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BY THE COMMITTEE CONSISTING OF

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DEDICATION

to:

My parents.

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Abstracts

The first chapter studies the relationship between democratization and production of knowledge. Using bibliographic and patents data, we show that there is a positive and strong impact of democratization on the formation of knowledge in social sciences and business but not in other fields and patenting activity. We confirm these findings using an instrumental variable approach to correct for the endogeneity problems, originating from the unobservables that affect both innovation and democratization and from measurement errors in quantifying democracy indices. Our instrumental variable results are in line with our baseline results. In fact, they indicate that there is a downward bias in the baseline result, which is likely to stem from measurement errors in quantifying democracy indices and unobservables. Finally, our results are robust to a number of estimation methods, outliers, alternative construction of our IV, and different measures of human capital.

In the second chapter, we examine the effect of mortgage credit market conditions on U.S. elections. During the financial crisis of 2008, the U.S economy experienced a sudden drop in mortgage credit supply. According to the previous research, voters responded to the financial crisis of 2007-2008 by punishing the incumbent party in the presidential election, meaning that the vote share of the incumbent party decreased. To further investigate the effects of the financial crisis on elections, we employ an individual-level dataset of loan application outcomes to examine the effects of the contraction in the mortgage credit market on the House and Gubernatorial elections of 2008. A two-stage

approach is employed in order to estimate the impact of the mortgage market conditions on election outcomes. In the first-stage regression, a measure of the change in mortgage credit supply from 2004 to 2008 is derived by taking into account the demand for credit. In the second stage, we estimate the effects of the change in mortgage credit supply on the change in votes for the candidate of the democratic party as well as the candidate of the challenger party. We find no significant impact of the shrinkage in mortgage credits on House and Gubernatorial elections' outcomes. This finding suggests that voters only punish the president for the change in mortgage credits as they may believe lower-level officials are not responsible for this shift.

In the third chapter, we study the effects of elections on the changes in the supply of mortgage credits around elections. According to the literature, politicians have incentives to change economic policies in order to attract voters. We consider a particular type of credit offered through financial institutions and a specific kind of election: mortgage credits supply and Gubernatorial elections. We conduct a spatial regression discontinuity design and explore the financial consequences of gubernatorial elections. We focus on census tracts adjacent to one another yet in two different states. We find that census tracts in states where gubernatorial elections are held and governors have full control over both chambers of state legislatures, lending growth rates increase dramatically. Our results are robust to different specifications.

Chapter 1

How Does Democratization Affect the Production of Knowledge?

The effects of democratic institutions on economic growth have been the subject of a wide range of economic studies. However, until recently, almost all studies found either a null or a negative effect. Gerring et al. (2005), in an extensive review of the literature on the effects of democracy, argue that the literature until the mid-2000s has found a negative or null impact of democracy on economic growth cross-nationally. This view has gained more attention in both academia and the public as the economic growth under an autocratic regime in China and some other nondemocratic states continues to be stable over time and as the consequences of the Arab Spring are dismal.

A handful of recent works have found a positive effect of democracy on economic growth. Papaioannou and Siourounis (2008), in an influential paper, show that democratization is associated with a 1% increase in GDP per capita growth. They propose a novel way of measuring democratization that has become very popular in subsequent works. A more recent paper by Acemoglu et al. (2019) also shows a positive relationship between democracy and income level.

The economic outcomes of democratic institutions in the literature have not been limited to economic growth. A vast literature have shown a positive relationship be-

tween democracy and other economic indicators, including but not limited to total factor productivity, corruption, government expenditure (for instance, Acemoglu et al., 2019; Baum and Lake, 2003; Alesina et al., 2016; Kotera and Okada, 2017; Bhattacharyya and Hodler, 2015). However, the effects of democracy on one of the most important determinants of economic growth, innovation, has been neglected. The formation of knowledge and new technologies can have a significant impact on economic growth according to the endogenous growth theory (Solow, 1957; Romer, 1990; Jones, 2009).

We contribute to the literature by investigating the effects of democratization on knowledge formation in different fields all over the world in the period of 1980 to 2017. We put together a novel dataset of citations received by academic papers and granted patents in various fields to study how democratization affects innovation. Our findings from a panel of countries between 1980 and 2017 show that democratization is associated with an increase in the level of formation of new ideas in social sciences and business fields while its effects in other fields are not conclusive.

Establishing a causal relationship between economic growth and innovation is challenging for several reasons. First, measurement error in reporting democracy indices is considerable. Glaeser et al. (2004) argue that democracy indices do not show perpetual characteristics of the environment. As Acemoglu et al. (2019) point out, this can lead to spurious changes in democracy indices while governments structures remain unchanged.

Second, correctly identifying the role of unobserved characteristics of democracies that also affect knowledge formation remains a challenging problem. For example, there are significant differences between democracies and autocracies in terms of cultural, historical, and institutional features. As a result, cross country estimations are less likely to show the correct causal relationship, if any, between democracy and innovation as they are susceptible to a number of biases.

Third, year and country fixed effects control for time-invariant characteristics of

democracies and non-democracies. However, there is an endogeneity problem originating from time varying unobservables that might simultaneously affect democracy and knowledge formation. This leads to the omitted variable bias.

Forth, the majority of works that study the determinants of the knowledge formation function make use of patenting data as the outcome variable. On the one hand, patents do not include knowledge production in social sciences, business, and humanities. On the other hand, governments in authoritarian regimes are more likely to suppress publications and academicians in social sciences and business as the scientific findings in these disciplines might directly question the competency of authorities.

In our paper, we address all of these challenges. We build on the works by Acemoglu et al. (2019) and Bosetti et al. (2015) and construct a new dataset to examine the effects of democracy on innovation. We employ a dichotomous measure of democracy, first proposed by Acemoglu et al. (2019), to tackle the first concern. In the data section, we carefully review the construction of this measure and its differences with other democracy indices. We address the second concern by employing a difference-in-differences approach with country and year fixed effects.

The instrumental variable approach (IV) motivated by Persson and Tabellini (2008), Aidt and Jensen (2013), and Acemoglu et al. (2019) is used to overcome the endogeneity issue. We discuss the validity of our instrument later in this paper. The instrumental approach relies on the fact that a transition to democracy for a given country often spreads to the other countries in a region. More specifically, we assume that shifts from autocracy to democracy in a given country can diffuse to the neighboring countries but do not have a direct impact on the level of knowledge formation in neighboring countries. We find similar results to the difference-in-differences estimations using the IV estimation approach though the magnitude of the coefficients is higher.

Finally, we employ the yearly citation data reported by Scopus to measure the level

of knowledge production in all fields, including social sciences, business, and humanities. We complement our data with the data on patenting activity to further investigate the effects of democracy.

The rest of the paper is organized as follows. Section 2 reviews the related literature. In section 3, we propose a simple model of knowledge formation. Section 4 describes the construction of our data. In section 5, we present the empirical approach of the paper. Section 6 shows the results of the difference-in-differences estimations. Section 7 develops and presents the estimation results from the IV estimation approach. In section 8, we present the results of the robustness checks. Section 9 further explores the validity of our instrument. Section 10 concludes.

1.1 Literature Review

This paper relates to the literature in several ways. First, it contributes to the growing literature on the political economy of growth. Within this area, it mostly links to the works in which governments sometimes block innovation and economic development due to vested economic interests. Krusell and Rios-Rull (1996) formalize a model in which technology adaptation plays a self-destructive role. According to their model, incumbent innovators have a decent amount of political power to block new technologies. Acemoglu and Robinson (2006) show that political elites tend to prevent economic development due to their fear of replacement that can be potentially brought by technological progress. Besides these two influential works, a handful of papers put emphasis on the role of conflicts between agents on economic development (e.g., Restuccia, 2004; Bellettini and Ottaviano, 2005; and Bridgman et al., 2007).

Second, our work adds to the literature on the persistence of power, policies, and the role of institutions in economic development. Acemoglu (2008), Acemoglu and Robinson

(2006), Acemoglu et al. (2008, 2009, 2015, 2019), Papaioannou and Siourounis (2008), and Persson and Tabellini (2006, 2008) emphasize the positive role of institutions in general, and democracy, in particular, on economic growth. Other papers have linked institutions and economic development, government expenditure, corruption, patent intensity, etc. Bhattacharyya and Hodler (2015), employing a difference-in-differences approach, find that democratization has a positive impact on the reduction of political corruption. Using the same approach, Kotera and Okada (2017) do not find a significant effect of democratization on total government expenditures, but a positive effect on governments expenditures on health. In this regard, a number of papers have focused on the impacts of population diversity on economic development. Alesina et al. (2016) consider total factor productivity (TFP) per capita and patent intensity as the indicators of economic development to examine the relationship between birthplace diversity and economic prosperity.

A large body of literature have investigated the effects of micro-level institutions (e.g., universities, research institutions, etc.) on knowledge formation. Institutions can facilitate access to knowledge by improving the tools via which reliable knowledge is obtained (Mokyr et al., 2002). Furman and Stern (2011) refer to these institutions as research-enhancing institutions. Most of the work investigating the role of research-enhancing institutions make use of citations to scientific papers or the number of granted patents to study the effects of existing knowledge on current developments. Among others, Jaffe et al. (1993) and Henderson et al. (1998) examine whether citations to patents received by universities with a broader geographical scope are more considerable than "control" patents extracted from comparable geographic locations. They all find a direct relationship between research-enhancing institutions and the formation of knowledge. Furman and Stern (2011) examine the impact of a particular type of institution, a biological resource center, that is responsible for verifying and distributing knowledge. Using a

difference-in-differences methodology, they discover that institutions can intensify the effect of individual knowledge formation.

An essential aspect of the role of institutions in economic development is the property and individual rights. Property rights are one of the components of both the Freedom House and Polity IV scores. Patent rights can be considered as a central part of property rights, and they are the subject of a wide range of economic studies on knowledge formation. Most of the research in this area, however, has been inconclusive. Theoretically speaking, the "prospecting theory" suggested by Kitch (1977) argues that patent rights amplify cumulative innovation. In contrast, a handful of papers have argued that patent rights can impede the formation of new ideas when bargaining between the patent parties is not efficient (see, for example, Bessen and Maskin, 2009 and Galasso and Schankerman, 2010). As to the empirical works on the topic, an influential paper by Murray and Stern (2007) is the first work to provide causal evidence of the negative impact of intellectual property rights on subsequent research in the biomedical field. In addition, Galasso and Schankerman (2015) show that patent rights block innovation in most fields and mainly depend on the bargaining environment.

Third, this paper also contributes to the literature on the determinants of knowledge production function and the endogenous growth theory. The formation of knowledge and new technologies can have a significant impact on economic growth according to the endogenous growth theory. (Romer, 1990; Grossman and Helpman, 1991). Hence, A wide range of studies have investigated the determinants of innovation. From the theoretical point of view, the association between innovation and its determinants has not been clearly defined despite a large number of empirical studies on the topic. Stern et al. (2000), propose a fundamental function for national innovative capacity. Following Stern et al. (2000), most of the empirical pieces of research done on this area have assumed that knowledge production is a function of R&D and human capital (HK)

levels in a given country as well as its regional neighborhood countries (e.g., Bode, 2004; Ponds et al., 2009; Fagerberg et al., 2014, and Charlot et al., 2015). Crescenzi and Rodríguez-Pose (2013) add a socio-economic index to the function of knowledge formation that includes the sectoral composition of the economy. They conclude that social conditions are necessary to boost the productivity of innovation efforts. Migration is another factor that may impact knowledge production. Bosetti et al. (2015) show that the number of granted patents and citations received by academic papers increases as the rate of migration to the European countries grows. Baum and Lake (2003) find that democracy has a positive effect on secondary education in non-poor countries.

1.2 Data

We construct two unbalanced annual panel datasets to measure knowledge production, citation information, and patent. Because the citation data is available from 1996, our first dataset comprises 128 countries from 1996 to 2017. As for the patenting activity, our dataset contains 129 countries from 1980 to 2018.[1]

We mainly make use of the dichotomous measure of democracy developed by Acemoglu et al. (2019) to quantify democracy. Although they construct their measure based on the index introduced by Papaioannou and Siourounis (2008), the two indices are not the same. The main difference is that Papaioannou and Siourounis (2008) only consider permanent transitions from nondemocracy to democracy, while Acemoglu et al. (2019) take into account all types of democratization, including reversal transitions. The major drawback of Papaioannou and Siourounis (2008)'s index is that when reversals are not considered, their index worsens the endogeneity problem by encoding information on the future status of democratic institutions. Hence, we believe that the democracy index

[1] These are the number of countries for which all data points are populated.

developed by Acemoglu et al. (2019) measures democracy more efficiently.

As we basically utilize the dichotomous index by Acemoglu et al. (2019), we briefly review how they form their index. This index relies on Freedom House and Polity IV scores. Freedom House publishes the institutional score in three different categories. The first one is the general level of democracy in a given country and codes countries as "Free", "Partially Free", and "Not Free". The other two measures are Civil Liberties and Political Rights scores. The two latter ones range from one to seven. A score of one represents the highest level of freedom and seven the lowest level of freedom. On the other hand, the Polity IV score shows the political regime democracy level on a 21-point scale ranging from -10 (full autocracy) to +10 (full democracy). Considering Freedom House's general score and Polity IV index, a country is coded as free if its general Freedom House score is either "Free" or "Partially Free", and it also has a positive Polity IV value. Finally, if a country is coded as "Free" or "Partially Free", its score is one. Otherwise, it receives a score of zero. This democracy index measures a wide range of modern democratic institutions' characteristics, including free elections, constraints on executive power, and more importantly for our work, civil rights as the latter is one of the main components of the Freedom House's score.

The data provides democracy information for 184 countries. Out of 4,049 country/year observations, 2,757 are indexed as democratic while 1,292 are autocratic. Once we consider countries for which other variables of interest are available, we are limited to 129 countries. This results in 38 and 29 events of democratization and reversals, respectively. In addition, Figure 1.1 shows the evolution of the dichotomous measure of democracy, along with two other indices for the whole world and seven regional categorizations that we employ for our IV estimation (Sub-Saharan Africa, East Asia and the Pacific, Europe Central Asia, Latin America and the Caribbean, Middle East and North Africa, and North America).

Our main outcome variable is innovation. Finding a valid proxy for knowledge formation is cumbersome and is still a subject of debate. Patents data are the most commonly employed proxy for innovation. Patents are legal titles for a specific product or an idea to be protected by patenting authorities when granted to the assignee. The major drawback of patents data in the context of our paper, however, is that not all new ideas are patented, and that the patenting activity is endogenous to the level of income and economic development of a given country, especially given the presence of financial and institutional entry barriers. In addition, the patent granting system is constructed in such a way that a patent office grants monopoly rights for a short period of time to innovations that are applicable in industry. However, knowledge formation is a concept much broader than the number of patents granted. More importantly, patents are mostly granted to the STEM fields. Consequently, by only considering patents data, we would not be able to capture the effects of democracy in social sciences, business, and humanities. Hence, we make use of another measure of innovation, bibliographic data, that is more strictly linked to pure research and entry barriers are lower than those in patenting activity. We can analyze the effects of democracy by academic fields using these measurements.

Utilizing patent and bibliographic data as proxies for knowledge production raises a significant concern regarding the quality of new ideas. To address this concern, we carefully choose two measures that are highly associated with the impact and quality of innovation. As for the patents data, we take into account the layout of the patent system to choose our variable of interest. There are different routes for inventors in order to protect their innovation. Specifically, applying for a patent can be done in a national office or via the Patent Cooperation Treaty (PCT). The former will result in receiving patent rights in a single country or market while the latter gives the applicant patent rights in more than one country, and in some cases, globally. It needs to be mentioned

that applying via PCT is both time and money consuming. We only employ PCT data in order to consider the high-quality patents and remove the lower quality ones from our sample. With regard to bibliographic data, we consider an assumption employed by Bosetti et al. (2015), assuming that influential knowledge has a strong influence when subsequent knowledge uses it to proceed. According to this assumption, we conclude that highly-cited publications are of higher quality as well. In addition, countries with a high number of researchers publish more. As a result, we use citations per document in order to adjust our measure for the number of publications.[2] Citation data have been previously used in the literature as an indicator of the impacts of universities or national output (see, for example, Stuen et al., 2012 and King, 2004). The World Intellectual Property Organization, (WIPO, 2020), provides detailed data on patenting activity. We collect data on the number of papers and the number of citations by year, country, and academic disciplines from The SCImago Journal & Country Rank database.

According to our model, we need data on the available stock of knowledge to researchers and the total number of researchers. A good proxy for the available stock of knowledge is the past innovation efforts.[3] As for the total number of researchers, these data are not available for most developing countries.[4] We address this concern by adding a human capital index and a variable for the total number of people who are engaged in the labor market. Also, our measure of innovation in the citation sample partly addresses this issue.

As for other control variables in our investigation, we use GDP per capita (constant 2010 $US), openness (measured by real exports and imports as a percentage of GDP),

[2]Our analysis for the citation data stops at 2017, while the SCImago (2020) dataset reports citations to all scientific papers published from 1996 to 2020. According to the literature, citations reach their peak after three years. Therefore, our analysis is reasonable. Although not reported here, our estimations were robust to excluding years 2016 and 2017.

[3]We also formed a weighted average of past innovation efforts, giving the highest weights to the most recent years, but our results did not change.

[4]These data are only available for one thousand observations which are one-third of our sample.

population, human capital, the number of persons engaged in the labor market from the Penn World Tables (PWT 9.1), and tertiary education from the World Development Indicators (2019). Descriptive statistics for all the variables we use in this paper are presented in Table 1.1. This table reveals important patterns regarding the difference between democratic and non-democratic countries. Democratic states have a higher level of income and publish more high-impact documents.

1.3 A Simple Model of Knowledge Formation

A wide range of studies have examined the effects of freedoms on economic growth. The effect of freedom on productivity, however, has only been investigated in a few papers. The majority of studies view productivity as a factor that affects economic growth through freedom. Moreover, in most cases, productivity has been assessed by evaluating an economy as a whole using TFP.

Economic growth and productivity are influenced by the formation of knowledge and new technologies, as per endogenous growth theory. We make use of the model developed by Bosetti et al. (2015) and present a simple model to describe the relationship between freedoms and knowledge formation.

According to the model, innovation, I, depends on two variables: number of researchers, N, and their average productivity, ζ.

$$I = N\zeta \tag{1.1}$$

Our model is also relies on another assumption that the average productivity depends on past knowledge formations, P, number of researchers, N, and freedoms, F. As a result:

$$\zeta = (P^{\alpha})(F^{\beta})(N^{\gamma-1}) \tag{1.2}$$

And finally:

$$I = (P^{\alpha})(F^{\beta})(N^{\gamma}) \tag{1.3}$$

Equation (1.3) clearly explains the relationship between freedoms and knowledge formation.

1.4 Empirical Methods

In this section, we use the annual data for 128 countries over the period 1996–2017 to investigate the effect of democratization on citations per document. Following Papaioannou and Siourounis (2008) and Acemoglu et al. (2019), the main linear regression model takes the following form in Equation (1.4).

$$ln(y_{it}) = \beta_1 D_{it} + X_{it}^{'}\Gamma + \alpha_i + \gamma_t + \epsilon_{it} \tag{1.4}$$

The dependent variable, $ln(y_{it})$ is the total number of citations per document or total patents granted in country i in year t. Our main variable of interest is D_{it} which is the dichotomous measure of democracy in country i in time t. $X_{it}^{'}$ is a vector of control variables in country i in year j including two indices for the level of human capital and available stock of knowledge to researchers. We also control for other variables that can affect our outcome variable, including GDP per capita, openness, and the total number of people engaged in the labor market. Additionally, year fixed-effects (γ_t) and (α_i) country-fixed effects control for global trends and time-invariant country characteristics, respectively.[5]

[5] Murray et al. (2016) argue that an increase in the level of openness motivates researchers to enter another country.

Equation (1.4) forms a difference-in-difference model where democratized countries are the treated group, and non-democratized countries serve as the control group. As we include country and year fixed-effects, D captures the effect of democracy in democratized countries compared to the general level of knowledge production in non-reforming countries. Difference-in-difference models have gained more attention in macroeconomic works recently since they address a wide range of limitations of cross-country estimations. With regard to our paper, they can account for time-invariant country characteristics, such as social norms and geography, that may affect both innovation and institutional development. Finally, the model also accounts for global trends that are common in all countries.

The difference-in-differences regression can be biased if the treatments are not random, or if the treated and control countries are systematically different from one another. Such biases may still be present in a macro context and cannot be completely ruled out. As a result, we need to be careful in interpreting our results. However, we make progress by addressing these concerns to be confident we are capturing the causal effects. As discussed earlier, country and time fixed-effects already eliminate biases arising from time-invariant country characteristics and common global trends that may influence both innovation and treatment.

The treated group may be systematically different from untreated countries in the absence of parallel trends before the treatment. Figure 1.2 shows the average number of citations per document in treated and control countries. It provides explicit support for the parallel trend assumption. It is not possible to present a figure like those shown in micro contexts. This is mainly due to the fact that democratization happens in countries at different years, and there is not an exact treatment time. As a result, we go through a different path to test this assumption. The top panel of Figure 1.2 presents the average number of citations per document in control countries from 1996 to 2017.

In the bottom panel, we plot the same variable in treated countries from seven years before to seven years after the treatment year. In the bottom panel, we plot the same variable in treated countries from seven years before to seven years after the treatment year. In our analysis, we need to take into account that previous publications can always be cited by subsequent publications. Hence, the total number of citations received by a given paper increases over time. That is why the graph is downward sloping for the control group. On the other hand, the graph for the treated group displays a downward sloping line before the treatment for all fields. However, after the treatment, we only observe a clear upward trend for two fields: social sciences and business.

Figure 1.3 presents the same analysis for the patents data. However, it displays a different pattern. Patenting activity shows a sharp fall following the democratization time and then gradually increases over time. As mentioned earlier, applying for PCT patents is highly costly, and researchers need to be supported by governments. One reason might be what Papaioannou and Siourounis (2008) refer to as "costs of transitions". They argue that countries experience a fall in the income level right after an event of democratization. Another reason might be the fact that in many cases, a transition from autocracy to democracy is following a crisis (Haggard and Kaufman, 1997) which suggests that we may need to be cautious in interpreting the patents regressions results.

1.5 IV Approach

As discussed above, our estimation method controls for the effects of time-invariant unobserved characteristics. In this section, We employ an instrumental-variables (IV) strategy developed by Acemoglu et al. (2019) to address the endogeneity concerns in this section. There are two issues that can be addressed by a valid instrument. First, there might be some time-varying omitted variables that can affect the occurrence of a

democratization event and subsequent knowledge formation at the same time. Second, the democracy index is subject to measurement error or more specifically, endogenous selection into democracy.

Transitions from an autocracy to a democracy usually happen in regional waves. A wave of democratization in the 1970s turned a large number of countries in Latin America and the Caribbean into democracies. This is also followed by another wave in the 1980s and 1990s. At the same time, after the disintegration of the Soviet Union in the 1990s, a large number of countries in Central Asia, Africa, and Eastern Europe underwent a democratization process. The most recent wave of democratization is the experience of the Arab Spring.

There is a vast literature on the factors affecting such waves. Acemoglu et al. (2019) argue that economic trends cannot account for democratization waves. Bonhomme and Manresa (2015) provide evidence that democratization is highly correlated within regions. As discussed theoretically and empirically in the literature, it suggests that democratization scatters across countries in the same region, which share similar backgrounds, governing structures, and close social norms.

Though Acemoglu et al. (2019) construct their IV based on the work of Persson and Tabellini (2008), these two are notably different. Persson and Tabellini (2008) use neighbors' democracy according to an inverse distance-weighted measure. But Acemoglu et al. (2019) assume that democratization occurs regionally and exploit the regional classification of countries based on the World Bank categorization. Equation (1.5) represents the formal specification of the IV.

$$Z_{it} = \frac{1}{N_{rinit} - 1} \sum_{i' \in r, D_{i'init} = D_{iinit}, i' \neq i} D_{i't} \tag{1.5}$$

Here, r represents one of the seven regions classified by the World Bank.[6] D_{iinit} is a

[6]East Asia and the Pacific, Europe and Central Asia, Latin America and the Caribbean, Middle

the dichotomous measure of democracy taking a value of one ($D_{iinit} = 1$) if the country was initially democratic ($D_{iinit} = 1$) or zero ($D_{iinit} = 0$) if the country was autocratic at the first time it becomes available in the sample. Finally, N_{rinit} measures the number of countries in that region. Because democratization in a given country does not affect political regimes in the neighboring countries immediately, we make use of the first lag of Z as the instrument for democratization. The two-stage least squares ($2SLS$) model can be estimated by

$$ln(y_{it}) = \beta_1 D_{it} + X_{it}^{'}\Gamma + \alpha_i + \gamma_t + \epsilon_{it} \tag{1.6}$$

$$D_{it} = \pi_j Z_{it-1} + X_{it}^{'}\Psi + \theta_i + \delta_i + \nu_{it} \tag{1.7}$$

The most important assumption here is that the exclusion restriction condition holds.[7] The economic justification is that, conditional on the other covariates, year and country fixed effects, democratization has no direct impact on innovation.

1.6 Emperical Results

Table 1.2 reports the results of the estimation of equation (1.4) controlling for a numbers of variables for a sample of 127 countries between 1996 and 2017. We cluster standard errors in all regressions at the country level to control for possible serial correlation within countries. The dependent variable is the total number of citations per document. Column 1 regresses the number of citations in all fields on the democracy index developed by Acemoglu et al. (2019). While we control for income levels, we do not include the other control variables in this regression. The main reason for excluding other control variables is the fact that we might be over-controlling for the other factors that can

East and North Africa, North America, South Asia, and Sub-Saharan Africa

[7] We later statistically show that the instrument is valid and strong.

affect knowledge formation. In particular, one might argue against including human capital index as it is a very slow-moving variable. Column 2, which is our preferred specification, shows the effect of democracy on citations including all control variables. We find no significant effect of democracy on the total level of knowledge production using the citations data. Also, the estimated coefficient on democracy in column 2 is twice as much as the one in the first column, although they are both insignificant. For comparison, we report the effects of the other measures of democracy on knowledge formation in columns 3-7. Other than the polity IV score, which is marginally significant, the effect of democracy on citations per document is not significant.[8] Nevertheless, the estimated coefficient on democracy in our preferred specification is greater than the other democracy indices.

With regard to the control variables, the effect of the existing stock of knowledge, as expected, on the current level of knowledge production is positive and significant in all columns. To control for the effects of the number of skilled researchers, we include two variables for human capital and the total number of people engaged in the labor market. In addition, since our main dependent variable is total number of citations per document, it also captures part of the effects of the number of researchers. Our results indicate that there is no statistically significant effect of a larger pool of researchers on knowledge formation. As mentioned earlier, one possible explanation is that the stock of knowledge and the structure of the dependent variable already partially account for the researchers' population. Another reason could be the effect of decreasing return to labor. As suggested by Bosetti et al. (2015), duplications, overlaps, and negative congestion externalities are the results of an increase in the number of skilled researchers.

We now turn to the heterogeneous effects of democratization on knowledge production. Table 1.3 displays the effect of democracy on citations by fields. Scopus annually

[8]It also shows that the correlation between democracy indices is high.

reports the total number of citations and publications for a given country in 27 different fields. However, many journals are assigned to more than one field. For instance, the Journal of Econometrics is counted in three different fields, including Economics, Mathematics, and Humanities. As a result, this categorization prevents us from forming an aggregated measure of our preferred classification. Hence, we choose five different fields of study which are assumed to be representative of all fields.[9]

As discussed above, we first exclude control variables and then do the same exercise, including all of them. Columns 1 through 6 report the regression results without the control variables. Columns 1 and 7 show the result of our preferred specification in Table 1.2, and the rest of the table explores the heterogeneous effects of democracy. While the magnitude of the coefficients does not change much once we include all control variables, the estimated coefficient on democracy on business becomes insignificant.[10] Other than social sciences and business, the effect of democratization on knowledge production is not significant in any fields. The coefficient on the democracy variable for Social Sciences is 0.155 (standard error=0.0782) and significant at 5% level, meaning that academic publications in social sciences in democracies receive around 16% more citations per document than the ones in non-democracies. The coefficient of democracy for business is close to the one for social sciences. The estimated coefficients of democracy in the other fields are positive and of a smaller magnitude compared to social sciences and business. Even though, to the best of our knowledge, there is no academic work on how governments in authoritarian regimes target professors and academicians, the evidence is abundant in policy discourse. In one case, Berat Albayrak, the Minister of Treasury of Turkey, after an economic turmoil, threatened economists by saying

[9]Business is an exception here. We chose it due to its important implications in the context of our paper.

[10]It is exclusively the effect of the human capital index. Although not reported here, we ran a regression by only excluding the human capital index and the effect of democracy on business was significant at 10% level.

"Economists and academics who state that the economy is getting worse are terrorists".[11] Likewise, the Study International argues that "academic papers by university professors are rigorously monitored in China, and anything critical of how the country is run is immediately censored".[12]

With regard to human capital and available stock of knowledge to researchers, the results are mostly similar to those in column 1. Once again, we do not find any significant effect of human capital on knowledge formation, while the effect of the available stock of knowledge (not reported) to researchers is again positive and significant.

We now turn to the results of our IV estimation. Table 1.4 presents the effects of democratization on innovation using a $2SLS$ estimation method as specified in equations (1.6) and (1.7). The identification is based on the fact that transitions to democracy are influenced by democratization in neighboring countries. In addition, democratization in neighboring countries is unlikely to influence innovation directly. Consequently, the constructed instrument for democratization is valid as the correlation between the IV and democratization in a given country is positive. Moreover, the IV does not directly impact the outcome variable. First, we examine the statistical validity of the instrument. The bottom panel of Table 1.4 presents the first-stage regression for democratization. As expected, the correlation between the constructed instrument and democratization is positive. The F-values in all columns for the tests of the excluded instruments in first-stage regressions surpass 10.[13] It satisfies the "rule of thumb" suggested by Staiger and Stock (1997).

We again report regressions with and without control variables and find similar results. In the seventh column of the top panel, we find that democratization, contrary

[11] YANICAG, November 7, 2019. Link

[12] Study International, August 5, 2019. Link

[13] The F-Stat here has been proposed by Kleibergen and Paap since the Cragg-Donald Wald F statistic is not valid for robust standard errors.

to the OLS estimation, is positively correlated with the total number of citations per document. Moreover, the magnitude of the estimated coefficient on democracy is much higher compared to the one estimated in the OLS regression. The coefficient on the democracy variable is 0.345 (standard error=0.284) and significant at 10% level, meaning that academic publications in democracies receive around 35% more citations per document than the ones in non-democracies. The fact that our IV estimations produce greater effects of democracy on citations might be due to two factors. First, it might be due to some of the factors causing downward biases as mentioned before. Second, measurement error led to attenuation bias. In addition, the IV estimation method controls for the effects unobservables simultaneously affecting both the democratization in different countries as well as the evolution of scientific papers. Another reason for the difference between our OLS and 2SLS results is the Local Average Treatment Effect. Even though our OLS results show the average difference in citations per document, the 2SLS results is only for those whose democratization was affected by the instrument in use (for regional changes).

The rest of Table 1.4 focuses on the heterogeneous impacts of democratization. Column 8 presents the effects of democracy on the total number of citations per document in the field of engineering. Unlike the OLS results, the impact of democratization on this field is significant and positive. The estimated coefficient is 1.369 (standard error=0.639). We later discuss some possible explanations for this change. As for other fields, we find no significant impact of democracy on the field of medicine, while this impact is positive and significant in agriculture. Among all fields, the estimated coefficients of democracy on social sciences and business are highly significant and of a higher magnitude. The coefficient on the democracy variable for social sciences is 1.885 (standard error=0.759) and significant at 5% level, meaning that academic publications in social sciences in democracies receive around 190% more citations per document than

the ones in non-democracies. The estimated coefficient on democracy for business is even higher. In Particular, academic publications in business in democracies are cited around 216% more compared to the ones in autocracies. This confirms our findings in the OLS regression.

Thus far, we have discussed our findings on the effects of democratization on citations. We now turn to explore the effects of democracy on patenting activity. Table 1.5 summarizes Tables 1.2, 1.3, and 1.4 for the patent sample. Our patents sample consists of 129 countries between the years 1980 and 2017. Column 6 focuses on the effects of democratization on the total number of patents granted employing the democracy index proposed by Acemoglu et al. (2019). Column 6 reports the estimated coefficient on democratization patents using the OLS estimation. We find a very small, negative, and insignificant impact of democracy on patenting activity. Columns 1-5 explore the same effect employing the alternative measures of democratization. In Column 7, we estimate the effects of democratization using the IV approach. The estimated coefficient on democracy is again negative and insignificant but of a higher magnitude. This confirms our findings in Figure 1.3. As shown in Figure 1.3, democratization is followed by a sharp fall in patenting activity. One possible explanation could be what we mentioned earlier as the "costs of transitions". The other possible reason might lie on government expenditures. As applying for patents is highly costly and it associates with a high level of risk since the application could be rejected, researchers need to be supported by governments. However, as it has been discussed in the literature, democratization has no significant impact on government spending on education (for example, Mulligan et al., 2010; Kotera and Okada, 2017). On the other hand, many works have found a negative and significant impact of democracy on government expenditure on defense (Dunne et al., 2008; Albalate et al., 2012; and Ünal Töngür et al., 2015). Since many patent fields are military-related, the decrease in government expenditure might be the

other possible reason. To sum up, our findings on the effects of democratization on patenting activity need to be interpreted with caution.

1.7 Robustness Checks

One concern regarding the citation data is the issue of self-citations. We check the sensitivity of our findings by subtracting self-citations from total citations and re-estimate the model. The results are presented in Table 1.6. Our results indicate that self-citations are not a major issue as our results are mainly the same as what we found in Table 1.4.

To control for the number of researchers, we employed the human capital measure from the PWT, along with the total number of people engaged in the labor market. We now replace the human capital index with an index for tertiary education. The results are shown in Table 1.6. While the estimated coefficient of democracy on the total number of citations per document in social sciences, agriculture, and business remains almost unchanged compared to our results from Table 1.4, the coefficient of democracy for engineering changed considerably. As shown in column 2, the effect of democracy on total number of citations per document in the field of engineering is not significant. Furthermore, unlike Table 1.4, the effect of our proxy for the number of researchers is significant. It suggests that human capital level is a major driver of publications in engineering, and we need to be careful about our earlier findings.

In Tables 1.8 and 1.9, the sensitivity of our findings to outliers is checked. Particularly, we exclude countries with a standardized residual above 1.96 or below -1.96 and re-estimate our main specification. As another check to explore the sensitivity of the results, we exclude couturiers with a Cook's distance above the common threshold.[14] The results, in both cases, are quite similar to our findings in the main section, suggesting

[14]Four divided by the number of observations.

that our results are not affected by outliers.

Finally, we check the robustness of our findings regarding the patenting activity. The results are presented in Table 1.10. Column 1 shows our baseline specification.[15] Column 2 replaces the human capital index with tertiary education. In Columns 3 and 4, we control for the effects of ateliers as specified above. In all columns, the effects of democratization on patenting activity is fairly small, negative, and insignificant.

1.8 Additional Checks on the IV Estimation

Although we believe that the construction of our instrument is intuitive as it has been discussed in the literature, there might be some concerns over the results of the IV estimation results. A major concern is the fact that the magnitude of the coefficients inflates dramatically. Another concern arises from the difference between the significance levels of the estimated coefficients in our baseline estimation and IV ones. In particular, the estimated coefficients on total number of citations per document, and in the fields of engineering and agriculture become significant in our IV estimations. In this section, we address these concerns. We first explore the intuition behind the construction of our instrument to make sure our instrument is a good one and then present the estimation results from alternative instruments.

We first check whether democracy is more likely to diffuse within region $\times$ initial regime cells as specified in the IV approach or if it diffuses more rapidly to neighbors. To do so, we employ the data on the latitude and longitude of countries to calculate the average of democracy in neighboring countries, and the weighted average of democracy in other countries given their inverse distances to a certain country. We also employ the data from CNTS to construct a variable of political unrest as a proxy for political

[15] Also, as human capital is slow-moving indicator, we dropped this index and re-estimated all the previous estimations, but we did not observe any major changes in our main findings.

discontent to explore the diffusion patterns of unrest.

Table 1.11 shows the results of regressing the dichotomous measure of democracy of a given country on its own lagged level of democracy, lagged regional democracy, lagged distance weighted democracy, and lagged average democracy in neighboring countries. A full set of country and year fixed effects are included in all regressions. The results of the estimations suggest that transitions to democracy are highly correlated with its own lags but not greatly with lagged regional democracy and lagged distance weighted democracy. Our results contradict the findings of Acemoglu et al. (2019)'s paper as they find lagged regional democracy is the main determinant of transitions to democracy following lagged democracy. There are two reasons that can explain this difference: first, our dataset is not as lengthy as theirs and second, we cover eight years that are not included in their dataset. More specifically, our dataset covers the years after the occurrence of the Arab Spring, while theirs ends right before this phenomenon.

To further investigate the determinants of the innovations to democracy, we study the diffusion patterns of unrest. Table 1.12 shows the results of the spatial diffusion patterns of democracy. Interestingly, we cannot make a conclusive conclusion regarding the main factor responsible for transitions to democracy. Depending on the specification of the model, either of lagged regional democracy, lagged distance weighted democracy, or lagged neighbors' democracy might explain transitions to democracy. Once again, our findings are not consistent with Acemoglu et al. (2019)'s paper.

We replace our instrument with a new one and repeat the same exercise as specified in equations (1.6) and (1.7). The new instrument is the lagged neighbors' democracy.[16] Table 1.13 shows the estimation results. While the effect of democracy on total number of citations per document is almost the same as Table 1.4, some important changes are

[16] We could not do the same exercise with lagged distance weighted democracy as it was not statistically a valid instrument.

observed.[17] First, the magnitude of the coefficients for all fields are less than half of the ones we found in Table 1.4. For example, the estimated coefficient of democracy on citations per document in social sciences is 0.887, meaning that transitions to democracy, on average, increase the number of citations per document in social science by around 90% while it was around 200% in Table 1.4. Second, the effect of democracy on citations per document in engineering is not significant anymore and its magnitude is much lower than what we observed in Table 1.4.

In short, while our main argument still strongly holds as the effect of democracy on social sciences and business is highly significant and sizable, our new results cast doubt on our finding in Table 1.4 on the effects of democracy on other fields. It should also be noted that once we changed the human capital index, knowledge formation in engineering was not impacted by democracy. Our findings in Table 1.13 are consistent with our findings in our baseline regressions.

1.9 Conclusion

While the effects of democracy on a wide range of economic indicators have been studied in the literature, its effects on innovation have been neglected. As suggested by the endogenous growth theory, new ideas are essential for economic growth. In this paper, we explored the effects of democratization on knowledge formation using the democracy index developed by Acemoglu et al. (2019). To quantify the formation of new ideas, we made use of two indices proposed by the literature: citations to academic papers and patenting activity.

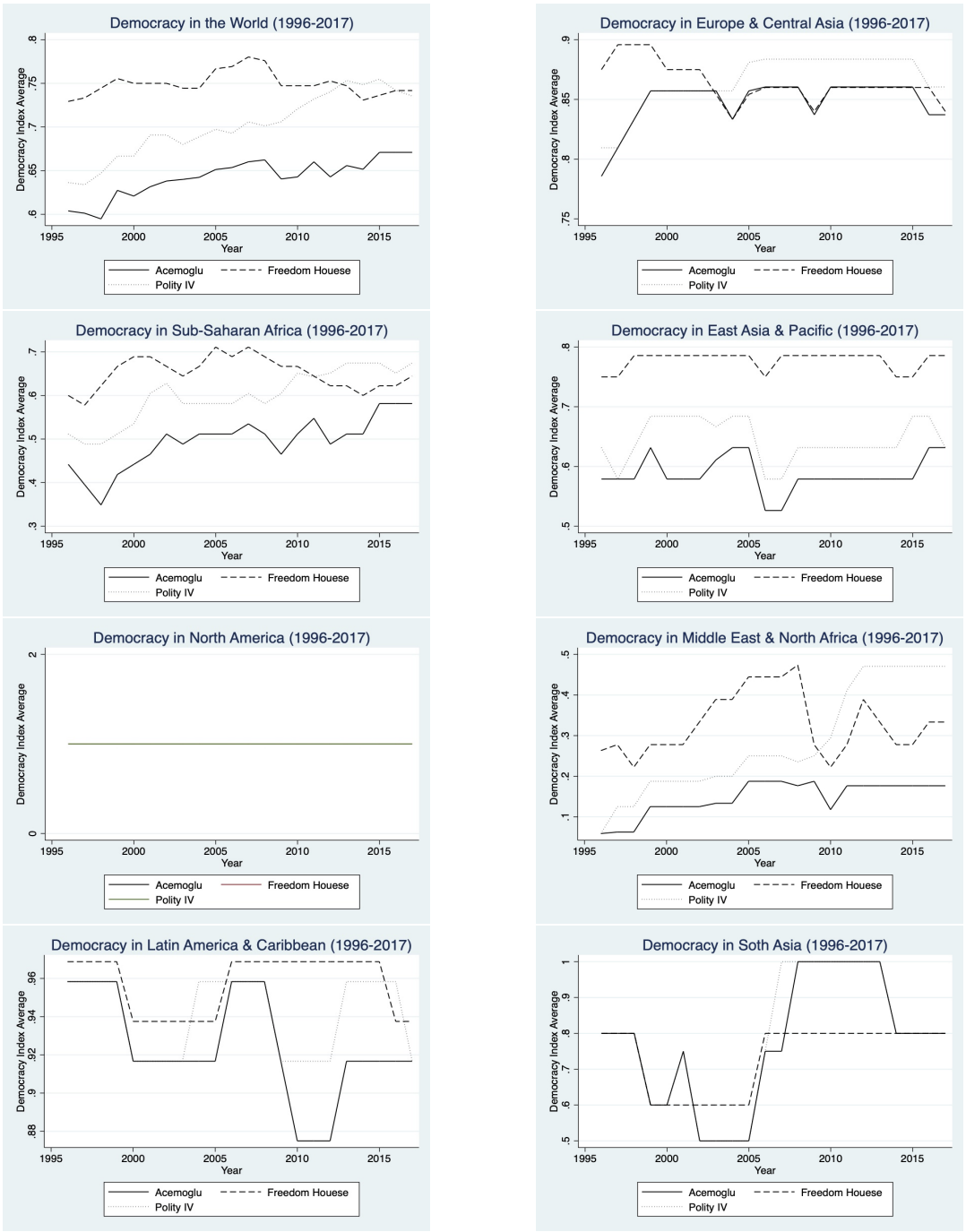
To address the endogeneity problem originating from the measurement error in quantifying the democracy index and observable factors which simultaneously affect both de-

[17] We also repeated the same exercise for our patent dataset, but it was almost similar to our previous findings.

mocratization and knowledge formation, we employed an IV estimation strategy. Our IV method relies on the fact that democratization can spread to the neighboring countries while it cannot directly affect knowledge production in other countries.

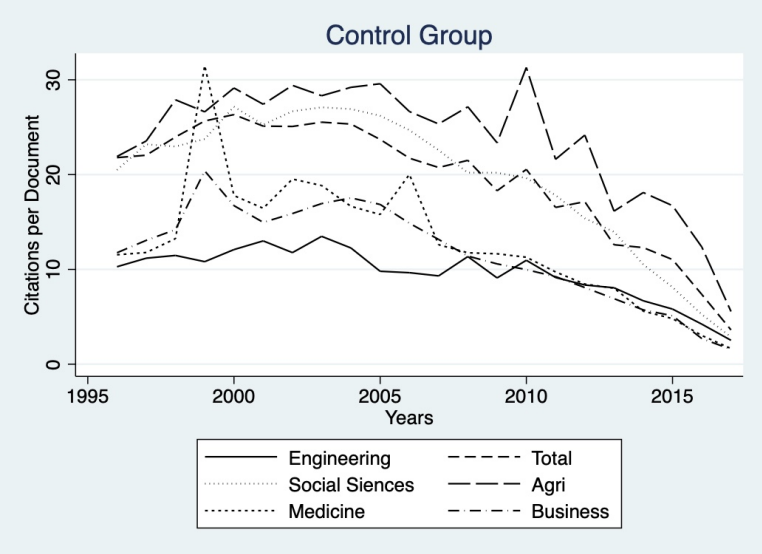
We found a significantly positive impact of democracy on total citations per document. As for the heterogeneous impact of democratization, we detected a pronounced and robust positive effect of democracy on citations per document in social sciences and business. We also found positive effects for the other fields but we need to be cautious in how we interpret them. While the effects of democracy on the number of citations received by publications in the field of engineering is positive in our main specification, it is highly sensitive to the measure of human capital and becomes insignificant once we control for tertiary education. Regarding our patents data, we did not find any significant effect of democratization on patenting activity. While we provided possible explanations for our results, we believe further research needs to be done to reach conclusive conclusions.

Figure 1.1: Democracy Measures Across Regions Over Time

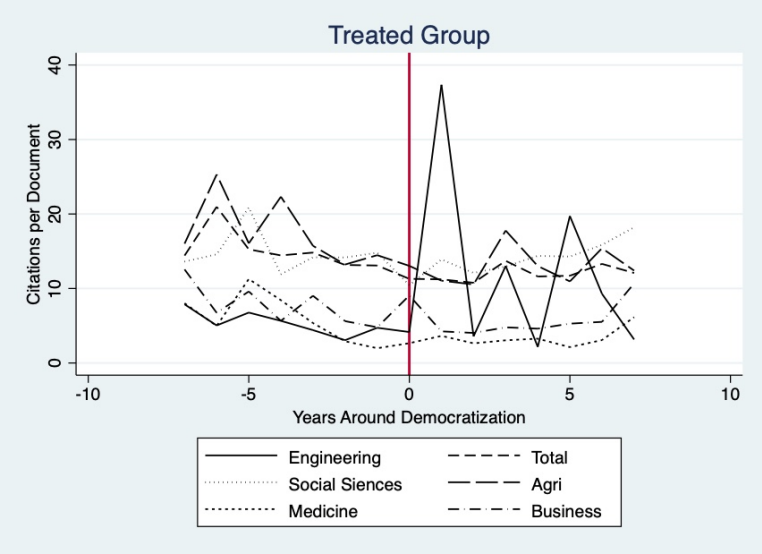


Notes: The dichotomous democracy index for non-democracies and democracies is 0 and 1, respectively. Categorization is based on the World Band definition. Source: Author's calculations based on Acemoglu et al. (2019)'s approach.

Figure 1.2: Average Citations Per Document in Control and Treated Countries



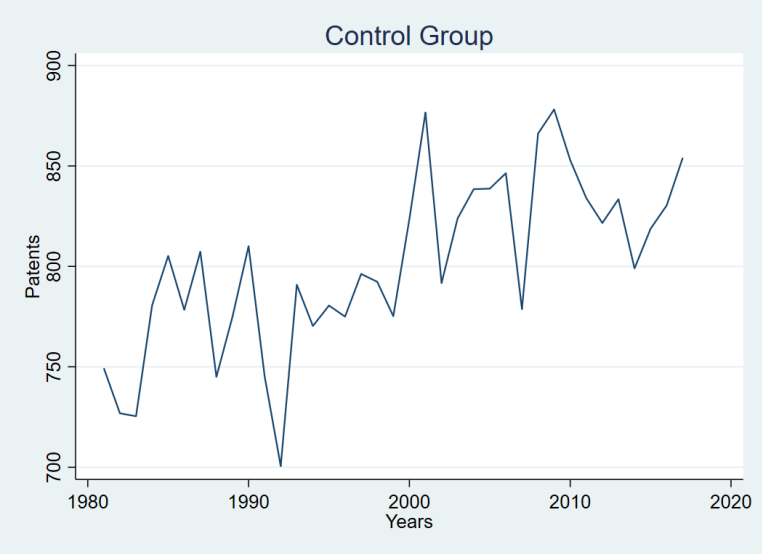
(a)



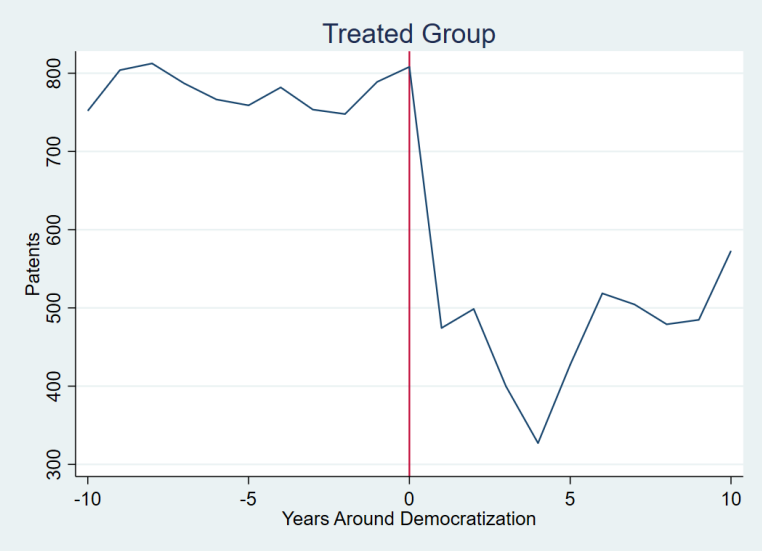
(b)

Notes: The top panel presents the average number of citations for democracies and the bottom panel presents the same variable for non-democracies. Source: Author's calculations using SCImago data.

Figure 1.3: Average Number of Patents Granted in Control and Treated Countries



(a)



(b)

Notes: The top panel presents the average number of citations for democracies and the bottom panel presents the same variable for nondemocracies. Source: Author's calculations using WIPO data.

Table 1.1: Descriptive Statistics

	Democracies			***Nondemocracies***		
	Mean	Std.Dev.	Obs	Mean	Std.Dev.	Obs
CPD Agriculture	18.66	15.32	2,757	15.74	14.19	1,292
CPD Arts and Humanities	17.67	40.92	2,757	15.41	41.64	1,292
CDP Bio Chemistry	24.92	22.02	2,757	20.37	20.93	1,292
CPD Bussiness	11.21	22.36	2,757	7.92	21.90	1,292
CPD Chemistry	16.64	24.25	2,757	12.75	22.96	1,292
CPD Chemical Engineering	14.92	27.14	2,757	10.65	17.31	1,292
CPD Computer Science	7.51	14.87	2,757	6.43	19.87	1,292
CPD Decision Science	8.94	15.26	2,757	6.98	15.87	1,292
CPD Dentistry	10.85	16.81	2,757	6.75	12.73	1,292
CPD Earth Sciences	18.80	21.50	2,757	14.92	20.24	1,292
CPD Economics	11.19	16.78	2,757	8.02	15.34	1,292
CPD Energy	10.65	13.57	2,757	8.73	12.97	1,292
CPD Engineering	8.59	9.63	2,757	7.74	13.03	1,292
CPD Environmetal Sciences	19.84	19.06	2,757	16.30	20.63	1,292
CPD Health	10.56	15.27	2,757	6.81	13.98	1,292
CPD Immunology	24.21	26.12	2,757	19.73	18.35	1,292
CPD Material Sciences	10.71	12.44	2,757	8.78	10.31	1,292
CPD Mathematics	7.18	9.18	2,757	6.43	14.55	1,292
CPD Medicine	23.06	21.28	2,757	18.95	34.58	1,292
CPD Multidisciplinary	65.23	111.80	2,757	35.16	126.49	1,292
CPD Neuroscience	17.52	20.62	2,757	11.48	18.36	1,292
CPD Nursing	14.70	23.40	2,757	10.49	18.97	1,292
CPD Pharmacology	17.21	17.38	2,757	15.19	17.91	1,292
CPD Psychology	15.77	54.00	2,757	13.08	37.21	1,292
CPD Physics	12.62	13.65	2,757	9.42	14.72	1,292
CPD Total	19.49	14.62	2,757	15.63	16.44	1,292
CPD Veterinary	10.14	12.68	2,757	10.13	13.42	1,292
CPD Social Sciences	10.58	12.88	2,757	9.38	17.36	1,292
Patents	775.28	490.97	3,168	624.43	515.77	2,338
Human Capital	2.63	0.68	2,076	2.04	0.56	886
Tertiary Education	38.19	27.52	1474	18.43	20.59	698
GDP per Capita	$15,709	$14,577	2,400	$11,947	$19,956	1,080
No. People Engaged	15.20	49.13	2,362	24.05	106.83	1,080

Notes: The table shows the summary statistics of the citation, patent, and other control variables. CPD stands for "citation per document" and is calculated by dividing the number of citations by number of documents. GDP per capita is in constant $US 2010, openness is measured by real exports and imports as percentage of GDP. The data for population, human capital, and the number of persons engaged in labor market are from the Penn World Tables (PWT 9.1). The data for tertiary education comes from the World Development Indicators (2019).

Table 1.2: Baseline Regression Results

	(1) Total	(2) Total	(3) Total	(4) Total	(5) Total	(6) Total	(7) Total
Democracy	0.026	0.042					
	(0.040)	(0.039)					
polity2			0.007*				
			(0.004)				
Civil Liberties				-0.007			
				(0.012)			
Political Rights					-0.004		
					(0.009)		
Polity IV (Dummy)						0.038	
						(0.037)	
Freedom House (Dummy)							-0.002
							(0.030)
Knowledge Stock	0.175***	0.295***	0.294***	0.279***	0.279***	0.295***	0.279***
	(0.065)	(0.043)	(0.042)	(0.043)	(0.043)	(0.043)	(0.043)
GDP Per Capita	0.083	0.055	0.052	0.040	0.041	0.057	0.042
	(0.050)	(0.038)	(0.037)	(0.037)	(0.037)	(0.033)	(0.034)
Human Capital		0.061	0.047	0.062	0.064	0.050	0.066
		(0.221)	(0.221)	(0.220)	(0.218)	(0.220)	(0.219)
Openness		-0.236**	-0.229**	-0.301***	-0.300***	-0.234***	-0.301***
		(0.091)	(0.090)	(0.091)	(0.091)	(0.089)	(0.090)
People Engaged		0.110	0.106	0.120	0.117	0.110	0.117
		(0.088)	(0.090)	(0.089)	(0.088)	(0.089)	(0.088)
Observations	3,001	2,673	2,673	2,806	2,806	2,673	2,806
Country FE	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y
No. Countries	144	128	128	134	134	128	134
R2	0.654	0.770	0.770	0.749	0.749	0.769	0.749

Notes: The dependent variable is the natural logarithm of total number of citations per document. Democratization is created using Acemoglu et al. (2019)'s approach. All other independent variables are in the natural logarithm. All specifications include year and country fixed effects, as well as a constant term, but the results are not presented here. ***, **, and * represent p<0.01, p<0.05, p<0.1, respectively. Robust standard errors, clustered at country-level, are in parentheses.

Table 1.3: The Heterogeneous Effects of Democratization on Citations

	(1) Total	(2) Engineering	(3) Social Sciences	(4) Agriculture	(5) Business	(6) Medical	(7) Total	(8) Engineering	(9) Social Sciences	(10) Agriculture	(11) Business	(12) Medical
Democracy	0.026	0.104	0.155*	-0.039	0.151*	0.051	0.042	0.065	0.155**	0.011	0.138	0.076
	(0.040)	(0.082)	(0.093)	(0.082)	(0.087)	(0.057)	(0.039)	(0.076)	(0.078)	(0.065)	(0.088)	(0.062)
GDP Per Capita	0.083	-0.102	0.006	-0.100*	0.255**	0.169**	0.055	-0.213*	0.044	-0.036	0.288**	0.142**
	(0.050)	(0.101)	(0.107)	(0.057)	(0.115)	(0.066)	(0.038)	(0.112)	(0.127)	(0.045)	(0.142)	(0.062)
Human Capital							0.060	0.858	-0.225	-1.052***	-0.129	-0.419
							(0.221)	(0.538)	(0.541)	(0.361)	(1.009)	(0.374)
Openness							-0.236**	-0.183	-0.722***	-0.357**	-0.480	-0.448***
							(0.091)	(0.228)	(0.229)	(0.153)	(0.389)	(0.122)
People Engaged							0.110	0.427***	0.490***	0.279***	0.319	0.107
							(0.088)	(0.158)	(0.131)	(0.090)	(0.302)	(0.138)
Observations	3,001	3,001	3,001	3,001	3,001	3,001	2,673	2,673	2,673	2,673	2,673	2,673
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Country FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
No. Countries	144	144	144	144	144	144	128	128	128	128	128	128
R2	0.654	0.125	0.308	0.526	0.141	0.404	0.770	0.169	0.389	0.629	0.157	0.524

Notes: The dependent variable is the natural logarithm of total number of citations per document. Democratization is created using Acemoglu et al. (2019)'s approach. All other independent variables are in the natural logarithm. All specifications include year and country fixed effects, as well as a constant term, but the results are not presented here. ***, **, and * represent p<0.01, p<0.05, p<0.1, respectively. Robust standard errors, clustered at country-level, are in parentheses.

Table 1.4: The Heterogeneous Effects of Democratization on Citations: IV Estimations

	(1) Total	(2) Engineering	(3) Social Sciences	(4) Agriculture	(5) Business	(6) Medical	(7) Total	(8) Engineering	(9) Social Sciences	(10) Agriculture	(11) Business	(12) Medical
Second Stage												
Democracy	0.576**	1.537**	2.553***	1.026**	2.200***	0.726*	0.345*	1.369**	1.888**	0.807**	2.167***	0.317
	(0.201)	(0.690)	(0.867)	(0.407)	(0.764)	(0.409)	(0.284)	(0.639)	(0.759)	(0.342)	(0.783)	(0.298)
GDP Per Capita	0.103	-0.053	0.095	-0.0598	0.326*	0.197**	0.062	-0.187	0.087	-0.015	0.336	0.149**
	(0.046)	(0.133)	(0.184)	(0.082)	(0.177)	(0.085)	(0.063)	(0.142)	(0.187)	(0.073)	(0.211)	(0.068)
Human Capital							-0.025	0.549	-0.682	-1.300***	-0.655	-0.512
							(0.251)	(0.687)	(0.713)	(0.439)	(1.201)	(0.392)
Openness							-0.198*	-0.0147	-0.529	-0.256	-0.222	-0.419***
							(0.114)	(0.300)	(0.362)	(0.223)	(0.501)	(0.133)
People Engaged							0.096	0.361**	0.424***	0.243***	0.223	0.097
							(0.096)	(0.156)	(0.157)	(0.094)	(0.307)	(0.142)
First Stage												
First-lagged Z	0.477***	0.479***	0.496***	0.496***	0.480***	0.477***	0.520***	0.525***	0.512***	0.526***	0.524***	0.520***
	(0.119)	(0.118)	(0.116)	(0.120)	(0.119)	(0.140)	(0.139)	(0.139)	(0.140)	(0.140)	(0.140)	(0.140)
Observations	2,975	2,975	2,975	2,975	2,975	2,975	2,975	2,676	2,676	2,676	2,676	2,676
KP F-Stat	16.009	16.286	16.259	17.005	16.17	15.907	15.54	15.77	15.37	15.6	15.59	15.31
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Country FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
No. Countries	144	144	144	144	144	144	144	128	128	128	128	128

Notes: The dependent variable is the natural logarithm of total number of citations per document. Democratization is created using Acemoglu et al. (2019)'s approach. All other independent variables are in the natural logarithm. All specifications in the second-stage regressions include year and country fixed effects, as well as a constant term, but the results are not presented here. The instrument for democratization is the first lag of the created instrument. All exogenous variables in the second-stage regressions are also included in the first-stage regressions, but we do not report the results here. ***, **, and * represent p<0.01, p<0.05, p<0.1, respectively. Robust standard errors, clustered at country-level, are in parentheses.

Table 1.5: The Effects of Democratization on Patenting Activity

	(1) Patents	(2) Patents	(3) Patents	(4) Patents	(5) Patents	(6) Patents	(7) Patents
Polity IV	-0.005						
	(0.003)						
Civil Liberties		0.024*					
		(0.014)					
Political Rights			0.014				
			(0.010)				
Polity IV Dummy				-0.031			
				(0.035)			
Freedom House D					-0.040		
					(0.039)		
Democracy						-0.0240	-0.195
						(0.040)	(0.142)
							(0.118)
Knowledge Stock	0.299***	0.305***	0.305***	0.300***	0.306***	0.300***	0.294***
	(0.031)	(0.030)	(0.029)	(0.031)	(0.029)	(0.031)	(0.030)
Human Capital	0.302	0.192	0.175	0.288	0.175	0.276	0.111
	(0.227)	(0.212)	(0.214)	(0.226)	(0.213)	(0.228)	(0.340)
GDP per Capita	-0.049	-0.046	-0.047	-0.050	-0.050	-0.044	-0.048
	(0.040)	(0.040)	(0.040)	(0.040)	(0.040)	(0.040)	(0.040)
Openness	0.142	0.117	0.115	0.149	0.118	0.149	0.089
	(0.137)	(0.127)	(0.126)	(0.138)	(0.127)	(0.135)	(0.142)
People Engaged	0.059	0.052	0.060	0.062	0.063	0.063	0.078
	(0.087)	(0.082)	(0.081)	(0.086)	(0.079)	(0.086)	(0.085)
First Stage							
First-lagged Z							0.745***
							(0.095)
Observations	4,523	4,897	4,897	4,523	4,897	4,523	4,486
KP F-Stat Stat							60.796
Year FE	Y	Y	Y	Y	Y	Y	Y
Country FE	Y	Y	Y	Y	Y	Y	Y
No. Countries	129	140	140	129	140	129	129
R2	0.110	0.111	0.111	0.109	0.110	0.109	

Notes: The dependent variable is the natural logarithm of total number of patents granted. Democratization is created using Acemoglu et al. (2019)'s approach. All other variables are in the natural logarithm. All specifications in columns 1-6 and the specification in the second-stage regression include year and country fixed effects, as well as a constant term, but the results are not presented here. The instrument for democratization is the first lag of the created instrument. All exogenous variables in the second-stage regression are also included in the first-stage regressions, but we do not report the results here. ***, **, and * represent p<0.01, p<0.05, p<0.1, respectively. Robust standard errors, clustered at country-level, are in parentheses.

Table 1.6: The Heterogeneous Effects of Democratization on Citations: Removing Self-Citations

	(1) Total	(2) Engineering	(3) Social Sciences	(4) Agriculture	(5) Business	(6) Medicine
Democracy	0.325	1.376**	1.972***	0.833**	2.279***	0.304
	(0.203)	(0.641)	(0.759)	(0.342)	(0.777)	(0.297)
Human Capital	-0.047	0.211	-0.777	-1.193***	-0.819	-0.530
	(0.253)	(0.705)	(0.718)	(0.402)	(1.248)	(0.393)
GDP per Capita	0.060	-0.150	0.130	-0.006	0.293	0.154**
	(0.041)	(0.139)	(0.179)	(0.070)	(0.193)	(0.068)
Openness	-0.198*	-0.153	-0.437	-0.338*	-0.726*	-0.536***
	(0.119)	(0.312)	(0.373)	(0.180)	(0.437)	(0.166)
People Engaged	0.100	0.370**	0.458***	0.194*	0.207	0.099
	(0.091)	(0.149)	(0.148)	(0.100)	(0.310)	(0.150)
Observations	2,676	2,676	2,676	2,676	2,676	2,676
KP F-Stat	15.55	15.764	15.334	15.556	15.582	15.309
Year FE	Y	Y	Y	Y	Y	Y
Country FE	Y	Y	Y	Y	Y	Y
No. Countries	127	127	127	127	127	127

Notes: This table shows the second-stage estimation results. The dependent variable is the natural logarithm of the number of citations per document in each filed. Democratization is created using Acemoglu et al. (2019)'s approach. All other independent variables are in the natural logarithm. All specifications in the second-stage regressions include year and country fixed effects, as well as a constant term, but the results are not presented here. The instrument for democratization is the first lag of the created instrument. All exogenous variables in the second-stage regressions are also included in the first-stage regressions, but we do not report the results here. ***, **, and * represent $p<0.01$, $p<0.05$, $p<0.1$, respectively. Robust standard errors, clustered at country-level, are in parentheses.

Table 1.7: The Heterogeneous Effects of Democratization on Citations with Alternative Measure of Human Capital

	(1) Total	(2) Engineering	(3) Social Sciences	(4) Agriculture	(5) Business	(6) Medicine
Democracy	0.448	0.555	2.122**	1.381**	2.151**	0.168
	(0.419)	(0.715)	(0.949)	(0.588)	(0.970)	(0.482)
Tertiary Education	0.003	0.046**	-0.003	0.019	0.051*	-0.0100
	(0.012)	(0.020)	(0.022)	(0.019)	(0.029)	(0.011)
GDP per Capita	0.178**	0.145	0.219	0.013	0.598***	0.283***
	(0.084)	(0.151)	(0.168)	(0.120)	(0.206)	(0.086)
Openness	-0.352**	-0.240	-0.089	0.176	-0.440	-0.453*
	(0.149)	(0.304)	(0.331)	(0.250)	(0.429)	(0.246)
People Engaged	0.064	0.213	0.429***	0.165**	0.286	0.057
	(0.110)	(0.131)	(0.130)	(0.079)	(0.241)	(0.120)
Observations	2,005	2,005	2,005	2,005	2,005	2,005
KP F-Stat Stat	12.369	12.314	11.707	12.432	12.136	12.034
Year FE	Y	Y	Y	Y	Y	Y
Country FE	Y	Y	Y	Y	Y	Y
No.Countries	134	134	134	134	134	134

Notes: This table shows the second-stage estimation results. The dependent variable is the natural logarithm of the number of citations per document in each filed. Democratization is created using Acemoglu et al. (2019)'s approach. All other independent variables are in the natural logarithm. All specifications in the second-stage regressions include year and country fixed effects, as well as a constant term, but the results are not presented here. The instrument for democratization is the first lag of the created instrument. All exogenous variables in the second-stage regressions are also included in the first-stage regressions, but we do not report the results here. ***, **, and * represent $p<0.01$, $p<0.05$, $p<0.1$, respectively. Robust standard errors, clustered at country-level, are in parentheses.

Table 1.8: The Heterogeneous Effects of Democratization on Citations: Removing Outliers

	(1) Total	(2) Engineering	(3) Social Sciences	(4) Agriculture	(5) Business	(6) Medicine
Democracy	-0.008	0.024	0.095***	-0.047***	0.111**	0.016
	(0.014)	(0.036)	(0.035)	(0.018)	(0.048)	(0.024)
Human Capital	0.143	0.341*	-0.627**	-0.070	-0.541	0.014
	(0.107)	(0.176)	(0.240)	(0.137)	(0.471)	(0.165)
Openness	-0.099**	-0.443***	-0.084	-0.143**	-0.18	-0.060
	(0.048)	(0.089)	(0.113)	(0.065)	(0.126)	(0.068)
People Engaged	-0.035	0.0220	0.153***	0.065	0.150	0.012
	(0.043)	(0.053)	(0.058)	(0.046)	(0.127)	(0.101)
GDP per Capita	0.040*	-0.010	0.057	0.022	0.272***	0.104***
	(0.023)	(0.030)	(0.045)	(0.029)	(0.076)	(0.034)
Observations	2,571	2,371	2,509	2,552	2,523	2,582
F Stat	0.000	0.000	0.000	0.000	0.000	0.000
Year FE	Y	Y	Y	Y	Y	Y
Country FE	Y	Y	Y	Y	Y	Y
No. Countries	127	127	127	127	127	127
R2	0.964	0.934	0.873	0.953	0.900	0.922

Notes: The dependent variable is the natural logarithm of total number of citations per document. Democratization is created using Acemoglu et al. (2019)'s approach. All other independent variables are in the natural logarithm. All specifications include year and country fixed effects, as well as a constant term, but the results are not presented here. ***, **, and * represent $p<0.01$, $p<0.05$, $p<0.1$, respectively. Robust standard errors, clustered at country-level, are in parentheses.

Table 1.9: The Heterogeneous Effects of Democratization on Citations: Removing Outliers

	(1) Total	(2) Engineering	(3) Social Sciences	(4) Agriculture	(5) Business	(6) Medicine
Democracy	0.007	0.104*	0.183***	-0.0203	0.159*	0.002
	(0.020)	(0.053)	(0.067)	(0.026)	(0.080)	(0.027)
Human Capital	-0.091	0.585	-0.577	-0.489**	-0.496	-0.369
	(0.166)	(0.438)	(0.363)	(0.206)	(0.798)	(0.225)
Openness	-0.180**	-0.495***	-0.667***	-0.171*	-0.662***	-0.216**
	(0.078)	(0.154)	(0.215)	(0.091)	(0.214)	(0.101)
People Engaged	0.071	0.246**	0.356***	0.130**	0.374*	0.134
	(0.079)	(0.100)	(0.098)	(0.065)	(0.216)	(0.133)
GDP per Capita	0.067**	-0.032	0.068	0.002	0.343***	0.093**
	(0.031)	(0.073)	(0.077)	(0.035)	(0.131)	(0.045)
Observations	2,554	2,508	2,513	2,565	2,522	2,564
F Stat	0.000	0.000	0.000	0.000	0.000	0.000
Year FE	Y	Y	Y	Y	Y	Y
Country FE	Y	Y	Y	Y	Y	Y
No. Countries	127	127	127	127	127	127
R2	0.914	0.466	0.674	0.876	0.366	0.806

Notes: The dependent variable is the natural logarithm of total number of citations per document. Democratization is created using Acemoglu et al. (2019)'s approach. All other independent variables are in the natural logarithm. All specifications include year and country fixed effects, as well as a constant term, but the results are not presented here. ***, **, and * represent $p<0.01$, $p<0.05$, $p<0.1$, respectively. Robust standard errors, clustered at country-level, are in parentheses.

Table 1.10: The Effects of Democratization on Patents: Robustness Checks

	(1) Patents	(2) Patents	(3) Patents	(4) Patents
Democracy	-0.024	-0.018	-0.030	-0.001
	(0.040)	(0.040)	(0.003)	(0.031)
Knowledge Stock	0.300***	0.277***	0.976***	0.472***
	(0.031)	(0.041)	(0.004)	(0.027)
Human Capital	0.276	-0.027	-0.009	0.173
	(0.228)	(0.018)	(0.018)	(0.181)
GDP per Capita	-0.044	-0.053	0.002	-0.040
	(0.040)	(0.046)	(0.004)	(0.026)
Openness	0.149	0.057	0.019	0.025
	(0.135)	(0.156)	(0.012)	(0.098)
People Engaged	0.063	0.102	0.002	0.036
	(0.09)	(0.115)	(0.008)	(0.061)
Observations	4,523	2,842	3,275	4,162
F Stat	0.000	0.000	0.000	0.000
Year FE	Y	Y	Y	Y
Country FE	Y	Y	Y	Y
No. Countries	130	129	129	129
R2	0.110	0.096	0.997	0.334

Notes: The dependent variable is the natural logarithm of total number of patents granted. Democratization is created using Acemoglu et al. (2019)'s approach. All other independent variables are in the natural logarithm. All specifications include year and country fixed effects, as well as a constant term, but the results are not presented here. ***, **, and * represent $p<0.01$, $p<0.05$, $p<0.1$, respectively. Robust standard errors, clustered at country-level, are in parentheses.

Table 1.11: Diffusion Patterns of Democracy

	(1)	(2)	(3)	(4)	(5)
Lagged democracy	0.713***	0.715***	0.712***	0.715***	0.713***
	(0.033)	(0.032)	(0.034)	(0.033)	(0.034)
Lagged regional democracy	0.020			0.007	0.017
	(0.045)			(0.048)	(0.049)
Lagged distance-weighted democracy		0.263**		0.259**	
		(0.123)		(0.130)	
Lagged neighbors' average democracy			0.070		0.051
			(0.194)		(0.210)
Observations	3,131	3,155	3,155	3,131	3,131
No. Countries	154	149	149	149	149
R2	0.520	0.524	0.524	0.524	0.524

Notes: The dependent variable is the democratization index created using Acemoglu et al. (2019)'s approach. All specifications include year and country fixed effects, as well as a constant term, but the results are not presented here. ***, **, and * represent p<0.01, p<0.05, p<0.1, respectively. Robust standard errors, clustered at country-level, are in parentheses.

Table 1.12: Diffusion Patterns of Unrest

	(1)	(2)	(3)	(4)	(5)
Lagged unrest	0.229***	0.200***	0.201***	0.199***	0.199***
	(0.023)	(0.024)	(0.024)	(0.024)	(0.024)
Lagged regional unrest	0.275***			0.060	0.111
	(0.051)			(0.070)	(0.070)
Lagged distance-weighted unrest		0.584***		0.515***	
		(0.133)		(0.141)	
Lagged neighbors' average unrest			0.107***		0.090***
			(0.029)		(0.031)
Observations	5,101	3,271	3,271	3,269	3,269
No. Countries	233	150	150	150	150
R2	0.181	0.216	0.214	0.216	0.216

Notes: The dependent variable the unrest index reported by CNTS. All specifications include year and country fixed effects, as well as a constant term, but the results are not presented here. ***, **, and * represent p<0.01, p<0.05, p<0.1, respectively. Robust standard errors, clustered at country-level, are in parentheses.

Table 1.13: The Effects of Democratization on Citations: Alternative IV Approach

	(1) Total	(2) Engineering	(3) Agriculture	(4) Social Sciences	(5) Business	(6) Medicine
Second Stage						
Democracy	0.342***	0.559	0.574*	0.887***	0.834**	0.119
	(0.122)	(0.348)	(0.314)	(0.301)	(0.349)	(0.144)
Human Capital	-0.0278	0.930	-1.087***	-0.224	-0.00175	-0.404
	(0.249)	(0.577)	(0.410)	(0.569)	(1.069)	(0.395)
GDP per Capita	0.061	-0.197	-0.017	0.097	0.311*	0.126**
	(0.045)	(0.120)	(0.065)	(0.148)	(0.162)	(0.061)
Openness	-0.204*	-0.138	-0.332	-0.621**	-0.393	-0.441***
	(0.116)	(0.257)	(0.219)	(0.289)	(0.435)	(0.128)
People Employed	0.0937	0.368**	0.215**	0.439***	0.277	0.139
	(0.095)	(0.156)	(0.092)	(0.138)	(0.311)	(0.135)
First Stage						
First-lagged Z	3.404***	3.406***	3.406***	3.379***	3.404***	3.395***
	(0.478)	(0.481)	(0.469)	(0.477)	(0.478)	(0.481)
Observations	2,596	2,596	2,596	2,596	2,596	2,596
KP F-Stat	49.602	50.788	49.985	51.998	50.886	50.437
Year FE	Y	Y	Y	Y	Y	Y
Country FE	Y	Y	Y	Y	Y	Y
No. Countries	127	127	127	127	127	127

Notes: The dependent variable is the natural logarithm of total number of citations per document. Democratization is created using Acemoglu et al. (2019)'s approach. All other independent variables are in the natural logarithm. All specifications in the second-stage regressions include year and country fixed effects, as well as a constant term, but the results are not presented here. The instrument for democratization is the first lag of the created instrument. All exogenous variables in the second-stage regressions are also included in the first-stage regressions, but we do not report the results here. ***, **, and * represent p<0.01, p<0.05, p<0.1, respectively. Robust standard errors, clustered at country-level, are in parentheses.

Chapter 2

The Electoral Outcomes of Contractions in Mortgage Credits: Evidence from Gubernatorial and House Elections

Voting behavior is influenced by the economy according to economic voter theory.[1] In contrast to the abundance of literature on the link between the macro-state of the economy and political outcomes (Fair, 1978, 1996, 1998, 2020; and Lewis-Beck and Stegmaier, 2000), there is little literature on the micro-level indicators. Recently, studies have focused on the housing market in an attempt to fill this gap.

Credit subsidies in the housing market have been linked with voting behavior as home-ownership policies have influenced politics and elections in the U.S. and all over the world. While politicians are punished directly by voters for bad economic conditions, they may be rewarded through mechanisms other than voting when they support credit subsidies, as Antoniades and Calomiris (2020) argue. During the financial crisis of 2007-2008, the U.S. experienced an unexpected shift from its most generous mortgage underwriting standards in 2004-2006 to a strict curtailment of mortgage credits. Regarding the political outcomes of the 2008 election, the Republican presidential candidate

[1] Lewis-Beck and Stegmaier (2013) provide an extensive review of the literature on this subject.

lost 9 states that had been carried by his predecessor in 2004. Furthermore, Democrats gained 8 seats in the Senate, 21 seats in the House, and 1 seat in the Gubernatorial races.

Hall et al. (2021), using individual-level data on foreclosure rates for Ohio, find that people whose homes were foreclosed were less likely to participate in voting in the 2008 election cycle. In another work, Antoniades and Calomiris (2020) find that the contraction in mortgage credits during the financial crisis of 2008 led the Republican Presidential candidate to lose the 2008 Presidential election. In this paper, we build on Antoniades and Calomiris (2020)'s work to investigate the effects of the contraction in mortgage credit supply on the Gubernatorial and House elections of 2008 and provide a of micro-level evidence on the relationship between the economy and politics. We employ a large dataset on millions of mortgage application outcomes provided by the Home Mortgage Disclosure Act (HMDA) to identify how changes in mortgage credits might have impacted the outcomes of House and Gubernatorial elections of 2008.

Our goal is to examine if the contraction in mortgage credits had electoral consequences. We make use of a two-stage estimation strategy proposed by Antoniades and Calomiris (2020) to isolate the effects of the shrinkage in mortgage credits. In the first stage, we estimate a model of the application outcome (reject - not reject) and track the change of the credit from 2004 to 2008. In other words, for each year, we regress the application outcome, which is a binary variable equal to one if a loan application is declined and zero otherwise, on a set of borrowers' characteristics. Additionally, we add a series of county and bank fixed effects. An isolated measure of mortgage credit supply growth is derived from the first stage regression. Specifically, the first stage controls for credit demand. For each year in the model, the coefficients of bank-year fixed effects are used to calculate county-specific changes in mortgage credit supply. In the second stage, we regress the change in the Democratic candidates vote shares change from 2004

to 2008 on our calculated metric of the difference in mortgage credit supply.[2]

We find that the contraction in mortgage credits had a negative effect on the vote shares received by the Democratic candidates in the Gubernatorial election of 2008. As for the House election, we find a positive but insignificant impact. We then consider the fact that voters respond to bad economic conditions by voting against incumbents and modify the dependent variable of the second stage to the change in the vote shares received by the challenger candidates.[3] We find no significant effect of the contraction in the mortgage credits in either elections.

Our results are robust to a different specification of the second stage regression where all variables except the variable of interest are in levels. Finally, we add more control variables with regard to the status of the housing market in 2008, including vacancy rates, loan rates, OFHEO Price Change, and foreclosure rates, but we do not find different results. Our findings are in line with studies showing that the status of the economy has no effect on Congressional elections. Notably, our work closely links to the studies of Erikson (1990) and Alesina, Londregan and Rosenthal (1993) to the extent that economic conditions can affect Presidential election outcomes but not Congressional ones.

The rest of the paper is organized as follows. Section 2 reviews the related literature. Section 3 describes the construction of our data. In section 4, we present the identification strategy of the paper. In Section 5, we present our estimation results and discuss them. In section 6, we present the results of the robustness checks. Section 7 concludes.

[2]We mainly do this for two reasons: first, we try to closely follow the approach proposed by Antoniades and Calomiris (2020), and second, the challenger of the Presidential election of 2008 as the top-of-the-ballot candidate was a Democrat.

[3]The main reason that Antoniades and Calomiris (2020) use the change in the Democratic vote shares as the outcome variable is that the Democratic candidate in the 2008 Presidential election was the challenger.

2.1 Literature Review

This paper relates to the literature in several ways. First, it contributes to the large body of literature on the relationship between economic conditions and voter behavior. This relationship has been extensively investigated in the literature. Economic and political science both provide ample evidence regarding voting behavior's sensitivity to economic conditions (See, for example, Key, 1996; Hibbs, 1987*a*,*b*; Alesina, Londregan and Rosenthal, 1993; and Persson and Tabellini, 2002). Furthermore, Fair (1978, 1996, 1998, 2009*a*, 2020) has done a significant amount of research examining the role that macroeconomic variables play in both Presidential and Congressional elections. Economic conditions are found to be positively correlated with presidential election outcomes when the economy is growing, inflation is low, and no adverse shocks are present, and negatively correlated when the environment is the opposite. The literature does not provide conclusive evidence on the effects of economic conditions on Congressional elections. While Erikson (1990), Alesina, Londregan and Rosenthal (1993), and Lynch (2002) show that there is little or no relationship between the state of the economy on voter behavior, Kiewiet and Udell (1998) and Grier and McGarrity (2002) find a positive one.

Economic conditions may also asymmetrically influence voter behavior. Several studies have shown that incumbents are penalized when the economy is not well, but are not rewarded in the same way when the economy is strong (e.g., Bloom and Price, 1975; Lewis-Beck and Stegmaier, 2013; Quinn and Woolley, 2001; and Jensen et al., 2017). Political psychology literature discusses the asymmetry in this relationship. Often, a voter will conclude that a good job or a mortgage approval is due to their own qualifications. However, they often blame others, especially politicians, if they are laid off or have their loan applications get rejected.

Second, our work contributes to the growing body of literature on the political out-

comes of the housing market and credit subsidies. Although the economic aspects of the housing market has been vastly studied in the literature, there is little evidence about how the housing market affects politics.

American politics has been shaped by homeownership policies for almost a century. Despite their wide range of forms, most of these policies are designed to reduce mortgage credit risks. Aside from the United States, mortgage subsidies are present in many other countries as well. "Help-to-buy" was part of Prime Minister Cameron's last election campaign in the U.K. President Roussef's "Minha Casa Minha Vida" program helped her win the 2014 election in Brazil.

Credit subsidies are the most common way for politicians to target their preferred groups. This is mostly because other direct ways, for example, tax credits, are blocked by governmental and legal institutions and they do not link to banking regulations (Rajan et al., 2010 and Calomiris and Haber, 2014). In a similar way, those who bear the costs of granting credit subsidies cannot identify them clearly (Coate and Morris, 1995). Also, there is ample evidence that government-owned or controlled lenders are receiving credit subsidies (for instance, Sapienza, 2004; Dinç, 2005; Khwaja and Mian, 2005; Claessens et al., 2008; and Carvalho, 2014*a*).

Another strand of the literature, examines how the behaviors of politicians are shaped by the notion that voters will reward them for delivering cheap credit to voters. State-owned banks lend to farmers in India during election years more in competitive districts, according to Cole (2009). In the U.S. Liu and Ngo (2014) show that in states whose governors are up for re-election, the likelihood of a banking failure declines in the year leading up to an election. Governors are more likely to magnify this effect if they have control over both chambers of state legislatures.

Finally, this paper is among the first works to make use of micro-level data to study the relationship between the economy and political outcomes. Antoniades and Calomiris

(2020) find that the contractions in mortgage credits during the financial crisis of 2008 cost the incumbent party the Presidential election. Mabud (2016) finds that the increase in mortgage credits in post-2000 elections helped incumbents in low-income counties to win elections. Hall et al. (2021) find that an increase in foreclosure rates was associated with lower turnout in Ohio.

2.2 Data

To explore the electoral outcomes of the contraction in mortgage credits, we collect data from several sources. The first part of our data comes from the Home Mortgage Disclosure Act (HMDA). HMDA requires all financial institutions to collect and report detailed data regarding applications for mortgage loans. HMDA is a very rich dataset that includes loan-level information about the outcome of loan applications, as well as information on the applicants' personal attributes, including gender, race, ethnicity, and income. It also reports information on loans' characteristics, including the location of the property and the purpose of the loan. In our baseline analysis, we make use of the HMDA data for years 2004 and 2008.

Pooling together, there are more than 50 million loan applications in these two years. However, we drop around 65 percent of the observations for several reasons. First, we drop withdrawn loans as well as loans purchased from other institutions. Second, we remove non-conventional loans as they do not follow traditional mortgage loan requirements. Third, we drop loans with missing information. Finally, we exclude the smallest banks from our data for computational purposes in our first-stage regression. We define the smallest banks as those receiving less than 1 percent of all loan applications received within a state-year. Table 2.1 reports the summary statistics of the HMDA data for years 2004 and 2008. Loan applications decreased by around 4 million from 2004 to

2008 while the rate of rejection increased from 25 percent to 37 percent, a clear sign of a reduction in mortgage credits during this time.

We also collect county-level data on personal income and unemployment rate from the Bureau of Labor Statistics, median age, the share of the black population, the share of bachelor's graduates, sex ratio, age dependency ratio, and Gini coefficient from the Census and the American Community Survey databases, and the share of Evangelical Protestants from the Religious Congregations and Membership Survey database.

As for election returns, we obtain data on them from two sources. We have county-level election returns on Presidential and Gubernatorial elections from the CQ Voting and Elections Collection and House elections from the Dave Leip's Atlas of US elections. As shown in Table 2.1, the vote shares received by Democratic candidates increased from 2004 to 2008 in both Gubernatorial and House elections.

Finally, we collect data on foreclosure rate, vacancy rate, high-cost loan rate, home-ownership rate, and home price change rate from the Department of Housing and Urban Development database and use them in our robustness analysis.

2.3 Identification Strategy

Using HMDA's loan-level data, we investigate the effects of the shrinkage in mortgage credits from 2004 to 2008 on election outcomes. In particular, we focus on Gubernatorial and House elections in the 2008 election cycle.

We closely follow the estimation strategy proposed by Antoniades and Calomiris (2020) and estimate a two-stage econometric model to link the shrinkage of mortgage credits to electoral outcomes. In the first stage, we estimate a model of the application outcome (reject - not reject) and track the change of the credit from 2004 to 2008. In this model, we also include several variables to control for the deamnd for credit. We

estimate the following equation for each year in the first stage:

$$LAR_{ijkt} = \alpha + \beta_{kt} + \gamma_{it} + \delta X_{it} + \zeta X_{it} \times After_t + \eta Y_{jt} + \lambda Y_{jt} \times After_t + \epsilon_{ijkt} \quad (2.1)$$

where LAR is the loan application outcome (reject - not reject) for bank i, borrower j, county k, year t. β_{kt} and γ_{it} are time interacted county and bank fixed effect, respectively. X and Y are two vectors of borrowers and loans characteristics, respectively. *After* is a binary variable equal to one if year is 2008 and zero otherwise. Finally, ϵ is the error term. We estimate Equation (2.1) using an OLS estimation method.[4]

As we include personal characteristics of applicants in our estimation, Equation (2.1) controls for variation in borrower attributes. As a result, all the demand-side shifts that are linked to the varying compositions of borrowers are removed. By including time-interacted county fixed effects, it also controls for differences in the economic environments of counties. Moreover, it might remove some of the supply-side effects that are associated with the location of lenders. For instance, banks may treat borrowers differently according to their locations with regard to variations in location-specific risks.[5]

After estimating Equation (2.1), we make use of time interacted bank fixed effects to extract a metric of credit supply change from 2004 to 2008 at the county level. In order to do so, we follow two steps: In the first step, a weighted average of the estimated time interacted bank fixed effects is calculated at the county-year level. We make use of the number of mortgage applications as weights. In the second step, we take the difference between the two computed averages.

We then link the shift in county-specific mortgage credit supply to county-level shift in voting behavior through a second-stage OLS regression. The second-stage equation

[4]This estimation includes more than 700 fixed effects, and as a result, we estimate it using an OLS approach to obtain consistent estimates. However, a logistic regression produces incontinent estimates. See Wooldridge (2004, p 484).

[5]For instance, expected house price appreciation can vary based on location.

takes the form of the following:

$$\Delta\%DemCanVote_{kt} = \alpha + \beta(\Delta MortgageCreditSupply_{kt}) + \gamma Z_{kt} + \delta S_{zt} + q_{kt}, \quad (2.2)$$

where $DemCanVote_{kt}$ is the change in votes a Democrat candidate receives from 2004 to 2008 in county k, year t (2008). Z_{kt} and S_{zt} are county-level controls and state fixed effects, respectively. Finally, q is the residual.

In the second-stage, we include county and state fixed effects as well as county characteristics to control for other county-level shifts that can affect voting behavior, such as shifts in economic indicators and voter demographics.

2.4 Results

2.4.1 Main Findings

Table 2.2 represents the results of the first-stage regression. The dependent variable is loan application rejection, which is a dummy variable equal to one a loan application gets rejected and zero otherwise. As expected, loan application denial is correlated with a wide range of applicant characteristics, as well as bank and county fixed effects.[6] The results from Table 2.3 suggest that it is important to control for borrowers' characteristics when using a rejection model, and it is not enough to only consider the originator's lending capacity.

Using bank-time fixed effects in the first stage, we measure the credit supply change from 2004 to 2008, as explained in section 4. Figure 2.1 shows the change in credit supply from 2004 to 2008 at county level. We employ the *negative* of the weighted time interacted bank dummies in our calculations. Figure 2.2 displays the density function of the change in mortgage credits. According to our calculation of the change in mortgage

[6]This table is mainly the replication of the Antoniades and Calomiris (2020)'s work.

credits, only 4 percent of counties experienced an increase in mortgage credits from 2004 to 2008 experienced an increase in mortgage credits.[7].

We now turn to the results of the second-stage estimation. In our second-stage regression, we explore the effects of the contraction in mortgage credits on the change in the Democratic vote shares (i.e., the share of votes going to the Democratic candidate in 2008, minus the vote share of the Democratic candidate in 2004 at the county level) in the Gubernatorial and House elections of 2008 as it appears in Equation (2.2).

The Gubernatorial elections of 2008 were held in 11 states, out of which 6 had a Democratic incumbent governor (Delaware, Montana, New Hampshire, North Carolina, Washington, and West Virginia) and 5 had a Republican incumbent one (Indiana, Missouri, North Dakota, Utah, and Vermont). Except for Missouri, where the Democratic party gained a seat, all other seats were held by the incumbent party. There are 565 counties in all of those states. However, the data are available only for 277 counties. The House elections were held nationally, and the Democratic party gained 21 seats. The availability of data for the House election is limited to 1510 counties.

Table 2.3 shows the estimation results for the Gubernatorial election. The first column shows the effect of the dependent variable on the set of control variables we make use of in our estimation. We find that economic conditions affect the change in the Democratic candidate vote shares as the estimated coefficient on the change in personal income is positive and statistically significant, and the estimated coefficient on the change in the unemployment rate is positive and significant. Younger people and minorities are associated with gain for the Democratic candidates while the Evangelical share has a negative impact. According to the results, the presence of the educated people has a negative effect on the Democratic vote share, which is not expected. However, our sample includes only 277 counties, and we need to be cautious in interpreting our findings

[7]That is why we cannot observe credit increase in Figure 2.1

in this regard. The estimated coefficient on the sex ratio is positive but not significant, meaning that men are more likely to support the Democratic party. It is also unexpected but in line with the findings of Antoniades and Calomiris (2020).

Column 2 of Table 2.4 includes the change in the raw mortgage credit rejection rate. The coefficient on the change in the raw mortgage credit rate is negative (the "wrong" sign) but not statistically significant from zero, meaning that a greater decline in the mortgage credit approval rate is associated with a decrease in the Democratic party candidate vote share. Column 3 represents our main finding for the Gubernatorial election: the contraction in mortgage credits from 2004 to 2008 had a negative and statistically significant impact on the change in the Democratic party candidates vote shares. Our results contradict the findings of Antoniades and Calomiris (2020) as they find that the contraction had a positive impact on the vote share received by the Democratic candidate as the challenger.

Before we discuss the possible explanations as to why we find a different effect of the contraction in the mortgage credits on electoral outcomes than Antoniades and Calomiris (2020), we present our estimation results for the House elections of 2008 in Table 2.4. The first column shows the effects of the control variables on the vote shares received by the Democratic candidates. As Expected, our findings here are almost similar to the ones in the first column of Table 2.4. We include the change in the raw mortgage credit rate in column 2 of Table 2.4. While the estimates coefficient on the change in the raw mortgage credit rate is positive, it is not significant. In the third column of Table 2.4, we include the calculated measure of the change in the mortgage credit supply. Contrary to our findings for the Gubernatorial election but in line with Antoniades and Calomiris (2020), the estimated coefficient on the change in mortgage credit supply is negative, meaning that the contraction in the mortgage credits had a positive effect on the change in the Democratic party candidates vote shares. However, this effect is not statistically

significant.

2.4.2 Discussion

Contrary to Antoniades and Calomiris (2020), we find that the contraction in the mortgage credits from 2004 to 2008 had a negative effect on the vote shares received by the Democratic candidates in the Gubernatorial election. As for the House election, while the effect of the contraction is positive, it is not significantly different from zero. In this section, we discuss our findings and the way they link to the literature, especially to the work of Antoniades and Calomiris (2020).

We first review the intuition behind the dependent variable and then repeat our estimations with a new dependent variable. The incumbent president at the time of the Presidential election of 2008 was a Republican meaning that, the Democratic candidate of the Presidential election was the challenger. In line with the literature, Antoniades and Calomiris (2020) argue that bad economic conditions gave the challenger of the Presidential election an advantage. As a result, the contraction in the mortgage credits from 2004 to 2008 helped the challenger to win the election. Our dependent variable is also the change in the vote share received by the Democratic candidates in the Gubernatorial and House elections. However, not in all regions, the challenger is a Democrat. To address this issue, we change our dependent variable to the change in the votes received by challengers and re-estimate Equation (2.2).[8]

The estimation results for the Gubernatorial and House elections are presented in Tables 2.5 and 2.6, respectively. We should note that we need to interpret the estimated coefficients on some of the control variables differently than our previous estimations.[9]

[8] We lose around 400 observations for the House election mainly because in many cases seats are uncontested.

[9] For instance, an increase in the share of BA graduates is not necessarily associated with an increase in the change of the challenger vote shares.

The estimated coefficient on the change in mortgage credit supply becomes insignificant in the Gubernatorial election but negative. Moreover, we find a significant effect of the change in personal income on the change of challenger vote shares. As for the unemployment rate, the effect is negative but insignificant. Regarding the House election, the coefficient is almost the same. As a result, we can conclude that the way we construct our dependent variable is not responsible for finding different results than Antoniades and Calomiris (2020). Nonetheless, we do conclude that our findings for the Gubernatorial election in Table 2.3 are not robust as they are sensitive to a different but quite plausible construction of the dependent variable.

Although we do not find significant results at the general level, the results might change in competitive elections. In line with Hall et al. (2021), we examine if our results change in competitive counties. Following Hall et al. (2021), we define competitive counties where the vote share for the Democratic candidate is between 40 and 60 percent. It should be noted, this test is not an ideal one, as there might be an approximately equal number of partisan voters of either party rather a large pool of swing voters in competitive counties. The results for Gubernatorial and House elections are presented in Tables 2.7 and 2.8, respectively. We also include the change in the vote shares received by challengers here. Similar to our previous findings, we do not find a significant impact.

Our findings are in line with Erikson (1990), Alesina, Londregan and Rosenthal (1993), and Lynch (2002), as they find that economy has either a negative or insignificant effect on the congressional elections. Among those papers, our work is a nice parallel to the studies of Erikson (1990) and Alesina, Londregan and Rosenthal (1993) with regard to finding different impacts of the economy on Presidential and Congressional elections. Our work also provides anecdotal evidence in favor of the so-called coattail effect, which suggests that the top-of-the-ballot candidate attracts voters for down-ballot candidates. The literature on this effect is not conclusive as there are a wide range of papers both

in favor and against the effect. Although the vote shares received by the Democrat candidates increased in both the Gubernatorial and House elections, it might be the effect of the Presidential candidate, not the effect of the economic factors as Antoniades and Calomiris (2020) suggest.[10]

2.4.3 The Role of Location

We now test the sensitivity of our results to the urban and rural areas.[11] Following Antoniades and Calomiris (2020), we remove all the control variables from our estimation to obtain a larger sample. Using the reports from the 2010 census, we consider a county urban if 75 percent or more of the population is classified as urban. The estimation results for the Gubernatorial and House elections are presented in Tables 2.9 and 2.10, respectively. It appears that our findings for the Gubernatorial sample are mainly driven by rural areas. Interestingly, the estimated coefficient on the change in the credit supply for urban areas is negative and much smaller than the one for rural areas, but still insignificant. As for the House election, while all the estimated coefficients are insignificant, the magnitude of the coefficient for the urban areas is almost twice as much as the one for the rural areas.

2.5 Robustness Checks

To check the robustness of our results, we conduct two sets of robustness checks. First, we add more control variables to our baseline estimation to address the omitted variable bias. Tables 2.11 and 2.12 show the estimation results for the Gubernatorial and House elections, respectively. The first columns of both tables show the estimation results of

[10] The vote shares received by the Democratic candidates increased by 8 and 2.5 percentage points in the Gubernatorial and House elections, respectively.

[11] Hereafter, our dependent variable is the change in the Democratic vote share.

Equation (2.2), including all previous controls. The actual concern of voters might be the increase in foreclosure rates, rather than the contraction in mortgage credits. To address this concern, we add foreclosure rates to our estimations. This appears to be the case in the House elections but not in the Gubernatorial elections. We also control for the variation in the characteristics of the housing market by including loan rates, vacancy rates, and the peak-to-trough change in housing prices. Columns 2 through 5 of Tables 2.11 and 2.12 show the estimation results. We only find a significant effect of loan rates on the change in the vote shares received by the Democratic candidates in the House elections. Once we include all control variables together, none are significant in either election (column 6 in tables 2.11 and 2.12). With regard to our main variable of interest, the magnitude of the estimated coefficients and the levels of significance are almost the same across all estimations in both elections.

Second, we consider a different specification of Equation (2.2), where all variables are in levels in 2008. Tables 2.13 and 2.14 presents the estimation results of the alternative specification for the Gubernatorial and House elections, respectively. Once again, the magnitude and the levels of significance of the main variable of interest is the same as our baseline estimations.

2.6 Conclusion

The economic vote theory suggests that the condition of the economy impacts voter behavior. While there is ample evidence regarding the relationship between macro-level variables and political outcomes, little attention has been devoted to micro-level variables. Moreover, the literature on the relationship between the housing market and politics is scarce. In this paper, we studied the political outcomes of the contraction in the mortgage credits during the financial crisis of 2008 in the Gubernatorial and House

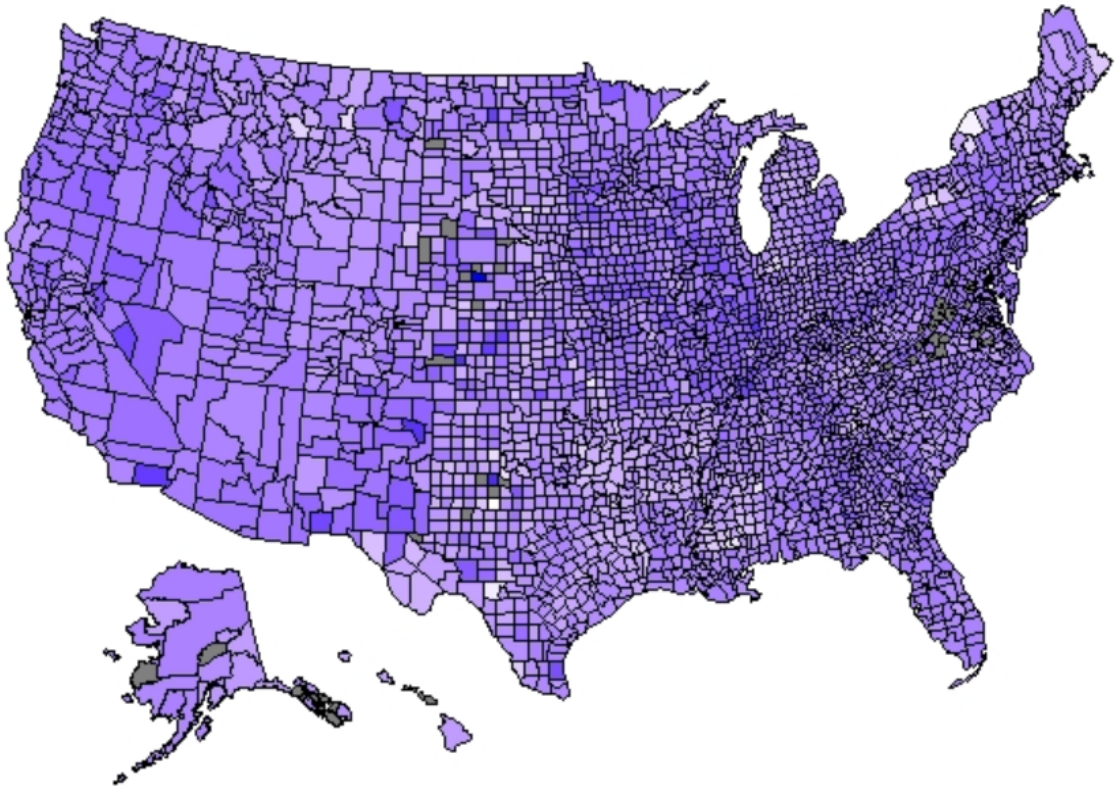
elections using micro-level data provided by HMDA.

We estimated a two-stage regression model to isolate the effects of the contractions in mortgage credits on political outcomes. In the first stage, we estimated an OLS model of loan application ejection for years 2004 and 2008. In our estimations, we controlled for borrower and location characteristics to get rid of the demand side variations that are linked to the changing compositions of borrowers. Making use of the extracted bank-year fixed effects from the first-stage regression, we calculated the contractions in mortgage credits from 2004 to 2008. In the second stage, we regressed the change in the Democratic vote shares from 2004 to 2008 on the calculated measure of the change in mortgage credits. We also controlled for other factors that can affect voter behavior, including income, unemployment rate, the share black population, the share of people with at least a Bachelors degree, etc.

While we did not find any significant effect of the contraction in mortgage credits on the Democratic vote shares for the House elections, we found a negative and significant effect for the Gubernatorial election. Once we replaced our dependent variable with the change in challenger vote shares, we did not find a significant effect in either election. The advantage of the change in the challenger vote shares over the change in the Democratic vote shares is that voters respond to bad economic conditions by voting against incumbents, not Republicans. In addition, the number of observations in our Gubernatorial sample was limited due to the fact that the Gubernatorial elections were held in only 11 states in 2008, and most of them are not populous. As a result, we cannot be sure about the effect of the contraction in mortgage credits in the Gubernatorial elections.

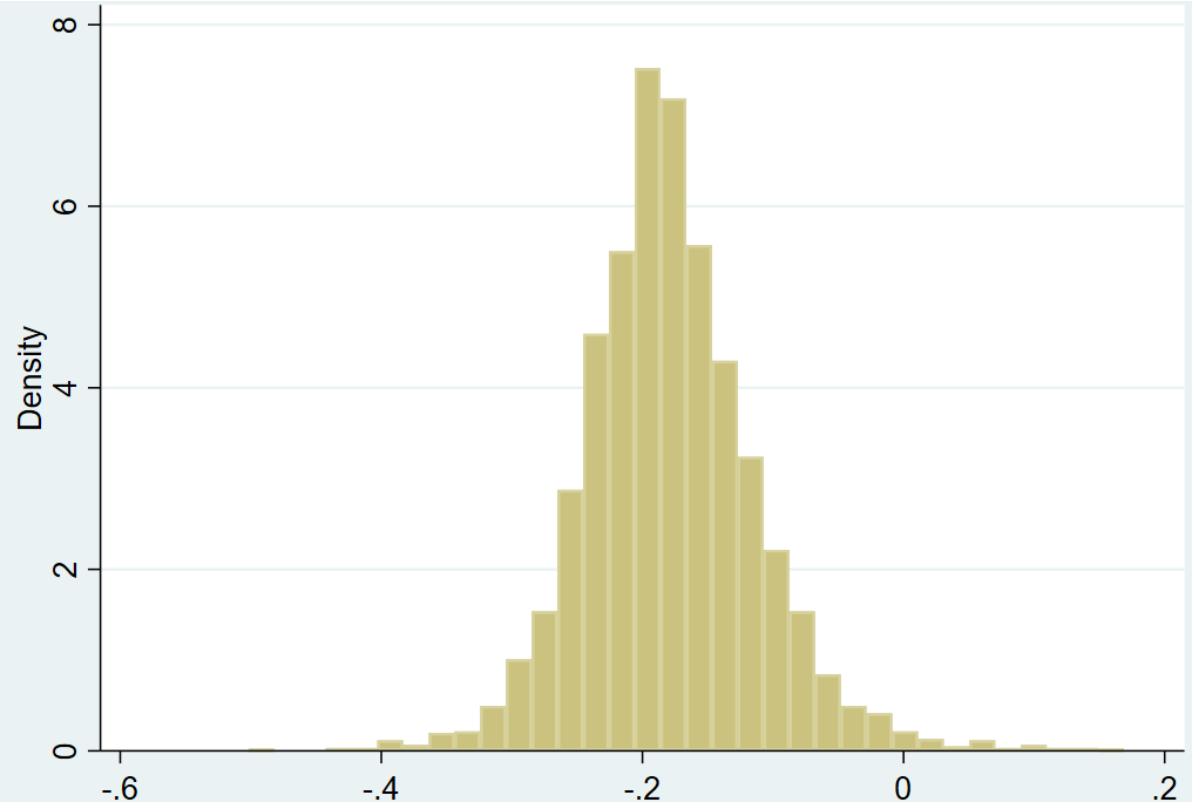
Figures and Tables

Figure 2.1: Change in Mortgage Credit Supply from 2004 to 2008



Notes: Each cell represents a county. The change in credit supply is calculated as explained in section 4. The magnitude of the change is shaded from white (no change) to dark blue (most negative change). Source: Author's calculations.

Figure 2.2: Density Function of Mortgage Credit Growth from 2004 to 2008



Notes: The change in credit supply is calculated as explained in section 4.

Table 2.1: Descriptive Statistics

Variable	**2004**	**2008**
States	51	51
Counties	3,180	3,186
Census Tracts	19,011	18,971
Financial Institutions (Banks)	317	369
Loan Applications	8,557,111	4,811,881
Applicant Income (average, in thousand USD)	87.04	100.84
Loan Amount (average, in thousand USD)	165.19	192.63
Loan to Income Ratio	2.25	2.37
Type of Loan: Home Purchase (% of total)	35	28
Type of Loan: Home Improvement (% of total)	8	12
Type of Loan: Home Refinance (% of total)	56	60
Female Applicants (% of total)	32	33
Hispanic Applicants (% of total)	12	10
Minority Applicants (% of total)	18	17
Applications with Co-Applicant (% of total)	46	48
Loan Rejection Rate (% of total)	25	37
Change in Democratic Vote shares(House) (Percentage Points)		8.06
Change in challenger Vote shares(House) (Percentage Points)		0.44
Change in Democratic Vote shares (Gubernatorial) (Percentage Points)		2.52
Change in challenger Vote shares (Gubernatorial) (Percentage Points)		-1.70

Notes: States, Counties, and Census Tracts are number of states, counties, and census in the sample, respectively. Financial Institutions (Banks) is number of banks. Loan Applications is total number of loans applied by borrowers. Applicant Income is the average of all applicants' income in the sample. Loan Amount is the average of all applicants' requested loan amount in the sample. Female Applicants, Hispanic Applicants, Minority Applicants, and Applications with Co-Applicant are percentage of female applicants, percentage of Hispanic applicants, percentage of minority applicants, and percentage of applicants with co-applicants in the sample, respectively. Loan Rejection Rate is percentage of loans that are rejected.

Table 2.2: First-Stage Regression Results

	Loan Application Rejection	
		S.E.
Female Applicant	-0.003	(0.003)
Female Applicant $\times After$	0.009**	(0.004)
Hispanic Ethnicity	0.049***	(0.01)
Hispanic Ethnicity $\times After$	0.055***	(0.016)
Minority Race	0.066***	(0.006)
Minority Race $\times After$	0.017	(0.012)
Log(Income)	0.024	(0.016)
Log(Income) $\times After$	0.022	(0.026)
Log(Loan)	-0.063***	(0.017)
Log(Loan) $\times After$	-0.006	(0.027)
Loan to Income	0.039***	(0.006)
Loan to Income $\times After$	0.013	(0.012)
Co Applicant	-0.029***	(0.005)
Co Applicant $\times After$	-0.002	(0.008)
Purpose: Home Purchase	-0.05***	(0.007)
Purpose: Home Purchase $\times After$	-0.054**	(0.021)
Purpose: Home Improvement	0.047	(0.034)
Purpose: Home Improvement $\times After$	-0.012	(0.041)
Observations	13,090,171	
R2	28.5	
County-year Fixed Effects	Yes	
Banks-year Fixed Effects	Yes	

Notes: The dependent variable is loan application rejection. Female Applicant is a binary variable equal to one if an applicant is female and zero otherwise. Hispanic is a binary variable equal to one if an applicant is Hispanic and zero otherwise. Minority is a binary variable equal to one if an applicant is minority and zero otherwise. Female Applicant is a binary variable equal to one if an applicant is female and zero otherwise. Log(Income) in the natural logarithm of an applicant's income. Log(Loan) in the natural logarithm of an applicant's requested loan. Co Applicant is a binary variable equal to one if an applicant has a co applicant and zero otherwise. Purpose is a binary variable equal to one according to the purpose specified and zero otherwise. All specifications include bank and county fixed effects, as well as a constant term, but the results are not presented here. ***, **, and * represent $p<0.01$, $p<0.05$, $p<0.1$, respectively. Robust standard errors, clustered at bank-level, are in parentheses. This tables replicates Table 3 in the Antoniades and Calomiris (2020)'s work.

Table 2.3: Second-Stage Regression Results: The Effects of the Change in Mortgage Credits on the Change in Democratic Vote Shares in the Gubernatorial Election, 2008

	(1)	(2)	(3)
Δ(*Mortgage Credit Supply*)			0.259***
			(0.090)
Δ(*Personal Income*)	0.147**	0.134	0.110
	(0.074)	(0.088)	(0.076)
Δ(*Unemployment Rate*)	0.012***	0.0116*	0.009
	(0.005)	(0.007)	(0.006)
Median Age	-0.002	-0.002*	-0.002*
	(0.001)	(0.0009)	(0.001)
Black	0.091	0.086	0.080
	(0.056)	(0.054)	(0.057)
Evangelical	0.000004	-0.0000009	-0.000006
	(0.00005)	(0.00005)	(0.00004)
BA Share	-0.0004	-0.0003	-0.0004
	(0.0005)	(0.0004)	(0.0005)
Sex Ratio	0.0002	0.0002	0.0001
	(0.0009)	(0.0008)	(0.0007)
Age Dependency Ratio	0.001**	0.001**	0.001**
	(0.0005)	(0.0006)	(0.0006)
Votes(t-1)	-0.04	-0.05	-0.043
	(0.055)	(0.057)	(0.061)
Δ(*Raw Mortgage Rejection Rate*)		-0.087	
		(0.056)	
Observations	277	277	277
R2	82.1	82.3	83.0
State Fixed Effects	Yes	Yes	Yes

Notes: The dependent variable is the change in Democratic vote shares in the Gubernatorial election in 2008. All specifications include bank and county fixed effects, as well as a constant term, but the results are not presented here. ***, **, and * represent p<0.01, p<0.05, p<0.1, respectively. Robust standard errors, clustered at MSA-level, are in parentheses.

Table 2.4: Second-Stage Regression Results: The Effects of the Change in Mortgage Credits on the Change in Democratic Vote Shares in the House Election, 2008

	(1)	(2)	(3)
Δ(*Mortgage Credit Supply*)			-0.123
			(0.134)
Δ(*Personal Income*)	-0.235***	-0.229***	-0.233***
	(0.085)	(0.085)	(0.087)
Δ(*Unemployment Rate*)	-0.008	-0.008*	-0.008
	(0.005)	(0.005)	(0.007)
Median Age	-0.003	-0.002	-0.002
	(0.002)	(0.002)	(0.002)
Black	0.185***	0.185**	0.186***
	(0.060)	(0.073)	(0.069)
Evangelical	-0.0002***	-0.0002***	-0.0002**
	(0.00006)	(0.00006)	(0.00007)
BA Share	-0.00006	-0.00003	-0.00003
	(0.0006)	(0.0005)	(0.0002)
Sex Ratio	0.0006	0.00008	0.00009
	(0.0006)	(0.0006)	(0.0006)
Age Dependency Ratio	0.0006	0.0006	0.0006
	(0.001)	(0.001)	(0.001)
Votes(t-1)	-0.409***	-0.410***	-0.410***
	(0.042)	(0.044)	(0.043)
Δ(*Raw Mortgage Rejection Rate*)		0.05	
		(0.070)	
Observations	1,510	1,510	1,510
R2	50.4	50.5	50.5
State Fixed Effects	Yes	Yes	Yes

Notes: The dependent variable is the change in Democratic vote shares in the House election in 2008. All specifications include bank and county fixed effects, as well as a constant term, but the results are not presented here. ***, **, and * represent p<0.01, p<0.05, p<0.1, respectively. Robust standard errors, clustered at MSA-level, are in parentheses.

Table 2.5: Second-Stage Regression Results: The Effects of the Change in Mortgage Credits on the Change in Challenger Vote Shares in the Gubernatorial Election, 2008

	(1)	(2)	(3)
Δ(*Mortgage Credit Supply*)			-0.120
			(0.077)
Δ(*Personal Income*)	-0.215	-0.215*	-0.198
	(0.141)	(0.120)	(0.118)
Δ(*Unemployment Rate*)	-0.016***	-0.016**	-0.014*
	(0.006)	(0.006)	(0.008)
Median Age	-0.0007	-0.0007	-0.0006
	(0.003)	(0.003)	(0.003)
Balack	-0.384***	-0.383***	-0.380**
	(0.105)	(0.145)	(0.143)
Evangelical	-0.00004	-0.00004	-0.00004
	(0.00008)	(0.00007)	(0.0001)
BA Share	-0.002	-0.002	-0.002
	(0.0013)	(0.0014)	(0.0013)
Sex Ratio	0.00003	0.00003	0.00009
	(0.002)	(0.002)	(0.001)
Age Dependency Ratio	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)
Votes(t-1)	-0.723***	-0.723***	-0.724***
	(0.223)	(0.213)	(0.239)
Δ(*Raw Mortgage Rejection Rate*)		0.002	
		(0.101)	
Observations	277	277	277
R2	73.9	73.9	74.0
State Fixed Effects	Yes	Yes	Yes

Notes: The dependent variable is the Change in challenger vote shares in the Gubernatorial election in 2008. All specifications include bank and county fixed effects, as well as a constant term, but the results are not presented here. ***, **, and * represent p<0.01, p<0.05, p<0.1, respectively. Robust standard errors, clustered at MSA-level, are in parentheses.

Table 2.6: Second-Stage Regression Results: The Effects of the Change in Mortgage Credits on the Change in Challenger Vote Shares in the House Election, 2008

	(1)	(2)	(3)
Δ(*Mortgage Credit Supply*)			-0.125
			(0.078)
Δ(*Personal Income*)	-0.091	-0.092	-0.086
	(0.091)	(0.096)	(0.082)
Δ(*Unemployment Rate*)	0.003	0.002	0.003
	(0.005)	(0.006)	(0.006)
Median Age	0.0007	0.0007	0.0007
	(0.001)	(0.001)	(0.001)
Black	0.033	0.032	0.033
	(0.082)	(0.066)	(0.07)
Evangelical	-0.00002	-0.00002	-0.00001
	(0.00002)	(0.00005)	(0.00004)
BA Share	0.0004	0.0004	0.0004
	(0.0004)	(0.0006)	(0.0005)
Sex Ratio	0.002**	0.002**	0.002**
	(0.0007)	(0.0008)	(0.0008)
Age Dependency Ratio	0.0002	0.0002	0.0003
	(0.0009)	(0.0008)	(0.0008)
Votes(t-1)	-0.752***	-0.751***	-0.754***
	(0.035)	(0.049)	(0.037)
Δ(*Raw Mortgage Rejection Rate*)		-0.01	
		(0.06)	
Observations	1,182	1,182	1,182
R2	48.5	48.5	48.6
State Fixed Effects	Yes	Yes	Yes

Notes: The dependent variable is the change in challenger vote shares in the House election in 2008. All specifications include bank and county fixed effects, as well as a constant term, but the results are not presented here. ***, **, and * represent $p<0.01$, $p<0.05$, $p<0.1$, respectively. Robust standard errors, clustered at MSA-level, are in parentheses.

Table 2.7: Second-Stage Regression Result for Competitive Elections: The Effects of the Change in Mortgage Credits on the Change in Democratic and Challenger Vote Shares in the Gubernatorial Election, 2008

	(1) Democratic	(2) Challenger
Δ(*Mortgage Credit Supply*)	0.166	-0.0815
	(0.147)	(0.156)
Δ(*Personal Income*)	0.139	0.127
	(0.134)	(0.138)
Δ(*Unemployment Rate*)	-0.006	-0.006
	(0.006)	(0.007)
Median Age	-0.002	-0.0002
	(0.002)	(0.003)
Black	-0.015	-0.143
	(0.068)	(0.097)
Evangelical	0.00003	-0.00007
	(0.00005)	(0.0007)
BA Share	0.0006	-0.0007
	(0.0005)	(0.0006)
Sex Ratio	0.0008	-0.002*
	(0.0007)	(0.001)
Age Dependency Ratio	0.001	-0.0001
	(0.001)	(0.001)
Votes Democrat(t-1)	-0.370***	
	(0.077)	
Votes Challenger(t-1)		-0.701***
		(0.209)
Observations	124	124
R2	86.4	80.4
State Fixed Effects	Yes	Yes

Notes: The dependent variables in the first and second columns are the change in Democratic and challenger vote shares in the Gubernatorial election in 2008, respectively. All specifications include bank and county fixed effects, as well as a constant term, but the results are not presented here. ***, **, and * represent p<0.01, p<0.05, p<0.1, respectively. Robust standard errors, clustered at MSA-level, are in parentheses.

Table 2.8: Second-Stage Regression Results for Competitive Elections: The Effects of the Change in Mortgage Credits on the Change in Democratic or Challenger Vote Shares in the House Election, 2008

	(1) Democratic	(2) Challanger
Δ(*Mortgage Credit Supply*)	0.016	-0.0341
	(0.107)	(0.132)
Δ(*Personal Income*)	0.103	-0.129
	(0.126)	(0.125)
Δ(*Unemployment Rate*)	-0.012	0.01
	(0.008)	(0.008)
Median Age	-0.002	0.003
	(0.0017)	(0.002)
Black	0.093**	0.067
	(0.044)	(0.089)
Evangelical	-0.00002	-0.00005
	(0.00006)	(0.00008)
BA Share	-0.0004	0.0009
	(0.0007)	(0.001)
Sex Ratio	-0.001	0.0008
	(0.0007)	(0.001)
Age Dependency Ratio	0.001	-0.0005
	(0.001)	(0.001)
Votes Democrat(t-1)	-0.708***	
	(0.046)	
Votes Challenger(t-1)		-0.496***
		(0.081)
Observations	442	348
R2	84.0	43.6
State Fixed Effects	Yes	Yes

Notes: The dependent variables in the first and second columns are the change in Democratic and challenger vote shares in the House election in 2008, respectively. All specifications include bank and county fixed effects, as well as a constant term, but the results are not presented here. ***, **, and * represent p<0.01, p<0.05, p<0.1, respectively. Robust standard errors, clustered at MSA-level, are in parentheses.

Table 2.9: Second-Stage Regression Results: The Effects of the Change in Mortgage Credits on the Change in Democratic Vote Shares in the Gubernatorial Election, 2008: Break-down by Location

	(1) All	(2) Urban	(3) Rural
Δ(*Mortgage Credit Supply*)	0.06	-0.003	0.276**
	(0.06)	(0.07)	(0.112)
Votes(t-1)	-0.065	-0.094*	-0.002
	(0.05)	(0.05)	(0.072)
Observations	565	360	205
R2	79.9	80.9	80.4
State Fixed Effects	Yes	Yes	Yes

Notes: The dependent variable is the change in Democratic vote shares in the Gubernatorial election in 2008. All specifications include bank and county fixed effects, as well as a constant term, but the results are not presented here. ***, **, and * represent p<0.01, p<0.05, p<0.1, respectively. Robust standard errors, clustered at MSA-level, are in parentheses.

Table 2.10: Second-Stage Regression Results: The Effects of the Change in Mortgage Credits on the Change in Democratic Vote Shares in the House Election, 2008: Breakdown by Location

	(1)	(2)	(3)
	All	Urban	Rural
Δ(*Mortgage Credit Supply*)	-0.080	-0.078	-0.043
	(0.066)	(0.051)	(0.138)
Votes(t-1)	-0.322***	-0.293***	-0.402***
	(0.041)	(0.042)	(0.059)
Observations	2,946	1,923	1,023
R2	44.2	40.4	53.8
State Fixed Effects	Yes	Yes	Yes

Notes: The dependent variable is the change in Democratic vote shares in the House election in 2008. All specifications include bank and county fixed effects, as well as a constant term, but the results are not presented here. ***, **, and * represent p<0.01, p<0.05, p<0.1, respectively. Robust standard errors, clustered at MSA-level, are in parentheses.

Table 2.11: Second-Stage Regression Results: The Effects of the Change in Mortgage Credits on the Change in Democratic Vote Shares in the Gubernatorial Election, 2008: Additional Controls

	(1)	(2)	(3)	(4)	(5)	(6)
Δ(*Mortgage Credit Supply*)	0.259**	0.262**	0.259**	0.257**	0.266**	0.262**
	(0.127)	(0.115)	(0.113)	(0.110)	(0.124)	(0.122)
Foreclosure Rate		0.212				0.574
		(0.621)				(0.801)
Loan Rate			0.005			-0.098
			(0.126)			(0.142)
Vacancy Rate				-0.0585		-0.092
				(0.195)		(0.153)
OFHEO Price Change					-0.299	-0.172
					(0.373)	(0.417)
Observations	277	277	277	277	277	277
R2	83.0	83.0	83.0	83.0	83.0	83.1
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variable is the change in Democratic vote shares in the House election in 2008. All specifications include bank and county fixed effects, as well as a constant term, but the results are not presented here. ***, **, and * represent $p<0.01$, $p<0.05$, $p<0.1$, respectively. Robust standard errors, clustered at MSA-level, are in parentheses.

Table 2.12: Second-Stage Regression Results: The Effects of the Change in Mortgage Credits on the Change in Democratic Vote Shares in the House Election, 2008: Additional Controls

	(1)	(2)	(3)	(4)	(5)	(6)
Δ(*Mortgage Credit Supply*)	-0.123	-0.117	-0.119	-0.122	-0.124	-0.120
	(0.125)	(0.120)	(0.111)	(0.119)	(0.124)	(0.142)
Foreclosure Rate		0.742**				0.373
		(0.347)				(0.894)
Loan Rate			0.215**			0.150
			(0.095)			(0.183)
Vacancy Rate				0.0930		0.011
				(0.205)		(0.147)
OFHEO Price Change					0.0349	0.119
					(0.140)	(0.227)
Observations	1,510	1,510	1,510	1,510	1,510	1,510
R2	50.5	50.7	50.8	50.5	50.5	50.8
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variable is the change in Democratic vote shares in the House election in 2008. All specifications include bank and county fixed effects, as well as a constant term, but the results are not presented here. ***, **, and * represent p<0.01, p<0.05, p<0.1, respectively. Robust standard errors, clustered at MSA-level, are in parentheses.

Table 2.13: Second-Stage Regression Results: The Effects of the Change in Mortgage Credits on the Change in Democratic Vote Shares in the Gubernatorial Election, 2008: Alternative Specification

	(1) Baseline Specification		(2) In Levels (2008)
Δ(*Mortgage Credit Supply*)	0.259**	Δ(*Mortgage Credit Supply*)	0.286***
	(0.114)		(0.103)
Δ(*Personal Income*)	0.110	Personal Income	-0.000002
	(0.0923)		(0.000001)
Δ(*Unemployment Rate*)	0.009*	Unemployment Rate	-0.003
	(0.005)		(0.004)
Median Age	-0.002*	Median Age	-0.001
	(0.001)		(0.001)
Black	0.0804	Black	0.087
	(0.057)		(0.055)
Evangelical	-0.000006	Evangelical	-0.00002
	(0.00005)		(0.00004)
BA Share	-0.0004	BA Share	0.0002
	(0.0005)		(0.0007)
Sex Ratio	0.0001	Sex Ratio	0.0004
	(0.00064)		(0.0008)
Age Dependency Ratio	0.001*	Age Dependency Ratio	0.001**
	(0.0006)		(0.0006)
Votes(t-1)	-0.0434	Votes(t-1)	0.960***
	(0.052)		(0.064)
Observations	277		277
R2	83.0		93.1
State Fixed Effects	Yes		Yes

Notes: The dependent variable is the change in Democratic vote shares in the House election in 2008. All specifications include bank and county fixed effects, as well as a constant term, but the results are not presented here. The asterisks ***,**, and * indicate the 1%, 5%, and 10% significance levels, respectively. ***, **, and * represent $p<0.01$, $p<0.05$, $p<0.1$, respectively. Robust standard errors, clustered at MSA-level, are in parentheses.

Table 2.14: Second-Stage Regression Results: The Effects of the Change in Mortgage Credits on the Change in Democratic Vote Shares in the House Election, 2008: Alternative Specification

	(1) Baseline Specification		(2) In Levels (2008)
Δ(*Mortgage Credit Supply*)	-0.123	Δ(*Mortgage Credit Supply*)	-0.136
	(0.104)		(0.103)
Δ(*Personal Income*)	-0.233**	Personal Income	-0.000002***
	(0.093)		(0.0000008)
Δ(*Unemployment Rate*)	-0.008	Unemployment Rate	0.006
	(0.006)		(0.004)
Median Age	-0.002	Median Age	-0.001
	(0.002)		(0.002)
Black	0.186***	Black	0.191***
	(0.072)		(0.068)
Evangelical	-0.0002**	Evangelical	-0.0001**
	(0.00008)		(0.00006)
BA Share	-0.00003	BA Share	0.002**
	(0.0005)		(0.0008)
Sex Ratio	0.0001	Sex Ratio	0.00009
	(0.0005)		(0.0006)
Age Dependency Ratio	0.0006	Age Dependency Ratio	0.0007
	(0.001)		(0.001)
Votes(t-1)	-0.410***	Votes(t-1)	0.586***
	(0.039)		(0.045)
Observations	1,510		1,510
R2	48.5		64.3
State Fixed Effects	Yes		Yes

Notes: The dependent variable is the change in Democratic vote shares in the House election in 2008. All specifications include bank and county fixed effects, as well as a constant term, but the results are not presented here. ***, **, and * represent $p<0.01$, $p<0.05$, $p<0.1$, respectively. Robust standard errors, clustered at MSA-level, are in parentheses.

Chapter 3

Elections, Political Races, and Mortgage Credit Market

A large body of the literature documents that politicians interfere with the financial services industry to create favorable economic outcomes in their electorates. It can be done, for instance, by decreasing forclosure rates (Agarwal et al., 2018), decreasing unemployment (Faccio and Hsu, 2017), increasing mortgage credits (Chu and Zhang, 2020), or increasing federal expenditure in certain states (Cohen et al., 2011).

In this regard, the relationship between the banking industry and politics is special. Regulations and licensing can be used by governments to limit the supply of banks. Moreover, the government plays a direct role in the establishment of institutions that provide the basis for a banking system. Banks are also regarded as a tool for political survival. There is a positive relationship between the supply of credit via the banking sector and the outcome of elections (e.g., Hall et al., 2021; Antoniades and Calomiris, 2020). Politicians are therefore encouraged to interfere in the banking sector. This study focuses on a particular type of credit that financial institutions offer and a certain type of election: mortgage credit supply and gubernatorial elections.

Building on the works of Chu and Zhang (2020) and Liu and Ngo (2014), we examine the effect of elections on changes in the supply of mortgage credits around gubernatorial

elections by exploiting exogenous variation in the timing of elections. We hypothesize that politicians have enough incentive and power to affect the supply of credit in order to seek favorable electoral outcomes. More specifically, we explore if either mortgage approval rates or mortgage lending volume change during a year leading to a gubernatorial election. The timing of elections has been used in the literature frequently as a source of exogenous variation (e.g., Goodell and Vähämaa, 2013; Jens, 2017; Gao et al., 2019). In line with the literature, we make use of the timing of the U.S elections in this study to examine the financial consequences of gubernatorial elections. We believe the timing of the U.S gubernatorial elections provides enough exogenous variations for two reasons: first, the timing is determined by law exogenously, and second, not all states hold gubernatorial elections at the same time.

We collect data from several sources. Our loan-level data comes from the Home Mortgage Discourse Act (HMDA). We also collect data on banks' performance from the Reports of Condition and Income (Call Reports). Using the bank identifier item in the Call Reports, we merge them with the HMDA data. We also collect data on local economic conditions from the Bureau of Economic Analysis and the Federal Housing Finance Agency. Our sample covers the years from 2000 to 2016.

In our baseline regression analysis, employing the identifier for Bank Holding Company in Call Reports, we aggregate the data at the BHC-county-year level. Our baseline results indicate that gubernatorial elections have no effects on lending decisions measured by approval rates or lending growth rates, suggesting that mortgage credits do not change in the years leading to a gubernatorial election. We further explore the effects of gubernatorial elections on lending decisions when a governor has full control over a state legislature (i.e., when a governor shares the same party affiliation with the majority of seats in both chambers of a state legislature). Similar to our baseline findings, we do not find any significant relationship between gubernatorial elections and the supply of

mortgage credits.

A wide range of studies focus on the incentives of incumbent politicians to change economic policies in order to attract voters for their next term (e.g., Carvalho, 2014*b*; Drazen, 2000). In line with this strand of literature, we examine if incumbent governors who are up for re-election affect lending decisions. We find that mortgage approval rates increase by 30 basis points in years an incumbent governor is up for re-election. However, this effect does not depend on whether they have full control over state legislatures or not.

As mentioned above, the law establishes election dates exogenously. However, Jens (2017) notes that reverse causality still poses some concerns. A "quality challenger" is more likely to challenge an incumbent when local economic conditions are poor, according to Van Dunk (1997). To address this concern, we conduct a spatial regression discontinuity method and explore the financial consequences of gubernatorial elections. We focus on census tracts that are adjacent to one another yet in two different states. We find that census tracts in states where a gubernatorial election is held, lending growth rates increase dramatically. However, this effect is only present when a governor has full control over the state legislature.

As our results from the RD design estimations indicate that the supply of mortgage credits increases during the years leading to a gubernatorial election, there should be some costs for banks in the subsequent years since they make risky decisions. We test this hypothesis and find that banks temporarily benefit from their lending decisions. However, they finally pay for the costs of their decisions. More specifically, their performance metrics, including return on assets, return on equity, and capital ratio, first increase but subsequently drop.

Finally, we conduct a series of robustness checks to test the sensitivity of our results. First, our results are robust to outliers, and we find similar results when we drop regions

whose total loan applications are less than 10. Second, we double-cluster all regressions, and the results do not change. Third, we drop midterm elections and find that those incumbent governors who have full control over state legislatures and are up for re-elections no longer affect lending decisions. However, we find similar results by focusing solely on mid-term elections. Our findings suggest that governors are more likely to intervene in the banking sector during midterm elections than during on-time elections. Finally, our finding are robust to dropping elections held during the financial crisis of 2008-2009.

The rest of the paper is organized as follows. In section 2, we review the related literature. Section 3 gives an overview of elections in the U.S. Section 4 describes the data. In section 5, we present our methodology and estimation results. In section 6, the results of the robustness checks are presented. Section 7 concludes.

3.1 Literature Review

This work contributes to several strands of the literature. First, this paper is directly related to a large body of literature arguing that economic conditions are correlated with political outcomes. Studies in this regard can be divided into two parts. At the macro-level, the literature has focused on macroeconomic variables, including economic growth, inflation, government spending, and unemployment (for instance, MacRae, 1977; Alesina, Cohen and Roubini, 1993; Persson et al., 2000; Fair, 1978, 1996, 2009*b*). With the availability of micro-level data, recent studies have focused on micro contexts such as foreclosure rates and mortgage approval rates (for example, Antoniades and Calomiris, 2020; Hall et al., 2021).

Second, our paper adds to the works on how politicians interfere with the financial sector in order to seek favorable outcomes. It is easier for politicians in developing

countries where the level of corruption is generally high to intervene in the financial sector. Using a sample of developing countries, Brown and Dinc (2005) find that banking failure is less likely during election years. State-owned banks lend to farmers in India during election years more in competitive districts, according to Cole (2009). This behavior, however, is not limited to developing countries. Liu and Ngo (2014), as one of the few studies in the U.S, document that banking failure is less likely to occur during gubernatorial election years. This effect is more pronounced if a governor has full control over both state legislative chambers. In another study, Delatte et al. (2020), using corporate data in France, find that the credit supply of independent private banks changes in the constituency of contested political incumbents to help them get re-elected.

Finally, this paper also adds to the growing literature on the relationship between housing markets, especially the mortgage industry, and political outcomes. Mian et al. (2010) document that during the expansion years of the mortgage industry (i.e., from 2002 to 2007), campaign donations from this industry rapidly increased, which might have affected the U.S. government policy. Mabud (2016) finds that the increase in mortgage credits in post-2000 elections helped incumbents in low-income counties to win elections. Hall et al. (2021) find that an increase in foreclosure rates was associated with lower turnout in Ohio. In another work, Antoniades and Calomiris (2020) find that the contraction in mortgage credits during the financial crisis of 2008 led the Republican Presidential candidate to lose the 2008 Presidential election. Finally, Chu and Zhang (2020) find that mortgage approval rates increase in the home state of the chairs of the Banking Committee of the U.S Senate.

3.2 Elections in the U.S.

In the U.S, the election time is determined exogenously by law. The first Tuesday after the first Monday in November has been the date of elections since 1845. Therefore, elections take place between November 2 and November 8. The most important elections, the presidential elections, are held every four years. Congress and the Senate hold their elections every two years. Generally, most states hold gubernatorial elections around the same time as federal elections (either the midterm election or the presidential election). Five states, however, hold their gubernatorial elections in different years. Except in New Hampshire and Vermont, governor elections are held every four years. As a result, a pattern emerges: Not every state holds gubernatorial elections in the same year. Governorship elections take place at varying times in contrast to presidential elections. These exogenous variations are used to examine whether electoral factors can explain changes in mortgage credit.

3.3 Data

In this paper, we collect data from various sources to explore the effects of gubernatorial elections on mortgage credits. The first part of our data comes from the Home Mortgage Disclosure Act (HMDA). HMDA requires all financial institutions to collect and report detailed data regarding applications for mortgage loans. Although the HMDA data is a loan-level dataset, the identifier of each loan is the financial agency via which the loan has been applied for or issued. HMDA is a very rich dataset that includes loan-level information about the status of mortgage applications, as well as information on the borrowers' personal characteristics, including gender, race, ethnicity, and income. It also reports information on loans' characteristics, including the location of the property

and the purpose of the loan. We obtain data from HMDA from 2000 to 2016.

Pulling together, our loan-level dataset contains more than 400 million observations. Our final goal is to form a dataset at the BHC-county-year level. However, before aggregating our data, we drop around 50 percent of the loan-level observations. First, we drop withdrawn loans as well as loans purchased from other institutions. Second, we remove non-conventional loans as they do not follow traditional mortgage loan requirements. Third, we drop loans with missing information.

The second part of our data comes from the Reports of Condition and Income for commercial banks known as "Call Reports", which is a bank-level dataset. All financial institutions are required to file their financial information periodically. We obtain the Call Reports from 2000 to 2016 from the work by Drechsler et al. (2017). These data are also publicly available via the Federal Financial Institutions Examination Council.[1] We then merge Call Reports with the HMDA data using the procedure first employed by Loutskina and Strahan (2009). We make use of loan "respondent id" item as reported by HMDA and match it with the Call Reports using two different identifiers, depending on the regulator agency of banks. We consider two types of banks: those that are regulated either by the Federal Reserve (FR) or the Federal Deposit Insurance Corporation (FDIC). The former is matched using item RSSD9050 and the latter using RSSD9055 in Call Reports.

The third part of our data is election data. We collect the data on gubernatorial election years for all states from the CQ Voting and Elections Collection. We also collect the data on state legislative chambers, the party that holds the majority and governors' party affiliation from the National Conference of State Legislatures website.[2]

The fourth part of our data is the data on local economic conditions. In all of our

[1] See https://cdr.ffiec.gov/public/ManageFacsimiles.aspx.
[2] See https://www.ncsl.org/research/about-state-legislatures/partisan-composition.aspx.

regressions, we control for economic conditions at either the county level or state level, depending on the availability of data. We collect data on counties' personal income and its growth rate from the Bureau of Economic Analysis. We also obtain data on House Price Index (HPI) from the Federal Housing Financial Agency. These data are available at the state level.

Finally, we obtain data on adjacent census tracts for two decennial censuses done in 2000 and 2010 from Brown University. We use this dataset to determine if two census tracts are adjacent to each other.[3]

To construct our dataset, we aggregate the data in either BHC-county-year or BHC-census tract-year levels, depending on our specifications.

Table 3.2 provides a brief description of the variables we use in this paper. Table 3.1 presents the descriptive statistics of the variables.

In Panel A, the summary statistics of the HMDA, election, and county economic conditions data are shown at the BHC-county-year level. The mean of *Gubernatorial Election* is 0.26 indicating there was a gubernatorial election in around 25 percent of all data points. The variable *Incumbency* measures the percentage of instances in which a governor is up for re-election. This happens in 16 percent of observations.[4] The last variable regarding the election data is *Full Control*. It is a dummy variable equal to one if a governor has full control of both state legislative chambers and zero otherwise. The mean is equal to 0.59.

The next eight variables are from the HMDA data. *Approval Rate* is a dummy variable equal to one if a loan application has been approved and zero otherwise. This

[3]See https://s4.ad.brown.edu/Projects/Diversity/Researcher/Pooling.html.

[4]We consider Gray Davis and Scott Walker an incumbents in the 2003 California gubernatorial recall election and the 2012 Wisconsin gubernatorial recall election, respectively as they faced a recall. We also consider Earl Ray Tomblin and Kate Brown incumbents in the in the 2003 West Virginia gubernatorial special election and the 2015 Oregon gubernatorial special election as they were the acting governors at the time of the election.

variable is one of the two outcome variables we use in this paper. On average, 79 percent of loans have been approved in our sample. *Lending Growth Rate* is the other outcome variable and is calculated by taking the difference between the natural logarithm of lending volume in year t and year $t-1$. We only consider approved loans in calculating this variable. We take the natural logarithm of applicants' income to calculate *Log (Income)* as reported by HMDA. *Female* is a dummy variable equal to one if an applicant is female and zero otherwise. 25 percent of all applicants in our sample have been female applicants. Finally, *Minority* reports the percentage of applicants with a minority background. As reported by HMDA, 9 percent of all applicants have a minority background. Starting 2004, HMDA reports ethnicity data. However, we do not use it as our sample starts in 2000.

Panel B of Table 3.1 reports banks' fundamentals at the BHC-year level. We employ these data to check if lending decisions are costly for banks. All metrics are in line with the literature.

3.4 Methodology and Main Results

3.4.1 Baseline Estimations

We closely follow the estimation strategy proposed by Chu and Zhang (2020) to investigate the effects of gubernatorial elections on mortgage lending. Gubernatorial elections are held at the state-level, but in line with literature (e.g., Favara and Imbs, 2015; Chavaz and Rose, 2019; Chu and Zhang, 2020), we estimate our baseline estimations at the county level. Specifically, we estimate the following equation:

$$Y_{ict} = \beta\, Gubernatorial\ Election_{st} + \gamma X_{ct} + \delta Z_{ict} + \theta_{ict} + \epsilon_{ict}, \tag{3.1}$$

where Y is either the lending growth rate or loan approval rate at bank i, state c, year t.

Lending growth rate is calculated by taking the difference between the natural logarithm of lending volume in year t and year $t-1$. Loan approval rate, on the other hand, is measured by dividing the number of approved loans by total number of loans.

Gubernatorial Election is a dummy variable that takes a value of one if there is a gubernatorial election in year t at state s and zero otherwise. The coefficient of interest is β that shows the effects of gubernatorial election on either mortgage approval rate or mortgage lending growth. X_{ct} is a vector of county level economic conditions, including personal income and personal income growth rate. We also include the Home Price Index here, but it is measured at the state level, so it is the same for all counties in a given state. Z_{ict} is a vector of borrowers' characteristics as reported by the HMDA. It includes borrowers' race, gender, income, and loan to income ratio aggregated at the county level. θ_{ict} is a vector of fixed effects. For the baseline regressions, we generally include BHC $\times$ year and BHC $\times$ state fixed effects. However, depending on the specification, we later add more fixed effects.[5]

As we include personal characteristics of applicants in our estimation, Equation (3.1) controls for variations in borrower attributes. As a result, all the demand-side shifts that are linked to the varying compositions of borrowers are removed. By including time-interacted state fixed effects, it also controls for differences in the economic environments of states. Moreover, it might remove some of the supply-side effects that are associated with the location of lenders. For instance, banks may treat borrowers differently according to their locations with regard to variations in location-specific risks.[6]

Table 3.3 presents the results of estimating equation (3.1). The dependent variable in the first two columns is mortgage approval rate, and lending growth rate in columns 3 and 4. In columns 2 and 4, we additionally add county $\times$ legislature fixed effects and

[5]In all regressions hereafter we drop regions with fewer than 10 loans in order to mitigate the effects of outliers.

[6]For instance, expected house price appreciation can vary based on location.

county × governor fixed effect to control for the party that holds the majority in a state legislature and governors' party affiliation, respectively. In none of our specifications we find a significant impact of gubernatorial elections on mortgage lending decisions as the estimated coefficients are statistically insignificant. Although the directions of coefficients are negative, they are too small. All other estimated coefficients are in line with literature. For example, the estimated coefficients on *Female* and *Minority* are negative and statistically significant, suggesting that women and people with minority backgrounds are less likely to be approved for loan applications. Local economic conditions positively affect lending decisions as reported by estimated coefficients on *Personal Income* and *Growth of Personal Income*.

3.4.2 Full Control of State Legislature and Lending Decisions

Following Liu and Ngo (2014), we explore the effects of gubernatorial elections on lending decisions when a governor has full control of a state legislature. As reported in Table 3.1, governors' party affiliations are the same as both state legislative champers in 59 percent of all data points. This allows governors to intervene with the banking sector to a higher extent as they are less likely to be questioned by state representatives and senators. In order to investigate this effect, we add two more variables to equation (3.1): *Full Control* and *Gubernatorial Election* × *Full Control*. Specifically, we estimate the following equation:

$$\begin{aligned} Y_{ict} = {} & \beta\, Gubernatorial\ Election_{st} + \zeta\, Full\ Control_{st} \\ & + \eta\, Gubernatorial\ Election_{st} \times Full\ Control_{st} + \gamma X_{ct} + \delta Z_{ict} + \theta_{ict} + \epsilon_{ict}, \end{aligned} \tag{3.2}$$

where $FullControl$ is a dummy variable equal to one if a governor in year t at state s has full control on both state legislative chambers and zero otherwise. All other variables are the same as equation (3.1). Of particular interest is *Gubernatorial Elections*

$\times$ *Full Control*, capturing the effects of gubernatorial elections on lending decisions when governors have full control. Table 3.4 presents the results of estimating Equation (3.2). Our results are in line with the baseline estimation. The estimated coefficient on *Gubernatorial Elections* $\times$ *Full Control* is negative but statistically insignificant. Economically speaking, it is also near zero. All other variables are in line with what we find in Table 3.3.

3.4.3 Incumbency and Lending Decisions

A large body of literature focus on the incentives of incumbent politicians to change economic policies in order to attract voters for their next term (e.g., Carvalho, 2014*b*; Drazen, 2000). We now explore if incumbent governors who are up for re-election have more incentives, and probably more power, to interfere with the banking sector. As shown in Table 3.1, in 16 percent of all observations, incumbent governors are up for re-election. That is about 60 percent of all gubernatorial elections in our sample. As a result, we alter equations (3.1) and (1.2) to add the incumbency status of governors. Specifically, we estimate the following equations (1.3) and (1.4):

$$Y_{ict} = \beta Incumbency_{st} + \gamma X_{ct} + \delta Z_{ict} + \theta_{ict} + \epsilon_{ict}, \tag{3.3}$$

$$\begin{aligned} Y_{ict} = {} & \beta Incumbency_{st} + \zeta Full\ Control_{st} + \eta Incumbency_{st} \times Full\ Control_{st} + \gamma X_{ct} \\ & + \delta Z_{ict} + \theta_{ict} + \epsilon_{ict}, \end{aligned} \tag{3.4}$$

where *Incumbency* is a dummy variable equal to one a governor in year t at state s is up for re-election and zero otherwise. The variable of interest in equation (3.4) is *Incumbency* $\times$ *Full Control* capturing the effects the effects of gubernatorial elections in which a governor is up for re-election and have the full control on both state legislative chambers on lending decisions. The first two columns of table 3.5 presents the results

of estimating Equation (3.3). The estimated coefficient on *Incumbency* is positive and statistically significant at the 90 percent level. It is also economically significant: in states where a governor is up for re-election, mortgage approval rate increases by 30 basis points. This is line with the studies of Carvalho (2014*b*) and Drazen (2000). Turning to column 2, the estimated coefficient on *Lending Growth* is not statistically significant. As for the interaction terms, none of them are significant, suggesting incumbent governors who share the same party with both state legislative chambers do not interfere with lending decisions. All other estimated coefficients are close to what we find earlier.

3.4.4 Endogeneity Concerns and Spatial Regression Discontinuity Design

As discussed earlier, election dates are exogenously determined by law. However, as noted by Jens (2017), there might still be some concerns regarding reverse causality. According to Van Dunk (1997), "quality challengers" are more likely to challenge an incumbent when the local economy is performing poorly. This clearly affects lending decisions. In addition, Chu and Zhang (2020) argue that there might be some concerns regarding the omitted variable bias, as powerful politicians are more likely to increase government spending in their home states.

As mentioned above, not all states hold gubernatorial elections in the same years. Figure 3.1 illustrates this situation clearly. It shows the gubernatorial elections held in 2012. As shown on the map, most neighboring states do not hold elections in the same year. We make use of this pattern to conduct a spatial RD design. we focus on state borders and compare either mortgage approval rates or lending growth rate across borders by focusing on adjacent census tracts.[7] We aggregate the data at BHC-census

[7]Using census tracts in RD designs have recently been popular in the literature. See, for example, Di Maggio and Kermani (2017) and Chavaz and Rose (2019) .

tract-year level and run the following regressions:

$$Y_{ijt} = \beta Gubernatorial\ Election_{st} + \gamma X_{ijt} + \delta Z_{it} + \theta_{it} + \zeta_{jp} + \epsilon_{ijt}, \tag{3.5}$$

$$Y_{ijt} = \beta Incumbency_{st} + \gamma X_{ijt} + \delta Z_{it} + \theta_{it} + \zeta_{jp} + \epsilon_{ijt}, \tag{3.6}$$

where the dependent variable is either approval rate or lending growth rate in bank i, census tract j, year t. To ensure we compare census tracts that are immediately adjacent to one another, we include census tract pair fixed effects as shown by ζ_{jp}. All other variables are the same as equations (3.1) and (3.2). Figure 3.2 illustrates our analysis. Census tract 1 is in the state of Indiana where there was a gubernatorial election in 2012. Census tracts 2 and 3 are in the state of Illinois where there was no election at the same time.

Table 3.6 presents the results from estimating equations (3.5) and (3.6). In line with our baseline estimations, none of the estimated coefficients are significant. All other estimated coefficients are almost the same as our baseline results.

We also explore the effects of gubernatorial elections on lending decisions when governors hold the control of both state legislative chambers. Therefore, we add interaction terms to equation (3.5) and (1.6) to investigate this effect. The results are presented in Table 3.7. Of interest in column 1 is *Gubernatorial* $\times$ *Full Control*. The estimated coefficient on this interaction term is positive and statistically significant. Its effect is also economically large and significant: in states where a governor holds the control of both state legislative chambers, mortgage lending growth rate increases by 360 basis points. We find similar results for the situations in which a government is up for re-election while having full control over the state legislature. This effect is now even more pronounced, indicating lending growth rate increases by 440 basis points.

Turning into effects of gubernatorial elections on approval rate when governors have full control over state legislative chambers, we do not find any significant effects for any

elections. As for elections in which an incumbent is up for re-election, we find that approval rates decrease by 80 basis points. While this effect is marginally significant and relatively small compare to what we find in the first two columns, it has important implications. It suggests that politicians might target nested interest groups as documented by the literature. Our results are in line with works by Chu and Zhang (2020), Liu and Ngo (2014), Bertrand et al. (2007), and Faccio and Hsu (2017) . They all find that favorable economic outcomes occur during election times.

3.4.5 The Costs for Banks

Together, our finding indicate that in states where governors, specially incumbent ones that hold the control of both state legislative chambers, lending decisions are generous in favor of certain groups. If banks make risky decisions, there should be some costs associated with their lending decisions. Following Chu and Zhang (2020), we make use banks' return on assets (ROA), return on equity (ROE), and capital ratio (CR) as metrics for banks' performance and explore the possibility of some losses until four years after a gubernatorial election. We limit the upper bound to four years in order not to coincide with the next gubernatorial election. In particular, we estimate the following equation:

$$Performance_{i,t+\phi} = \beta\, Gubernatorial\ Election_{st} + \theta_i + \zeta_t + \epsilon_{ijt}, \tag{3.7}$$

where the dependent variable is one of the ROA, ROE, or CR. We also include year and bank fixed effects. Table 3.8 shows the results from estimating equation (3.7). Although other than three coefficients, the rest are statistically insignificant, there is a clear pattern in timing of banks' performance. For example, in Panel C, the estimated coefficient on *Gubernatorial Election* is positive and statically significant for year *t+1*, it becomes negative while insignificant for the following years. There is a similar pattern in

all other panels. Together, these results indicate that banks enjoy an instant benefit from their lending decisions during election years and then incur some losses in subsequent years. It should also be noted that mortgage default rates peak around five years after their originations, but we limited our upper bound to 4 year as mentioned above.

3.5 Robustness Analysis

To check the robustness of our results, we conduct several sets of sensitivity checks. We focus on the robustness of our finding in Table 3.7 which present our benchmark findings. First, we add all loans to our sample and re-estimate equations (3.5) and (3.6) with interactions.[8] As mentioned above, we remove census tracts with less than 10 loans. Including all observations, we find almost similar results. More precisely, there results are more pronounced here.

Second, in all previous estimations we clustered standard errors at the state level. In column 2 of Table 3.9, we double cluster our estimations at both state and year levels and still find similar results.

Third, we differentiate between on-time and midterm elections. There might be a difference between our results depending on the type of the election. On-time elections are held at the same time as presidential elections and presidents also have some incentives to interfere with the financial sector. In column 3, we only include on-time elections and find interesting results. While the estimated coefficients on the interaction term for gubernatorial elections is statistically significant, it is not the case for our incumbency sample. Moreover, the interaction term for the gubernatorial sample is marginally significant. In column 4, we only include data points from midterm elections. The estimated coefficients are larger and the significance level increases. Together, our results indicate

[8] In all specifications, we include a set of borrowers' characteristics, county controls, and fixed effects although we do not report them.

that governors are more likely to intervene with the banking sector in midterm elections than on-time elections. Although the channel is not clear, it might be because of the fact that governors decisions are highly impacted by presidents' decisions.

Finally, we identify years 2008 and 2009 as crises years and exclude them from our sample. As presented in column 5 of Table 3.9, our results are robust to removing these two years.

3.6 Conclusion

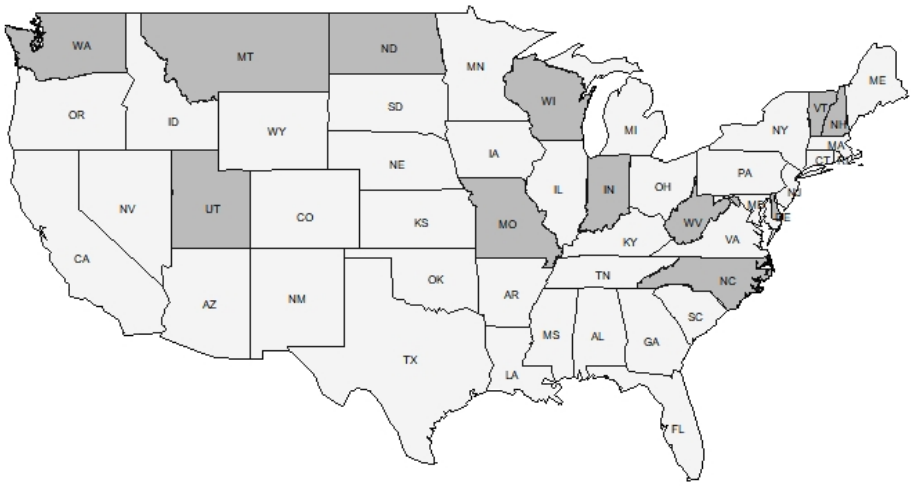
The economic voting theory suggests that the condition of the economy impacts voter behavior. As such, politicians have incentives to create favorable economic conditions in order to attract voters. According to the literature, politicians might take different actions to benefit their electorate. In this paper, we examine if governors interfere with the mortgage industry in order to induce them to be more generous in their lending decisions.

To examine our question, we collect data on gubernatorial elections, mortgage application outcomes, and local economic conditions. We first make use of the exogenous variation in the timing of the U.S elections and find no significant effects from gubernatorial elections on mortgage lending decisions. We further argue that although elections are believed to be exogenous, there still might be some endogeneity concerns, including reverse casality. In order to address this issue, we conduct a regression discontinuity design and find that governors do intervene in the banking sector. In particular, we find that mortgage lending growth rates increase significantly during a year leading to a gubernatorial election.

We further identify that banks' lending decisions are associated with some costs. Although banks enjoy a temporary improvement in their performance, they later pay

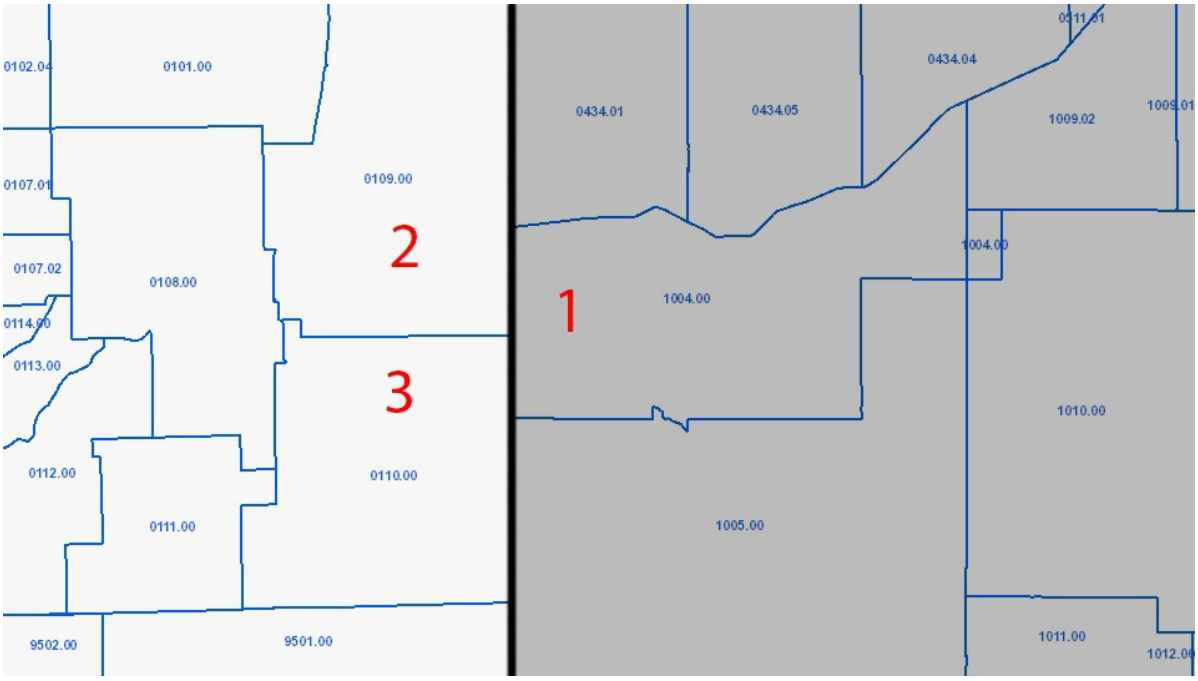
the costs of their generous lending decisions. Finally, we check the robustness of our results. Our results are robust to different specifications.

Figure 3.1: Gubernatorial Elections in 2012: State-level Data



Notes: The figure shows gubernatorial elections held in 2012 in the U.S. States in which a gubernatorial election was held are colored with grey. States include: Delaware, Indiana, Missouri, Montana, New Hampshire, North Carolina, Utah, Vermont, Washington, Wisconsin, and West Virginia.The election in Wisconsin was a special election as the incumbent governor faced a re-call. Source: The FFIEC map and author's calculations.

Figure 3.2: Gubernatorial Elections in 2012: Tract-level Data



Notes: This plot shows the adjacent census tracts at the border of Indiana and Illinois. Census tract 1 is in Indiana and 2 and 3 are in Illinois. Indiana is colored with grey as there was a gubernatorial election in the state in 2012.

Table 3.1: Descriptive Statistics

Panel A. BHC-County-Year level					
Variable	N	Mean	SD	P25	P75
Gubernatorial Election	329,206	0.26	0.44	0	1
Incumbency	329,206	0.16	0.36	0	0
Full Control	329,206	0.59	0.49	0	1
Approval Rate	329,206	0.79	0.19	0.676	0.942
Lending Growth Rate	329,206	0.03	0.61	-0.214	0.263
Total Loans	329,206	987.45	2,384.65	34	725
Log (Income)	293,384	4.49	0.51	4.171	4.742
Loan to Income Ratio	293,384	1.45	0.88	1.009	1.789
Female	329,206	0.25	0.17	0.143	0.33
Minority	329,128	0.09	0.15	0	0.105
Log (Personal Income)	329,206	14.98	1.59	13.761	16.073
Growth of Personal Income	329,206	0.04	0.04	.0167	.058
HPI Growth	329,206	0.03	0.05	-0.003	.052
Lag of HPI Growth	329,206	0.03	0.05	-0.005	0.053
Panel B. BHC-Year level					
Gubernatorial Election	30,389	0.24	0.41	0	0.341
Log (Assets)	30,389	12.57	1.33	11.673	13.210
Capital Ratio	30,389	10.00	2.62	8.421	11.095
ROA (%)	30,388	0.17	1.01	0.118	0.313
ROE (%)	30,388	1.22	15.39	1.162	3.239
Deposits/Assets	30,389	0.83	0.07	0.797	0.879
RE Loans/Assets	19,079	0.49	0.15	0.386	0.599
CI Loans/ Assets	30,179	0.09	0.06	0.049	0.124

Notes: Full description of variables are presented in Table 3.1

Table 3.2: Data Description

Panel A. **Election Data**	
Variable	Description
Gubernatorial Election	Dummy Variable – 1 if there is a gubernatorial election in a state, 0 otherwise.
Incumbency	Dummy Variable – 1 if the incumbent is up for reelection, 0 otherwise.
Full Control	Dummy Variable – 1 if a governor has the full control of both state legislative chambers, 0 otherwise.
Panel B. **HMDA Data**	
Race: Minority	Percentage of minority applicants
Female	Percentage of female applicants
Loan to Income	Requested loan amount over applicants' income (total income for application with co-applicant).
Log (Income)	Natural logarithm of Applicants' Income.
Log (Loan Amount)	Natural logarithm of requested loan amount.
Approval Rate	Percentage of approved loans.
Lending Growth Rate	Growth rate of total lending volume from previous year.
Panel C. **Banking Data**	
Log (Assets)	Natural logarithm of banks' total assets
Capital Ratio	Ratio of total equity to total assets
ROA (%)	Share of net income to total assets
ROE (%)	Share of net income to total equity
Deposits/Assets	Share of total banks' deposits to total assets
RE Loans/Assets	Share of total real state loans to to total assets
CI Loans/ Assets	Share of commercial and industrial loans to total assets
Panel D. **County Characteristics Data**	
Log (Personal Income)	Natural logarithm of counties' personal income
Growth of Personal Income	Growth rate of personal income from the previous year
HPI Growth	Growth rate of the housing price index
Lag of HPI Growth	Lag of growth rate of the housing price index

Table 3.3: Baseline Estimation Results

	(1) Approval Rate	(2) Approval Rate	(3) Lending Growth	(4) Lending Growth
Gubernatorial Election	-0.0006	-0.0003	-0.005	-0.006
	(0.001)	(0.001)	(0.005)	(0.005)
Log (Personal Income)	0.0001	0.028***	-0.034***	-0.147***
	(0.0007)	(0.007)	(0.003)	(0.024)
Growth of Personal Income	0.053***	-0.003	-0.246***	0.143**
	(0.015)	(0.014)	(0.067)	(0.056)
Log (Applicant Income)	0.045***	0.041***	0.370***	0.491***
	(0.002)	(0.002)	(0.017)	(0.027)
Female	-0.042***	-0.039***	-0.075***	-0.062***
	(0.003)	(0.003)	(0.01)	(0.011)
Minority	-0.132***	-0.123***	-0.058**	-0.114***
	(0.009)	(0.009)	(0.024)	(0.024)
HPI Growth	0.132***	0.124***	0.638***	0.614***
	(0.030)	(0.026)	(0.093)	(0.086)
Lag. HPI Growth	0.069**	0.070**	0.018	0.039
	(0.031)	(0.031)	(0.090)	(0.086)
Loan to Income	0.009***	0.007***	0.145***	0.169***
	(0.002)	(0.001)	(0.031)	(0.041)
Observations	284,977	281,500	284,977	281,500
F-stat	88.37	77.29	218.2	586.3
R2	0.516	0.548	0.181	0.216
BHC × Year FE	Yes	Yes	Yes	Yes
BHC × State FE	Yes	Yes	Yes	Yes
County × legislature FE	No	Yes	No	Yes
County × Governor FE	No	Yes	No	Yes

Notes: The dependent variable in columns 1 and 2 is the approval rate, and in columns 3 and 4 is the lending growth rate. A constants is included in all specifications, but we do not report it. Robust state clustered standard errors are reported in the parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Table 3.4: The Role of Having the Full Control of the State Legislative Chambers

	(1) Approval Rate	(2) Approval Rate	(3) Lending Growth	(4) Lending Growth
Gubernatorial Election	0.0002	0.001	-0.0005	-0.005
	(0.002)	(0.002)	(0.006)	(0.006)
Full Control	0.002	0.001	0.004	0.005
	(0.003)	(0.003)	(0.006)	(0.012)
Gubernatorial * Full Control	-0.002	-0.003	-0.008	-0.002
	(0.003)	(0.003)	(0.010)	(0.009)
Log (Personal Income)	0.0001	0.027***	-0.034***	-0.147***
	(0.0007)	(0.007)	(0.003)	(0.025)
Growth of Personal Income	0.053***	-0.003	-0.246***	0.143**
	(0.015)	(0.014)	(0.067)	(0.056)
Log (Applicant Income)	0.045***	0.041***	0.370***	0.491***
	(0.002)	(0.002)	(0.017)	(0.027)
Female	-0.042***	-0.039***	-0.075***	-0.063***
	(0.003)	(0.003)	(0.012)	(0.011)
Minority	-0.132***	-0.123***	-0.058**	-0.114***
	(0.01)	(0.009)	(0.02)	(0.025)
HPI Growth	0.133***	0.124***	0.639***	0.614***
	(0.030)	(0.026)	(0.093)	(0.086)
Lag. HPI Growth	0.071**	0.071**	0.020	0.04
	(0.035)	(0.032)	(0.090)	(0.087)
Loan to Income	0.01***	0.008***	0.145***	0.169***
	(0.002)	(0.002)	(0.031)	(0.041)
Observations	284,977	281,500	28,4977	281,500
F	75.59	72.43	197.5	495.2
R2	0.516	0.548	0.181	0.216
BHC × Year FE	Yes	Yes	Yes	Yes
BHC × State FE	Yes	Yes	Yes	Yes
County × legislature FE	No	Yes	No	Yes
County × Governor FE	No	Yes	No	Yes

Notes: The dependent variable in columns 1 and 2 is the approval rate, and in columns 3 and 4 is the lending growth rate. A constants is included in all specifications, but we do not report it. Robust state clustered standard errors are reported in the parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Table 3.5: The Role of Incumbency

	(1) Approval Rate	(2) Lending Growth	(3) Approval Rate	(4) Lending Growth
Incumbency	0.003*	-0.007	0.003	-0.015
	(0.002)	(0.008)	(0.002)	(0.011)
Full Control			0.002	0.001
			(0.003)	(0.006)
Incumbency * Full Control			-0.0003	0.012
			(0.003)	(0.015)
Log (Personal Income)	0.0002	-0.034***	0.0002	-0.034***
	(0.0008)	(0.003)	(0.0008)	(0.004)
Growth of Personal Income	0.053***	-0.247***	0.053***	-0.248***
	(0.017)	(0.067)	(0.016)	(0.0671)
Log (Applicant Income)	0.046***	0.370***	0.046***	0.370***
	(0.003)	(0.017)	(0.003)	(0.0171)
Female	-0.042***	-0.075***	-0.042***	-0.075***
	(0.0034)	(0.012)	(0.003)	(0.012)
Minority	-0.132***	-0.059**	-0.132***	-0.059**
	(0.01)	(0.025)	(0.01)	(0.025)
HPI Growth	0.132***	0.639***	0.132***	0.636***
	(0.031)	(0.093)	(0.031)	(0.094)
Lag. HPI Growth	0.068**	0.016	0.069**	0.020
	(0.036)	(0.091)	(0.032)	(0.090)
Loan to Income	0.01***	0.145***	0.01***	0.145***
	(0.002)	(0.032)	(0.002)	(0.032)
Observations	284,977	284,977	284,977	284,977
F-stat	87.16	218.1	72.11	205.0
R2	0.516	0.181	0.516	0.181
BHC × Year FE	Yes	Yes	Yes	Yes
BHC × State FE	Yes	Yes	Yes	Yes

Notes: The dependent variable in columns 1 and 2 is the approval rate, and in columns 3 and 4 is the lending growth rate. A constants is included in all specifications, but we do not report it. Robust state clustered standard errors are reported in the parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Table 3.6: Spatial RD Design, Baseline Estimations

	(1) Approval Rate	(2) Lending Growth	(3) Approval Rate	(4) Lending Growth
Gubernatorial Election	-0.001	-0.009		
	(0.002)	(0.009)		
Incumbency			0.001	-0.001
			(0.002)	(0.009)
Log (Personal Income)	0.035**	-0.069	0.035**	-0.069
	(0.015)	(0.056)	(0.015)	(0.056)
Growth of Personal Income	-0.023	0.036	-0.022	0.037
	(0.024)	(0.117)	(0.023)	(0.117)
Log (Applicant Income)	0.0300***	0.469***	0.030***	0.469***
	(0.002)	(0.032)	(0.002)	(0.032)
Female	-0.023***	-0.087***	-0.023***	-0.087***
	(0.003)	(0.014)	(0.003)	(0.014)
Minority	-0.08***	-0.091**	-0.08***	-0.091**
	(0.01)	(0.037)	(0.01)	(0.037)
HPI Growth	0.066*	0.342***	0.064*	0.337***
	(0.033)	(0.098)	(0.033)	(0.099)
Loan to Income	0.0022*	0.055	0.002*	0.055
	(0.001)	(0.036)	(0.001)	(0.036)
Observations	370,115	370,115	370,115	370,115
F-stat	87.34	478.3	92.31	456.7
R2	0.305	0.186	0.305	0.186
BHC × Year FE	Yes	Yes	Yes	Yes
Census Tract-pair FE	Yes	Yes	Yes	Yes

Notes: The dependent variable in columns 1 and 3 is the approval rate, and in columns 2 and 4 is the lending growth rate. A constants is included in all specifications, but we do not report it. Robust state clustered standard errors are reported in the parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Table 3.7: Spatial RD Design, Incumbency and Full Control

	(1) Lending Growth	(2) Lending Growth	(3) Approval Rate	(4) Approval Rate
Gubernatorial Election	-0.029**		-0.002	
	(0.013)		(0.003)	
Full Control	-0.004	-0.0003	0.001	0.003
	(0.009)	(0.008)	(0.002)	(0.002)
Gubernatorial * Full Control	0.036*		0.0009	
	(0.02)		(0.003)	
Incumbency		-0.028**		0.006*
		(0.014)		(0.003)
Incumbency * Full Control		0.044**		-0.008*
		(0.021)		(0.004)
Log (Personal Income)	-0.067	-0.068	0.035**	0.035**
	(0.056)	(0.056)	(0.015)	(0.015)
Growth of Personal Income	0.034	0.03	-0.022	-0.020
	(0.117)	(0.116)	(0.024)	(0.023)
Log (Applicant Income)	0.469***	0.469***	0.030***	0.030***
	(0.032)	(0.032)	(0.002)	(0.002)
Female	-0.087***	-0.087***	-0.023***	-0.023***
	(0.014)	(0.018)	(0.003)	(0.003)
Minority	-0.091**	-0.091**	-0.079***	-0.08***
	(0.037)	(0.037)	(0.009)	(0.009)
HPI Growth	0.339***	0.340***	0.068*	0.067*
	(0.103)	(0.102)	(0.034)	(0.034)
Loan to Income	0.055	0.055	0.002*	0.002*
	(0.036)	(0.036)	(0.001)	(0.001)
Observations	370,115	370,115	370,115	370,115
F-stat	381.4	374.3	74.12	75.66
R2	0.186	0.186	0.305	0.305
BHC × Year FE	Yes	Yes	Yes	Yes
Census Tract-pair FE	Yes	Yes	Yes	Yes

Notes: The dependent variable in columns 1 and 2 is the lending growth rate, and in columns 3 and 4 is the approval rate. A constants is included in all specifications, but we do not report it. Robust state clustered standard errors are reported in the parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Table 3.8: The Costs for Banks

	(1) t+1	(2) t+2	(3) t+3	(4) t+4
Panel A: ROA/Gubernatorial Election				
Gubernatorial Election	-0.033	0.01	0.006	-0.015
	(0.034)	(0.009)	(0.007)	(0.008)
Observations	31,431	27,583	24,247	21,156
R2	0.209	0.271	0.356	0.365
Panel B: ROA/Incumbency				
Incumbency	-0.008	-0.006	-0.015	-0.017*
	(0.024)	(0.010)	(0.01)	(0.010)
Observations	31,431	27,583	24,247	21,156
R2	0.209	0.271	0.356	0.365
Panel C: ROE/Gubernatorial Election				
Gubernatorial Election	0.385*	0.086	-0.288	-0.187
	(0.206)	(0.221)	(0.215)	(0.265)
Observations	31,431	27,583	24,247	21,156
R2	0.215	0.233	0.265	0.305
Panel D: ROE/Incumbency				
Incumbency	0.354**	-0.293	-0.026	-0.310
	(0.177)	(0.312)	(0.338)	(0.368)
Observations	31,431	27,583	24,247	21,156
R2	0.215	0.233	0.265	0.305
Panel E: CR/Gubernatorial Election				
Gubernatorial Election	-0.016	0.012	-0.018	-0.003
	(0.02)	(0.017)	(0.019)	(0.019)
Observations	31,432	27,584	24,248	21,157
R2	0.743	0.754	0.785	0.793
Panel E: CR/Incumbency				
Incumbency	0.013	-0.024	-0.020	-0.03
	(0.028)	(0.026)	(0.029)	(0.030)
Observations	31,432	27,584	24,248	21,157
R2	0.743	0.754	0.785	0.793

Notes: The dependent variable in columns 1 to 4 is a measure of costs for banks from year t+1 to year t+4, respectively. A constants is included in all specifications, but we do not report it. Robust BHC clustered standard errors are reported in the parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Table 3.9: Robustness Checks

	(1) Full Sample	(2) Double Cluster	(3) On-time Elections	(4) Midterm Elections	(5) Non-crisis Years
Panel A. Gubernatorial Election					
Gubernatorial Election	-0.03**	-0.03**	-0.087	-0.018	-0.028**
	(0.012)	(0.012)	(0.069)	(0.014)	(0.013)
Full Control	-0.005	-0.005	-0.012	-0.013	-0.002
	(0.009)	(0.009)	(0.014)	(0.012)	(0.012)
Gubernatorial * Full Control	0.038**	0.038*	0.063*	0.049**	0.040**
	(0.018)	(0.018)	(0.034)	(0.021)	(0.02)
Observations	412,390	412,390	124,529	287,249	358,699
R2	0.197	0.197	0.276	0.210	0.196
Panel B. Incumbency					
Incumbency	-0.029**	-0.029**	-0.015	-0.029*	-0.028**
	(0.013)	(0.012)	(0.044)	(0.015)	(0.012)
Full Control	-0.0002	-0.0003	0.0016	-0.008	0.004
	(0.008)	(0.008)	(0.014)	(0.011)	(0.009)
Incumbency * Full Control	0.045**	0.045**	0.039	0.059***	0.047**
	(0.018)	(0.019)	(0.050)	(0.021)	(0.019)
Observations	412,390	412,390	124,529	287,249	358,699
R2	0.197	0.197	0.276	0.210	0.196

Notes: The dependent variable in columns is the lending growth rate. In all specifications, we control for borrows, loan, and county characteristic, but we do not report it. A constants is included in all specifications, but we do not report it. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. The numbers in parentheses are robust standard errors clustered at the state level except for column 2 in which we additionally cluster at the year level.

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