict and protests, we have built a department-year dataset. Cameroon contains 360 arrondissements that are part of 49 constituencies (departments) in 1992. As discussed below, the variation that we exploit takes place at the department level. In what follows, we describe the data used for assembling our dataset.

3.3.1 Elections

The results for the National Assembly elections of 1992 stem from Albaugh (2001). This data is at the constituency (department) level. It contains information on the number of seats allocated to each constituency, the number of registered voters, the number of votes cast, as well as the number of votes received by each political party. This information allows us to disentangle between constituencies that are single-member (i.e. that have only one seat allocated in the National Assembly) and multi-member ones (i.e. have more than one seat allocated per constituency in the National Assembly). Our sample focuses exclusively on multi-seat constituencies, as for single-seat constituencies by construction majoritarian representation always applies. The aforementioned data on electoral results also allows us to know whether the absolute majority threshold has been reached or not. As mentioned above, in multi-seat constituencies there was only majoritarian representation if a party won the absolute majority of votes. In contrast, in the absence of absolute majority, the list (party) with most votes received half of the available seats and the rest of seats were distributed proportionally to the rest of lists. We refer to a given constituency as "treated" when this specific type of proportional representation applies, while it is dubbed "non-treated" if the winner takes all seats.

3.3.2 Conflict and protests

Data on conflict and protests occurring in Cameroon from 1987 to 1996 comes from Global Data on Events, Location and Tone (GDELT, Leetaru and Schrodt (2013)), which records worldwide events from international broadcast, print, and web news in over 100 languages.[6] An appealing feature of this data is that it distinguishes protests from military conflicts. In particular, military (material) conflicts refer to situations that exhibit military or police power, coerce, assault, fight and use of unconventional mass violence (ex. mass expulsion, mass killings, ethnic cleansing, attack with unconventional weapons that are meant to cause massive destruction and casualties, etc). In contrast, protests are defined as episodes such as "civilian demonstrations and other collective actions carried out as protest against the target actor", and "demonstrations for

[6]Note that GDELT data is available until today, yet we focus on the period before the second multi-party elections, as explained above.

leadership, institutional or policy change, rights" (Gerner et al. (2009)). Note that we do not include violent protests or riots within the definition of pacific protests.[7] From this geolocalized data we construct a measure of conflict at arrondissement level (administrative level 3). Thus, in a nutshell, we have information on the number of conflict or protest occurrences for each Cameroonian arrondissement by year throughout the period 1987-1996.[8] For the purpose of illustration, we present the conflict and protest data graphically in the Appendix. In particular, the dots depicted in Figures 3.A1a and 3.A1b show the distribution of conflict events and pacific protests, respectively, throughout the Cameroonian territory during the period analyzed in the present paper, 1987-1996. Overall, relatively many incidents are located, apart from the capital Yaounde, in the Northern and Western part of the country, specially in the English-speaking provinces of Northwest and Southwest. We also present the number of conflict events and pacific protests per year in Figure 3.A2. As outlined in Section 3.2, demands for democratization occurred in different parts of the country starting in 1990, year in which multi-party system was introduced.

3.3.3 Other data

In terms of other data, we include as control variables the percentage of unemployed people, as well as the share of people in a polygamous union in each department. This data comes from census information from the years 1987 and 2005, available at IPUMS International (Ruggles et al. (2003)). We also control for terrain ruggedness, calculated using data from Nunn and Puga (2012), and distance to the coast, computed using data from Patterson and Kelso.

3.3.4 Descriptive summary statistics and balancing

We have relegated to the Appendix descriptive summary statistics and balancing tables. In particular, Appendix Table 3.A1 provides descriptive statistics of the main variables that form our sample and Appendix Table 3.A2 provides a balancing test of these variables, comparing the departments in which Majoritarianism applies with respect to those where Proportional Representation (PR) is implemented.

[7] To be concrete, events classified as conflicts in our data for Cameroon include for example violent repression, property destruction, assaults, assasinations, territory occupation, inter-ethnic conflicts (such as in the Far North province), as well as fights that include any weapons ranging from small arms to tanks. Peaceful protests comprise demonstrations for policy or leadership change (such as on May 26 1990 in the Cameroonian capital of Yaoundé) or hunger strikes.

[8] Note that we are not able to use alternative conflict datasets such as ACLED (the Armed Conflict Location & Event Data Project), UCDP GED, RAND Database or the Global Terrorism Database as they do not cover the time period we are interested in, contrary to GDELT.

3.4 Model

3.4.1 The setting

In what follows we present a formal game-theoretic model of how the mode of representation (proportional versus majoritarian) may affect the scope for "voice" (peaceful protests) versus "violence" (armed conflict events). The setting features two groups, i (group holding government) and j (group in opposition). We think of groups as collective players, focusing on the group members at large (rather than their officials/delegates/narrow elites). The population share of group j is θ, and accordingly $(1-\theta)$ for group i. As depicted graphically in the game tree of Figure 3.1, in the first stage j selects between accepting the status quo ("Peace") versus challenging it ("Conflict").

Then in the second stage, in the "Peace" node the government group i receives a share $(1-\alpha)$ of the rents, while the opposition j obtains accordingly α. In this node both groups decide, simultaneously and independently, whether to tackle corruption by the elite / officials of their group (by engaging in protests within the members of their own group, $m=1$) or whether to accept embezzlement of part of the group rents (i.e. a share μ of rents gets appropriated by group elites /officials, and are hence lost to the group at large). Staging protests eliminates this stealing but entails that a share d of the group rents is lost. For simplicity, we model this as a within-group issue, where payoffs of group j (resp., i) do not depend on the demonstration decision of the other group i (resp. j). As tie-breaking rule, we assume that when indifferent, $m=0$ is selected.

In contrast, in the "Conflict" node, a standard strategic contest arises between the two groups with a ratio-form contest success function where both groups simultaneously and non-cooperatively select fighting efforts f_i and f_j. One can think of an implicit time constraint, as the time spent for fighting is lost for productive work (i.e. the unit cost c of fighting can be thought of as foregone wages). Without loss of generality, our setting also allows for a lump-sum cost of conflict ψ (e.g. destruction), unrelated to a group's own fighting efforts (note that our results are qualitatively unchanged if we set $\psi=0$). The groups trade off a concave function of appropriation of rents with a linear opportunity cost of conflict.

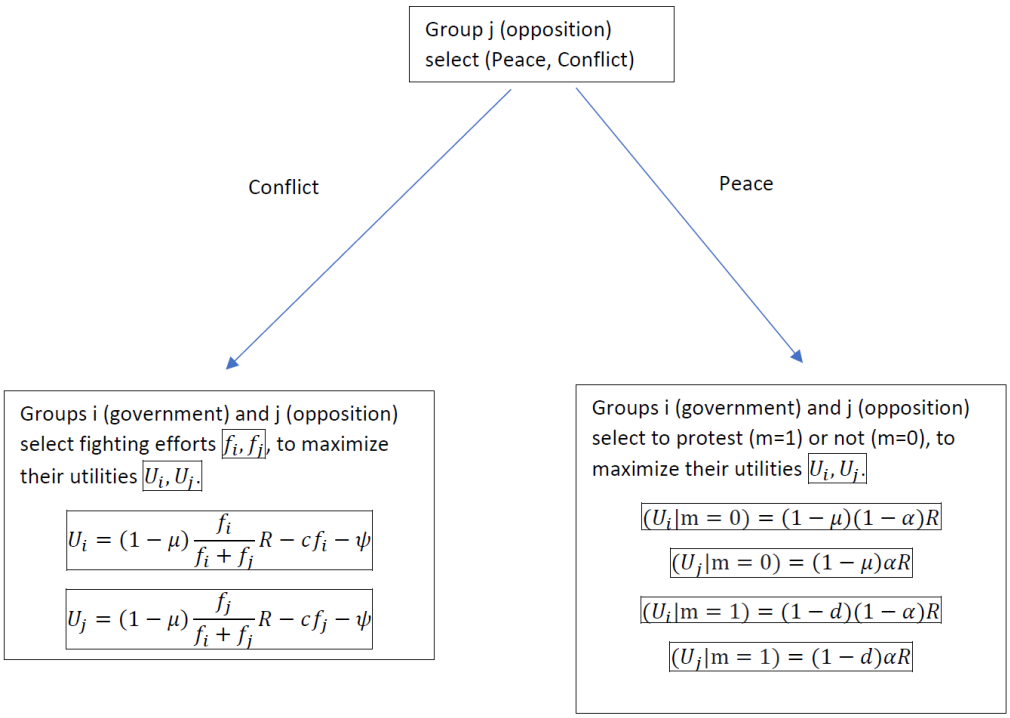
The framework features perfect and complete information. The equilibrium concept used is the Nash Equilibrium.

There are two main axioms made:

Axiom 1: Peaceful protests are prohibitively costly under conflict, ruling out option $m=1$ in the conflict node.

Axiom 2: Under majoritarian representation, the larger group wins all seats in a given region (hence, $\alpha = 0$), while under proportional representation the seats are distributed closer to group size, yielding $\alpha > 0$.

Figure 3.1: Game tree



To briefly discuss the features and underlying assumptions of the model. First of all, note that we have not included fighting technologies in the contest success function and have assumed the same unit cost of fighting for both groups. We also assume a linear cost of demonstrations and presume that demonstrations always completely remove embezzlement from group elites. Relaxing these simplifications would make the expressions more complex but leave the results qualitatively identical. Hence, for the sake of being parsimonious we prefer to simplify all features unrelated to the research question we focus on.

Regarding the key assumptions, Axiom 1 simply reflects the notion that protesting within groups in the midst of battle-field fighting is dangerous, and that hence often under martial law scrutiny for government corruption is lower. Note that we could easily relax this assumption and our results would go through as long as protesting is somewhat more expensive under conflict than under peace.

Finally, Axiom 2 reflects typical outcomes when politics are determined by group membership. With winner takes all, (single-district) majoritarian representation typically hands the lion's

share of seats to the majority group, while with proportional representation (PR) the shares of seats received by given minority groups is much more in line with their population shares (it is typically larger than under majoritarianism, and may be $\alpha = \theta$ in case there are no quorum or other specific constraints). Again, making this assumption less stark would leave the qualitative results unchanged, as long as minority groups have somewhat stronger political representation under PR than under majoritarianism.

3.4.2 The equilibrium

We solve the game through backward induction. Starting from the "Conflict" node, the equilibrium fighting efforts and utilities are given by, respectively:

$$f_i^* = f_j^* = \frac{R(1-\mu)}{4c}, \tag{3.1}$$

$$U_i = U_j = \frac{R(1-\mu)}{4} - \psi, \tag{3.2}$$

where half of the rents are lost in the fighting.

Considering now the "Peace" node, one can easily see that the condition for any group to prefer protests ($m = 1$) over inaction ($m = 0$) is $d < \mu$. Quite intuitively, when protest costs are low, they are selected, otherwise not.

Moving backwards to stage 1, we can characterize the conditions for "Conflict" versus "Peace".

Proposition 1: When demonstration costs are low ($d < \mu$), then "Peace" (with protests) is selected in the top decision node iff

$$\alpha > \frac{(1-\mu)}{4(1-d)} + \frac{\psi}{(1-d)R}. \tag{3.3}$$

When demonstration costs are high ($d \geq \mu$), then "Peace" (without protests) is selected in the top decision node iff

$$\alpha > \frac{1}{4} - \frac{\psi}{R(1-\mu)}. \tag{3.4}$$

Proof: Follows directly of payoff comparisons, applying backward induction.

The focus of the current contribution lies on the comparative statics with respect to α, which is larger under proportional representation (PR) than majoritarianism.

Corollary 1: Proportional representation (implying a higher α) decreases the likelihood of conflict and increases the likelihood of protests.

Proof: Follows directly from Proposition 1.

In what follows we shall confront these simple predictions with the empirical evidence from Cameroon.

3.5 Identification Strategy

In what follows we discuss the empirical strategy and identification. To start with, the regression equation we estimate corresponds to

$$y_{iT} = \beta PR_i * Post_{iT} + \gamma_i + \theta_T + X_i * \theta_T + u_{iT} \tag{3.5}$$

where i denotes a department, T denotes the time period (before treatment from 1987 to 1991 or after it, from 1992 to 1996).[9] The dependent variable y_{iT} is a dummy variable taking a value of 1 if at least one conflict event or pacific protest occurred in department i at time period T. In terms of explanatory variables, PR_i is a dummy variable indicating that proportional representation (PR) applies in department i, while $Post_{iT}$ is an indicator variable that takes the value of 1 for the post-treatment period, i.e. after the year 1992. Further, γ_i, θ_T are department and period fixed effects, respectively, and X_i is a vector of time-invariant control variables that enter in the equation interacted with period fixed effects. Bootstrapped standard errors are clustered in all specifications at the level of the 45 departments.

The rationale for this specification is as follows. In constituencies with Majoritarian representation the 1992 election represented a continuity in terms of (lack of) minority representation, as before and after the reform all seats went to just one party. In contrast, in constituencies with Proportional Representation (PR) after 1992 a new way of representing even small groups emerged. Importantly, our fixed effects structure filters out time invariant heterogeneity (such as e.g. the level of ethnic polarization or the climate zone in a given department), while the time dummies control for global shocks. Note that an important identifying assumption of this difference-in-difference estimation is that there was no pre-trend before the election, which we document below.

[9]Note that we focus on the two periods dimension because this structure is more accommodating with the fact that conflict and riots episodes are not very frequent and it is more consistent with the type of variation we are exploiting.

3.6 Results

In what follows we discuss the baseline results of the paper, as well as a series of robustness checks.

3.6.1 Baseline Results

Table 3.1 presents the conflict results in its simplest form, performing a pure diff-in-diff analysis with as unit of analysis the department level and collapsing the sample into two periods (pre- versus post-). Column 1 shows that for the basic specification PR reduces the likelihood of a conflict event by 0.30 percent, which corresponds to roughly 80 percent of the baseline risk. In the following specifications we add a series of controls, interacting key potential confounders with the post-period dummy. Column 2 controls for the share of unemployed people in each department. Column 3 controls for distance to the coast, column 4 adds the ruggedness index, column 5 includes the share of polygamous unions within the deparment and the last column adds the pre-sample mean of the dependent variable. The coefficient of interest changes very little throughout the inclusion of the different control variables.

Table 3.2 performs the analogous analysis, but for pacific protests. In line with the model, we find in all specifications that the likelihood of protests goes up under PR. The effect is again quantitatively very large, corresponding to basically the full baseline risk. Overall, the results presented in these two tables are consistent with the theoretical model described in section 3.4: proportional representation decreases the propensity to engage in violent events such as assaults or coercion while it increases the probability of seeing pacific protests in a given Cameroonian department.

Table 3.1: Effect of Proportional Representation (PR) on conflict

	Conflict				
Proportional Representation	-0.295**	-0.296**	-0.273*	-0.282**	-0.290**
	(0.139)	(0.144)	(0.141)	(0.139)	(0.141)
Deparment FEs	Yes	Yes	Yes	Yes	Yes
Time dummy	Yes	Yes	Yes	Yes	Yes
Dist. Coast × Time dummy	No	Yes	Yes	Yes	Yes
Ruggedness × Time dummy	No	No	Yes	Yes	Yes
Polyg. × Time dummy	No	No	No	Yes	Yes
Pre-sample mean × Time dummy	No	No	No	No	Yes
Observations	90	90	90	90	90
N. Clusters	45	45	45	45	45
R-Squared	0.801	0.801	0.817	0.823	0.828
Mean of Dep. Variable	0.378	0.378	0.378	0.378	0.378

NOTES: This table shows the difference-in-difference estimates of the effect of PR on conflict. The unit of observation is a department i in the pre- and post-treatment periods. The pre-treatment period covers the years 1987-1991, while the post-treatment period the years 1992-1996. The dependent variable is a dummy taking a value of 1 if at least one episode of conflict occurred in department i in the pre- and post-treatment periods. Column 1 controls for deparment and time fixed effects. Column 2 includes distance to the coast, column 3 adds terrain ruggedness. Column 4 controls for the share of people in a polygamous union in the department. The last column adds the pre-sample mean of the dependent variable. Bootstrapped clustered standard errors at department level are shown in parentheses. Statistical significance is represented by * p < 0.10, ** p < 0.05, *** p < 0.01.

Table 3.2: Effect of Proportional Representation (PR) on pacific protests

	Pacific Protests				
Proportional Representation	0.364**	0.376***	0.362***	0.358**	0.358**
	(0.162)	(0.140)	(0.136)	(0.147)	(0.147)
Department FEs	Yes	Yes	Yes	Yes	Yes
Time dummy	Yes	Yes	Yes	Yes	Yes
Dist. Coast × Time dummy	No	Yes	Yes	Yes	Yes
Ruggedness × Time dummy	No	No	Yes	Yes	Yes
Polyg. × Time dummy	No	No	No	Yes	Yes
Pre-sample mean × Time dummy	No	Yes	Yes	Yes	Yes
Observations	90	90	90	90	90
N. Clusters	45	45	45	45	45
R-Squared	0.679	0.734	0.742	0.743	0.743
Mean of Dep. Variable	0.211	0.211	0.211	0.211	0.211

NOTES: This table shows the difference-in-difference estimates of the effect of PR on pacific protests. The unit of observation is a department i in the pre- and post-treatment periods. The pre-treatment period covers the years 1987-1991, while the post-treatment period the years 1992-1996. The dependent variable is a dummy taking a value of 1 if at least one pacific protest took place in department i in the pre- and post-treatment periods. Column 1 controls for deparment and time fixed effects. Column 2 includes distance to the coast, column 3 adds terrain ruggedness. Column 4 controls for the share of people in a polygamous union in the department. The last column adds the pre-sample mean of the dependent variable. Bootstrapped clustered standard errors at department level are shown in parentheses. Statistical significance is represented by * p < 0.10, ** p < 0.05, *** p < 0.01.

3.6.2 Robustness Checks

In what follows we discuss how robust our findings are with respect to some sensitivity checks.

First, Appendix Table 3.B1 restricts the sample to departments in which the main political party obtained a vote share around the 50% threshold. Columns 1 and 2 (3 and 4) analyze the effect of PR on conflict (pacific protests) for departments where the vote share obtained by the biggest political party in each constituency is within the range 30-70%. Further, columns 5 and 6 (7 and 8) perform an analogous exercise for observations within the range of 40-60% vote share for the biggest party. Although quantitatively the results remain very similar, the level of significance is reduced. This is explained by the very substantial drop in the number of observations with respect to the baseline estimations.

Second, Table 3.B2 performs a placebo analysis, where the placebo treatment occurs at a different year, i.e. in the year 1984 instead of 1992. The resulting sample period becomes 1980-1988, so there are five pre- and post-treatment years, as the original sample. Columns 1 and 2 depict the baseline estimates for comparison, while columns 3 and 4 display the estimated coefficients for the falsification exercise. Reassuringly, effects are only found for the actual treatment but not for the placebo.

3.7 Conclusion

In this paper we have started out from a game-theoretical model of conflict and protest, which has yielded the main prediction that Proportional Representation leads to more "Voice" (peaceful protests) and less "Violence" (armed conflict), compared to Majoritarian Representation. Consistent with the theoretical model, Proportional Representation is found to be associated with a lower propensity to participate in violent events, while increasing the likelihood of peacefully demonstrating in the streets.

Overall our findings are in line with the notion that in settings featuring severe social tensions there is a key role to play for Proportional Representation. Guaranteeing a proportional political representation for even small groups allows everybody to express their discontent peacefully within the limits of the law, and curbs incentives for engaging in political violence.

While the current paper is a useful step in the direction on unpacking the role of specific institutional features for preventing conflict, much work lies ahead. We very much encourage contributions that studies the same distinction between PR and Majoritarianism for other context or that study different institutional features.

Bibliography

Acemoglu, D. and Robinson, J. A. (2005). Economic origins of dictatorship and democracy. Cambridge University Press.

Albaugh, E. A. (2001). An autocrat's toolkit: adaptation and manipulation in 'democratic' cameroon. Democratization, 18(2):388–414.

Anderton, C. H. and Brauer, J. (2021). Mass atrocities and their prevention. Journal of Economic Literature, 59(4):1240–92.

Besley, T. and Persson, T. (2011). The logic of political violence. The Quarterly Journal of Economics, 126(3):1411–1445.

Bidner, C., Francois, P., and Trebbi, F. (2014). A theory of minimalist democracy. NBER Working Paper, (w20552).

Boone, C. and Wahman, M. (2015). Rural bias in african electoral systems: Legacies of unequal representation in african democracies. Electoral Studies, 40:335–346.

Bursztyn, L., Cantoni, D., Yang, D. Y., Yuchtman, N., and Zhang, Y. J. (2021). Persistent political engagement: Social interactions and the dynamics of protest movements. American Economic Review: Insights, 3(2):233–50.

Cederman, L.-E., Gleditsch, K. S., and Buhaug, H. (2013a). Inequality, grievances, and civil war. Cambridge University Press.

Cederman, L.-E., Gleditsch, K. S., and Hug, S. (2013b). Elections and ethnic civil war. Comparative Political Studies, 46(3):387–417.

Cervellati, M. and Sunde, U. (2013). Democratizing for peace? The effect of democratization on civil conflicts. Oxford Economic Papers, 66(3):774–797.

Collier, P. and Rohner, D. (2008). Democracy, development, and conflict. Journal of the European Economic Association, 6(2-3):531–540.

Collier, P. and Vicente, P. C. (2014). Votes and violence: Evidence from a field experiment in Nigeria. The Economic Journal, 124(574):F327–F355.

DeLancey, M. D., DeLancey, M. W., and Mbuh, R. N. (2019). Historical dictionary of the Republic of Cameroon. Rowman & Littlefield.

Dercon, S. and Gutiérrez-Romero, R. (2012). Triggers and characteristics of the 2007 Kenyan electoral violence. World Development, 40(4):731–744.

Dike DeLancey, M., Neh Mbuh, R., and DeLancey, M. Historical dictionary of the Republic of Cameroon. The Scarecrow Press, Inc.

Duverger, M. (1959). Political parties: Their organization and activity in the modern state. Metheun & Co. Ltd.

Enikolopov, R., Makarin, A., and Petrova, M. (2020). Social media and protest participation: Evidence from russia. Econometrica, 88(4):1479–1514.

Enonchong, L.-S. E. (2021). The Constitution and Governance in Cameroon. Routledge.

Esteban, J., Morelli, M., and Rohner, D. (2015). Strategic mass killings. Journal of Political Economy, 123(5):1087–1132.

Facchini, G., Knight, B. G., and Testa, C. (2020). The franchise, policing, and race: Evidence from arrests data and the Voting Rights Act.

Fearon, J. D. (2011). Self-enforcing democracy. The Quarterly Journal of Economics, 126(4):1661–1708.

Fearon, J. D. and Laitin, D. D. (2003). Ethnicity, insurgency, and civil war. American Political Science Review, 97(1):75–90.

Fergusson, L., Querubin, P., Ruiz, N. A., and Vargas, J. F. (2020). The real winner's curse. American Journal of Political Science.

Fetzer, T. and Kyburz, S. (2018). Cohesive institutions and political violence. The Review of Economics and Statistics, pages 1–46.

Fujiwara, T. (2015). Voting technology, political responsiveness, and infant health: Evidence from brazil. Econometrica, 83(2):423–464.

Gerner, D. J., Schrodt, P. A., and Yilmaz, O. (2009). Conflict and Mediation Event Observations (CAMEO) Codebook. http://eventdata.psu.edu/data.dir/cameo.html.

Group, I. C. (2016). Cameroon: Confronting boko haram.

Hegre, H., Ellingsen, T., Gates, S., and Gleditsch, N. P. (2001). Toward a democratic civil peace? Democracy, political change, and civil war, 1816-1992. American Political Science Review, 95(1):33–48.

Lacroix, J. (2020). Ballots instead of bullets? The effect of the Voting Rights Act on political violence.

Laurent-Lucchetti, J., Rohner, D., and Thoenig, M. (2023). Ethnic conflicts and the informational dividend of democracy. Journal of the European Economic Association.

Leetaru, K. and Schrodt, P. A. (2013). Gdelt: Global data on events, location, and tone, 1979–2012. In ISA annual convention, volume 2, pages 1–49. Citeseer.

Marcucci, A., Rohner, D., and Saia, A. (2023). Ballot or bullet: The impact of uk's representation of the people act on peace and prosperity.

Mueller, H. and Rauh, C. (2022). Building bridges to peace: A quantitative evaluation of power-sharing agreements. Technical report.

Mueller, H. and Rohner, D. (2018). Can power-sharing foster peace? Evidence from Northern Ireland. Economic Policy, 33(95):447–484.

Nunn, N. and Puga, D. (2012). Ruggedness: The blessing of bad geography in africa. Review of Economics and Statistics, 94(1):20–36.

Passarelli, F. and Tabellini, G. (2017). Emotions and political unrest. Journal of Political Economy, 125(3):903–946.

Patterson, T. and Kelso, N. V. World coastlines, 1:50 million.

Reynal-Querol, M. (2002). Ethnicity, political systems, and civil wars. Journal of conflict resolution, 46(1):29–54.

Rohner, D. (2022). Mediation, military and money: The promises and pitfalls of outside interventions to end armed conflicts. Journal of Economic Literature.

Rohner, D. and Thoenig, M. (2021). The elusive peace dividend of development policy: From war traps to macro complementarities. Annual Review of Economics, 13:111–131.

Ruggles, S., King, M. L., Levison, D., McCaa, R., and Sobek, M. (2003). Ipums-international. Historical Methods: A Journal of Quantitative and Interdisciplinary History, 36(2):60–65.

Saideman, S. M., Lanoue, D. J., Campenni, M., and Stanton, S. (2002). Democratization, political institutions, and ethnic conflict: A pooled time-series analysis, 1985-1998. Comparative political studies, 35(1):103–129.

Appendix

In the Appendices we provide some descriptive statistics in Section A, as well as robustness checks to the main results in Section B. Finally, Section C explores possible channels of transmission.

A Data

In what follows, descriptive summary statistics (Table 3.A1) and a balancing table (Table 3.A2) are displayed. Figures 3.A1a and 3.A1b map the distribution of conflict events and pacific protests.

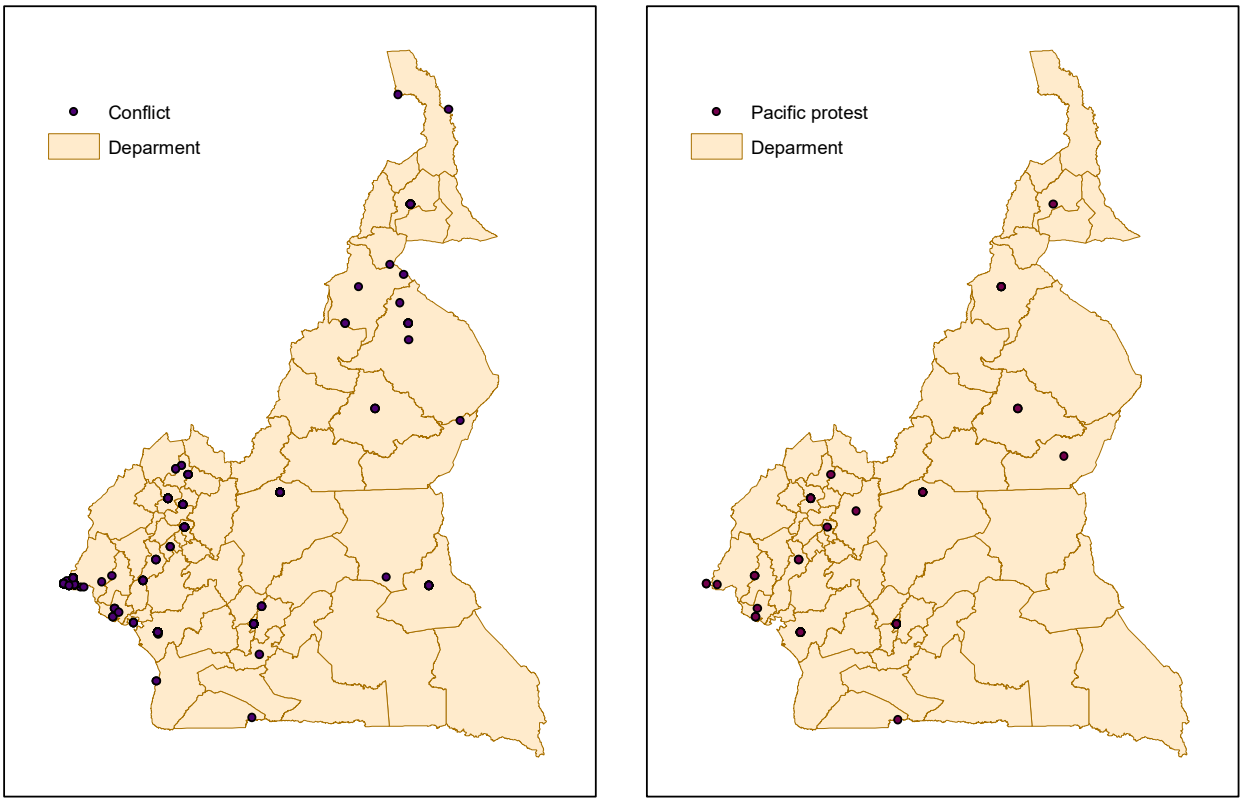
Table 3.A1: Descriptive statistics

	Mean	SD	Min	Max	N
Conflict (dummy)	0.35	0.48	0.00	1.00	98
Pacific protest (dummy)	0.19	0.40	0.00	1.00	98
Proportional Representation	0.12	0.33	0.00	1.00	98
Multimember constituency	0.92	0.28	0.00	1.00	98
% First Party	62.43	15.88	31.96	100.00	98
Geographical and socio-economic control variables					
Terrain Ruggedness Index	0.75	0.56	0.01	2.14	98
Distance to coast (in kilometers)	296.27	275.50	0.00	943.36	98
% unemployed people	0.05	0.03	0.01	0.14	96
Share polygamous marr.	0.80	0.04	0.74	0.89	96

Table 3.A2: Balancing Table

	Majoritarian		*PR*		Mean-Diff	
Vote share first party	69.53	(12.44)	42.29	(6.27)	27.25***	(0.000)
Terrain Ruggedness Index	0.73	(0.51)	0.83	(0.76)	-0.10	(0.678)
Distance Coast (in kilometers)	287.73	(267.02)	309.33	(336.72)	-21.61	(0.844)
Unemployment rate	0.05	(0.03)	0.05	(0.03)	0.00	(0.739)
Polyg. marriage	0.81	(0.03)	0.80	(0.04)	0.00	(0.938)
N	33		12		45	

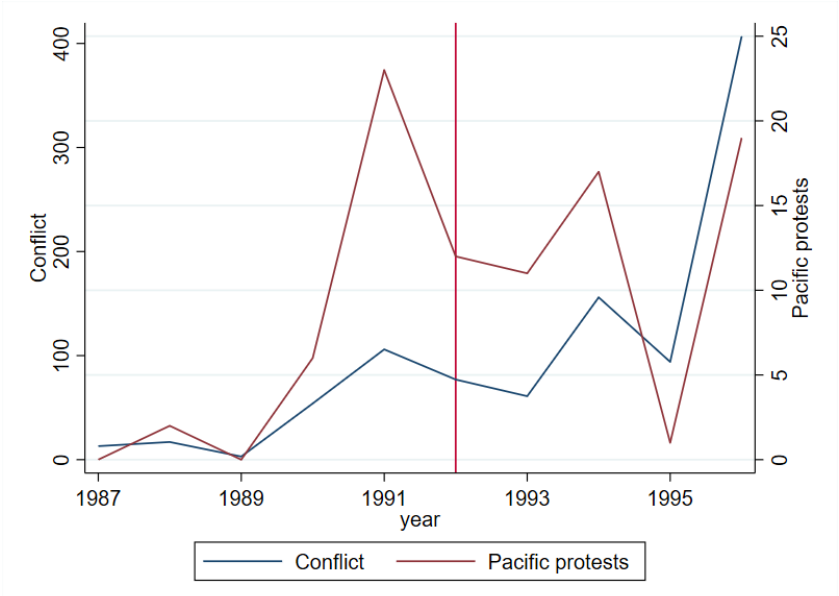
Figure 3.A1: Conflict and pacific protests



(a) Conflict (b) Pacific protests

NOTES: Figure 3.A1a (3.A1b) shows the incidence of conflict (pacific protests) at the department level during the period 1987-1996. Data source: Global Data on Events, Location and Tone (GDELT, Leetaru and Schrodt (2013)).

Figure 3.A2: Conflict and pacific protests



NOTES: The figure shows the number of conflict events (in blue- left axis) and pacific protests (in red- right axis) over the period 1987 and 1996. The vertical red line indicates the treatment year.

B Robustness Analysis

Below robustness tables investigate sensitivity to restricting the analysis to a narrower margin of victory threshold (Table 3.B1) and a placebo analysis (Table 3.B2)

Robustness Analysis: Around the threshold

Table 3.B1: Effect of PR on conflict and pacific protests

	Around threshold: 30-70%				Around threshold: 40-60%			
	Conflict	Conflict	Pacific protest	Pacific protest	Conflict	Conflict	Pacific protest	Pacific protest
PR	-0.241*	-0.240	0.491***	0.469***	-0.325	-0.291	0.675***	0.554
	(0.145)	(0.154)	(0.178)	(0.150)	(0.264)	(0.423)	(0.219)	(0.371)
Department FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time dummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-sample mean × Time dummy	No	Yes	No	No	No	Yes	No	No
Pre-sample mean × Time dummy	No	No	No	Yes	No	No	No	Yes
Ruggedness × Time dummy	No	Yes	No	Yes	No	Yes	No	Yes
Dist. Coast × Time dummy	No	Yes	No	Yes	No	Yes	No	Yes
Polyg. × Time dummy	No	Yes	No	Yes	No	Yes	No	Yes
Observations	62	62	62	62	36	36	36	36
N. Clusters	31	31	31	31	18	18	18	18
R-Squared	0.823	0.846	0.709	0.771	0.751	0.781	0.725	0.789
Mean of Dep. Variable	0.452	0.452	0.274	0.274	0.472	0.472	0.278	0.278

NOTES: This table shows the difference-in-difference estimates of the effect of PR on conflict and pacific protests. The unit of observation is a department i in the pre- and post-treatment periods. The pre-treatment period covers the years 1987-1991, while the post-treatment period the years 1992-1996. The dependent variable in columns 1-2 and 5-6 is a dummy taking a value of 1 if at least one episode of conflict occurred in department i in the pre- and post-treatment periods. Instead, in columns 3-4 and 7-8 the dependent variable is an indicator variable for the existence of a pacific protest in department i. Columns 1-4 (columns 5-8) restrict the sample to departments in which the biggest political party obtained a vote share around 30-70% (40-60%). Odd columns control for department and time fixed effects, while even columns add all our usual control variables: terrain ruggedness, distance to the coast, the share of people in a polygamous union in the department, as well as the pre-sample mean of the dependent variable, all interacted with period fixed effects. Bootstrapped clustered standard errors at department level are shown in parentheses. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Robustness Analysis: Placebo

Table 3.B2: Placebo exercise: PR occuring at a different year

	Baseline		Placebo	
	Conflict	Pacific protest	Conflict	Pacific protest
Proportional Representation	-0.290**	0.358**	0.147	-0.121**
	(0.141)	(0.147)	(0.141)	(0.051)
Department FEs	Yes	Yes	Yes	Yes
Time dummy	Yes	Yes	Yes	Yes
Pre-sample mean × Time dummy	Yes	No	Yes	No
Pre-sample mean × Time dummy	No	Yes	No	Yes
Dist. Coast × Time dummy	Yes	Yes	Yes	Yes
Ruggedness × Time dummy	Yes	Yes	Yes	Yes
Polyg. × Time dummy	Yes	Yes	Yes	Yes
Observations	90	90	90	90
N. Clusters	45	45	45	45
R-Squared	0.828	0.743	0.670	0.687
Mean of Dep. Variable	0.378	0.211	0.211	0.067

NOTES: This table shows the difference-in-difference estimates of the effect of PR on conflict and pacific protests. The unit of observation is a department i in the pre- and post-placebo treatment periods. The pre-placebo treatment period covers the years 1980-1983, while the post-treatment period the years 1984-1988. Columns 1 and 2 are the baseline estimations for conflict and pacific protests, respectively. Columns 3 and 4 show the placebo estimates. All the specifications control for department and time fixed effects, terrain ruggedness, distance to the coast, the share of people in a polygamous union in the department as well as the pre-sample mean of the dependent variable. Bootstrapped clustered standard errors at department level are shown in parentheses. Statistical significance is represented by * p < 0.10, ** p < 0.05, *** p < 0.01.

C Channels

The current section explores potential channels and mechanisms at work. In particular, we focus on whether the observed effects may be due to socio-economic differences arising due to PR versus Majoritarian Representation. For the purpose of this investigation, we first employ the available IPUMS census data of 1976, 1987 and 2005 at the department level. Next, we interpolate the data in order to be able to analyze our period of interest. Figure 3.C3 displays findings for potential mechanisms. First, we focus on educational outcomes (estimate 1 examines the school attendance rate and estimate 2 the share of people with primary education). Next, we investigate living conditions, such as the share of people who own a house (estimate 3), those who have access to electricity at home (estimate 4), those who have finished floor at home (estimate 5) and the ones with flush toilets (estimate 6). Lastly, we examine labor market outcomes, such as labor force participation (estimate 7) and the share of people in the agricultural sector (estimate 8). We do not find any significant effects on any of these outcomes.

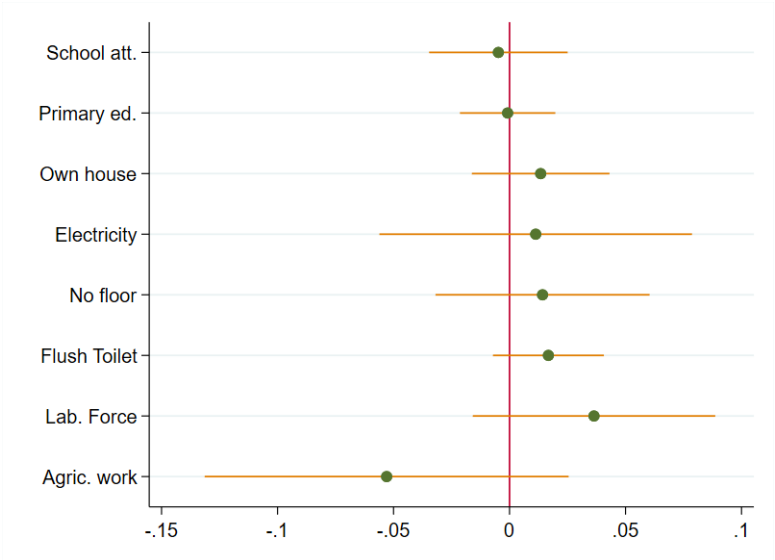
We further explore whether there may be more specific and targeted educational effects, such as the potential effect of being exposed to Proportional Representation during schooling age (i.e. being between 5 and 14 years old in 1992) on the labor force participation with respect to those individuals who were already part of the labor force when the change in the electoral system took place (i.e. from 15 to 64 years old in 1992). Table 3.C3 presents the results of this individual analysis, drawing on the 2005 Cameroonian census. The exposure to the treatment in this analysis occurs at the cohort and at the department level. This allows us to benefit from a large sample of over 740,000 observations. As before, we are not able to find significant results for this investigation.

Table 3.C3: Channels I

	Labor force participation	
Proportional Representation	0.006	0.008
	(0.010)	(0.011)
Arrond. FEs	Yes	No
Deparment FEs	No	Yes
Cohort FEs	Yes	Yes
Observations	741,570	741,570
N. Clusters	190	190
R-Squared	0.053	0.040
Mean of Dep. Variable	0.602	0.602

NOTES: This table shows the difference-in-difference estimates of the effect of PR on the labor force participation for individuals within the age range 18-64 in the 2005 census. The independent variable PR is a dummy variable taking a value of 1 for individuals' cohort with schooling age in 1992 and from a department where Proportional Representation applied in 1992 and 0 otherwise (i.e. individuals for that same cohort, but from a department with majoritarian representation and individuals out of schooling age in 1992). Bootstrapped clustered standard errors at arrondissement level are shown in parentheses. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 3.C3: Channels I



NOTES: The figure shows the estimated coefficients for the effects of PR on the dependent variables presented in the Appendix Table 3.C4 and their 95% confidence interval. All the regressions control for department and time fixed effects, terrain ruggedness, distance to the coast, unemployment rate and the share of people in a polygamous union in the department.

Below is presented Table 3.C4 that contains the underlying estimates of Figure 3.C3.

Table 3.C4: Channels II

	Education		Living cond.				Labor market	
	School att.	Primary ed.	Sh. own house	Electricity	No floor	Flush toil.	Lab. force	% People agric.
PR	-0.005	-0.001	0.013	0.011	0.014	0.017	0.036	-0.053
	(0.015)	(0.010)	(0.015)	(0.034)	(0.024)	(0.012)	(0.027)	(0.040)
Department FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time dummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ruggedness × Time dummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dist. Coast × Time dummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Polyg. × Time dummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	84	90	90	90	90	90	90	84
N. Clusters	42	45	45	45	45	45	45	42
R-Squared	0.918	0.992	0.965	0.957	0.978	0.936	0.884	0.931
Mean of Dep. Variable	0.293	0.389	0.782	0.303	0.578	0.076	0.599	0.415

NOTES: This table shows the difference-in-difference estimates of the effect of PR on a variety of channels. The unit of observation is a department i in the pre- and post-treatment periods. The pre-treatment period covers the years 1987-1991, while the post-treatment period the years 1992-1996. Clustered standard errors at department level are shown in parentheses. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.