Three Essays on Social and Corporate Finance

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Abstract

This thesis consists of three distinct chapters on empirical finance. In the first chapter, I examine how political ideology of the government shapes household's access to financial services. A rapidly growing literature has shown the importance of government political ideology for regulatory and socio-economic actions in the financial sector. Financial inclusion, the access to formal financial services, provides an entry key for people to participate in the economy. Using granular survey data of 65 countries, I find that financial inclusion is higher under right-wing regimes than under left-wing governments. I use regression discontinuity design and propensity-score matching to address endogeneity issues. I also show that right-wing regimes enhance mobile banking. Moreover, right-wing market-oriented policies induce people to save less and use accounts more frequently. I conclude that right-wing market-oriented policies are more successful in enhancing financial inclusion than left-wing societal policies.

Chapter 2 investigates why do managers preannounce asset sales. I find that 32% of the announcements of asset sales are preceded by a public statement of the intention to sell. I refer to these statements as preannouncements and find significant average announcement returns of 1.12%, which have not been documented in the literature. A key characteristic of the preannouncements is that corporate executives have discretion in timing the statement of their intention. As a result, the preannouncements prevail in specific situations: for assets outside the U.S., after poor stock performance, and when a new CEO has been appointed recently. I find that the preannouncement returns are explained by size and leverage. In contrast, returns on deals that were not preannounced have different explanations, such as past returns and the

buyer's identity. Most striking, the ultimate announcements of preannounced deals have low return impact, and this impact is also unrelated to standard explanatory variables. Finally, I observe opportunistic behaviour of managers who vest options around the preannouncements aiming to benefit from the uptick in stock prices. Investors account for this opportunism as returns upon these announcements are 2.9%-point lower.

Chapter 3 examines the relation between property crime and corporate debt covenant intensity. Uncertainty in borrowers' actions induces creditors to increase debt covenant intensity. This chapter examines whether the U.S. states' property crime rate is risk factor that also induce lenders to increase covenants. I find that greater crime exposure of the borrower leads lenders to impose more and tighter covenants. Instrumental variable analysis and various robustness tests confirm my findings. A difference-in-difference test shows that firm's relocation to a higher crime-prone state significantly increases the covenant intensity. I investigate two potential channels that drive the effect of property crime: earnings volatility and reduced collateral value of firms operating in crime-ridden states. I find that covenants and spreads are complementary factors, not substitutes in the presence of higher property crime.

Declaration

This thesis is an original work of my research and contains no material which has been accepted for the award of any other degree or diploma at any university or equivalent institution and that, to the best of my knowledge and belief, this thesis contains no material previously published or written by another person, except where due reference is made in the text of the thesis.

Signature: ……………………

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Date: 31 January 2022

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Chapter 1

Reaching Out to the Unbanked: The Role of Political Ideology in Financial Inclusion

Abstract

This paper studies the effect of political ideology on household financial inclusion. Financial inclusion is the access to formal financial services and provides an entry key for people to participate in the economy. Using granular data of 65 countries, we find that financial inclusion is higher under right-wing regimes than under left-wing governments. We use regression discontinuity design and propensity-score matching to address endogeneity issues. We investigate multiple channels for the effect and conclude that right-wing market-oriented policies are more successful in enhancing financial inclusion than left-wing societal policies.

Keywords: Financial inclusion, Government ideology, Political ideology, Mobile banking

JEL Codes: G2, G21, G51, P16

1.1. Introduction

Financial inclusion, defined as access to formal financial services, has received increasing attention from policymakers due to its potential positive impact on the financial health of the economy. An inclusive financial system is a precondition for achieving financial development (Beck et al., 2007). However, evidence suggests that financial development remains a challenge for many countries worldwide, especially in developing economies, where poverty and income inequality are pervasive. One of the primary reasons behind this is that many people are underserved by the formal financial sector. According to the 2017 Global Findex database, 69% of adults worldwide have an account in a formal financial institution, which has increased drastically from 51% in 2011. Despite the substantial increase, approximately 1.7 billion people worldwide are excluded from formal financial systems, which causes two sets of concerns. First, the lack of access to formal financial services encourages people to rely on the informal or quasi-formal financial sector creating economic inefficiencies (Hasan et al., 2020; Allen et al., 2021), deepening poverty (Bruhn and Love, 2014), and posing a severe threat to combating money laundering and terrorist financing (Financial Action Task Force, 2011). Second, formal financial institutions fail to benefit from economies of scale and reduced information asymmetry. Most importantly, financial institutions fail to pool and diversify risk that can be achieved easily by serving a wide range of clients (Allen et al., 2016). Therefore, it is important to understand the drivers of financial inclusion.

After the global financial crisis in 2008, researchers have criticized the inability of financial intermediaries to diversify risk across sectors (Klapper et al., 2013). They emphasize the necessity of financial inclusion that helps in diversifying risk and contributes to a country's sustainable financial development. Similarly, recent studies highlight the importance of government interventions in achieving a stable financial system (Allen et al., 2016; Chiu and Lee, 2019). Pagano and Volpin (2001) identify two channels through which governments can influence economic agents, especially the banking sector: policy formulation and direct intervention. Recent literature reiterated that the government controls the economic environment through policies, regulations, and taxes. Economic agents endorse these changes if the actions are aligned with their political preferences (Francis et al., 2016). In many cases, economic agents develop their strategies to benefit from this environment, even if it disrupts the agents' regularly planned activities, by managing their relationship with the government in power (Li et al., 2020). This study explores a political economy perspective and explains cross-country variation in financial inclusion. Specifically, we examine whether the government's political orientation, classified as left-wing and right-wing, affects household-level financial inclusion in a country. This question is nontrivial because it studies which political direction is more effective in augmenting financial inclusion.

Political economists locate politicians and political parties based on preferences on the magnitude of state control of the economy (Botero et al., 2004). In particular, leftist politicians prefer greater state control of the economy than right-wing politicians. Parties with different ideologies design different policy directives because of the distinctive redistributive impact on the economy. Left-wing parties are considered egalitarians who prioritize income redistribution (Hibbs, 1977; Alesina et al., 1997). They also increase government expenditure to channel spending to promote economic welfare and reduce unemployment. On the other hand, right-wing parties encourage a free-market economy with occasional intervention if required (White, 2013), emphasize price stability, and rely on fiscal spending cuts. By establishing their ideological position, left- and right-wing parties signal a commitment by undertaking policies favored by their constituencies. Traditionally, left-wing parties' core constituency consists of underprivileged groups in society, whereas the elite classes of society and the financial community are the main constituents of right-wing parties (Alesina et al., 1997; Dutt and Mitra, 2005). Therefore, the political economy literature uses the terms pro-labor and left-wing and the terms pro-capitalist and right-wing interchangeably.

Partisan theory (Hibbs, 1977; 1987) suggests that policymakers respond to electoral incentives as self-interested agents. Therefore, left- and right-wing governments pursue policies following the preferences of their median voters. Since the poor and underprivileged people benefit more from financial inclusion, the partisan theory implies that left-wing governments are more likely to promote financial inclusion. Despite such delegated roles of partisans according to their ideological standing, anecdotal evidence regarding which political party is more likely to promote financial inclusion is unclear. In India, for example, both left-wing and right-wing governments have played important roles in promoting financial inclusion, particularly among low-income households. The left-wing parties Indian National Congress and Janata Dal operated the world’s largest state-led bank branch expansion program throughout the 1970s and 1980s. As part of this program, 30,000 bank branches opened in unbanked rural locations in India (Burgess & Pande, 2005). However, also the right-wing Bharatiya Janata Party government launched an ambitious project in August 2014 to link every Indian household with the banking system through a digital agent banking network. As of April 2020, 380 million bank accounts have been opened as part of this program.[1] Both types of parties intend to remove demand and supply barriers to access financial services. Demand-side barriers restrict an individual's capacity to access available financial products. For example, a lack of education or income could deter an individual from demanding particular financial services (Allen et al., 2016). Supply-side barriers can emerge from the lack of infrastructure development or the reluctance to offer services to specific segments of society. For example, inadequate profit prospects can discourage financial institutions from opening branches in rural areas (Brown et al., 2015). Right-wing parties spend more heavily on education and infrastructure development (Herwartz and Theilen, 2017). In contrast, left-wing parties attempt to reduce unemployment (Hibbs, 1987) and encourage banks to open branches in rural areas (Burgess and Pande, 2005). All of these factors contribute positively to financial inclusion.[2] Therefore, the impact of governments' ideological leaning on financial inclusion is an empirical question.

We explore the link between government ideology and financial inclusion using multiple data sources. We first collect data on government ideology from the Database on Political Institutions (DPI) compiled by the World Bank. In particular, we collect information on the political ideology of the major party (i.e., the party with the highest vote share) in government and that of its chief executive. Data on financial inclusion is collected from the World Bank's Global Financial Index, or the Global Findex database, the most granular financial inclusion database available to date. Although financial institutions provide various services, we focus on account ownership as the primary measure of financial inclusion for the following reasons.

1 See https://pmjdy.gov.in/account.

2 For details, see Allen et al. (2016).

First, as Allen et al. (2016) pointed out, account ownership is more comparable among individuals and across countries. In contrast, many other services, such as credit and savings, are not comparable because these instruments vary in maturity and interest rate. Second, account ownership works as an entry key to the formal financial sector. After having an account, people can use various services offered by financial institutions. It is important to note that many people in developing countries do not have access to this essential service, let alone other sophisticated financial services such as saving, borrowing, and debit and credit card transactions.

We challenge well-established partisan theory and find robust evidence that countries under right-wing parties are more likely to observe higher levels of account ownership than countries with a left-wing government. While having an either right-wing or left-wing ideology is more conducive to financial inclusion than not having an ideology, our focus is on which ideology is more conducive. Our results are in favor of the right-wing parties. While financial inclusion is also likely to increase under a left-wing government, it is less pronounced than the right-wing parties and not robust. Our estimates show that the account ownership level in countries with a rightist government is 6.5% higher than in countries under a leftist regime. These results are reinforced by a regression-discontinuity design, where party control changes at 50% of the electoral seat share in parliamentary elections and right-wing candidate winning margin for presidential elections. Moreover, to examine whether right-wing parties merely capitalize on the groundwork laid by left-wing parties to augment financial inclusion, we test our results using the data of the last five and even ten years, considering the ideology-dominated political party in power for the most years during these times. We find the same results in favor of right-wing parties.

Our findings depart from the stylized observations presented in early work on political economy, that blue-collar working people make up the core constituency of left-wing parties (e.g., Hibbs, 1977). Instead, as recent evidence from India suggests, right-wing parties have received support from low- and middle-income groups in many countries, as reflected in their policymaking.

What are the potential channels through which a right-wing government affects financial inclusion, and why do governments pursue these policies? First, rightist policies are mostly comprised of non-social spending, such as education and infrastructure development. For example, the right-wing Social Democratic Party in Albania emphasized the importance of education and made nine years of schooling free and compulsory (International Monetary Fund, 2003).[3] Education improves the ability to make sound personal financial decisions (Klapper et al., 2013). Besides, right-wing parties' pro-innovation policies are likely to improve financial technology. Consistent with this channel of enhanced inclusion, we find that mobile banking has substantially increased financial inclusion in recent years, and this effect is most substantial under right-wing regimes. Right-wing economic policy initiatives aim to increase spending after enhanced financial inclusion. They aim to increase the frequency of account use and decrease savings, and thereby stimulate the economy. We find additional results consistent with this motivation since right-wing governments are associated with increased account usage

3 It is important to note that according to Global Findex, financial inclusion increased by ten percent point in Albania during the right-wing Social Democratic Party of Albania's regime and by only two percent point during the left-wing Socialist party of Albania's regime.

and spending savings. Furthermore, rightist and leftist parties use varying levels of economic intervention to achieve their distinct economic goals. Right-wing parties, associated with less interventionist policies, advocate trade openness, while left-wing parties favor protectionism (Milner and Judkins, 2004). Our results suggest that rightist parties increase financial inclusion by intervening less in the domestic market and improving the judicial system.

We employ several robustness tests of our results. We divide the robustness analyses into four categories, i.e., political system heterogeneity, sampling, economic environment, and econometric assumptions. Under political system heterogeneity, we consider (i) the electoral system, in particular plurality voting versus proportional representation; (ii) having a finite term in office; (iii) the duration of the party in power; (iv) the type of government (single party vs. coalition); and (v) we limit the ideological orientation of the government to at most three major parties. We also alter the sample, where we exclude populist governments and consider right and left parties with left parties as a reference group. In testing for robustness to the economic environment, we consider the role of Global Financial Crisis of 2008, bank competition, and the poorest 40% of the households. Finally, we vary our econometric assumptions by clustering the standard error at the country level, changing the bootstrapping, adjusting sampling weights, and considering regime changes. We find robust evidence that right-wing policies are more likely to promote financial inclusion.

Our study makes the following contributions to the literature. First, we are the first to examine the effect of government ideology on households' access to financial services. We provide evidence that right-wing parties are more likely to promote household-level financial inclusion in a country, thus extending the financial inclusion and political ideology literature (Alesina, 1987; Allen et al., 2016; Müller et al., 2016). Prior studies on political ideology primarily focus on the ideological distinction of political parties in formulating the fiscal policies, their redistributional concerns, and their effect on economic growth (Bjørnskov, 2008). We focus on the entry point, the individual's access to the financial system rather than the macroeconomic effect of ideological difference. Furthermore, recent literature on financial inclusion studies the socio-economic characteristics, bank branch proximity, or ATM proximity as determinants of financial inclusion (Brown et al., 2015; Horvath et al., 2017; Allen et al., 2021; Lu et al., 2021), while we focus on the political side.

Second, we attempt to identify the channels through which political ideology affects financial inclusion. We study a range of indicators, from individual savings and withdrawal behavior due to ideology-driven policy changes to the use of mobile banking. We find that these indicators serve as channels through which governments achieve their ideology-driven political economy goals.

Third, we extend the literature by studying individual-level data on financial inclusion, using a survey database that offers the most granular data on global financial inclusion to date. Existing studies on financial inclusion either use only country-level data (Beck et al., 2007) or create an index for financial inclusion (Morgan and Pontines, 2014).[4] It is thus challenging to disaggregate financial service users by income, education, or other characteristics.

Finally, our study contributes more broadly to the politics and finance literature (Myers, 1977; Pagano and Volpin, 2001). Prior studies have emphasized that politics significantly affect

4 Except for Allen et al. (2016), who use the Global Findex database.

public and corporate policy formulation and decision-making. Our study provides microeconomic evidence on the politics-finance relationship.

The remainder of the paper proceeds as follows: Section 2 provides an overview of the data. Section 3 discusses the methodology. Section 4 discusses the main results. Section 5 documents the potential channel. Section 6 presents the results of additional tests. Section 7 documents the country-level analysis, and Section 8 concludes the paper.

1.2. Data

We use data from several sources to investigate the relationship between political ideology and financial inclusion. We start our analysis with account ownership. In line with the literature, account ownership is measured by using the following survey question: "*An account can be used to save money, to make or receive payments, or to receive wages or financial help. Do you, either by yourself or together with someone else, currently have an account at a bank or another type of formal financial institution?"* This indicator is used as the primary measure of financial inclusion (Demirgüç-Kunt et al., 2015) .The financial inclusion data are collected from the World Bank's Global Financial Inclusion (Global Findex) database. The Findex data are drawn from 2011, 2014, and 2017 surveys carried out by the Gallup World Poll and represent more than 140 countries. The survey participants are randomly selected individuals at least 15 years of age. The data on political ideology are collected from the Database of Political Institutions (DPI).[5] DPI identifies party orientation for economic policies and defines

5 See Beck et al. (2001) for details.

a party as leftist if its name includes the term *communist, socialist, social democratic,* or *left-wing* in cross-checked sources and rightist if the party name includes *conservative, Christian-democratic,* or *right-wing* in cross-checked sources.[6] We use the ideology of the chief executive for a presidential political system or if an assembly elects the president, and the ideology of the largest government party if the political system is parliamentary, where the ideology of the chief executive is coded as zero (unelected) for the presidential system and replaced with the executive's political party, if available. The variable right-wing takes the value of one if the country is right-wing and zero otherwise. Similarly, left-wing takes the value of one if the country is left-wing and zero otherwise. The reference group represents the countries where the ideology of the government does not fall in either of the right or left-wing categories.

Figure 1(a) portrays a wide variation in account ownership across countries clustered by income level, high-income, upper-middle-income, middle-income, lower-middle-income, and low-income countries. 93% of adults living in high-income countries have an account in a formal financial institution, decreasing monotonically across subsequent clusters. We exclude high-income economies from the analysis since account ownership is almost universal in these countries with a gross national income (GNI) per capita of USD 12,056 or more.[7] The argument is that financial inclusion will not be a policy priority in these countries, irrespective of the government's ideology. Figure 1(b) shows the percent of financial inclusion across countries

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[6] DPI uses a rigorous process to identify party orientation. If the party name does not suggest its orientation immediately, it consults several other websites, Political Handbook or any other sources that specifically provides party orientation.

7 See also Demirgüç-Kunt et al. (2018).

by political ideology over the survey waves. The figure depicts that financial inclusion is higher in the right-wing countries than in left-wings across all survey waves.

[Insert Figure 1 here]

We apply several selection criteria to construct our sample. First, we exclude individuals missing demographic information such as education or income. Second, we only consider countries that have data on political ideology available. The final sample consists of 193,284 observations from 65 countries.[8] Additionally, we include other individual, macroeconomic, institutional, and regulatory variables that could affect financial inclusion. Individual-level data are collected from the Global Findex database. Data on macroeconomic and infrastructure-related variables are collected from the World Development Indicators (WDI). Variables related to institutions and politics are collected from World Governance Indicators (WGI). Table 1 provides the list of countries included in our sample, and Appendix B shows the definitions and sources of all the indicators used in this study.

[Insert Table 1 here]

Table 2 presents the summary statistics. It shows that, on average, 39% of adults in the sample have an account, 33% of adults use a formal account to save, and 23% frequently use an account when all income groups are considered. Moreover, average account ownership is lowest for the poorest 20% of the population, increasing monotonically with income levels.

8 Appendix A documents the sample selection.

[Insert Table 2 here]

Table 3 presents the mean values of the access to and use of respondents' accounts under left- and right-wing regimes.

[Insert Table 3 here]

A *t*-test shows that the respondents of a country under the rightist regime are likely to have higher account ownership, higher frequency of account use, and a higher number of mobile banking accounts across all income levels. Savings, however, is higher in left-wing countries.

1.3. Methodology

We conduct two sets of analyses. The first set uses the individual-level data, and the second set country-level data.

Account ownership in a financial institution depends on the individual's characteristics, such as the level of education and the economic or political characteristics of the country in which the individual lives. The relation between financial inclusion and political ideology thus spans multiple levels. Measures of financial inclusion are individual-level variables, whereas the ideology of the political party in power is a country-level characteristic in a particular year that does not vary across individuals. Thus, our data has a two-level hierarchical or multilevel structure, where the first-level (micro-level) variables are nested within the second-level (macro-level) variables. Therefore, we combine respondent-level (micro-level/first-level) and country-level (macro-level/second-level) information in our analysis. Failure to recognize the multilevel nature of the data would violate an important assumption of the Gaussian model, the assumption of the independence of the residuals (Hox, 2017). Individual-level observations are interdependent; the respondents of one country are likely to be more similar than those in other countries, resulting in underestimating the standard errors associated with the second-level variables.

We use a two-stage multilevel logistic regression model specifically equipped for modeling a hierarchical data structure (Hox, 2017). Since the dependent variables are binary, we use the following multilevel logit model, following Solt (2008) and Fairbrother (2014):

$$logit(FI_{i,t,c}) = \gamma_{000} + \gamma_1 R_{t-1,c} + \gamma_2 X_{i,t,c} + \gamma_3 Z_{t-1,c} + \delta_t + u_{0,t,c} + e_{i,t,c} \qquad (1)$$

where, i indexes individual respondents, c indexes countries and t indexes time, R is the primary explanatory variable, X is the vector of individual-level controls, Z is the vector of country-level controls, δ_t is the time fixed effects, and $u_{0,t,c}$ and $e_{i,t,c}$ are country- and individual-specific error terms, respectively. Thus, the multilevel model allows us to disentangle within- and between-cluster effects by considering clusters at both respondent- and country-level.

Additionally, the residual intraclass correlation is analyzed. In the multilevel model, the intraclass correlation coefficient (ICC) is used to analyze the degree of homogeneity in the outcome variable within the group with the following equation:

$$Variance\ Partition\ Coefficient = \frac{\sigma_u^2}{\sigma_u^2 + \sigma_e^2} \qquad (2)$$

where the numerator indicates the random intercept variance (second-level variance component or between-group variance) and the denominator indicates total variance (between- and within-group variance).

1.4. Results

1.4.1. Does Political Ideology Affect Account Ownership?

We start with results that can be interpreted as correlations between variables and not a causal relation per se because of the cross-sectional nature of the data. In subsequent analyses, we will introduce identification techniques to allow for causal inferences. We report the coefficients from the multilevel logistic regressions in the regression tables. To discuss the economic significance of the results, we use the margin effects that describe the likelihood of changes in the regressand due to a change in the regressor, holding the other variables constant.

We start our analysis by including countries irrespective of their government's democratic or autocratic characteristics. Table 4 documents the baseline results of the relation between political ideology and account ownership.

[Insert Table 4 here]

First, in column (1), we regress account ownership on government's political ideology without any control variables. The results show that the likelihood of owning an account is significantly higher when a right-wing party is in power. Specifically, the likelihood of account ownership is 6.5% higher under the right-wing regime than under any other government. In Table 4, the results for individual-level variables are in column (2), individual and macroeconomic variables in column (3), and individual, macroeconomic, political, and institutional variables in column (4), respectively. Even after controlling for these individual- and country-level variables, the account ownership is likely to be 6% higher under a right-wing regime. Individual-level control variables indicate that the probability of account ownership is higher among males, more affluent, older, and more educated individuals. Country-level control variables show that the higher levels of the gross domestic product (GDP) per capita, manufacturing value-added, control of corruption augment account ownership. In specifications (1) to (4), the log-likelihood levels decline, suggesting that each subsequent specification is a better fit than the previous one. The ICC in column (1) of Table 4 indicates that between-country differences explain 20.5% of the likelihood of owning an account, and the remaining 79.5% is explained by within-country (individual-level) differences. The variation remains consistent even after adding individual- and country-level characteristics in columns (2) to (4).

We include country fixed effects in column (5) to account for the country heterogeneity in the error term, i.e., the possibility that all the observations of a country exhibit the same error. However, including country fixed effects eliminates all the cross-national differences. Therefore, ICC is close to zero when country fixed effects are included.

1.4.2. Does this Relation Hold When We Consider Only Democratic Countries?

Authoritarian governments can have very different policy agendas relative to electoral democracies. Our sample includes authoritarian countries, which can induce noise in our estimations. Therefore, we exclude these governments from our sample to assess whether they affect our results meaningfully. We use data from Freedom House that classifies countries as free, partly free, or not free. We define a government as authoritarian if the country is classified as not free. Sixteen countries in our sample fall in this category and we exclude these countries from our sample. The results are reported in column (6) and (7) of Table 4, without and with country fixed effects. The estimates confirm that account ownership increases under a right-wing government, irrespective of the nature of the government.

1.4.3. Is the Politics–Inclusion Relation Causal?

Estimating causal effects of political ideology on financial inclusion is challenging due to potential identification problems. Because incumbent parties are not selected randomly, omitted variables and causality biases may influence our results. For instance, unobserved voter preferences could affect the selection of the ruling parties, which will induce an omitted-variable problem. Moreover, a correlation between the party ideology and a policy outcome does not necessarily suggest causation. For example, countries with a certain level of financial inclusion might prefer parties with particular ideologies, which will induce a reverse-causality problem in our analysis.

It is difficult to account for all variables that might affect an electorate's voting preferences, and therefore we cannot eradicate the omitted-variables problem. We mitigate this problem using the methodology proposed by Altonji et al. (2005). This approach compares coefficient estimates without controls with coefficients with elaborate controls and gauges the importance of omitted variables.[9] Specifically, this indicates what the magnitude of the influence of unobserved factors has to be, relative to the influence of observed factors, in order to nullify

9 This measure is calculated as $\frac{\widehat{\beta_F}}{\widehat{\beta_R}-\widehat{\beta_F}}$, where $\widehat{\beta_F}$ is obtained after including all the observables and $\widehat{\beta_R}$ is obtained only after considering the main variable of interest.

the statistical impact of the variable of interest. The estimation is less affected by the observation selection if the difference between the coefficients with and without controls is small.

In our baseline results in Table 4, we focus on a comparison between column (3), where a large number of controls are included, and column (1), where only the ideology variable is included. The value of the ratio is approximately 19, meaning that the omitted variable has to be 12 times greater than the observed variables. This makes it extremely unlikely that the inclusion of additional variables will explain the influence of political ideology on financial inclusion in the form of account ownership. Similar results hold for all other specifications considered.

Larcker and Rusticus (2010) question the suitability of using instrumental variables to address the reverse causality problem when the instruments are weak or not fully exogenous. In the absence of a good instrumental variable for political ideology, we follow Girardi (2020) and Pettersson‐Lidbom (2008) and perform a quasi-experiment using a regression discontinuity design (RDD) to deal with the (reverse) causality problem. RDD can produce "near" experimental causal estimates of the effect of party ideology on financial inclusion. The institutional features of an election system where parties with a majority of the votes can form the government provides an opportunity to implement the RDD. Following Girardi (2020), the assignment variable in the parliamentary elections is twice the percentage of seats a party gets with a treatment threshold of 50%. We estimate the treatment effect of electing a right-wing party on financial inclusion, as opposed to electing the other parties.

We can include 49 democratic countries because the threshold is the percentage of seats in an election, forcing us to exclude the autocratic countries from the RDD. We follow Pettersson-Lidbom (2008) and modify the bandwidth approach. We have a limited number of observations around the 50% threshold and use the control function approach to include all available data because this is the most efficient method in our context. The government party and the opposition cannot share the same ideology for the RDD estimates to be efficient and unbiased. So, we exclude three countries where the winning party and the opposition share the same ideology. We use the data of 46 countries in the RDD.

We use the DPI data with the percentage of seats the winning party receives to estimate the multilevel logistic regression model of the following form

$$logit(FI_{i,t,c}) = \gamma_{000} + \pi_1 treat_{t-1,c} + f(right-wing\ share)\varphi + \gamma_2 X_{i,t,c} + \gamma_3 Z_{t-1,c} + \delta_t + u_{0,t,c} + e_{i,t,c} \quad (3)$$

Equation (3) is similar to Equation (1) except $treat$, which is a dummy variable that takes the value of 1 if a right-wing party wins the majority of seats and 0 otherwise. The coefficient π reflects the party effect and is the parameter of interest, $f(right-wing\ share)$ is the control function or any low-order polynomial that denotes the percentage of seats won by the right-wing party. The results are reported in column (1) and (2) of Table 5. We use the first-order polynomial in column (1) for the control function and add control variables. In column (2), we add country fixed effects.

[Insert Table 5 here]

Estimates of Table 5 reinforce our baseline results, which implies that financial inclusion and the right-wing government have a positive association. Specifically, the results show a discontinuous jump at the threshold. The positive and significant coefficient of the treatment variable suggests a strong right-wing party effect on financial inclusion. The estimates change little when we add several control variables, providing further assurance of the validity of our baseline regression. We present a graphical visualization of our RDD in Figure 2(a) that depicts the marginal effect of the treatment and control group on financial inclusion.

[Insert Figure 2 here]

The right-side of the cut-off shows the right-wing treatment effect, and the left-side shows the control effect. The solid line in the graph shows an upward right-wing effect just after the threshold. The vertical distance between two parallel lines is measured in marginal terms and is 5.7%, implying a 5.7% jump in financial inclusion at the right-hand side of the cut-off.

For presidential elections, we follow Girardi (2020) and exclude elections in which the president is not elected by popular vote, presidential elections in purely parliamentary systems, or parliamentary elections held in the same month of a presidential election under a presidential system. We define right margin as the difference between the vote share of the first right candidate and the share of the first non-right candidate. We utilize the dataset assembled by Girardi (2020) to construct the right margin variable and complement this data by hand collecting additional information from publicly available sources for our sample. This exercise yields 76,975 observations for 39 countries. The estimates are reported in column (3) and (4) of Table 5 and are visually depicted in Figure 2(b). The results show a discontinuous jump of about 2% after a ring-wing president is elected. However, the effects are less strong than the elected right-wing government.

1.4.4. Are Parties Capitalizing on Their Predecessors?

To examine whether the results postulate that rightist parties are only capitalizing on the conducive groundwork laid by leftists predecessors, we use the last five and ten years of data to find which ideological party was in power most of the time, on average, during these periods. The variables are coded as one if a rightist party was in power most of the time and zero otherwise. Table 6 documents the results. In columns (1) to (3), we include the five-year averages and in columns (4) to (6) we include the ten-year averages of the respective country-level control variables.

[Insert Table 6 here]

Table 6 shows that the baseline results are not a mere manifestation of rightists using a leftist foundation to augment financial inclusion. These findings support our baseline results and demonstrate that account ownership is higher under a right-wing regime than a left-wing one, even when longer horizons of five and ten years are considered. The negative sign of manufacturing in column (2), seemingly counterintuitive at first glance, captures the fact that, because of the global financial crisis, access to banks declined substantially (Han and Melecky, 2013). In addition, when employment increased through economic recovery, account ownership increased at a much lower rate (Ardic et al., 2013). However, when the last ten years are considered, the relationship is positive and significant. Interestingly, the ICC is larger in column (5) for the ten-year average than in column (2) for the five-year average. This result means that country-level variance has declined over the past few years.

1.5. Understanding the Politics–Inclusion Relation

What are the potential channels through which a right-wing government affects financial inclusion? Given the widespread and multidimensional impact of political ideology, it is not easy to pin down the channels. In this section, we discuss four potential channels through which government ideology influences the demand and supply factors affecting financial inclusion; public spending policy, use of mobile banking, policies for the use of accounts, and degree of intervention in the economy.

1.5.1. Public Spending Policies

It is commonly known that right-wing government public spending comprises mainly of non-social spendings, such as education and infrastructure development (Herwartz and Theilen, 2014). These two factors contribute significantly to financial inclusion (Allen et al., 2016). In all regression tables, we already control for education across two levels; secondary and tertiary. We use the GDP per capita as a proxy for economic development and manufacturing value-added as a proxy for infrastructure development. Consistent with prior literature, we find that account ownership is higher among the more educated population. Additionally, the more developed a country's economy and infrastructure, the higher the account ownership is likely to be.[10]

10 Beck et al. (2016) finds a positive net effect of financial innovation on economic growth.

1.5.2. Use of Mobile Bank Accounts

The advent of digital finance, especially mobile banking, has primarily influenced individuals' access to the formal financial sector. It presents as a promising vehicle to include the unbanked and underbanked population in the mainstream economy. The success of mobile money requires innovative measures to build the necessary governance and institutions, which in turn, rely on government policies (Suri, 2017). Wang et al. (2019) investigate the impact of government ideology on the overall technical innovativeness of 110 countries from 1995 to 2015. They argue that leftist parties undertake expansionary monetary and fiscal policies in order to decrease unemployment. Consequently, they do not promote technical innovation because technical innovation increases automation. Left-wing parties are, therefore, likely to deter financial innovation. On the contrary, rightist parties, as advocates of a free-market economy, promote engagement in research and development and stimulate the progress of new technology. Evidence suggests that the adoption of new technology, such as mobile banking, has played a critical role in increasing financial inclusion (Suri and Jack, 2016). Still, mobile banking adoption is also an outcome of individual choices, and thus we present this as indirect evidence of innovation policies.

Two examples of effective government intervention have drawn attention in the literature. In 2010, the central bank of Kenya implemented agent banking regulations, which allowed banks and other financial institutions, previously limited to brick-and-mortar operations, to directly compete with the country's largest mobile money provider, M-PESA.[11] The central bank of

11 It could be argued that digital innovation such as the development of M-Pesa are initiated by technocrats rather than the politicians. However, literature suggests, specialty in the context of developing countries, that government plays a crucial role in sustaining these innovations. For example, M-Pesa was used a vehicle to strategically use state resources to earn loyalty of the general population in Kenya and the profits were transferred to the political elites (Tyce, 2020).

India issued licenses to several entities in 2014 to function as payments banks. The objective was to boost financial inclusion by enhancing mobile services in banking. Unlike regular banks, these new financial institutions are not allowed to extend credit, but they can take deposits, pay interest, facilitate transfer and remittances, and offer Forex services. In both cases, policy interventions have been taken by right-wing political parties-the Party of National Unity in Kenya and BJP in India. It is also worth mentioning that, this advancement in technological innovation has emerged as the primary productive force augmenting economic development (Lee and Deng, 2018). According to Wang et al. (2019), since leftist parties undertake expansionary monetary and fiscal policies in an attempt to decrease unemployment, they do not promote technical innovation since technical innovation increases automation. Left-wing parties are, therefore, likely to deter financial innovation. On the contrary, rightist parties, as advocates of a free-market economy, promote engagement in research and development and stimulate the progress of new technology. If this argument is correct, we should observe more mobile bank account ownership during a right-wing regime.

The Findex data report whether respondents had a mobile banking account.[12] The variable takes the value of one if the respondent reports having a mobile banking account and zero otherwise. The results are reported in Table 7.

[Insert Table 7 here]

12 In the 2014 and 2017 waves, Findex reports whether the respondent *"has a mobile money account."* In 2011, the question was as follows: *"In the past 12 months, have you used a mobile phone to pay bills, send money or receive money?"* For 2011, we used this question to measure mobile banking account ownership. Our results are similar, even after excluding 2011.

Our analysis supports the findings of Wang et al. (2019). Indeed, the likelihood of mobile bank account ownership increases significantly under a rightist party, which is indirect evidence of the effects of innovation policies. The results of columns (1) to column (4) in Table 7 suggest that mobile bank account ownership is less popular among older and female individuals but rises significantly with education, income, employment, domestic credit, regulatory quality, and freedom of expression measured by voice and accountability. One interesting finding is that mobile bank account ownership declines when the government becomes more effective or has more political stability. This result indicates that people prefer formal banking to mobile banking when there is political stability. In addition, as column (3) shows, between-country differences explain 68.4% of the likelihood of owning a mobile banking account.

1.5.3. Policies for the Use of Accounts

To gain a deeper understanding of the effect of political ideology, we next analyze its impact on the use of an account, that is, savings and the frequency of account use. Recent studies have emphasized that countries, where people save more and spend less, can experience secular stagnation and experience lackluster financial performance (Eggertsson et al., 2016). Moreover, the interventionist strategy of a left-wing government consists of raising the level of domestic savings (Boix, 1997). In contrast, rightist parties stimulate the economy by encouraging private consumption through tax cuts (Müller et al., 2016). Therefore, if the ideology–inclusion relation is driven by market-oriented considerations, we should observe a negative association between rightist ideology and savings and a positive relationship between rightist ideology and frequency of account use. On the contrary, the relationship should be the opposite under a government that pursues socialistic considerations.

Information on household savings is measured by using the following question" *"In the past 12 months, have you, personally, saved or set aside any money by using an account at a bank or another type of formal financial institution?"* Although a financial institution offers various services, having an account for savings is crucial since it indicates an individual's willingness to save in a formal financial institution. The variable for savings takes the value of one if the respondent reports having saved in a formal financial institution and zero otherwise.

For the frequency of account use, following Allen et al. (2016), we focus on the number of monthly withdrawals from the account rather than deposits. Deposits can be initiated by others (e.g., salary or gifts), whereas the account holder actively initiates withdrawals. The survey question is the following: *"In a typical month, about how many times is money taken out of your account(s)? This includes cash withdrawals, electronic payments or purchases, checks, or any other time money is removed from your account(s) by yourself or others."* The participants were asked if they made zero, one or two, three to five, or six or more withdrawals.[13] Withdrawing funds only once or twice could be an indication of the withdrawal of a salary. Individuals who withdraw three or more times are more likely to use cards or electronic payments. Therefore, following Allen et al. (2016), frequent account use is defined as making three or more withdrawals a month. Specifically, the frequency variable takes the value of one if the respondent makes three or more withdrawals a month and zero otherwise.

Table 8 documents the results, where we find that the results are in line with capitalistic considerations.

13 This question is not available for 2017 wave. The analysis of frequency of use is, thus, limited to 2011 and 2014.

[Insert Table 8 here]

Columns (1) to (3) of Table 8 show that savings are negatively correlated with the rightist ideology. Along with the findings of Boix (1997), these results for savings support the anecdotal evidence that, over the past decades, leftist parties in many developing countries, such as Bolivia, Ecuador, Nicaragua, Peru, and Brazil, have been using the existing banking channel and microfinance institutions to mobilize savings (Bédécarrats et al., 2012). Therefore, we expect savings to increase under a left-wing regime. Savings behavior depends significantly on individual ability and, as shown by the results in the table, increases monotonically as the level of education and income increases. Women are less likely to use formal financial accounts for savings purposes. The GDP per capita and manufacturing value added are negatively related to savings, consistent with the literature, while inflation is positively related.

Columns (4) to (6) of Table 8 document a positive relationship between a rightist orientation and frequency of account use. Right-wing policies to stimulate public expenditures are likely to increase account withdrawals. However, the frequency of use is complex and depends mainly on individual characteristics. As our results show, a more educated and wealthier economic group is more likely to use accounts frequently, but this usage declines significantly with age. Moreover, male account holders are more likely than female account holders to use the account frequently for the purpose of withdrawal. In contrast, when government efficiency declines, the frequency of account withdrawals increases significantly.

1.5.4. Degree of Economic Intervention

Rightist and leftist parties use varying levels of economic intervention to achieve their distinct economic goals. As discussed above, left-wing parties increase the level of economic intervention while rightist governments reduce intervention and amplify the disciplining effects of market mechanisms. Furthermore, right-wing parties, associated with less interventionist policies, advocate trade openness while left-wing parties favor protectionism (Milner and Judkins, 2004). In this section, we investigate the effect of these policies on financial inclusion.

We use two variables, regulatory requirements for starting a business, and trading across borders, to test the level of intervention in the economy and quality of the judicial processes to test the disciplining effect of the market mechanism. Regulatory requirements for starting a business measures the number or procedures, capital requirement, time, and cost required for firms to start and operate a business. Trading across borders measures the degree of trade openness. Quality of judicial process indicates the degree of efficiency of the judicial system in enforcing contracts and protecting property rights. We collect these data from the Doing Business indicators of the World Bank. All these variables range between 0 to 100. A lower value indicates the least business-friendly regulation. We conduct subsample analysis by dividing each indicator into two groups and assign a dummy variable one if the score of the indicator variable for an economy is above median and zero otherwise. Results are presented in Table 9.

[Insert Table 9 here]

The results of starting a business in column (1) and (2) show that more intervention has little association with financial inclusion while less intervention is likely to increase financial inclusion, and the results are more pronounced for the rightist parties. We observe similar results for a better judicial process. We also find evidence that the protectionist policy of the left-wing parties for international trade has a positive and significant effect on financial inclusion. Furthermore, an inefficient judicial system hinders financial inclusion and is more affected when leftist parties are in power. The results suggest that financial inclusion is likely driven by less government intervention in the domestic economy and a better judicial system.

1.6. Additional tests

We divide this section with robustness tests into four subsections based on political system heterogeneity, sampling, various economic environments, and changing econometric assumptions. We conduct various robustness tests to ensure specific design choices do not drive our results. The robustness tests are briefly reported in Table 10, while we provide the full results in the Online Appendix.

1.6.1. Heterogeneity in Political System

Political systems may play an important role in shaping public policies. This section explores whether government ideology has varying effects on financial inclusion depending on the electoral system (i.e., plurality voting versus proportional representation, having a finite term in office, and the duration of the party in power. In addition, we consider the type of government (single party vs. coalition). Furthermore, we conduct tests by limiting the ideological orientation of the government to three major parties.

Legislators are elected using a winner-take-all method under "plurality" systems (coded one if plurality, zero otherwise). In "proportional representation (PR)" candidates are elected based on the percent of votes received by their party (1 if PR, 0 otherwise). Finite term in office identifies whether there is a constitutional limit on how many years a president can serve before new elections are called (one if finite term, zero otherwise). The duration of the party in power measures the number of years the elected party been in office. We only include the democratic countries in this test as autocratic parties occupy office without election. We also divide the data into three groups (i) eight years or less, (ii) nine to 16 years, and (iii) more than 16 years in office to gain deeper insight of the effect party duration in office.[14] Column (1) to (4) of Panel (A) presents the results. We find that the right-wing coefficient is positive and significant, implying that a right-wing party has a positive effect on financial inclusion and as the right-wing party duration in office increases, so does the financial inclusion.

[Insert Table 10 here]

Governments can be formed by a single party, or by coalition. We run additional RDD analyses to examine whether the type of government affects our result. We report the results of RDD in column (5) and (6) in Panel (A) and visualize the analyses in Figure 3. Similar to the baseline results we observe a discontinuous jump in financial inclusion when right-wing parties win elections. The coefficient is 1.16 when a single party forms government. However, the coefficient

14 Many countries in our sample have their election held every four years. We divide the data to reflect such situations.

is 0.52 when the government is formed by coalition. This suggest that the effect is stronger for single-party governments than coalition governments.

[Insert Figure 3 here]

To win an election, some governments may form coalitions and implement policies that are diametrically opposed to their party ideology. Following Wang et al. (2019), we solve this potential problem by excluding countries with coalition governments with three or more parties over our sample period to limit the ideological representation of the government to three main coalition parties. We report the result in column (7) of Panel (A) and find a positive and significant right-wing effect, which is consistent with our original findings.

1.6.2. Altering the Sample

Many established democracies have seen populist parties rise and become institutionalized in recent decades. These "far-left" or "far-right" parties may undertake policies that depart from their common ideological position. For example, radical right parties can take policies that are both economically right-wing and socially conservative (Norris, 2020). We exclude these governments from our sample to remove the potential influences of populist parties on financial inclusion. We follow Norris (2020) and use the Global Party Survey Data 2019 from Harvard Dataverse that provides an ordinal measure of party populism, from strongly pluralist to strongly populist. We remove the strongly populist parties from our sample. We complement this data with the DPI data, remove the strongly populist parties in power from our sample, and conduct multilevel logistic regression. Results are reported in column (1) of Panel B. The results are not only similar to the baseline results reported in Table 4, but are stronger when we remove populist parties.

So far, our reference group includes all the parties that do no fall in the right and left categories. We test the robustness of this choice by considering only right and left governments, where the right party is our main variable of concern and thus the left-wing parties are the reference group. The results are reported in column (2) of Panel B. We find that the right-wing effect is positive and significant, and again our findings are stronger than the baseline results.

1.6.3. Economic Environment

We conduct three tests to assess whether our results are robust across varying economic environments. First, we exclude the 2011 wave. This year closely follows the global financial crisis, and it could have affected individuals' trust in the formal financial sector and affected their decision to own a bank account. Column (1) of Panel C reports the results. Interestingly, the left-wing effect on financial inclusion becomes negative after excluding 2011, but the right-wing coefficient is stronger.

Second, bank competition can be important, and financial inclusion is likely affected by the degree of competition among financial institutions. We use bank concentration and the Boone indicator to account for this competition.[15] Unfortunately, data for these two indicators are not available for all the countries during our sample period, and the number of observations suffers

[15] Bank concentration is the percent of asset concentration by three-largest banks in a country. Boone indicator is the elasticity of profits to marginal costs. Data of these two indicators are collected from the Global Financial Development database of the World Bank.

due to their inclusion in the regression. However, our results (not tabulated) are qualitatively similar after their inclusion.

Third, we consider the poorest 40% of the households. Prior literature argues that the poor segment of the economy is particularly deprived of access to financial services (Beck et al., 2007; Demirgüç-Kunt et al., 2018). Therefore, it could be argued that, since right-wing governments focus on the richer and middle-class population, the increase in access is driven by the increase in account ownership among the rich and middle-class population, not the poorest. Therefore, we next investigate whether the increase in account ownership under a right-oriented party results from the account ownership of the middle class and rich segments of society. To test this, we regress account ownership, savings, and the frequency of account use on ideology for the poorest 40% of households. The results are documented in Column (2) to (5) of Panel C.

Similar to the full-sample results, we find that financial inclusion for the poorest 40% of households is also likely to rise under a right-wing government. Specifically, in column (2) we regress account ownership on ideology and find that account ownership among the poorest 40% of households is likely to be 9% higher under a rightist regime, compared to a leftist regime. This magnitude is even larger for this segment than when the full sample in Table 4 is considered since there is greater scope for improvement. The individual- and country-level variables show qualitatively similar results to the baseline results in Table 4. This finding shows that access to the financial sector is more likely to rise during a rightist government across all income levels of the economy, and not only in the rich or middle-income group of the population.

1.6.4. Changing Econometric Assumptions

We use alternative econometric assumptions and test the robustness of our findings. Specifically, we cluster standard errors at the country level, use bootstrapping, adjust sampling weights, and consider regime changes.

Estimates based on aggregate and disaggregate regression when the data is nested in nature can be too liberal or too conservative. Therefore, estimation bias can run in either direction (Bliese, 2000). Although clustering standard errors provides better estimates than non-clustering, multilevel models for the clustering of the data is an even better approach than correcting the standard errors of the linear estimates (Cheah, 2009). Nonetheless, we check the robustness of our results by simply regressing account ownership on the right-wing variable and the controls, and cluster standard errors at the country level. The results are reported in Column (1) of Panel D. The coefficient is significant at 10% level, which provides further assurance that are original results are robust.

Our second-level variables are at the country level and we thus have relatively small numbers of cases, when compared to individual-level data. We apply bootstrapping to assess the robustness of the results of our multilevel model. The bootstrapping method resamples the existing data set many times and provides a simulated data set. We report the regression results of 100 and 500 bootstrapping replications in columns (2) and (3), respectively, of Panel D. We find that the results from the bootstrapping method are very close to our baseline estimates, providing further evidence that our baseline results are robust.

Sampling weights are used in surveys to ensure that the respondents are representative of the population. In Global Findex, primary sampling units are stratified by population size, geography, or both and clustered through one or more stages. Random route procedures are then used to select households to be surveyed. Therefore, by design and survey methodology, Global Findex data is representative of the national population.[16] However, sampling and non-response errors can still exist and bias our results. We use the sampling weights to correct for these errors and report the results in column (4) of Panel (D). Again, we find that our baseline results are robust.

To further strengthen our results, we examine the effect of change in regime on financial inclusion. Since the data of financial inclusion are available for three waves, we identify the countries that experienced regime changes from one wave to another. We require that the government of a specific ideology be in power for at least two years before a wave correctly identifies the impact of that government. For example, Paraguay had a left-wing government from 2009 to 2013 and a right-wing government from 2014 to 2017. We generate a new variable, change in government, and for Paraguay, it is coded as zero (left-wing) in 2014 and as one (right-wing) in 2017 to compare the impacts of right- and left-wing policies. A similar exercise is applied to the other countries, and we identify four other countries, namely Albania, Guatemala, India, Jamaica, that experienced a similar regime change. Again, using multilevel regression, we find in column (5) of Panel D that countries under a right-wing regime are likely

16 For detailed survey design and methodology, see Demirgüç-Kunt et al. (2018).

to have significantly greater financial inclusion than countries under a left-wing regime, supporting our original findings.[17]

1.7. Country-level Analysis and Propensity-Score Matching

We run several additional tests on the country-level data to examine the robustness of the baseline results. The World Bank provides the weighted average financial inclusion data at the country level for 2011, 2014 and 2017, which allows us to use Ordinary Least Squares (OLS) regression models. Similar to prior analysis, we exclude high-income countries.18 The primary variable of interest is the right-wing ideology. We exclude the individual-level control variables from the country-level analysis. Table 11 reports the results.

We begin by using the full country-level sample of account ownership as depicted in column (1) of Table 11. The dependent variable in columns (2) and (3) is account ownership among the poorest 40% of households and account ownership in rural areas, respectively. All models include country and time fixed effects. Standard errors are clustered by country and time. The results suggest that account ownership is 7.5% higher in countries under the right-wing government. Similarly, account ownership among the poorest 40% of the household and account ownership in rural areas is also higher in countries with right-wing regimes. Coefficients in specifications (2) and (3) are larger than the coefficient in specification (1), which is expected because there are more improvement opportunities for the poorer segment

17 A difference in mean test also provides similar results.

[18] Including the high-income countries in this analysis provides similar results and assures against selection bias.

and rural areas. In specification (4) we compare between right and left regime countries keeping the only left-wing countries as the base. Again, the results are similar to the baseline results.

To further alleviate endogeneity concerns, we use propensity-score matching (PSM) on the country-level data. We use a matched sample where the country characteristics are similar, but one has a right-wing government and the other, a left-wing government. We use PSM based on all country-level macroeconomic, institutions, and regulatory variables. For each country-year observation with a right-wing government (treatment group), we match it with an observation of a left-wing country (control group) in the same year, where the propensity score is closest. Thus, these countries are likely to have a similar probability of selecting a right-wing government. We document the summary statistics of the matched sample in Panel A of Table 12. The results show that the macroeconomic institutions and regulatory characteristics between the two groups of countries are not significantly different from zero. Thus, we observe no difference between the treatment and the control group. Table 12, Panel B reports the estimates from linear regression on the matched sample. The coefficient right-wing in column (1) suggests that right-wing governments increase the probability of having a bank account by 8 percentage points. After controlling for the macroeconomic and regulatory variables, we observe a 9 percentage point increase. These estimates are similar to our baseline country-level analysis in Table 11.

1.8. Conclusion

Financial inclusion, measured as access to formal financial services, has increased substantially over the years. Despite this increase, many individuals remain outside of the financial system, especially in developing economies. In this study, we use a political economy approach to examine whether the government's political ideology affects household-level financial inclusion and, if it does, why.

Based on partisan theory, one would expect higher access to finance during a leftist regime because such a regime would focus on income distribution and would be more pro-poor (Hibbs, 1977, 1987). Using individual survey data from Global Findex, we find that account ownership in a formal financial institution is more likely to increase under right-wing regimes than their left-wing counterparts. Regression discontinuity designs also reinforce these estimates. This result is robust even when a longer horizon, such as five and ten years, of the party's office occupancy, is considered. These effects are due to the market-oriented and pro-innovation policies of rightist parties, as opposed to the societal policies of their leftist counterparts. Therefore, we conclude that capitalist policies are more conducive to access to the financial sector than socialistic policies. These results are robust to various tests. It is also important to note that individual-level characteristics also play a significant role in financial inclusion, along with the macroeconomic, political, and institutional characteristics of a country.

The results of this article raise an important question about the policies undertaken by governments in different countries. Although the interventionist strategy of leftist parties is more likely to increase savings, access to the financial sector is likely to rise during a rightist regime. However, one caveat in interpreting these results is that each country is different, and a policy that works in a particular setting can backfire in another. Besides, identifying a policy directive that achieves a net improvement in financial access is different from supposing that a keen government will implement it. It is also possible that many individuals have access to formal financial services, but they choose to voluntarily exclude themselves because they have religious or cultural reasons for not using the services or do not need them (Demirgüç-Kunt et al., 2015). The distinction between voluntary and involuntary exclusion is important for policy reasons. Individuals who are voluntarily excluded pose less of a problem for policymakers, because they reflect lack of demand. Those who are involuntarily excluded require specific policy actions, since their exclusion could be due to discriminatory policies, inadequacies in contractual and informational backgrounds, or insufficient product features. Our results only suggest that politicians have an important role to play to increase financial inclusion.

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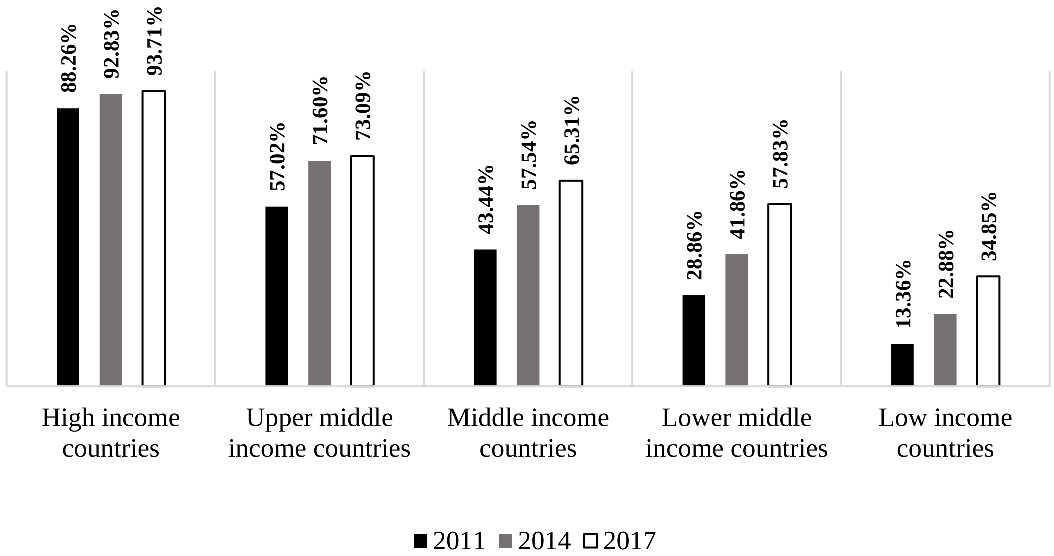
Figure 4: Differences in account ownership

Figure (a) plots differences in account ownership across countries by income level. Account ownership refers to the percentage of survey respondents who reported having an account in a formal financial institution. The income group classification is based on the World Bank Group's fiscal year from 2017 to 2018. Source: Demirgüç-Kunt et al. (2018), Global Findex Database, The World Bank. Figure (b) plots the percent of account ownership across countries by party ideology over the survey waves. Source: Global Findex Country-level Database, The World Bank.

(a) Differences in account ownership across countries by income group



(b) Differences in account ownership across countries by government party ideology

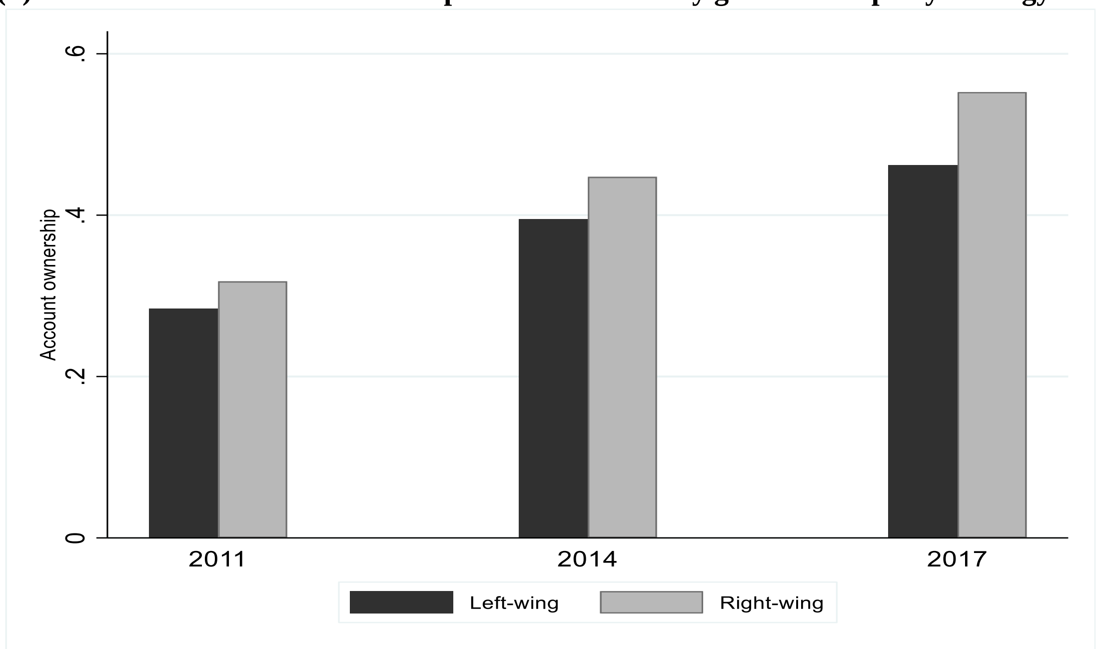
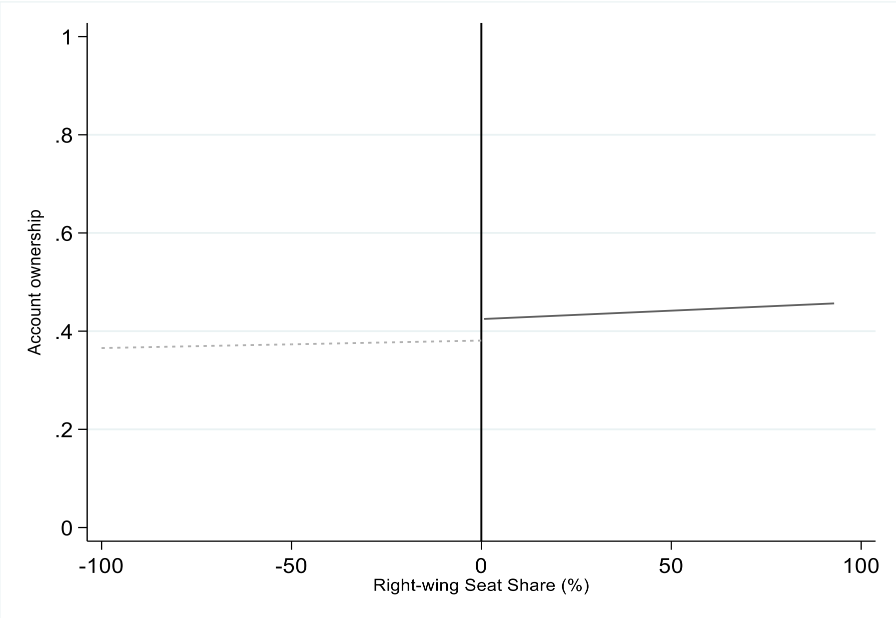


Figure 2: Regression discontinuity design based on the type of election

This figure presents a visualization of RDD. Figure (a) potrays the RDD in parliamentary elections. The horizontal axis indicates the margin of victory which is twice the difference in right-wing seat share and the majority threshold of 50%. Figure (b) shows the winning margin of the right-wing candidate in presidential elections. The vertical axis indicates the account ownership. The right-side of the threshold shows the treatment effect and the left-side depicts the control effect. The distance of the two parallel lines is portrayed in marginal terms.

(a) Right-wing seat share in parliamentary elections



(b) Right-wing margin in presidential elections

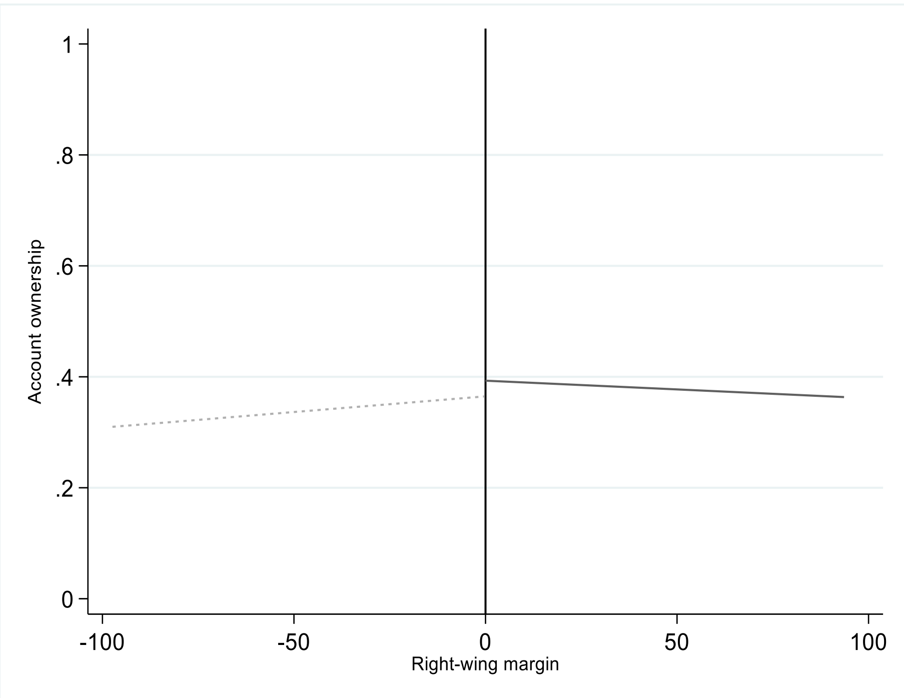
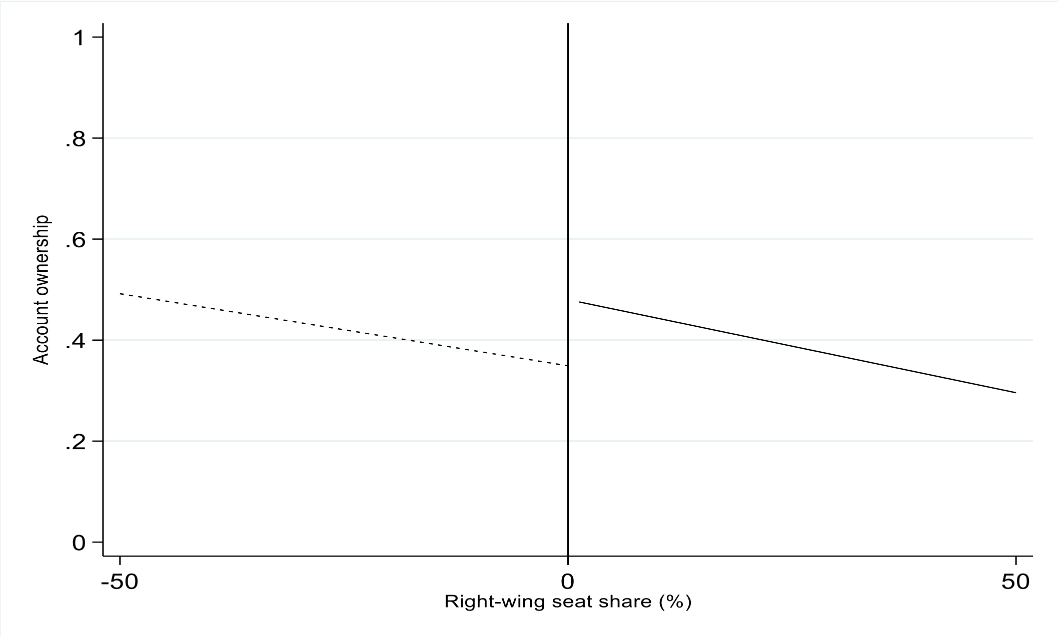


Figure 3: Regression discontinuity design based on the type of government

This figure presents a visualization of RDD based on the type of government Figure (3a) potrays the RDD when a single party forms the government. Figure (3b) shows the RDD when the government is formed by coalition. The horizontal axis indicates the margin of victory i.e. difference in right-wing seat share and the majority threshold of 50%. The vertical axis indicates the account ownership. The right-side of the threshold shows the treatment effect and the left-side depicts the control effect. The distance of the two parallel lines is portrayed in marginal terms.

(a) Right-wing seat share of single party government



(b) Right-wing seat share of coaltion government

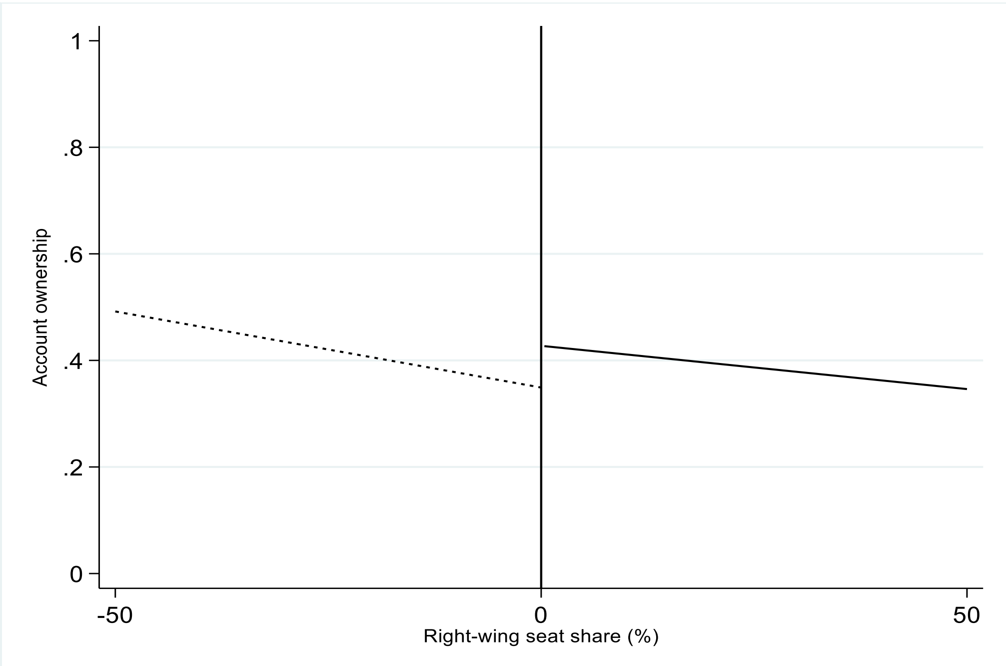


Table 1. Country list

This table presents the list of the countries in our sample. Since account ownership is almost universal in high-income economies, we exclude economies with a GNI per capita $12,056 or more. We include countries that have political ideology data available. Our final sample consists of 65 countries. All these countries are classified as developing by the World Bank.

Albania	Costa Rica	Malaysia	Rwanda
Algeria	Ecuador	Mauritania	Senegal
Angola	El Salvador	Mauritius	Sierra Leone
Armenia	Gabon	Mexico	Sri Lanka
Azerbaijan	Ghana	Moldova	Tajikistan
Bangladesh	Guatemala	Mongolia	Tanzania
Belarus	Honduras	Morocco	Thailand
Benin	India	Myanmar	Togo
Bolivia	Indonesia	Namibia	Tunisia
Botswana	Iraq	Nepal	Turkey
Brazil	Jordan	Nicaragua	Uganda
Bulgaria	Kazakhstan	Niger	Ukraine
Burkina Faso	Kenya	Nigeria	Vietnam
Cambodia	Lebanon	Pakistan	Zambia
Cameroon	Lesotho	Paraguay	
Chad	Liberia	Peru	
Colombia	Malawi	Philippines	

Table 2. Summary statistics

This table reports the individual- and country-level summary statistics. Panel A reports summary statistics for the individual respondents surveyed. Account represents the account ownership in a formal financial institution; *Savings* shows whether the respondent saved using a formal financial institution; *Frequency* is three of more withdrawals a month from an account, and *Mobile account* denotes whether the respondent has a mobile banking account. Panel B reports the summary statistics of country-level variables.

Panel A: Individual-level Variables					
Variables	**Obs.**	**Mean**	**Std. dev.**	**Min**	**Max**
Account	193,284	0.396	0.489	0	1
Savings	151,599	0.329	0.469	0	1
Frequency	35,699	0.230	0.420	0	1
Mobile account	169,112	0.108	0.310	0	1
Age	193,284	38.131	16.585	15	99
Primary education	193,284	0.460	0.498	0	1
Secondary education	193,284	0.096	0.295	0	1
Tertiary education	193,284	0.177	0.382	0	1
Income: poorest 20%	193,284	0.193	0.395	0	1
Income: second 20%	193,284	0.211	0.408	0	1
Income: Middle 20%	193,284	0.251	0.434	0	1
Income: Fourth 20%	193,284	0.541	0.498	0	1
Income: Richest 20%	193,284	38.131	16.585	0	1
Female	193,284	0.460	0.498	0	1

Panel B: Country-level variables						
Variables	**Obs.**	**Mean**	**Std. dev**	**p25**	**p50**	**p75**
Right-wing	181	0.133	0.340	0.000	0.000	0.000
Left-wing	181	0.343	0.476	0.000	0.000	1.000
Employment	181	0.044	0.206	0.000	0.000	0.000
Domestic Credit	181	59.207	12.425	50.712	59.680	68.241
Ln GDP Per Capita	181	40.365	31.484	17.584	32.605	50.558
Inflation	181	8.678	0.879	7.981	8.773	9.466
Manufacturing value added	181	5.176	4.392	2.105	4.448	7.135
Regulatory quality	181	12.620	6.492	7.659	12.581	16.121
Political Stability	181	-0.309	0.512	-0.700	-0.329	0.052
Voice and accountability	181	-0.504	0.764	-1.015	-0.402	0.008
Corruption Control	181	-0.338	0.595	-0.777	-0.255	0.036

Table 3. Government ideology and financial inclusion: Summary statistics

This table reports the results of the difference in means in account ownership, savings, frequency of account use and mobile banking in left- and right-wing countries, using a *t*-test.

	FI in countries where the major party in power is right-wing	FI in countries where the major party in power is left-wing	Difference *t*-test
Account	0.439	0.372	18.476
Savings	0.320	0.303	4.218
Frequency	0.257	0.227	4.247
Mobile account	0.101	0.086	5.973

Table 4. Baseline results

This table reports the estimates of multilevel logistic regression. The dependent variable in all the columns is account ownership or the level one variable, which refers to respondents who reported owning an account at a formal financial institution. Country-level clustering is level two variable. Column (1) regresses account ownership on party ideology Each subsequent column adds individual-level variables, macroeconomic variables, and political and institutional variables, respectively. Column (5) includes the country fixed effects. Column (6) and (7) documents results for the democratic countries only without and with country fixed effects. Standard errors are in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Variables	**(1) Account**	**(2) Account**	**(3) Account**	**(4) Account**	**(5) Account**	**(6) Account**	**(7) Account**
Right-wing	0.420***	0.428***	0.409***	0.387***	0.399***	0.374***	0.393***
	(0.037)	(0.040)	(0.041)	(0.041)	(0.042)	(0.043)	(0.043)
Left-wing	0.134***	0.036	0.093**	0.096**	0.107***	0.096**	0.116***
	(0.037)	(0.040)	(0.040)	(0.041)	(0.041)	(0.043)	(0.043)
Age		0.013***	0.013***	0.013***	0.013***	0.012***	0.012***
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Secondary education		0.906***	0.906***	0.905***	0.905***	0.871***	0.871***
		(0.013)	(0.013)	(0.013)	(0.013)	(0.015)	(0.015)
Tertiary education		2.067***	2.066***	2.066***	2.067***	2.086***	2.087***
		(0.022)	(0.022)	(0.023)	(0.023)	(0.026)	(0.026)
Income: second 20%		0.189***	0.191***	0.191***	0.191***	0.193***	0.193***
		(0.020)	(0.020)	(0.020)	(0.020)	(0.022)	(0.022)
Income: middle 20%		0.406***	0.408***	0.409***	0.409***	0.404***	0.405***
		(0.019)	(0.019)	(0.019)	(0.019)	(0.021)	(0.021)
Income: fourth 20%		0.670***	0.673***	0.674***	0.674***	0.680***	0.680***
		(0.019)	(0.019)	(0.019)	(0.019)	(0.021)	(0.021)
Income: richest 20%		1.132***	1.134***	1.136***	1.137***	1.161***	1.162***
		(0.018)	(0.018)	(0.018)	(0.018)	(0.021)	(0.021)
Female		-0.358***	-0.359***	-0.358***	-0.358***	-0.387***	-0.387***
		(0.011)	(0.011)	(0.011)	(0.011)	(0.013)	(0.013)
Employment			0.024***	0.020***	0.023***	0.021***	0.025***
			(0.005)	(0.005)	(0.005)	(0.005)	(0.006)
Domestic credit			-0.001*	-0.001	-0.001	-0.005***	-0.006***

			(0.000)	(0.000)	(0.000)	(0.001)	(0.001)
Ln GDP per capita			0.635***	0.480***	0.467***	0.791***	0.820***
			(0.093)	(0.099)	(0.142)	(0.121)	(0.159)
Inflation			-0.021***	-0.021***	-0.021***	-0.033***	-0.034***
			(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
Manufacturing			0.010*	0.009	0.008	-0.027***	-0.028***
			(0.006)	(0.006)	(0.006)	(0.008)	(0.008)
Regulatory quality				0.071	0.076	0.083	0.088
				(0.073)	(0.076)	(0.085)	(0.087)
Political stability				0.010	0.012	-0.031	-0.033
				(0.028)	(0.028)	(0.032)	(0.032)
Voice and accountability				-0.111*	-0.140**	0.289***	0.311***
				(0.066)	(0.069)	(0.079)	(0.082)
Corruption control				0.501***	0.493***	-0.035	-0.068
				(0.061)	(0.062)	(0.072)	(0.073)
Observations	193,284	193,284	193,284	193,284	193,284	149,790	149,790
Log-likelihood	-112560.73	-100982.37	-100982.78	-100894.24	-100627.42	-78419.676	-78247.59
ICC	0.205	0.216	0.216	0.189	-	0.196	-
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	No	No	No	No	Yes	No	Yes
No of countries	65	65	65	65	65	49	49

Table 5. Regression discontinuity design

This table reports the results of regression discontinuity design. Column (1) and (2) documents the results for the parliamentary elections. The assignment variable is the right-wing margin calculated as twice the difference in right-wing seat share and the majority threshold of 50% without and with country fixed effect. Column (3) and (4) reports results for presidential elections without and with country fixed effect. The assignment variable is the right-wing winning margin. The dependent variable in all the specifications is account ownership, which refers to respondents who reported owning an account at a formal financial institution. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	(1)	**(2)**	**(3)**	**(4)**
Variables	**Parliamentary elections**		**Presidential elections**	
	Account	**Account**	**Account**	**Account**
Right-wing	0.320***	0.326***	0.049**	0.103*
	(0.033)	(0.034)	(0.024)	(0.059)
Right-wing seat share	0.311***	0.336***		
	(0.031)	(0.038)		
Margin			0.002***	-0.000
			(0.000)	(0.001)
Age	0.012***	0.012***	0.011***	0.012***
	(0.000)	(0.000)	(0.001)	(0.001)
Secondary education	0.879***	0.880***	0.888***	0.982***
	(0.016)	(0.016)	(0.020)	(0.022)
Tertiary education	2.120***	2.121***	1.826***	2.141***
	(0.027)	(0.027)	(0.033)	(0.037)
Income: second 20%	0.199***	0.199***	0.120***	0.137***
	(0.023)	(0.023)	(0.031)	(0.033)
Income: middle 20%	0.413***	0.413***	0.300***	0.363***
	(0.022)	(0.022)	(0.029)	(0.031)
Income: fourth 20%	0.687***	0.687***	0.623***	0.693***
	(0.022)	(0.022)	(0.029)	(0.031)
Income: richest 20%	1.167***	1.167***	1.093***	1.190***
	(0.022)	(0.022)	(0.028)	(0.030)
Female	-0.385***	-0.385***	-0.245***	-0.270***
	(0.013)	(0.013)	(0.017)	(0.018)
Employment	0.027***	0.032***	-0.013***	0.092***
	(0.006)	(0.006)	(0.001)	(0.012)
Domestic credit	-0.009***	-0.011***	-0.001***	0.001*
	(0.002)	(0.002)	(0.000)	(0.001)
Ln GDP per capita	1.170***	1.360***	0.544***	-0.637**
	(0.131)	(0.164)	(0.015)	(0.300)
Inflation	-0.023***	-0.023***	0.033***	-0.018***
	(0.003)	(0.003)	(0.002)	(0.004)
Manufacturing	-0.035***	-0.036***	-0.014***	-0.025*
	(0.008)	(0.008)	(0.002)	(0.013)
Regulatory quality	-0.162*	-0.209**	-0.564***	0.489**
	(0.089)	(0.091)	(0.028)	(0.235)
Political stability	0.070*	0.071*	0.092***	-0.177**

	(0.036)	(0.037)	(0.015)	(0.076)
Voice and accountability	0.372***	0.437***	0.198***	-0.539***
	(0.092)	(0.094)	(0.030)	(0.203)
Corruption control	0.016	-0.029	0.758***	0.791***
	(0.079)	(0.080)	(0.038)	(0.159)
Observations	138,802	138,802	76,975	76,975
Log-likelihood	-72473.782	-72309.426	-41635.936	-38163.321
ICC	0.217	-	0.104	-
Time fixed effects	Yes	Yes	Yes	Yes
Country fixed effects	No	Yes	No	Yes
No of countries	46	46	39	39

Table 6. Baseline results with five- and ten-year averages

This table reports the results of a multilevel logistic regression. The dependent variable in all four columns is account ownership, which refers to respondents who reported having an account at a formal financial institution. Panel A reports the five-year averages and Panel B reports the ten-year averages. Column (1) includes the political ideology of the government in power for the majority of the time during the last five years, column (2) adds individual-level variables and the five-year average of macroeconomic, political, and institutional variables, and column (3) includes country fixed effects in addition to column (2) variables. Column (4) presents the political ideology of the government in power for the majority of the time during the last 10 years, column (5) adds individual-level variables and the ten -year average of macroeconomic, political, and institutional variables, and column (6) includes country fixed effects in addition to column (5) variables. Standard errors are in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Five-year averages				**Panel B: Ten-year averages**			
Variable	**(1) Account**	**(2) Account**	**(3) Account**	**Variable**	**(4) Account**	**(5) Account**	**(6) Account**
Right-wing (five-year average)	0.497***	0.459***	0.453***	Right-wing (ten-year average)	0.225***	0.209***	0.210***
	(0.036)	(0.040)	(0.041)		(0.039)	(0.043)	(0.043)
Left-wing (five-year average)	0.186***	0.079**	0.088**	Left-wing (ten-year average)	0.120***	0.097***	0.106***
	(0.035)	(0.038)	(0.038)		(0.030)	(0.033)	(0.033)
Age		0.013***	0.013***	Age		0.013***	0.013***
		(0.000)	(0.000)			(0.000)	(0.000)
Secondary education		0.906***	0.907***	Secondary education		0.902***	0.903***
		(0.013)	(0.013)			(0.013)	(0.013)
Tertiary education		2.067***	2.069***	Tertiary education		2.067***	2.070***
		(0.022)	(0.022)			(0.023)	(0.023)
Income: second 20%		0.190***	0.190***	Income: second 20%		0.189***	0.189***
		(0.020)	(0.020)			(0.020)	(0.020)
Income: middle 20%		0.408***	0.409***	Income: middle 20%		0.407***	0.407***
		(0.019)	(0.019)			(0.019)	(0.019)
Income: fourth 20%		0.673***	0.673***	Income: fourth 20%		0.671***	0.671***
		(0.019)	(0.019)			(0.019)	(0.019)
Income: richest 20%		1.134***	1.135***	Income: richest 20%		1.132***	1.132***
		(0.018)	(0.018)			(0.018)	(0.018)
Female		-0.357***	-0.357***	Female		-0.358***	-0.357***
		(0.011)	(0.011)			(0.011)	(0.011)

Employment (five-year average)		0.020***	0.023***	Employment (ten-year average)		-0.001	-0.012
		(0.005)	(0.006)			(0.007)	(0.007)
Domestic credit (five-year average)		-0.008***	-0.009***	Domestic credit (ten-year average)		-0.004***	-0.005***
		(0.001)	(0.001)			(0.001)	(0.001)
Ln GDP per capita (five-year average)		1.089***	1.516***	Ln GDP per capita (ten-year average)		1.349***	2.024***
		(0.130)	(0.182)			(0.152)	(0.166)
Inflation (five-year average)		0.010***	0.010***	Inflation (ten-year average)		0.020***	0.018***
		(0.003)	(0.003)			(0.003)	(0.003)
Manufacturing (five-year average)		-0.013*	-0.016**	Manufacturing (ten-year average)		0.029***	0.036***
		(0.007)	(0.007)			(0.007)	(0.008)
Regulatory quality (five-year average)		-0.396***	-0.482***	Regulatory quality (ten-year average)		-0.225**	-0.322***
		(0.088)	(0.092)			(0.107)	(0.109)
Political stability (five-year average)		0.038	0.021	Political Stability (ten-year average)		-0.020	-0.059
		(0.037)	(0.039)			(0.054)	(0.055)
Voice and accountability (five-year average)		-0.201**	-0.272***	Voice and accountability (ten-year average)		-0.059	-0.095
		(0.084)	(0.088)			(0.107)	(0.114)
Corruption control (five-year average)		0.177**	0.146*	Corruption control (ten-year average)		-0.466***	-0.593***
		(0.086)	(0.089)			(0.122)	(0.124)
Observations	193,284	193,284	193,284	Observations	193,284	193,284	193,284
Log-likelihood	-112526.59	-100887.29	-100626.88	Log-likelihood	-112615.35	-100954.30	-100675.35
ICC	0.210	0.250	-	ICC	0.209	0.323	-
Time fixed effects	Yes	Yes	Yes	Time fixed effects	Yes	Yes	Yes
Country fixed effects	No	No	Yes	Country fixed effects	No	No	Yes
No of countries	65	65	65	No of countries	65	65	65

Table 7. Mobile banking

This table reports the results of a multilevel logistic regression. The dependent variable in all three columns is mobile account ownership (Mobile), which refers to the response of the survey respondent in terms of owning a mobile bank account. Column (1) includes the political ideology, and each subsequent column adds individual-level variables, macroeconomic variables, and political and institutional variables. Standard errors are in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Variables	**(1)** **Mobile**	**(2)** **Mobile**	**(3)** **Mobile**	**(4)** **Mobile**
Right-wing	0.654***	0.828***	1.178***	1.154***
	(0.079)	(0.084)	(0.086)	(0.087)
Left-wing	-0.102	-0.078	0.209***	0.202***
	(0.065)	(0.069)	(0.074)	(0.075)
Age		-0.010***	-0.010***	-0.010***
		(0.001)	(0.001)	(0.001)
Secondary education		0.675***	0.658***	0.659***
		(0.022)	(0.022)	(0.022)
Tertiary education		1.260***	1.230***	1.231***
		(0.035)	(0.035)	(0.035)
Income: second 20%		0.230***	0.232***	0.232***
		(0.036)	(0.036)	(0.036)
Income: middle 20%		0.360***	0.366***	0.366***
		(0.034)	(0.035)	(0.035)
Income: fourth 20%		0.553***	0.561***	0.561***
		(0.033)	(0.033)	(0.033)
Income: richest 20%		0.877***	0.898***	0.898***
		(0.032)	(0.032)	(0.032)
Female		-0.290***	-0.285***	-0.284***
		(0.018)	(0.018)	(0.018)
Employment		-0.015**	-0.000	-0.002
		(0.007)	(0.008)	(0.008)
Domestic credit		0.004***	0.004***	0.004***
		(0.001)	(0.001)	(0.001)
Ln GDP per capita		0.091	0.256	1.030***
		(0.223)	(0.263)	(0.295)
Inflation		0.076***	0.065***	0.067***
		(0.005)	(0.005)	(0.005)
Manufacturing		0.106***	0.142***	0.154***
		(0.013)	(0.014)	(0.015)
Regulatory quality			0.936***	0.928***
			(0.138)	(0.142)
Political stability			-1.176***	-1.205***
			(0.058)	(0.058)
Voice and accountability			1.705***	1.828***
			(0.125)	(0.126)
Corruption control			0.064	0.023
			(0.113)	(0.115)

Observations	159,373	169,112	169,112	169,112
Log-likelihood	-43138.372	-42890.1	-42590.3	-42328.1
ICC	0.339	0.525	0.684	-
Time fixed effects	Yes	Yes	Yes	Yes
Country fixed effects	No	No	No	Yes
No of countries	65	65	65	65

Table 8. Savings and frequency of use

This table presents the results of a multilevel logistic regression. The dependent variable in the first three columns is savings, and that in the next three columns is frequency, where Savings refers to respondents who reported having saved money at a formal financial institution conditional upon having a bank account, and Frequency refers to whether the respondent withdrew money from an account three or more times a month. Columns (1) and (4) include the political ideology, and each subsequent column adds individual-level variables, macroeconomic variables, and political and institutional variables. Standard errors are in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Variable	(1) Savings	(2) Savings	(3) Savings	(4) Frequency	(5) Frequency	(6) Frequency
Right-wing	-0.216***	-0.117**	-0.082	0.524***	0.498***	0.570***
	(0.051)	(0.054)	(0.056)	(0.114)	(0.117)	(0.141)
Left-wing	0.308***	0.230***	0.252***	0.226*	0.252**	0.177
	(0.049)	(0.052)	(0.054)	(0.115)	(0.117)	(0.163)
Age		0.002***	0.002***		-0.009***	-0.009***
		(0.000)	(0.000)		(0.001)	(0.001)
Secondary education		0.557***	0.556***		0.349***	0.351***
		(0.015)	(0.015)		(0.038)	(0.039)
Tertiary education		1.219***	1.220***		0.846***	0.856***
		(0.023)	(0.023)		(0.046)	(0.047)
Income: second 20%		0.244***	0.244***		0.110*	0.109*
		(0.023)	(0.023)		(0.064)	(0.064)
Income: middle 20%		0.471***	0.472***		0.182***	0.181***
		(0.022)	(0.022)		(0.060)	(0.060)
Income: fourth 20%		0.689***	0.689***		0.370***	0.372***
		(0.022)	(0.022)		(0.056)	(0.056)
Income: richest 20%		1.074***	1.075***		0.717***	0.719***
		(0.021)	(0.021)		(0.054)	(0.054)
Female		-0.202***	-0.203***		-0.295***	-0.295***
		(0.012)	(0.012)		(0.027)	(0.027)
Employment		0.019***	0.023***		0.008	-0.023
		(0.005)	(0.007)		(0.006)	(0.015)

Domestic credit		0.003***	0.003***		0.000	-0.000
		(0.001)	(0.001)		(0.001)	(0.001)
Ln GDP per capita		-0.230**	-0.673***		0.229**	1.999***
		(0.097)	(0.176)		(0.106)	(0.401)
Inflation		0.022***	0.020***		-0.002	-0.002
		(0.003)	(0.003)		(0.007)	(0.007)
Manufacturing		0.023***	0.025***		-0.010	0.007
		(0.007)	(0.007)		(0.010)	(0.019)
Regulatory quality		-0.099	-0.073		0.179	0.636**
		(0.082)	(0.089)		(0.177)	(0.319)
Political stability		-0.370***	-0.381***		-0.120	-0.378***
		(0.036)	(0.038)		(0.081)	(0.134)
Voice and accountability		-0.018	-0.094		0.077	0.282
		(0.073)	(0.081)		(0.147)	(0.334)
Corruption control		0.263***	0.223***		0.448***	0.214
		(0.071)	(0.074)		(0.134)	(0.189)
Observations	151,599	151,599	151,599	35,699	35,699	35,699
Log- likelihood	-84849.426	-79883.088	-79677.948	-18342.206	-17670.41	17520.507
ICC	0.088	0.116	-	0.102	0.088	-
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	No	No	Yes	No	No	Yes
No of countries	65	65	65	65	65	65

Table 9: Economic Intervention

This table presents the results of a multilevel logistic regression. The dependent variable in all the columns is account ownership. Regulatory requirements for starting a business, and trading across borders test the level of intervention in the economy and quality of the judicial processes test the discipling effect of market mechanism. Standard errors are in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Variable	**Starting a business (business friendly regulation)**	**Starting a business (stringent regulation)**	**Trading across border (open)**	**Trading across border (protective)**	**Quality of judicial system (efficient)**	**Quality of judicial system (less efficient)**
Right-wing	0.779***	0.229	-0.121	1.395	4.106***	-0.342**
	(0.073)	(0.140)	(0.134)	(0.916)	(1.041)	(0.151)
Left-wing	0.674***	-0.383***	-0.443**	0.283**	2.530***	-0.810***
	(0.057)	(0.132)	(0.185)	(0.140)	(0.652)	(0.115)
Age	0.009***	0.017***	0.010***	0.016***	0.013***	0.014***
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Secondary education	0.800***	1.017***	0.851***	1.173***	0.788***	0.730***
	(0.019)	(0.020)	(0.025)	(0.026)	(0.033)	(0.028)
Tertiary education	2.009***	2.139***	2.118***	2.322***	1.932***	1.951***
	(0.030)	(0.037)	(0.041)	(0.044)	(0.057)	(0.051)
Income: second 20%	0.218***	0.158***	0.221***	0.126***	0.146***	0.272***
	(0.027)	(0.031)	(0.036)	(0.041)	(0.044)	(0.043)
Income: middle 20%	0.394***	0.433***	0.449***	0.386***	0.397***	0.423***
	(0.026)	(0.029)	(0.035)	(0.039)	(0.044)	(0.042)
Income: fourth 20%	0.663***	0.699***	0.743***	0.732***	0.551***	0.646***
	(0.026)	(0.029)	(0.035)	(0.037)	(0.043)	(0.041)
Income: richest 20%	1.099***	1.194***	1.202***	1.239***	1.037***	1.052***
	(0.026)	(0.028)	(0.035)	(0.036)	(0.044)	(0.040)
Female	-0.407***	-0.279***	-0.394***	-0.233***	-0.461***	-0.342***
	(0.016)	(0.017)	(0.021)	(0.021)	(0.027)	(0.025)
Employment	0.114***	-0.027***	-0.133***	0.074***	-0.163***	0.037***
	(0.011)	(0.008)	(0.028)	(0.011)	(0.049)	(0.004)
Domestic credit	-0.024***	-0.000	-0.019***	-0.002***	-0.138***	-0.034***
	(0.002)	(0.001)	(0.004)	(0.001)	(0.050)	(0.003)
Ln GDP per capita	0.832***	2.031***	0.415	-0.483	-6.988***	2.308***
	(0.260)	(0.377)	(0.456)	(0.359)	(2.277)	(0.157)
Inflation	-0.028***	-0.020***	0.142***	0.001	-0.728**	-0.114***
	(0.004)	(0.006)	(0.036)	(0.005)	(0.314)	(0.014)
Manufacturing	-0.069***	0.114***	-0.049*	0.044	0.899***	0.089***
	(0.015)	(0.019)	(0.027)	(0.027)	(0.290)	(0.014)
Regulatory quality	0.077	-0.768***	-0.980**	-0.522**	0.946*	-3.528***
	(0.138)	(0.148)	(0.413)	(0.260)	(0.536)	(0.294)
Political stability	-0.146***	-0.149**	-0.083	-0.173**	-7.070***	1.252***
	(0.047)	(0.071)	(0.179)	(0.082)	(2.379)	(0.073)
Voice and accountability	0.015	-0.454***	0.007	-0.615	6.801**	-0.223
	(0.109)	(0.176)	(0.280)	(0.374)	(2.876)	(0.207)
Corruption control	0.294***	0.569***	0.519**	1.612***	8.823***	0.402
	(0.113)	(0.122)	(0.215)	(0.177)	(3.208)	(0.406)

Observations	92,229	91,579	57,953	56,888	31,091	37,876
Log-likelihood	-49757.291	-45140.851	-29489.892	-27240.137	-16978.811	-20746.207
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
No of countries	43	42	35	34	25	36

Table 10: Robustness

This table presents the results of various robustness tests. Panel A reports the regression results based on heterogeneity in political system depending on the electoral system (plurality voting versus proportional representation), having a finite term in office and duration of the party in power. Additionally, it reports the RDD estimates based on the type of government (single party vs coalition) and the estimates limiting ideological orientation of the government to three major parties. Panel B documents the results excluding the populist parties, and considering only right and left parties. Panel C shows the results based on different economic environment such as excluding 2011 survey wave right after Global Financial Crisis and considering poorest 40% of the households. Panel D reports results from alternative econometric assumptions such as clustering standard error at the country-level, using bootstrap, adjusting sampling weights, and considering change in regime. Standard errors are in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel **A: Heterogeneity in political system**									
Variable	**(1) Plurality**	**(2) Proportional representation**	**(3) Finite term**	**(4) Duration of the party in power**			**(5) Single-party**	**(6) Coalition**	**(7) Limiting to three major parties**
				Duration (≤8)	**Duration (9 to 16)**	**Duration (>16)**			
Right-wing	1.709***	0.401*	0.570***	1.099***	2.927***	14.004*	1.160***	0.520***	0.616***
	(0.515)	(0.242)	(0.138)	(0.341)	(0.435)	(8.219)	(0.135)	(0.040)	(0.219)
Left-wing	1.469***	0.185	0.251*	-2.241***	-8.086***	11.934			0.187
	(0.515)	(0.251)	(0.138)	(0.613)	(2.110)	(7.367)			(0.241)
Observations	81,529	86,669	125,032	55,253	17,973	13,939	63,438	115,879	78,235
Log-likelihood	-41516.598	-43681.491	-63380.184	-27777.404	-9640.8839	-6550.726	-31730.455	-60985.196	-40287.681
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No of countries	33	37	52	30	14	6	30	46	42

Panel B: Altering sample		
Variable	**(1) Excluding populist government**	**(2) Considering only right and left**
Right-wing	0.401***	0.542***
	(0.044)	(0.043)
Left-wing	0.153***	
	(0.047)	
Observations	167,319	92,986
Log-likelihood	-87475.605	-49926.473
Controls	Yes	Yes
Time fixed effects	Yes	Yes
Country fixed effects	Yes	Yes
No of countries	42	34

Panel C: Economic environment					
		Financial inclusion in the poorest 40% of population			
Variable	**(1) Excluding 2011 wave**	**(2) Account**	**(3) Account**	**(4) Account**	**(5) Account**
Right-wing	0.319***	0.568***	0.547***	0.511***	0.561***
	(0.081)	(0.071)	(0.073)	(0.073)	(0.077)
Left-wing	-0.206**	0.069	0.105	0.068	0.123
	(0.088)	(0.076)	(0.076)	(0.078)	(0.081)
Observations	132,201	66,708	66,708	66,708	66,708
Log-likelihood	-70998.617	-32986.925	-31756.932	-31742.544	-31560.334
ICC	0.200	0.244	0.187	0.166	-
Time fixed effects	Yes	Yes	Yes	Yes	Yes
Country fixed effects	65	No	No	No	Yes
No of countries		65	65	65	65

Panel D: Changing econometric assumptions					
Variable	**(1) Clustering standard errors at country level**	**(2) Bootstrapping 100**	**(3) Bootstrapping 500**	**(4) Sampling weights**	**(5) Regime change**
Right-wing	0.078*	0.387***	0.387***	0.074***	
	(0.046)	(0.041)	(0.041)	(0.007)	
Left-wing	0.021	0.096**	0.096**	0.014**	
	(0.046)	(0.041)	(0.041)	(0.006)	
Change in regime					0.395***
					(0.110)
Observations	193,284	193,284	193,284	131,777	12,972
Pseudo R-squared	0.223				
Log-likelihood	-	-112560.73	-112560.73	-69807.384	-7449.382
Controls	Yes	0.205	0.205	0.227	-
Time fixed effects	Yes	Yes	Yes	Yes	Yes
No of countries	Yes	65	65	65	5

Table 11. Country level analysis

This table presents the estimates of OLS regression of account ownership on party ideology with country-level control variables. The dependent variable in column (1), (2) and (3) is account ownership, account ownership among the poorest 40% of the household and account ownership in rural area respectively, all at the aggregate level. The specification in column (4) similar to column (1) keeping only the left-wing countries in the base. The country level financial inclusion indicator refers to respondents who reported having an account at a financial institution. All the Models include country and time-fixed effects. Standard errors, clustered by country and time. are in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Variables	**(1) Account**	**(2) Account among poorest 40%**	**(3) Account in rural areas**	**(4) Account**
Right-wing	0.075***	0.081***	0.078**	0.098***
	(0.023)	(0.030)	(0.030)	(0.027)
Employment	0.003	0.003	0.005	0.000
	(0.004)	(0.004)	(0.005)	(0.006)
Domestic credit	-0.000	-0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.002)
Ln GDP per capita	0.135	0.203	0.115	0.215
	(0.115)	(0.125)	(0.122)	(0.194)
Inflation	0.000	-0.001	-0.001	0.001
	(0.002)	(0.002)	(0.002)	(0.003)
Manufacturing	0.005	0.000	0.002	0.006
	(0.004)	(0.005)	(0.004)	(0.006)
Regulatory quality	-0.089	-0.112	-0.087	-0.057
	(0.062)	(0.073)	(0.070)	(0.095)
Political stability	-0.034	-0.030	-0.019	0.023
	(0.023)	(0.023)	(0.026)	(0.047)
Voice and accountability	0.017	0.007	0.008	-0.014
	(0.063)	(0.064)	(0.073)	(0.139)
Corruption control	0.106**	0.088*	0.085	0.082
	(0.050)	(0.052)	(0.058)	(0.077)
Observations	188	183	188	87
R-squared	0.949	0.940	0.938	0.945
Time fixed effects	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes
No of countries	71	71	71	37

Table 12. Right-wing government and financial inclusion: PSM

Panels A and B report the results from the Propensity Score Matching (PSM). For each country with a right-wing government, we match the country with the left-wing government on the country-level macroeconomic and regulatory variables. Panel A documents the means of these variables of the group of countries with the right-wing government (treatment group) and countries with the left-wing government (control group). Panel B documents the estimates from the linear regressions on the matched sample. The dependent variable in columns (1), (2), and (3) is account ownership at the aggregate level. The country-level financial inclusion indicator refers to respondents who reported having an account at a financial institution. All the Models include country and time fixed effects. Standard errors, clustered by country and time. are in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Comparing treatment countries and control countries				
	Mean		**Test-t**	
	Control	**Treated**	**Diff**	**t-test**
Employment	59.125	58.720	0.405	-0.150
Domestic credit	38.269	35.537	2.732	-0.470
Ln GDP per capita	8.848	8.896	-0.048	0.220
Inflation	6.412	6.913	-0.501	0.300
Manufacturing	13.439	11.613	1.826	-1.410
Regulatory quality	-0.123	-0.089	-0.034	0.230
Political stability	-0.327	-0.293	-0.034	0.150
Voice and accountability	-0.154	-0.128	-0.027	0.130
Corruption control	-0.395	-0.445	0.049	-0.320

Panel B: Regressions on the matched sample			
	(1)	**(2)**	**(3)**
	Account	**Account**	**Account**
Right-wing	0.080***	0.073**	0.090**
	(0.027)	(0.029)	(0.035)
Employment		0.002	0.005
		(0.006)	(0.008)
Domestic credit		-0.002	-0.004
		(0.001)	(0.003)
Ln GDP per capita		0.375	0.488
		(0.287)	(0.461)
Inflation		-0.002	-0.001
		(0.003)	(0.004)
Manufacturing		0.005	-0.002
		(0.011)	(0.018)
Regulatory quality			-0.263
			(0.295)
Political stability			0.140
			(0.095)
Voice and accountability			-0.241
			(0.329)
Corruption control			0.030
			(0.124)
Observations	48	48	48
R-squared	0.952	0.959	0.969
Time fixed effects	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes

Appendix A. Sample selection

Description	Number of country-year observations
Global Findex data	451,372
Less: Unavailable DPI data	(96,847)
Combination of DPI and Global Findex data	354, 525
Less: Unavailable WDI data	(3,006)
Combination of Global Findex, DPI, and WDI data	351,519
Less: Unavailable WGI data	(0)
Combination of Global Findex, DPI, WDI and WGI data	351,519
Less: High-Income countries	(109,474)
Less: Missing individual and country-level observations	(44,259)
Less: Countries not having at least 2 years of data	(4,502)
Final sample: country-year observations	193,284

Appendix B. Data description and sources

Variable	Description	Source
Individual level variables		
Account ownership	Dummy equals 1 if the respondent has an account in a formal financial institution.	Global Findex
Savings (conditional on formal account)	Dummy equals to 1 if the respondent has saved or set aside money in the past 12 months using an account.	Global Findex
Frequency (conditional on formal account)	Dummy equals to 1 if the respondent has taken money out of an account three or more times in a typical month.	Global Findex
Mobile	Dummy equals 1 if the respondent has a mobile banking account.	Global Findex
Female	Dummy equals 1 if the respondent is female.	Global Findex
Age	Age of the respondent, in years.	Global Findex
Income: poorest 20%	Dummy that takes the value 1 if the respondent falls in the lowest income quintile, and 0 otherwise. Income quintiles are based on the incomes of the respondents in a country.	Global Findex
Income: second 20%	Dummy that takes the value 1 if the respondent falls in the second lowest income quintile, and 0 otherwise. Income quintiles are based on the incomes of the respondents in a country.	Global Findex
Income: middle 20%	Dummy that takes the value 1 if the respondent falls in the middle-income quintile, and 0 otherwise. Income quintiles are based on the incomes of the respondents in a country.	Global Findex
Income: fourth 20%	Dummy that takes the value 1 if the respondent falls in the second-highest income quintile, and 0 otherwise. Income quintiles are based on the incomes of the respondents in a country.	Global Findex
Income: richest 20%	Dummy that takes the value 1 if the respondent falls in the highest income quintile, and 0 otherwise. Income quintiles are based on the incomes of the respondents in a country.	Global Findex
Primary education	Dummy that takes the value 1 if the respondent completed up to 8 years of education, and 0 otherwise.	Global Findex
Secondary education	Dummy that takes the value 1 if the respondent completed 9–15 years of education, and 0 otherwise.	Global Findex
Tertiary education	Dummy that takes the value 1 if the respondent completed more than 15 years of education.	Global Findex

Country-level variables		
Right-wing	Takes the value of 1 if the party in office is right-wing.	Database of Political Institution (DPI)
Left-wing	Takes the value of 1 if the party in office is left- wing.	Database of Political Institution (DPI)
Employment rate	Labor force participants aged 15or above.	World Development Indicators (WDI)
Domestic credit to private sector (% GDP)	Indicator of financial sector development.	World Development Indicators (WDI)
Ln GDP per capita	Natural logarithm of the per capita real GDP.	World Development Indicators (WDI)
Plurality	Dummy that takes the value 1 if plurality, 0 otherwise	Database of Political Institution (DPI)
Proportional representation	Dummy that takes the value 1 if PR, 0 otherwise	Database of Political Institution (DPI)
Finite term	Dummy that takes the value 1 if finite term, 0 otherwise	Database of Political Institution (DPI)
Inflation	Measured by the Consumer Price Index.	World Development Indicators (WDI)
Manufacturing value added	Proxy for an alternative route of development associated with wage laborers.	World Development Indicators (WDI)
Regulatory quality	Captures perceptions of the extent to which government is able to formulate and implement sound policies. A higher score indicates higher ability.	World Governance Indicators (WGI)
Political stability	Political stability and absence of violence. The scale is from -2.5 to 2.5, where a higher value means a more stable government.	World Governance Indicators (WGI)
Voice and accountability	Captures the extent of freedom of expression of the country's citizen. The scale is from -2.5 to 2.5, where a higher value means a more freedom.	World Governance Indicators (WGI)
Corruption control	Indicates a country's ability to control corruption, with a scale from -2.5 to 2.5, where a higher value indicates less corruption.	World Governance Indicators (WGI)
Starting a business score	The simple average of the scores for the procedures, time, cost, and minimum capital requirement to start and formally operate a business.	Doing Business, the World Bank
Trading across borders	The score for trading across borders is the simple average of the scores for the time and cost for documentary compliance and border compliance for international trade	Doing Business, the World Bank
Quality of judicial processes index	The sum of the court structure and proceedings, case management, court automation and alternative dispute resolution.	Doing Business, the World Bank

Account ownership	Weighted average percentage of population having a bank account and are at least 15 years of age. Findex: Accountage15	Global financial Inclusion country level data
Account among poorest 40%	Weighted average percentage of population (poorest 40%) having a bank account and are at least 15 years of age. Findex: Accountincomepoorest40	Global financial Inclusion country level data
Account in rural areas	Weighted average percentage of rural population having a bank account and are at least 15 years of age. Findex: Accountruralage15	Global financial Inclusion country level data

Online Appendix for "Reaching Out to the Unbanked: The Role of Political Ideology in Financial Inclusion"

Table A.1: Robustness

This table presents the results of various robustness tests. Panel A reports the regression results based on heterogeneity in the political system depending on the electoral system (plurality voting versus proportional representation), having a finite term in office, and duration of the party in power. Additionally, it reports the RDD estimates based on the type of government (single party vs. coalition) and the estimates limiting the ideological orientation of the government to three major parties. Panel B documents the results excluding the populist parties and considering only right and left parties. Panel C shows the results based on different economic environments, such as excluding the 2011 survey wave right after the Global Financial Crisis and considering the poorest 40% of the households. Panel D reports results from alternative econometric assumptions such as clustering standard error at the country-level, using bootstrap, adjusting sampling weights, and considering the regime change. Standard errors are in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Heterogeneity in the political system

Variable	(1) Plurality	(2) Proportional representation	(3) Finite term	(4) Duration of the party in power			(5) Single-party	(6) Coalition	(7) Limiting to three major parties
				Duration (≤8)	Duration (9 to 16)	Duration (>16)			
Right-wing	1.709***	0.401*	0.570***	1.099***	2.927***	14.004*	1.160***	0.520***	0.616***
	(0.515)	(0.242)	(0.138)	(0.341)	(0.435)	(8.219)	(0.135)	(0.040)	(0.219)
Left-wing	1.469***	0.185	0.251*	-2.241***	-8.086***	11.934			0.187
	(0.515)	(0.251)	(0.138)	(0.613)	(2.110)	(7.367)			(0.241)
Right-wing seat share							-0.916***	0.750***	
							(0.281)	(0.084)	
Age	0.017***	0.011***	0.013***	0.009***	0.011***	0.021***	0.016***	0.011***	0.014***
	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)	(0.002)	(0.001)	(0.000)	(0.001)
Secondary education	1.020***	0.854***	0.938***	0.825***	0.888***	1.244***	1.014***	0.858***	0.896***
	(0.021)	(0.020)	(0.017)	(0.025)	(0.043)	(0.052)	(0.024)	(0.017)	(0.021)
Tertiary education	2.306***	1.996***	2.124***	2.029***	2.068***	2.970***	2.241***	2.027***	2.087***
	(0.036)	(0.034)	(0.029)	(0.042)	(0.079)	(0.135)	(0.042)	(0.029)	(0.036)
Income: second 20%	0.194***	0.133***	0.182***	0.139***	0.088	0.371***	0.226***	0.182***	0.126***
	(0.031)	(0.030)	(0.025)	(0.037)	(0.063)	(0.075)	(0.035)	(0.025)	(0.031)
Income: middle 20%	0.480***	0.334***	0.395***	0.338***	0.408***	0.455***	0.504***	0.383***	0.301***

	(0.030)	(0.029)	(0.024)	(0.036)	(0.060)	(0.074)	(0.034)	(0.024)	(0.030)
Income: fourth 20%	0.725***	0.585***	0.666***	0.588***	0.665***	0.873***	0.733***	0.660***	0.584***
	(0.030)	(0.028)	(0.024)	(0.035)	(0.059)	(0.074)	(0.033)	(0.024)	(0.029)
Income: richest 20%	1.197***	1.100***	1.141***	1.082***	1.102***	1.483***	1.161***	1.149***	1.081***
	(0.029)	(0.028)	(0.023)	(0.035)	(0.058)	(0.074)	(0.033)	(0.024)	(0.029)
Female	-0.341***	-0.361***	-0.310***	-0.296***	-0.480***	-0.134***	-0.377***	-0.354***	-0.349***
	(0.017)	(0.017)	(0.014)	(0.021)	(0.036)	(0.044)	(0.020)	(0.014)	(0.018)
Employment	0.017**	-0.069***	-0.000	0.134***	0.032***	-0.007	-0.013	0.087***	0.066***
	(0.008)	(0.013)	(0.007)	(0.020)	(0.011)	(0.009)	(0.009)	(0.010)	(0.017)
Domestic credit	0.007***	0.008***	0.006***	0.000	-0.176***	0.169*	-0.001	-0.000	-0.002
	(0.002)	(0.003)	(0.001)	(0.004)	(0.025)	(0.093)	(0.003)	(0.001)	(0.003)
Ln GDP per capita	-0.712**	-1.506***	-0.988***	-0.749**	2.660***	4.155**	2.088***	0.448	-3.343***
	(0.280)	(0.434)	(0.213)	(0.297)	(0.333)	(2.061)	(0.323)	(0.274)	(0.466)
Inflation	-0.034***	-0.023***	-0.025***	-0.051***	0.200***	-0.041***	0.032***	-0.026***	-0.042***
	(0.005)	(0.005)	(0.003)	(0.010)	(0.072)	(0.010)	(0.007)	(0.003)	(0.004)
Manufacturing	0.075***	0.079***	0.066***	0.079***	-0.028	-0.548	-0.033	0.020**	0.051***
	(0.018)	(0.010)	(0.008)	(0.027)	(0.061)	(0.370)	(0.025)	(0.008)	(0.011)
Regulatory quality	-0.745***	-0.120	-0.363***	-0.410*	0.488	-6.308**	-1.675***	0.657***	0.320*
	(0.167)	(0.175)	(0.114)	(0.213)	(0.554)	(3.129)	(0.203)	(0.123)	(0.188)
Political stability	-0.101	-0.074	-0.070*	0.124	4.492***	0.181	0.555***	-0.087*	0.100
	(0.069)	(0.046)	(0.037)	(0.117)	(1.222)	(0.325)	(0.082)	(0.045)	(0.072)
Voice and accountability	-0.030	-0.585***	-0.001	0.275	-7.368***	-9.365	-0.358**	-0.289**	0.238
	(0.125)	(0.140)	(0.089)	(0.256)	(1.629)	(6.945)	(0.171)	(0.122)	(0.164)
Corruption control	0.340***	0.789***	0.554***	0.264	5.819***	-1.290***	1.119***	0.094	0.078
Observations	81,529	86,669	125,032	55,253	17,973	13,939	63,438	115,879	78,235
Log-likelihood	-41516.598	-43681.491	-63380.184	-27777.404	-9640.8839	-6550.726	-31730.455	-60985.196	-40287.681
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No of countries	33	37	52	30	14	6	30	46	42

Panel B: Altering the sample		
Variable	**(1) Excluding populist government**	**(2) Considering only right and left**
Right-wing	0.401***	0.542***
	(0.044)	(0.043)
Left-wing	0.153***	
	(0.047)	
Age	0.012***	0.011***
	(0.000)	(0.001)
Secondary education	0.902***	0.881***
	(0.014)	(0.019)
Tertiary education	2.073***	2.058***
	(0.024)	(0.032)
Income: second 20%	0.186***	0.205***
	(0.021)	(0.028)
Income: middle 20%	0.413***	0.430***
	(0.020)	(0.027)
Income: fourth 20%	0.673***	0.678***
	(0.020)	(0.026)
Income: richest 20%	1.141***	1.103***
	(0.020)	(0.026)
Female	-0.353***	-0.314***
	(0.012)	(0.016)
Employment	0.019***	0.006
	(0.006)	(0.007)
Domestic credit	0.000	0.004**
	(0.001)	(0.002)
Ln GDP per capita	0.671***	0.542**
	(0.160)	(0.232)
Inflation	-0.022***	-0.002
	(0.003)	(0.004)
Manufacturing	0.010	-0.003
	(0.006)	(0.007)
Regulatory quality	0.223***	-0.582***
	(0.083)	(0.129)
Political stability	-0.109***	0.379***
	(0.031)	(0.057)
Voice and accountability	-0.313***	-0.202
	(0.086)	(0.135)
Corruption control	0.550***	1.365***
Observations	167,319	92,986
Log-likelihood	-87475.605	-49926.473
Controls	Yes	Yes
Time fixed effects	Yes	Yes
Country fixed effects	Yes	Yes
No of countries	42	34

Panel C: Economic environment					
		Financial inclusion in the poorest 40% of population			
Variable	**(1) Excluding 2011 wave**	**(2) Account**	**(3) Account**	**(4) Account**	**(5) Account**
Right-wing	0.319***	0.568***	0.547***	0.511***	0.561***
	(0.081)	(0.071)	(0.073)	(0.073)	(0.077)
Left-wing	-0.206**	0.069	0.105	0.068	0.123
	(0.088)	(0.076)	(0.076)	(0.078)	(0.081)
Age	0.319***	0.568***	0.547***	0.511***	0.561***
	(0.081)	(0.071)	(0.073)	(0.073)	(0.077)
Secondary education	0.013***	0.069	0.105	0.068	0.123
	(0.000)	(0.076)	(0.076)	(0.078)	(0.081)
Tertiary education	0.849***		0.010***	0.010***	0.010***
	(0.016)		(0.001)	(0.001)	(0.001)
Income: second 20%	2.019***		0.777***	0.778***	0.779***
	(0.027)		(0.023)	(0.023)	(0.023)
Income: middle 20%	0.185***		1.904***	1.907***	1.908***
	(0.023)		(0.048)	(0.048)	(0.048)
Income: fourth 20%	0.396***		-0.297***	-0.297***	-0.298***
	(0.022)		(0.020)	(0.020)	(0.020)
Income: richest 20%	0.633***		0.018***	0.018***	0.027**
	(0.022)		(0.007)	(0.007)	(0.010)
Female	1.076***		-0.001	-0.000	-0.001
	(0.022)		(0.001)	(0.001)	(0.001)
Employment	-0.371***		0.697***	0.639***	0.754***
	(0.013)		(0.120)	(0.126)	(0.259)
Domestic credit	0.021***		-0.019***	-0.018***	-0.020***
	(0.007)		(0.004)	(0.004)	(0.005)
Ln GDP per capita	-0.005***		0.009	0.008	0.005
	(0.002)		(0.009)	(0.009)	(0.011)
Inflation	0.812***			-0.191	-0.206
	(0.135)			(0.125)	(0.140)
Manufacturing	-0.015***			0.059	0.065
	(0.004)			(0.048)	(0.052)
Regulatory quality	-0.200**			0.105	0.079
	(0.094)			(0.109)	(0.128)
Political stability	-0.127***			0.473***	0.437***
	(0.045)			(0.106)	(0.115)
Voice and accountability	-0.056	0.568***	0.547***	0.511***	0.561***
	(0.085)	(0.071)	(0.073)	(0.073)	(0.077)
Corruption control	0.511***	0.069	0.105	0.068	0.123
	(0.091)	(0.076)	(0.076)	(0.078)	(0.081)
Observations	132,201	66,708	66,708	66,708	66,708
Log-likelihood	-70998.617	-32986.925	-31756.932	-31742.544	-31560.334
ICC	0.200	0.244	0.187	0.166	-
Time fixed effects	Yes	Yes	Yes	Yes	Yes
Country fixed effects	65	No	No	No	Yes
No of countries		65	65	65	65

Panel D: Changing econometric assumptions					
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Variable	**Clustering standard errors at country level**	**Bootstrapping 100**	**Bootstrapping 500**	**Sampling weights**	**Regime change**
Right-wing	0.078*	0.387***	0.387***	0.074***	
	(0.046)	(0.041)	(0.041)	(0.007)	
Left-wing	0.021	0.096**	0.096**	0.014**	
	(0.046)	(0.041)	(0.041)	(0.006)	
Change in regime					0.395***
					(0.110)
Age	0.002***	0.013***	0.013***	0.002***	0.014***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Secondary education	0.161***	0.905***	0.905***	0.152***	0.798***
	(0.006)	(0.013)	(0.013)	(0.002)	(0.048)
Tertiary education	0.379***	2.066***	2.066***	0.376***	1.828***
	(0.011)	(0.023)	(0.023)	(0.004)	(0.090)
Income: second 20%	0.028***	0.191***	0.191***	0.030***	0.050
	(0.004)	(0.020)	(0.020)	(0.002)	(0.067)
Income: middle 20%	0.065***	0.409***	0.409***	0.072***	0.232***
	(0.005)	(0.019)	(0.019)	(0.002)	(0.066)
Income: fourth 20%	0.113***	0.674***	0.674***	0.117***	0.449***
	(0.006)	(0.019)	(0.019)	(0.003)	(0.066)
Income: richest 20%	0.202***	1.136***	1.136***	0.192***	0.745***
	(0.007)	(0.018)	(0.018)	(0.003)	(0.066)
Female	-0.063***	-0.358***	-0.358***	-0.059***	-0.449***
	(0.007)	(0.011)	(0.011)	(0.001)	(0.040)
Employment	0.005	0.020***	0.020***	0.006***	-0.213***
	(0.004)	(0.005)	(0.005)	(0.000)	(0.047)
Domestic credit	-0.000	-0.001	-0.001	-0.000***	-0.010*
	(0.000)	(0.000)	(0.000)	(0.001)	(0.006)
Ln GDP per capita	0.058	0.480***	0.480***	0.129***	-5.650**
	(0.098)	(0.099)	(0.099)	(0.016)	(2.330)
Inflation	-0.003*	-0.021***	-0.021***	-0.002***	-0.444***
	(0.002)	(0.002)	(0.002)	(0.000)	(0.040)
Manufacturing	-0.001	0.009	0.009	-0.001	0.031
	(0.004)	(0.006)	(0.006)	(0.001)	(0.073)
Regulatory quality	0.023	0.071	0.071	0.029**	-10.218***
	(0.051)	(0.073)	(0.073)	(0.012)	(0.715)
Political stability	-0.006	0.010	0.010	-0.016***	3.528***
	(0.018)	(0.028)	(0.028)	(0.004)	(0.888)
Voice and accountability	-0.052	-0.111*	-0.111*	-0.042***	
	(0.041)	(0.066)	(0.066)	(0.011)	
Corruption control	0.094**	0.501***	0.501***	0.080***	
	(0.043)	(0.061)	(0.061)	(0.010)	
Observations	193,284	193,284	193,284	131,777	12,972
Pseudo *R*-squared	0.265				
Log-likelihood	-	-112560.73	-112560.73	-69807.384	-7449.382
Controls	Yes	0.205	0.205	0.227	-
Time fixed effects	Yes	Yes	Yes	Yes	Yes
No of countries	65	65	65	65	5

Chapter 2

Why do managers announce the intention to sell assets?

Abstract

We find that 32% of the announcements of asset sales are preceded by a public statement of the intention to sell. We refer to these statements as preannouncements and find significant average announcement returns of 1.12%, which have not been documented in the literature. A key characteristic of the preannouncements is that corporate executives have discretion in timing the statement of their intention. As a result, the preannouncements prevail in specific situations: for assets outside the U.S., after poor stock performance, and when a new CEO has been appointed recently. We find that the preannouncement returns are explained by size and leverage. In contrast, returns on deals that were not preannounced have different explanations, such as past returns and the buyer's identity. Most striking, the ultimate announcements of preannounced deals have low return impact, and this impact is also unrelated to standard explanatory variables. Finally, we observe opportunistic behavior of managers who vest options around the preannouncements aiming to benefit from the uptick in stock prices. Investors account for this opportunism as returns upon these announcements are 2.9%-point lower.

Key words: Asset sales, Preannouncements, Managerial opportunism

JEL: G30, G34, G40

2.1. Introduction

Asset sales play a pivotal role in corporate restructuring and contribute to corporate funding. A prime example of this emerges in the following company press release. On 23 October 2013, Bill Barrett Corporation sold the company's West Tavaputs natural gas property for $371 million. "Completing this transaction is consistent with our objectives to partially fund our capital program through asset sales, to end 2013 with total debt less than year-end 2012 and to divest of projects where the company is not actively investing, " stated Chief Executive Officer and President Scot Woodall, in a press release (Dow Jones Newswires, 2013). Now a days, asset sales play a more prominent role as a corporate funding source than the conventional sources of capital such as debt or equity. In 2012, non-financial firms in the United States reported $131 billion of asset sales against $81 billion of seasoned equity issues (Edmans and Mann, 2019). While some asset sales are motivated by financing requirements or operational reasons such as efficient reallocation of productive assets, others aim to transfer wealth to shareholders. For example, on 27 November 2018, Alliance Data Systems Corp announced the intention to sell its global media arm. Ed Heffernan, Alliance Data's president and chief executive officer said, "Today's announcement reflects the outcome of a lengthy study into Alliance Data's portfolio of businesses with the objective of unlocking increased value for Alliance Data stockholders […] by returning capital to stockholders through share repurchases and/or dividend"(PR Newswire, 2018).

Irrespective of the motivation, studies report that markets react positively to asset sales announcements (Jain, 1985; Hite et al., 1987; Borisova et al., 2013). These sales announcements can be categorized into two types: preannounced deals and non-preannounced deals. Preannounced deals constitute two subsequent events: the preannouncement, followed by the deal announcement. Deals that are not preannounced have a single announcement. Prior literature on asset sales focuses on deal announcements, disregarding the preannouncements. Further, while research into other corporate actions that generate a positive market reaction (e.g., stock splits, repurchases) studies managerial incentives to exploit the expected positive market reaction, little such inquiry has been conducted into asset sales. This paper fills this gap by investigating the prevalence of asset sales preannouncements.

Asset sale preannouncements are voluntary disclosures by firms of the intention to sell certain assets. We examine whether managers disclose their intention to sell assets in an attempt to maximize their personal wealth. Specifically, we study the timing of CEO option grants around the asset sales preannouncement. Several arguments motivate our analysis. First, given that asset sales alter the scope of the firm's operations and financial structure, there is a plausibly strong demand for information about asset sales (Hite et al., 1987). Second, managers tend to expedite news that is likely to affect stock price positively and delay information that is likely to spur an adverse market reaction (Shalev, 2009). Because management has discretion over the timing, they are likely to preannounce their intention to sell as long as the benefit of doing so outweighs the cost. There are opportunities for managers to strategically time the preannouncement to benefit from a firm's share price increase. Therefore, we argue that a preannouncement allows some of the expected positive market reactions to occur earlier. Third, prior studies analyse major corporate events such as stock split or corporate earnings announcements and present evidence that CEOs opportunistically time voluntary corporate disclosures and use option vesting as a tool to increase personal gains (Aboody and Kasznik, 2000; Devos et al., 2015). Since options are typically vested with an exercise price equal to the closing price on the grant day, asset sale announcements with positive reactions would yield an immediate increase in the option value. Therefore, managers have the motive to time the intention to sell assets opportunistically.

We examine disclosures of U.S. public firms and first document the prevalence of preannouncements in a sample of 635 completed asset sales between 2005 and 2019. In line with our argument and new to the asset sale literature, we find that 32% of asset sales are preceded by a public announcement of the intention to sell. Preannouncements are more prevalent among larger deals (transactions preceded by an announcement are on average 6.25% larger than non-announced transactions), and therefore their value-weighted proportion equals as much as 51%. We then investigate market' reactions to preannouncements. We find that preannounced sales elicit statistically and economically significant cumulative abnormal returns, which average 1.12% over a three-day event window (significant at 1% level). These abnormal returns have not been included in prior studies, leading to an underestimation of the market reaction to asset sales by 18%. The preannouncement, together with the deal-announcement of the preannounced deals (0.81% on average), induces a total 1.93% positive market reaction (significant at 1%). Interestingly, when deals have not been preannounced, returns are almost identical, i.e. 1.92% on average, but for a single announcement.

We investigate the determinants of the decision to preannounce a deal and find that the probability of preannouncement increases when managers have incentives to signal improved prospects of the firm's remaining assets. In particular, we find that asset sales are more likely to be preannounced when the selling firms are larger in terms of market capitalization. Firms are also likely to preannounce when deal values are larger and sold assets are closely related to the firm's operation. The opportunistic timing of preannouncements is evident, because they occur when new CEOs are appointed and in case the firm's stock has performed poorly in the year preceding the announcement.

In our next set of analyses, we study the market response to the three types of announcements: preannouncements, and deal-announcements of both preannounced and non-preannounced sales. We find that non-preannounced deals receive more positive market reactions when the selling firms have suffered from poor stock market performance in the year preceding the preannouncement or when the buyer of the assets is a private equity firm. Preannounced deals receive a similar reaction from the market when firms with lower market capitalization or highly levered firms preannounce asset sales. However, none of these determinants instigate any reaction from the market on the ultimate announcement day of the preannounced deals, which is consistent with the notion that the market does not receive much new information about the asset's characteristics on that day.

Managers have the discretion to strategically choose to preannounce and select the date because no other parties are involved yet. This discretion is absent for the managers of non-preannounced deals because of disclosures regulation and the buyer and seller have reached an agreement at the time of the announcement. Therefore, the risk of information leakage is high. Following Edmans et al. (2017), we use the timing of stock option vesting to examine managerial incentive to time preannouncement. Our finding suggests that the percentage of stock option vesting is significantly higher in a short window before asset sales preannouncements. We also investigate managerial behaviour after the asset sales preannouncement using the timing of option exercise and sell. We find that both the exercise and sell of stock options increase significantly after the preannouncement. These findings are consistent with our argument that managers opportunistically time the asset sales preannouncement.

Finally, we test the determinants of the overall market return. Here, we add the two announcement returns in preannounced deals and take the single announcement in other deals and investigate the effects of preannouncement on CAR over the three-day window [−1, 1]. We find that preannounced deals for which the preannouncement coincided with vesting stock options elicit less positive market reactions, which we interpret as follows. In the absence of managerial incentives driving the preannouncement, a preannouncement can be conducive to the selling process by, for instance, increasing the number of interested buyers and thereby improving the bidding process. However, if managerial incentives prompt the preannouncement, the market's recognition of the managers' opportunistic behavior may result in less positive market reactions. As we observe significantly lower returns at the preannouncement, clearly, the investors recognize the managers' opportunistic behaviour and restrain the manager's personal gain at the asset sales preannouncement.

The major contribution of this study is that we shed light on the asset sales preannouncement that prior studies have ignored. More specifically, we show that the current literature underestimates the market reaction to asset sales by 18% when not taking into account the reactions to the preannouncements. We also contribute to the literature on managerial opportunism. While many empirical studies document how managers opportunistically time various corporate events such as earnings announcement and stock splits (Michaely et al., 2014; Devos et al., 2015), this study empirically investigates managerial incentives to voluntary preannounce asset sales. We further add to the literature by documenting a significantly higher percentage of stock option vesting surrounding the asset sales preannouncement and a significant increase in excising and selling the stock options after the preannouncement. In addition, we document that stock market recognizes manager's motive and punish them by reducing abnormal return in the event of asset sales preannouncement coinciding with options vesting.

The rest of the paper proceeds as follows: Section 2 provides a brief literature review while Section 3 presents a description of the data and the sample. Section 4 to Section 9 presents our results and findings, and Section 10 concludes the paper.

2.2. Literature review

An asset sale is a mechanism that firms use to raise finance or to alter their scope by selling or divesting a portion of their business to new owners. The market for corporate assets is similar to other markets; a transaction ensues if a buyer and seller match. However, there is no organized market for all types of assets to be traded. A firm that wants to dispose of an asset has to search for a buyer. This search process is typically initiated by the seller who either seeks a buyer by announcing the intention to sell (asset sales preannouncement) or offers the asset for sale to an identified buyer. Alternatively, the process may start with a prospective buyer approaching the seller. We focus on asset and seller characteristics, including their motivation to sell the asset, with a minor focus on buyer characteristics because of the nature of the research question.

2.2.1 Asset Characteristics

The literature on asset characteristics can be categorized into a few key areas such as asset quality, industry cyclicality, asset performance and liquidity, asset type, relative size, and information asymmetry. An implicit assumption in earlier literature on the quality of assets to be sold is that the seller is better informed about the asset value than the buyer is (see, e.g., (Stiglitz and Weiss, 1981; Myers and Majluf, 1984). Although the legal and institutional characteristics of asset-sale markets around the world reveal a different picture that potential buyers could be better informed about the seller's asset value than the seller is (Curi and Murgia, 2020), asset quality, nonetheless, affects management's decision of which segments to divest.[19] Hege et al. (2009) theorize that asset quality determines the method of payment. Their model

[19] For example, a strategic acquirer such as a venture capitalist may be better than the founder at evaluating the future prospects of a firm's asset.

has a two-sided asymmetric information structure. Sellers of assets hold crucial private information about the intrinsic quality of the asset in terms of cash flow prospects and contingent liabilities. They initiate the selling process through competitive bidding via an auction-like process to screen buyers. Each potential buyer may also have private information about the value the asset can produce if it is conjoined with the 'buyer's existing assets. The seller then initiates bilateral negotiations with the buyer offering the highest bid and makes a counteroffer comprising buyer equity. The seller's counteroffer is deemed as a signal of asset quality that cannot be imitated by sellers with low-quality assets. Overall, their model suggests that firms selling a high-quality asset will accept equity as part of the payment for the asset due to the positive expected returns from the 'Buyer's ownership and management of the asset. Alternatively, sellers will be more likely to only accept cash if the asset is of relatively low quality in order to avoid further exposure to negative effects on equity value.

Pan et al. (2016) study the quality of firm's divested segments after CEO turnover. They find that newly appointed CEOs pursue a strategy of optimal disinvestment by selling lower-performing segments. Edmans and Mann (2017) added three new forces that affect asset sales, two of which focus on asset quality. The first is the camouflage effect that allows firms to hide the sale of a low-quality asset among the asset sales of other firms. In a market in which many firms are selling assets for operational reasons, firms with low-quality assets are able to camouflage their asset sales by selling at the same time as high-quality firms, thus camouflaging the true reason for the asset sale. The second force is the correlation effect. If equity is issued, the market infers that the equity is overvalued. This adverse market reaction affects the new issue as well as the outstanding equity of the firm. The advantage of asset sales is the existing equity need not be adversely affected by it because the sale of a low-quality asset need not imply that the rest of the firm is of low quality. A similar line of literature that studies asset performance finds that 66.8% of the firms divest the division with cash flows below the median of all segments of the firm.

However, Schlingemann et al. (2002) argue that asset liquidity is more important than that asset's performance or any other factors that determine which segment to sell. They show that the liquid assets will attract more buyers, and a firm would be more likely to sell it at or close to the net present value of its cash flows, whereas firms selling illiquid assets might be forced to sell at a discount. They measure liquidity by creating an industry liquidity index by taking the ratio of the value of the industry's corporate transactions to the value of the industry's total assets.

Another set of literature looks at macroeconomic shocks and industry cyclicality as determinates of asset sales. These factors play a pivotal role in the restructuring of firms' asset mix. Macroeconomic shock hits many economic sectors, and their level of individual cyclicality will affect the size of the capital reallocations they trigger. When a technological innovation induces specific industry shock, factors such as product market competition and financial leverage will have a prominent role in deciding which asset to dispose of and also how to price it. Shleifer and Vishny (1992) built a leverage decision model to account for an industry- or economic-wide negative demand shock that forces companies to liquidate assets. Under this setting, a firm faces credit constraints and sells off assets to service debt. However, firms from the same industry suffer from the same shock that prevents them from buying these assets. Asset sales under these conditions often involve industry outsiders. Maksimovic and Phillips (2001) suggest that firms buy assets unrelated to their core operations during recessions in an attempt to reduce risk and sell unrelated assets during economic growth.

Assets sold by firms can vary in type and value. Hite et al. (1987) define a sell-off as "the sale of a subsidiary, division, or other operating assets to a buyer for cash, securities, and/or other future consideration" These assets can be broadly categorized into two types: physical or financial assets. The sale could be one individual asset or a portfolio of real assets such as natural resources, buildings, factories, plants, and equipment. Alternatively, a firm may divest a single financial asset or a portfolio of them such as treasury securities, bank loans, equity stakes, etc. Existing research show that asset being studied varies by the type of data availability. For example, Maksimovic and Phillips (2001) use Longitudinal Research Database (LRD) to study the sale and purchase of manufacturing plants, whether in a partial segment sale, full segment sale, or M&A transaction. However, Pan et al. (2016) use asset sales data from Compustat, SDC, and Worldscope for segment and cash flow data, sale of business units data, and for international company asset sales data, respectively.

Asset size can also be an important determinant of asset sales, Prior studies show that firms are more likely to divest their smaller units. Dittmar and Shivdasani (2003) find that 68% of the firms divested their smallest segment. They also find that a divestiture is more likely to occur when the segment is small rather than when it performs poorly. Further, almost half (46.9%) of divested segments have sales of less than 10% of the firm's total sales.

Firms that operate in a diversified range of economic sectors are often susceptible to costly information asymmetries between stockholders and managers, making those firms' assets more difficult to evaluate by capital markets. When firms announce major divestitures, undervaluation is often mentioned as one of the major reasons for selling assets. Nanda and Narayanan (1999) propose a model where the diversifying cost in several lines of business arises because of asymmetric information between capital markets and management. External investors cannot clearly observe cash flows split within a conglomerate firm, so they will rationally update the overall quality of the firm as if each division's performance were industry average. This model implies that the market will undervalue the successful division and overvalue the poorly performing division, leading to the overall discounting of the selling firm. Because managers aware of which division has more or less informative cashflows, they are able to determine if the firm is over- or undervalued. Hege et al. (2009) suggest that sellers have specific knowledge about the intrinsic quality of the asset and that buyers have private information about the value they expect to generate from their management of the asset. Their model accounts for this situation in a double signalling game that will result in the settlement on a purchase price of the asset.

2.2.2 Seller Characteristics

Seller characteristics are pivotal in determining the assets to be sold. This is why asset sales literature often emphasizes these characteristics in explaining agency issues, motivation to sell an asset, and use of proceeds.

Agency costs associated with asset sales can be significant. Myers and Majluf (1984) made an early attempt to model agency conflict where the critical assumption is that asset sales are undertaken in the industries where the managers have high probability of losing their jobs. These are the industries under significant restructuring threats because of exogenous shifts in the technological or business environment. The higher the job risk, the higher the incentives for managers to divest assets. However, Boot (1992) proposed an alternative model and argued that managers are reluctant to divest assets because it indicates inappropriate prior investment choices. Therefore, alternative governance mechanisms are required to force managers to sell unproductive assets. Stulz (1990) and Lang et al. (1995) find that, due to agency problems, managers will retain sale proceeds rather than distribute to shareholders or pay off debt.

Determining the correct motivation behind an asset sale can be difficult. Various reasons can drive the managers to sell an asset. Some studies, similar to ours, attempt to identify the reason for the sale by hand collecting what has been reported, either directly from the firm or from the newspaper headlines.[20] However, one limitation of this approach is that some firms may be less likely to report motivations that may be negatively viewed by the markets, such as asset sales due to financial distress. From prior literature, Borisova et al. (2013) identified potential motivations for asset sales and use of proceed such as focus attention and resources on core business and assets, synergies through distribution or service agreement, pay or reduce outstanding debt, raise cash, distribution to preferred or common shareholders, reinvest to enhance asset quality, cost efficiency through lower operating expenses, sale to comply with regulatory requirements or antitrust approval.

2.2.3 Buyer Characteristics

Although we focus exclusively on why managers preannounce intention to sell, we briefly discuss the buyers' characteristics because it has received considerable attention in the literature and, thus, is important as control variables.

Although asset seller is assumed to be more informed about the asset they intend to sell, buyers may be able to value the asset accurately, especially when from the same industry (Hite et al., 1987). Unlike equity investors who have to value claims on the firm, the Buyer is likely to have a comparative advantage in valuing the asset. Further, anticipated synergies for the Buyer can determine asset value. Kaplan and Weisbach (1992) find that roughly 43% of divestitures are sold to buyers in the related industry. It is also advantageous for asset sellers because the Buyer and the asset are often better fit, and the Buyer is more likely to pay a higher premium (John and Ofek, 1995). This positive synergy also translates to the share market. Amira et al. (2013)

20 See also, (Lang et al., 1995).

find that the cumulative abnormal returns for their sample of asset buyers are positive and significant at 1.58% over a 3-day window, indicating that asset purchases enhance buyer firm value. Borisova et al. (2013) also find positive abnormal returns for buyers in asset sales, but they find that returns are larger for the seller than for the Buyer in general.

2.2.4 Asset Sell-Off Preannouncement

Prior literature agrees that there is a substantial price appreciation at the announcement of asset sales for the selling firm. This is because this announcement indicates that resources are being reallocated to higher-valued uses. Alexander et al. (1984) state that firms' voluntary asset sell-off can be viewed as a positive-net-present-value investment decision. Therefore, the announcement of such a decision should result in an upward movement in the equity price of the firm. They document substantial positive abnormal returns on the announcement date of a voluntary sell-off. Interestingly, they also find that sell-off announcements are often followed by a period of abnormal negative returns, suggesting that voluntary sell-offs typically take place after other negative information about the firm is released.

Hite et al. (1987) investigate two types of voluntary corporate restructuring: partial sell-offs and total liquidations. They document that the initial announcements of sell-offs (preannouncements) are associated with 1.5% increase in the market value of the equity of the selling firms. Successful sellers augment the initial gains with positive abnormal returns when they announce the completion of the sell-offs (deal announcement). However, if the asset sale is unsuccessful, the selling firm loses the total initial gains. This is more pronounced when subsequent bids do not follow the terminations.

Boot (1992) argues that one should distinguish the announcement of an intention to divest (preannouncement) from the announcement of an actual divestiture (deal announcement). They show that potentially distorted managerial incentives often lead them to hang on to projects that should be divested in the interest of value maximization. A manager is disinclined to sell an asset because the announcement of an unanticipated divestiture signals to the market that he initially made a poor project choice which adversely affects perceptions of his ability. This is clearly bad news, and the market punishes the selling firm upon announcement of the intention to sell. However, the announcement of the actual divestiture is good news because it indicates the presence of a compatible user and is followed by a positive market reaction.

2.2.5 Voluntary Disclosure and Managerial Opportunism

An opportunistic manager is one who makes decisions for their personal benefit rather than the benefit of the company. The decision is advantageous as well as well timed. The persistence of managerial opportunism is facilitated by information asymmetry. Agency theory contends that information asymmetry between managers and shareholders generate the potential for managers to perform opportunistically at the expense of the latter group. Increased information asymmetry translates into secondary market stock prices and investors react by adding a discount to the stock price to mitigate these concerns (Corwin, 2003). However, managers often use the timing and the content of information announcements to their advantage and alter market reaction to their favour.

Releasing more information, nonetheless, involves cost. For example, by disclosing more information the firm runs the risk of divulging its trade secret known as proprietary information. These costs may also heighten the investors' concern that the competitor will use this proprietary information for their benefit which will lead to the disclosing firms' performance decline. Therefore, competitive dynamics theory argue that (1) managers do not want to disclose information for the fear of incurring the proprietary cost (Chen and Miller, 2015) and (2) when they do disclose information, they do so to shape the perception and reaction of it's competitors (Gao et al., 2016).

From the above discussion, managers voluntarily disclose information only when the benefit of doing so out weight the cost. Prior literature document that managers strategically time the voluntary disclosure of information about the major corporate events such as stock split, earning announcement or seasoned equity offering to maximize their personal gains. For example, there is evidence that these announcements coincide with CEO stock option grants or vesting (Aboody and Kasznik, 2000; Devos et al., 2015). Stock options are usually granted with a fixed exercise price equal to the stock price on the award date and at this price the options vests to the managers. If they can influence the timing of a grant, they might therefore time it to occur (i) after an anticipated stock price decline, (ii) after a recent price decline not perceived to be justified by company fundamentals or (iii) before an anticipated stock price increase. In any of these cases, the opportunistic behavior by managers should manifest itself in stock price decreases before stock option grants (Lie, 2005). However, investors often recognize this opportunism and punishes the managers by reacting adversely in the stock market. For example, Holderness and Pontiff (2016) find that in case of right offering, the market rationally infers negative information about a firm conducting a nontransferable rights offering and reacts negatively at the onset of its announcements.

2.3. Data and Sample Selection

We draw our sample from the Mergers and Acquisition database available from the Securities Data Corporation (SDC). We select all completed divestitures from January 1st, 2005 to December 31st, 2019 by public firms incorporated in the U.S. Following previous studies (e.g., Schlingemann et al. (2002), we exclude deals of regulated utilities (SIC 4900-4999) and financial firms (SIC 6000-6999), deals where the acquirer and either target or selling firm are the same, deals designated as being part of a bankruptcy procedure, and deals that are not asset sales.[21] This leads to a preliminary sample of 5784 deals. We match this sample with Compustat (annual, quarterly, and segment files), Center for Research in Security Prices (CRSP), IBES, and Execucomp and require the data necessary to construct the variables of interest is not missing.[22] We further require that the relative deal size, defined as the proportion of deal value to the market value of equity nine months prior to the deal, is between 5% and 95% unless the deal value is higher than $1 billion. These steps reduce our sample to 1021 deals.

We then manually look up the deals in Factiva, most importantly to determine whether the selling firm has preannounced the intention to sell the asset in question. We use information retrieved from Factiva and clean the sample in the following ways: (1) we confirm that the date the deal announcement was made public as reported in SDC, (2) we verify that the deal is an asset sale, (3) we confirm that the preannouncement was made voluntarily, which entails that we drop deals that were preceded by rumors, were mandated by the FTC[23]or were part of a bankruptcy[24], (4) we drop deals that coincide with other major events other than quarterly earnings announcements (e.g., acquisitions by selling firm), and (5) we drop deals that were part of a general divestiture plan.[25] Next, we link the sold asset to its reported segment using

[21] More specifically, we exclude deals where the acquisition technique is designated as “Joint venture”, “Sale and Leaseback”, “Carveout”, “Asset Swap”, “Pooling”, “Reverse Takeover”, “Reverse Morris Trust”, “Internal Reorganization”, or “Spinoff”.

[22] We use the CRSP file “stocknames” to link the CUSIP identifier provided by SDC to the identifiers used in the WRDS datasets.

[23] In order to approve a merger, FTC often demands that a party to the proposed merger divests operations where the combination would otherwise gain too much market power. In these cases it is public knowledge which assets are to be divested, while the seller has not voluntary offered this information. Also, the information on the deal cannot be disentangled from the consequences of the merger that given the asset sale can follow.

[24] It is mandated by the Chapter 11 proceedings to publicly look for potential buyers, even for assets that are already pursued by potential buyers. The same arguments as above dictate the omission of these deals.

[25] This is the case when a firm announces plans to divest a certain dollar amount of asset sales, without specifying which assets will be sold. Generally, these plans involve the sale of multiple assets. Given the substantial dollar amounts that are involved, these plans generate large market reactions. Empirically, this poses a problem as the

10-K fillings available on EDGAR and drop deals for which this is not possible. We also drop deals where more than one event occurs on the same day (i.e., the preannouncement of one deal and deal announcement of another), where the preannouncement and deal announcement are less than 20 days apart, and where we do not have the necessary data to construct the variables of interest.[26] Finally, we drop deals where the three-day cumulative announcement returns are higher (lower) than 50% (-50%). This procedure leads to a final sample to 635 deals, of which 201 are preannounced.

In Figure 1, we depict the annual distribution of the number of deals over the sample period 2005-2019, delineated by whether they were preceded by the preannouncement of asset sales. The results imply that the number of non-preannounced deals over the years is much more stable than the number of preannounced deals. Stating from the year 2010, the number of preannounced deals dropped substantially in comparison to prior years before rising again from 2014.

[Insert Figure 1 here]

2.4. Variable Description and Summary Statistics

We categorize asset sales into two types: (1) preannounced deals and (2) non-preannounced deals. Preannounced deals have two events: (1) public announcements of intended deals (preannouncements) and (2) definitive agreement of the deal (deal announcement). Non-preannounced deals only have one event, deal announcement. This is depicted in Figure 2.

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market's reaction to the sale of a certain asset cannot be disentangled from other assets that are sold as part of the same plan.

[26] While we already applied this screen in relation to the deal announcement, given that some pre-announcements relate to other fiscal years, it happens that in some cases we do not have sufficient data to construct all necessary variables.

To measure the market reaction to an asset sale, we construct the three-day CAR [-1; +1] window for the selling firm around preannouncements (PreAnn CAR) and deal announcements (Deal CAR). For preannounced deals, we also sum the CAR of both events to capture the total market reaction (Total CAR). In line with conventional event-study methodology, we use the market-model specification with the CRSP value-weighted index as the market portfolio, with market model parameters estimated over the window from 252 to 46 trading days prior to the event.

Our empirical investigation starts with the deal-level variables. Our primary variable of interest is PreAnn is an indicator variable that takes on the value of one in case the deal was preannounced and zero otherwise. We calculate the consideration paid in millions of U.S. dollars (Deal value) and the ratio of the deal value to the seller's market value of equity at the end of the previous fiscal year-end (Relative Size). Given that announcements may be bundled with other news, we create the indicator variables E.A. (Deal) that takes on the value of 1 if the three-day window surrounding preannouncement coincides with the annual earnings announcement, zero otherwise. E.A. (PreAnn) is an indicator variable that takes on the value of 1 if three-day window surrounding either deal or preannouncement coincides with the annual earnings announcement, zero otherwise. E.A. (Either) takes on the value of 1 if three-day window surrounding deal announcement coincides with annual earnings announcement, zero otherwise. For preannounced deals, we measure the time in days between the preannouncement and the deal-announcement (Time-to-Completion). We also create Timing that takes on the value of 1 if preannouncement and deal announcement are less than two months apart, zero otherwise. Foreign Asset is an indicator variable that takes on the value of 1 if non-U.S. asset is sold, zero otherwise. P.E. is an indicator variable that takes on the value of 1 if Buyer is a private equity firm, zero otherwise. Main Industry is an indicator variable that takes on the value of 1 if sold asset has the same 4-digit SIC code as the selling firm, zero otherwise. Related

Buyer is an indicator variable that takes on the value of 1 if Buyer has the same 4-digit SIC code as sold asset, zero otherwise. Nr. Potential Buyer indicated the number of firms in sold asset's industry that have a credit rating.

We create several variables aimed to capture the characteristics of the sellers. Size indicates natural log of market value of equity, Nr. Analyst denotes the number of analysts following the firm, Leverage is the ratio of total debt to book value of total asset, Ext. Fin. Dependence is the Measure of a firm's need for external finance based on Rajan and Zingales (1998). Tobin's Q refers to the firm's demand shock. Pre-BHAR is the buy and- hold abnormal return to the selling firm is the size-adjusted buy-and-hold abnormal return for the period [-130; -2] relative to preannouncement for preannounced deals and [-230; -100] relative to deal announcement for non-preannounced deals, TCR is the credit risk score, Board Size refers to the number of board members, and Executive Ratio is the ratio of executive to total board members.

[Insert Table 2.1 here]

The mean of PreAnn indicates that 32% of asset sales are preannounced, which shows the pervasiveness of prior information dissemination by firms in the market of corporate asset sales and the empirical importance of taking into account these preannouncements. Relative size of the deals is 16%, which indicates that sold assets are usually large in values. In line with managers having more discretion regarding the timing of the preannouncement, the results in Table 2.1 indicate that 18% of preannouncements are bundled with the earnings announcement. In contrast, only 10% of deal announcements coincide with earnings announcements. Furthermore, the average preannouncement precedes the deal-announcement by 182 days. The results in Table 2.1 further show that 46% of the deals in our sample involve the sale of assets from the same industry as the seller, 31% buyers of the asset sold are from the same industry, 28% buyers are private equity firms, and, on average, an asset to be sold have 28 potential buyers.

2.5. Why do firms preannounce?

2.5.1 Bivariate Analysis

As the first step in our analysis of the determinants of preannouncement, we compare deal characteristics (Panel A), firm characteristics (Panel B), stated motives of the selling firms and use of proceeds (Panel C), as well as proportions of deals across the industry (Panel D), for the two deal types and report the results in Table 2.2. Importantly, from Panel A, we find that on average, preannounced deals are 2.2 times larger than non-preannounced deals. This is in line with attempts to increase the pool of earnest buyers. A key determinant of the number of potential buyers is the financial ability of potential buyers to acquire a selling firm's assets, which is negatively related to the size of the intended deal (Shleifer and Vishny, 1992). The difference in the relative size of the deals, however, is statistically indistinguishable from zero. Furthermore, the significant difference between the deal values implies that the value-weighted proportion of preannounced asset sales equals 51%.

[Insert Table 2.2 here]

The results in Table 2.2 further indicate that preannounced deals more often involve selling foreign or non-US. This finding supports the expectation that preannouncements are instigated by improved prospects in the selling firm's remaining operations. Furthermore, private equity firms are major buyers when asset sales are preannounced. Asset sales are often preannounced conjointly with earning announcements. On the contrary, non-preannounced assets are purchased more often by the within-industry buyers indicating the ease of negotiating with an informed buyer diminishes the necessity of preannouncement.

Panel B shows that preannounced deals often involve significantly more analyst following, higher return on assets, highly levered firms, newly appointed CEOs, and better corporate governance, as shown by significantly larger board size. Further, we find that firms that preannounce their asset sales have worse stock performance prior to the preannouncement than their non-pre-announcing counterparts. More specifically, the results show that preannouncing firms underperform non-pre-announcing firms by a statistically and economically significant 4%. Overall, we find that preannounced asset sales involve larger deals in absolute value conducted by firms with more unsatisfactory stock performance and more improved prospects in their remaining operations.

Panel C lists the stated motives and use of proceeds (UoP) across these two deal types. We use the definition of Borisova et al. (2013) to construct these variables, and the detailed definitions are provided in the variable list in Appendix A2**.** The results show that 70% of the firm that preannounced mentions focus as the primary motive to sell assets. Specifically, the firms indicate that they will focus on core operations by selling non-core or non-strategic assets. One typical example is Blucora, Inc., a leading provider of technology-enabled financial solutions to consumers, small businesses and tax professionals. After announcing sell of its Infospace business on 05 July 2016, John Clendening, President and Chief Executive Officer of the firm said " With this sale, we will monetize a non-core asset, allowing us to pay down debt […]"(Dow Jones Newswires, 2016). However, 26% of the preannouncing firms and 45% of their non-preannouncing counterparts do not mention any motive for selling the assets. As for the use of proceeds, 60% of the preannouncing firm mentions raising cash as the reason for selling assets as opposed to 36% non-preannouncing firms, 19% of firms preannouncing firms sell assets to buy back equity. However, 25% of non-preannouncing firms do not disclose the use of proceed while selling assets.

Panel D lists the proportion of deals across the industry using one-digit SIC code as per announcement type. Among all the industries, firms in construction and wholesale trade industries preannounce assets sales more. On the contrary, firms in the agriculture, services, and manufacturing industries rarely preannounce asset sales.

2.5.2 Probit Regressions

We estimate the following Probit regression as the second step in our analysis of the determinants of preannouncement:

$$Probit(PreAnn_{i,t}) = \alpha + \beta\ determinants_{i,t} + \theta_k + \delta_t + \varepsilon_{i,t} \tag{2.1}$$

Where $PreAnn_{it}$ is the outcome variable that takes the value of 1 if the firm i preannounces asset sales at year t, α is the intercept, β_i denotes the coefficients of the determinants of asset sales identified by prior literature, δ_t and θ_k are time and industry fixed effects, respectively, and ε_{it} is the error term.

Table 2.3 shows estimations of the determinants that induce a manager to disclose an intended transaction. We run our Probit specification in stages. In specification (1), we only include seller and deal characteristics. In subsequent specifications, we add asset characteristics, managerial characteristics, buyers' characteristics, and governance variables. As the estimated coefficients across the specifications, i.e., specification (1) to (6), are essentially identical, we will discuss the results of specification (6).

[Insert Table 2.3 here]

From Table 2.3, we find that Size and Relative Size have significant effects on the decision to preannounce assets sale. The positive and significant coefficient of Size indicates that larger firms are more likely to preannounce the intention to sell an asset. We offer two explanations for this. First, it is relatively less costly for large firms to provide disclosures (Bamber and Cheon, 1998). Second, due to our sample selection criteria (i.e., deal value is required to be at least 5% of the seller's market value of equity), assets sold by firms in our sample are larger. As there are fewer potential buyers for large assets, the benefits of a preannouncement may be higher for larger firms. We also provide two explanations for the positive coefficient of Relative Size. First, the importance of informing investors in a timely fashion is positively related to the materiality of the information, which is, in turn, increasing the relative size of the firm's operations that are discontinued. Second, the relative size of the asset sale is likely to be positively related to the expected improvements in the remaining firm's operations. That is, in case the sold asset is the culprit to the negative past performance of the selling firm, the improvement post-sale should be increasing in the size of the sold asset. In case the asset is sold due to improved prospects of the remaining operations of the firm, the willingness to sell a large portion of the firm is both a stronger signal as well as a larger influx of capital which can be used to finance future growth. The positive and significant coefficient of Foreign Asset also supports the notion that when firms sell non-U.S. assets, it's more likely to preannounce to signal the intention to improve or focus on its existing domestic operation.

The estimated coefficient of New CEO is positive and significant. This is also consistent with the findings of Weisbach (1995), who suggests that newly appointed managers may have a strong incentive to dispose of any poor performing assets that their predecessors had invested in. This is because any accounting write-downs on these assets will lower the benchmark against which the future performance of the newly appointed managers is evaluated, potentially increasing the amount of performance-related compensation of the manager. Pre-BHAR is negative and significant (-0.908, t-value: -3.23). This is in line with the argument that managers that sell a part of the firm that contributes to the poor past performance have an incentive to promptly inform markets of this. This signals that the firm is actively reallocating its assets to the best possible use that will contribute to wealth maximizing in the future. Further, firms also preannounce sales when the number of potential buyers in the same industry is low, indicating that firms inform potential buyers about their intention and also reduce buyer's search costs. Interestingly, the number of analysts following only plays a role as a determinant of preannouncement when the potential buyers are also included in the model.

2.6. Wealth effects of asset sales

In this section, we report and compare the stock market's reaction to the preannouncement and deal announcements. Panel A of Table 2.5 reports the average cumulative abnormal return to the deal-announcement for the entire sample, i.e., both the preannounced and non-preannounced deals. The magnitude of the market's reaction (1.57%) is similar to those reported in other studies (Borisova et al., 2013), confirming that asset sales evoke a positive reaction by shareholders. However, when we compare the returns to preannounced and non-preannounced deals, we find that the deal announcement return of the preannounced deals (1.93%) is higher than those that accrue to the non-preannounced deals (1.92%). When we further segregate preannounced deal returns into deal-announcement and preannouncement returns, we find that the preannouncement return (1.12%) is higher than the deal-announcement returns of the preannounced deals (0.81%). Anecdotal evidence also supports this notion. For example, when oil and gas producer Penn Virginia announced its intention to sell the East Texas assets on 26 February 2015, its share price rose as much as 15 % in morning trading. However, on the deal announcement the share price rose by 7.5 % at closing. The difference between the market's reaction to the preannouncement and deal-announcement translates into an underestimation of market reaction to the full sample of asset sales of 18.43%. It entails that, markets consider the disclosure of the intention to sell to be value-relevant news and deem the completion of the deal as very likely as they incorporate over 58% of the total effect on the preannouncement date.

[Insert Table 2.5 here]

Despite the market's larger positive reaction to the preannounced deals, the total return (preannouncement return + deal-announcement return) of the preannounced deals is not significantly smaller than the market's reaction to non-preannounced deals, both economically (1.93% vs. 1.92%) and statistically (do not differ significantly from zero). The results of the statistical tests of these comparisons are reported in Panel B.

In panel C, we compare the cumulative abnormal returns if either preannouncement or deal announcement event coincides with annual earnings announcements. We observe no significant difference in the abnormal returns for these events. Additionally, in Panel C, we examine the cumulative abnormal returns of the event when insiders' stock options vest in the [-21, 21] window surrounding preannouncement. The preannouncement CAR is positive, and the deal announcement CAR is negative. Again, we observe no significant abnormal returns when insiders' stock options vest during this event window surrounding preannouncement.

2.7. Timing of preannouncements

To investigate whether managerial trading incentives affect the *timing* of preannouncements, we examine how they relate to the vesting, exercises, and sales of stock options. While strategic timing of preannouncements benefits insiders, if they sell after the price increase following the preannouncement, investigating insider selling directly would be hard to interpret as evidence of timing due to reverse causality concerns, i.e., insiders selling, maybe prompted by the positive market reaction to the preannouncement rather than motivate the announcement or its timing. As such, we follow Edmans et al. (2017) and use the timing of vesting of stock options that were granted at least one year prior to preannouncement. More specifically, we test whether more options vest in the month immediately before and after the preannouncement in the 22-month surrounding the preannouncement.[27] The argument here is that the time at which these options vest are determined by grants made several years prior to the announcement; it is unlikely that they are driven by the timing of the preannouncement. However, managers can benefit from the (expected) positive market reaction to the preannouncement by using their discretion in timing the preannouncement such that it either precedes or shortly follows the vesting of their stock options.[28] To corroborate that selling incentives drive the timing, we subsequently test whether more vested options are indeed exercised and sold after the preannouncement.

To run the analysis, we calculate the total number of stock options for each deal that vest in the 22 months (467 trading days) centered around the preannouncement where the CEO has been granted stock options at least a full year prior to the preannouncement. We then calculate the proportion of stock options that vest in each trading day. For example, if there is only one day during this period on which stock options of the CEO vest, this day is assigned the value of 1 (1/x), and the remaining days are assigned the value of zero (0/x).[29] We then calculate the average value of this proportion for each of the 467 trading days. We expect that if managers time preannouncements opportunistically to benefit from the positive market reaction around the time that their stock options vest, we should observe a higher proportion of options vesting

[27] Given that most granted options vest on a yearly basis, we exclude month -12 and +12.

[28] While managers generally exercise and sell their options upon vesting, they can wait a short period to benefit from the price increase due to the pre-announcement. This is in line with the evidence reported in Edmans et al (2018, Table OA1) that selling either occurs in the month of or after the vesting of options

[29] The x is the number of trading days.

around the preannouncement. We then regress the average proportion on a dummy for the -/+21 trading day window and report the results in panel A of Table 2.4.

[Insert Table 2.4 here]

Alternatively, we split this 42-trading day window into two dummies, one for the period before (-21, -1) and one for the period after (0, 21) the preannouncement. While we expect more options vesting both shortly before and after the preannouncement, we apply this distinction as we do expect to find more of these vested options to be exercised and sold after the preannouncement. The results for the proportion of vested options are reported in columns (1) and (2), the results for the proportions exercised in columns (3) and (4), and the results for the proportions sold in columns (5) and (6). We effectively run the same regression in panel B of Table 2.4, with the important distinction that we do not calculate the average proportions per relative trading day but rather keep each relative trading day as the unit of analyses. This allows us to link specific characteristics, such as firm size or whether trading coincides with a blackout period or earnings announcement, to each deal. As the results reveal the same pattern, we only discuss the results on Panel A, given its more intuitive interpretation. In particular, the constant captures the expected average proportion for each day (i.e., 1/467). The positive and significant coefficients in columns (1) and (2) reveal that the proportion of stock options vesting prior to the preannouncement are almost 1.36 times larger than the average (i.e., the sum of all coefficients equals 0.558 and the average is 0.196). Notably, the results in the remaining columns (3) to (6) reveal that managers exercise and sell these higher-than-average number of vested options only after the preannouncement. Thus, by either having the preannouncement precede or shortly following the vesting of their stock options, managers can exercise and sell these after benefiting from the positive market reaction to the preannouncement.

2.8. Stock market reactions to option vesting and other determinants of asset sales

How does the stock market react to voluntary asset sales preannouncement when it coincides with managers option vesting? To rationally react to the asset sale preannouncement coinciding with option vesting, investors need to (1) know the timing of the managers' option vesting and their intent, and (2) calculate the magnitude of wealth transfer to the managers. However, this information may never be fully known to the market. Therefore, investors may react in one of the two following ways. First, investors might not be aware of the manager's option vesting and react positively at the asset sales preannouncement considering the event value maximizing for the firm. Second, investors might rationally infer the managerial motive. Therefore, irrespective of the reason to preannounce asset sales, investors infer bad news when the asset sale preannouncements coincide with managers option vesting as they anticipate a large wealth transfer to manager. They react by selling their shares upon asset sale preannouncement coinciding with option vesting which is likely to induce a negative pressure on the stock price.

To investigate the market reaction in these two scenarios, we proceed by investigating the determinants of the market reactions to announcements for each event separately by using the following Ordinary Least Square (OLS) regression:

$$CAR_i = \alpha + \beta\ Vesting\ deals_i + X\ determinants_i + \theta_k + \delta_t + \varepsilon_i \qquad (2.2)$$

where the dependent variable CAR is the cumulative abnormal returns over the three-day window $[-1, 1]$ for the firm i, α is the intercept, β is coefficient of the deal vested surrounding the preannouncement, X_i denotes the coefficients of the determinants of asset sales identified by prior literature, δ_t and θ_k are time and industry fixed effects, respectively, and ε_i is the error term. The main variable of interest is vesting deals, an indicator variable that takes on the value of 1 if insiders' stock options vest in the [-21, 21] window surrounding preannouncement, zero otherwise. The determinants of asset sales follow prior literature. In all specifications, we control for industry (at the 1-digit SIC level) and year-fixed effects. Table 2.6 presents the regression results.

[Insert Table 2.6 here]

In column 1, we show the estimates of the non-preannounced deals. We find that markets react positively when the relative size of the deal is larger, and the selling firm's stock performs more poorly in the year preceding the preannouncement. In column (2), we add the motive for assets sales and use of proceed variables. Both these variables are positive and significant, meaning that investors also welcome the asset sales when the prospect of increasing efficiency of the existing assets or lowering the amount of leverage is high. Adjusted R-squared values show that including the motive and the use of proceed variables increase the explanatory power of the model from 0.058 to 0.083.

Column (3) to (5) reports the results of the deal announcements of the preannounced deals. Consistent with the efficient market hypotheses, we observe no market reaction at this event. This is also evident from the negative adjusted R-squared. This is expected because the market does not receive any new information about the asset to be sold. Only if the market receives information about the positive synergy or the prospect of receiving cash from the deal, it reacts positively.

Columns (6) to (8) show the estimates of the preannouncement evet for the preannounced deals, and columns (9) to (11) show the results of total CAR of the preannounced deals (preannouncement + deal announcement). Since the results of these columns are broadly similar, we will only discuss the coefficients of the preannouncement events. The results show that when large firms or highly levered firms announce selling of an asset, the market reacts negatively. Further, option vesting is negative and significant, meaning negative abnormal return before option vesting surrounding preannouncement. Finally, it indicates that when managers strategically time asset sales preannouncements that coincide with their option vesting, the market rationally recognizes the manager's motive and reacts adversely during the event. This is in line with the findings of Holderness and Pontiff (2016) in the right offering. They argue that if right offers are more attractive to managers than to shareholders, shareholders react adversely upon the announcement of the right offer, refrain from participating in the offer or even sell their shares, resulting into negative announcement returns.

Interestingly, none of the motives to asset sales or use of proceed variables are significant during the preannouncement event implying that investors do not react to the stated motives or use of proceed when stated in the preannouncements as these are subject to change. Rather, they wait until the deal announcements and react to positive synergy and cash proceed as evident in column (5). The lower adjusted R-squared value in column (8) relative to column (7) also supports this notion as the model's predictive power decline.

2.9. Robustness

2.9.1. Including Additional Determinants of Preannouncements

We test the robustness of our findings by including several control variables such as segment information and the credit risk score along with the variables controlled in Table 2.3. We did not include them in the specification of Table 2.3 because these variables do not significantly determine asset sales preannouncements but substantially reduce our sample size.

[Insert Table 2.7 here]

2.9.2. Including Additional Variables as Determinants of Stock Market Reaction

We include several control variables along with the variables controlled in Table 2.6. Specifically, we include the number of analysts following as a proxy for information asymmetry, firm performance measure (ROA), Corporate governance measures such as board size and executive ratio, segment variables such as segment gone and segment ROA, and the credit rating of firms. However, one caveat is that including these variables substantially reduces our sample size. Therefore, we did not incorporate these variables in our analysis in Table 2.6. The results of corporate governance variables are noteworthy in this Table because only board size and executive ratio variables are significant. The coefficient of board size is positive, and the executive ratio is negative, implying that the market reacts positively when a firm with good governance preannounces asset sales but reacts negatively when the opposite occurs. This is consistent with the corporate governance literature. For example, in their influential paper, Gompers et al. (2003) argue that poor governance unexpectedly increases agency costs through a combination of inefficient investment, reduced operational efficiency, or self-dealing, which translates into large negative abnormal returns. On the contrary, good-governance firms enjoy the positive abnormal return. In terms of asset sales preannouncement, our results suggest the same.

[Insert Table 2.8 here]

2.10. Conclusion

We show that asset sales preannouncement is prevalent among the U.S. publicly listed firms. 32% of the asset sales are accompanied by public announcement of the intention to sell and almost always receives positive investor's reaction. We examine the managerial opportunism behind these preannouncements by using CEO option vesting. Since the exercise price of the stock options are generally set to the closing price on the day the options are granted, options grated before an assets sales preannouncement benefit managers from a lower exercise price relative to options granted after the pre- announcement. We show that managers strategically time the asset sale preannouncements. 26.6% of options vests right before the asset sales preannounces, and both option exercise and sell increase right after it. Although we find evidence of managers' opportunism through asset sale preannouncement, we also find striking evidence that investors recognize these motives. If asset sale preannouncement coincides with options vesting, the market punishes the firm by reacting adversely, which is clear from the 2.9%-point decline in a three-day CAR window.

Although we find compelling evidence about the managers opportunistic timing using asset sales preannouncement, some qualification remains. First, we cannot directly test managers' intentions or motives. It is possible that firms simply preannounce asset sales to signal private information, improve the firm's prospect, or to benefit shareholders when the firm's past stock price performance has been poor. Second, a firm might not sell assets every year, but when it does, the options vested to managers in the prior years would also gain in value. However, due to the nature of the data available, we cannot capture this phenomenon in our analysis except to estimate current option vesting. This will lead to an underestimation of our results. We leave these questions for future research.

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Appendix A2: Variable Descriptions

Variable	Description	Source
BHAR	Size-adjusted buy-and-hold abnormal return for the period between pre-announcement and deal announcement for pre-announced deals, minus the average size-adjusted buy-and-hold abnormal return for non-pre-announced deals for the same period	CRSP; Kenneth R. French data library
Blackout	Indicator variable that takes on the value of 1 if the day falls within [-46; +1] days of annual earnings announcement, zero otherwise	Compustat Fundamentals Quarterly (rdq)
Board Size	Number of board members	BoardEx
EA (Deal)	Indicator variable that takes on the value of 1 if three-day window surrounding pre-announcement coincides with annual earnings announcement, zero otherwise	Compustat Fundamentals Quarterly (rdq)
EA (Either)	Indicator variable that takes on the value of 1 if three-day window surrounding deal announcement coincides with annual earnings announcement, zero otherwise	Compustat Fundamentals Quarterly (rdq)
EA (PreAnn)	Indicator variable that takes on the value of 1 if three-day window surrounding either deal or pre-announcement coincides with annual earnings announcement, zero otherwise	Compustat Fundamentals Quarterly (rdq)
Deal Value	Value of deal	SDC
EA Day	Indicator variable that takes on the value of 1 if annual earnings announcement occurs on a trading day, zero otherwise	Compustat Fundamentals Quarterly (rdq)
Executive Ratio	Ratio of executive to total board members	BoardEx
Ext. Fin. Dependence	Measure of a firm's need for external finance based on Rajan and Zingales (1998) ((capx - oancf)/capx)	Compustat Fundamentals Annual
Foreign Asset	Indicator variable that takes on the value of 1 if sold asset has the same 4-digit SIC code as the selling firm, zero otherwise	SDC
Leverage	Book leverage ((dlc+dltt)/at)	Compustat Fundamentals Annual
Main Industry	Indicator variable that takes on the value of 1 if buyer is private equity, zero otherwise	SDC

New CEO	Indicator variable that takes on the value of 1 if CEO was appointed less than 1 year relative to deal announcement, zero otherwise	Execucomp; SDC
Nr. Analysts	Number of analists following the firm	IBES
Nr. Pot. Buyers	Number of potential buyers (number of firms in sold asset's industry that have a credit rating)	Compustat Fundamentals Annual; Capital IQ; Moodys; SDC
PE	Indicator variable that takes on the value of 1 if non-US asset is sold, zero otherwise	SDC
PreAnn	Indicator variable that takes on the value of 1 if deal was pre-announced, zero otherwise	SDC; Factiva
Pre-BHAR	Size-adjusted buy-and-hold abnormal return for the period [-130; -2] relative to pre-announcement for pre-announced deals and [-230; -100] relative to deal announcement for non-pre-announced deals	CRSP; Kenneth R. French data library
Prop. Equity	Proportion of CEO compensation consisting of equity ((rstkgrnt+ option_awards_blk_value)/tdc1) prior t0 2006, ((stock_awards_fv+option_awards_fv)/tdc1) after to 2006,	Execucomp
Related Buyer	Indicator variable that takes on the value of 1 if buyer has the same 4-digit SIC code as sold asset, zero otherwise	SDC
Relative Size	Deal value divided by market value of equity (timed at x month prior to deal)	SDC; CRSP
RoA	Return on assets (oibdp/lagged at)	Compustat Fundamentals Annual
Size	Natural log of market value of equity	CRSP
TCR Score	Credit risk score	https://joshualeeacct.wixsite.com/joshualee/data
Time to Completion	Number of days between pre- and deal announcement	SDC; Factiva
Timing	Indicator variable that takes on the value of 1 if pre-announcement and deal announcement are less than two months apart, zero otherwise	SDC; Factiva
Tobin's Q	Tobin's Q ((at - ceq)+(prcc_f*csho))/at	Compustat Fundamentals Annual

Vesting Deal	Indicator variable that takes on the value of 1 if insiders' stock options vest in the [-21, 21] window surrounding pre-announcement, zero otherwise	Thomson/Refinitiv - Insider Data; SDC; Factiva

Figure 2.1

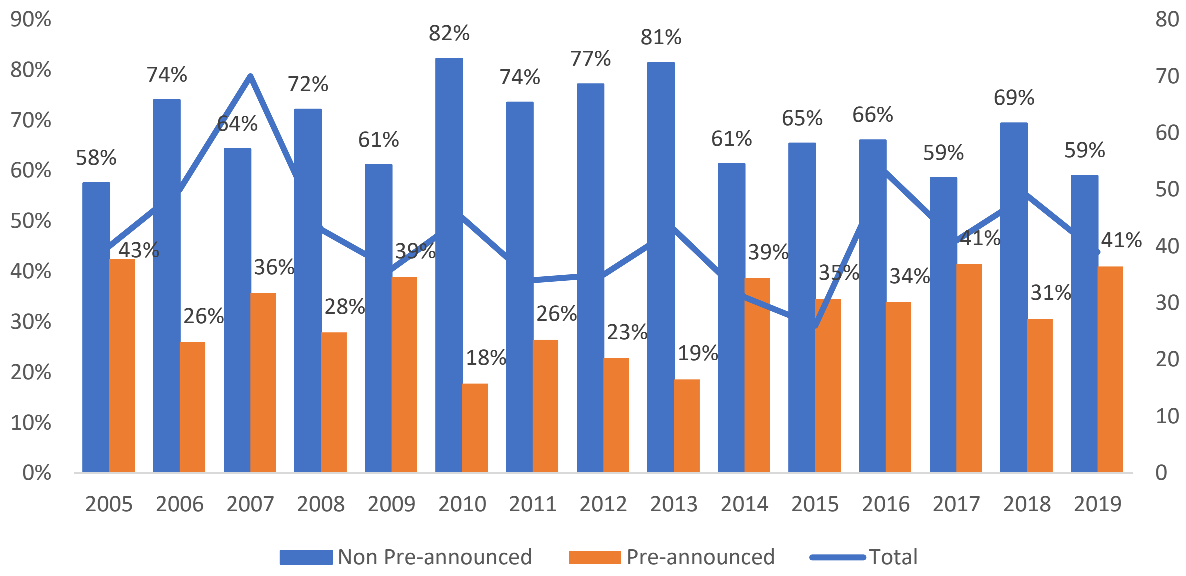

Fig. 2.1. Percentage of asset sales by deal type. The figure shows the percentage of asset sales by publicly listed U.S. firms delineated by deal type, firms that preannounced asset sales and firm that did not preannounce asset sales. The sample spans from 2005-2019. The secondary vertical axis shows the total number of assets sold each year.

Figure 2.2

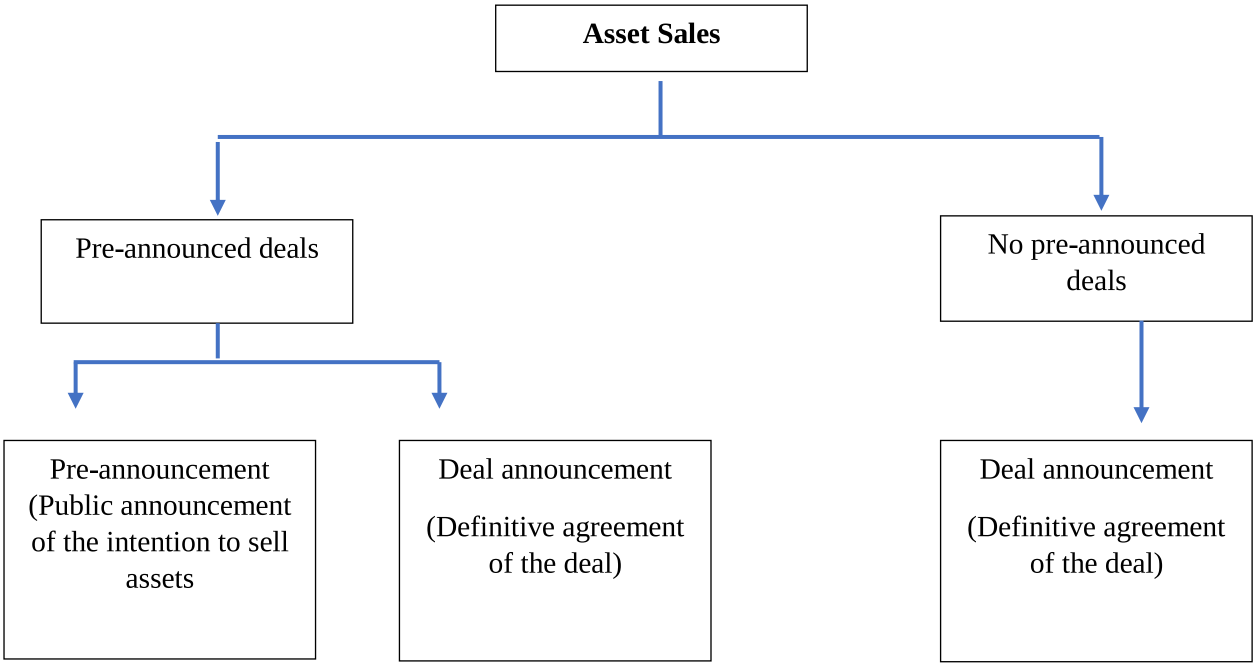

Fig. 2.2 Asset sales announcements delineated by deal type. The figure shows asset sales delineated by deal type based on the preannouncement and non-preannouncement.

Table 2.1 Summary statistics

This table reports the summary statistics of the sample, which consists of asset sales by U.S. public firms from 2005-2019. Panel A lists the deal characteristics, and Panel B lists the asset seller characteristics. All variables are defined in Appendix A1. Variables sample size varies depending on data availability.

	N	*Mean*	*StDev*	*p1*	*p25*	*p50*	*p75*	*p99*
Panel A: Deal Characteristics								
PreAnn	635	0.317	0.465	0	0	0	1	1
Deal Value	635	986.4	1976.2	12.3	115.0	343.7	1000.0	9630.0
Relative Size	635	0.163	0.153	0.004	0.064	0.108	0.207	0.734
EA (Deal)	635	0.102	0.303	0	0	0	0	1
EA (PreAnn)	201	0.179	0.384	0	0	0	0	1
EA (Either)	635	0.151	0.359	0	0	0	0	1
Time to Completion	201	182.4	140.2	27.0	89.0	149.0	238.0	553.0
Vesting Deal	201	0.159	0.367	0	0	0	0	1
Timing	201	0.080	0.271	0	0	0	0	1
Foreign Asset	635	0.154	0.362	0	0	0	0	1
Main Industry	635	0.457	0.499	0	0	0	1	1
PE	635	0.277	0.448	0	0	0	1	1
Related Buyer	635	0.310	0.463	0	0	0	1	1
Nr. Pot. Buyers	635	27.650	28.871	0	5	14	42	106
Panel B: Seller Characteristics								
Size	635	8.0	2.0	4.1	6.6	7.7	9.1	12.8
Nr. Analysts	635	11.140	7.843	1	5	10	16	35
RoA	635	0.104	0.106	-0.211	0.060	0.109	0.153	0.370
Tobin's Q	635	1.466	0.591	0.729	1.090	1.296	1.637	3.549
New CEO	635	0.172	0.377	0.000	0.000	0.000	0.000	1.000
Pre-BHAR	632	-0.030	0.269	-0.664	-0.157	-0.033	0.092	0.910
Prop. Equity	632	0.490	0.270	0.000	0.330	0.560	0.695	0.942
Leverage	635	0.315	0.212	0.000	0.187	0.303	0.431	0.940
Ext. Fin. Dependence	635	0.257	25.329	-12.788	-2.348	-0.840	0.271	11.580
TCR Score	531	-0.047	1.227	-3.188	-0.712	-0.009	0.664	3.540
Board Size	603	9.846	2.699	5.000	8.000	10.000	11.000	18.000
Executive Ratio	603	0.862	0.067	0.625	0.833	0.875	0.900	1.000

Table 2.2 Summary statistics across deal type

This table reports the difference in mean of the deal and firm characteristics of the sample delineated by deal characteristics (Panel A), seller characteristics (Panel B), stated motive and use of proceed (Panel C), and proportion of preannounced and non-preannounced deals per industry. The sample consists of asset sales by U.S. public firms from 2005-2019 as further described in the sample selection section. All variables are defined in Appendix A2. Significance levels of the two sample mean comparison tests are denoted by ***, **, and * indicating p<0.01, p<0.05, and p<0.010 levels, respectively.

Panel A: Deal Characteristics

	Preannounced		*Non-preannounced*		*Difference*	
	N	Mean	N	Mean	Mean	*p*-value
PreAnn	201	1.000	434	0.000	1	-
Deal Value	201	1587.9	434	707.8	880.1	0.000
Relative Size	201	0.170	434	0.159	0.011	0.433
Foreign Asset	201	0.249	434	0.111	0.138	0.000
Main Industry	201	0.473	434	0.449	0.023	0.584
PE	201	0.338	434	0.249	0.089	0.019
Related Buyer	201	0.259	434	0.334	-0.075	0.056
Nr. Pot. Buyers	201	26.443	434	28.210	-1.767	0.474
EA (Deal)	201	0.100	434	0.104	-0.004	0.872
EA (Either)	201	0.254	434	0.104	0.150	0.000

Panel B: Seller Characteristics

	Preannounced		*Non-preannounced*		*Difference*	
	N	Mean	N	Mean	Mean	*p*-value
Size	201	8.724	434	7.623	1.101	0.000
Nr. Analysts	201	13.572	434	10.014	3.558	0.000
RoA	201	0.118	434	0.097	0.021	0.022
Tobin's Q	201	1.515	434	1.443	0.072	0.155
Leverage	201	0.338	434	0.305	0.034	0.063
Ext. Fin. Dependence	201	-0.550	434	0.632	-1.182	0.585
TCR Score	169	-0.203	362	0.026	-0.229	0.045
Pre-BHAR	199	-0.058	433	-0.018	-0.040	0.083
Board Size	192	10.641	411	9.474	1.166	0.000
Executive Ratio	192	0.866	411	0.860	0.006	0.329
New CEO	201	0.234	434	0.143	0.091	0.005
Prop. Equity	199	0.525	433	0.474	0.051	0.026

Panel C: Stated Motive and Use of Proceeds						
	Preannounced		*Non-preannounced*		*Difference*	
	N	Mean	N	Mean	Mean	*p*-value
Focus	201	70%	434	50%	20%	0.000
Efficiency	201	14%	434	8%	6%	0.013
Synergy	201	5%	434	3%	2%	0.283
No Motive	201	26%	434	45%	-19%	0.000
Debt	201	28%	434	30%	-2%	0.682
Equity	201	19%	434	12%	7%	0.021
Cash	201	60%	434	36%	24%	0.000
Reinvest	201	40%	434	37%	3%	0.479
No UoP	201	13%	434	25%	-12%	0.001

Panel D: Proportion of Deals per 1-digit SIC Industry							
		Preannounced		*Non-preannounced*		*Total*	
SIC	*Industry*	N	%	N	%	N	%
0	Agri, Forestry & Fishing	0	0.0%	3	100.0%	3	100%
1	Mining	38	31.9%	81	68.1%	119	100%
2	Construction	65	45.8%	77	54.2%	142	100%
3	Manufacturing	45	23.7%	145	76.3%	190	100%
4	Transportation	13	37.1%	22	62.9%	35	100%
5	Wholesale Trade	17	41.5%	24	58.5%	41	100%
7	Retail Trade	17	23.9%	54	76.1%	71	100%
8	Services	6	17.6%	28	82.4%	34	100%

Table 2.3 Determinants of preannouncements

This table presents results of Probit regressions for the determinants of preannouncing an asset sales. The dependent variable in all models is PreAnn, an indicator variable that takes on the value of one in case the deal was preannounced and zero otherwise. The sample consists of asset sales by U.S. listed firms from 2005-2019, as further described in the sample section. All variables are defined in Appendix A2. The symbols ***, **, and * denote statistical significance at p<0.01, p<0.05, and p<0.010 levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Size	0.165***	0.154***	0.163***	0.144***	0.149***	0.163***
	[3.97]	[3.55]	[3.66]	[3.15]	[3.24]	[2.87]
Relative Size	1.304***	1.271***	1.320***	1.359***	1.288***	1.522***
	[3.27]	[3.16]	[3.23]	[3.33]	[3.12]	[3.53]
Nr. Analysts	0.016	0.016	0.016	0.024**	0.022*	0.020*
	[1.59]	[1.50]	[1.50]	[2.06]	[1.85]	[1.65]
RoA	0.489	0.496	0.792	0.743	0.676	0.629
	[0.80]	[0.81]	[1.25]	[1.17]	[1.06]	[0.96]
Tobin's Q	-0.032	-0.001	0.033	0.045	0.066	0.100
	[-0.30]	[-0.01]	[0.30]	[0.41]	[0.59]	[0.83]
Leverage	0.317	0.265	0.167	0.164	0.211	0.014
	[1.15]	[0.94]	[0.57]	[0.56]	[0.71]	[0.04]
Ext. Fin. Dependence	-0.001	-0.001	-0.000	0.000	-0.000	-0.000
	[-0.26]	[-0.23]	[-0.11]	[0.01]	[-0.06]	[-0.09]
Foreign Asset		0.529***	0.564***	0.553***	0.572***	0.605***
		[3.53]	[3.70]	[3.61]	[3.71]	[3.76]
Main Industry		0.098	0.078	0.105	0.097	0.173
		[0.77]	[0.60]	[0.80]	[0.74]	[1.26]
New CEO			0.389***	0.378**	0.390***	0.444***
			[2.59]	[2.51]	[2.58]	[2.79]
Pre-BHAR			-0.644***	-0.655***	-0.760***	-0.908***
			[-2.61]	[-2.63]	[-2.94]	[-3.23]
Nr. Pot. Buyers				-0.005*	-0.005*	-0.006*
				[-1.82]	[-1.78]	[-1.89]
Prop. Equity					0.272	0.263
					[1.19]	[1.11]
Board Size						0.027
						[0.86]
Executive Ratio						-1.166
						[-1.20]
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	635	635	632	632	629	597
Pseudo R-squared	0.108	0.125	0.144	0.148	0.153	0.172

Table 2.4 Wealth effects of asset sales

This table reports and compares the cumulative abnormal returns to the events related to asset sales (i.e., pre and deal announcement) in panel A and the difference in mean in the pre and non-pre announced deals in Panel B. Additionally, panel C reports and compares the cumulative abnormal returns if (either) event coincides with earning announcements. Panel D lists the cumulative abnormal returns of the event when insiders' stock options vest in the [-21, 21] window surrounding preannouncement. The sample consists of asset sales by U.S. listed firms from 2005-2019, as further described in the sample selection section. All variables are defined in Appendix A2. Significance levels of the two sample mean comparison tests are denoted by ***, **, and *, indicating p<0.01, p<0.05, and p<0.010 levels, respectively.

Panel A: Cumulative Abnormal Returns					
	(1)	(2)	(3)	(4)	(5)
Sample	*Full*	*Non Pre-Announced*	*Pre-Announced*	*Pre-Announced*	*Pre-Announced*
N	635	434	201	201	201
Variable	*Deal CAR*	*Deal CAR*	*Deal CAR*	*PreAnn CAR*	*Total CAR*
Mean	1.57%	1.92%	0.81%	1.12%	1.93%
(p-value)	0.000	0.000	0.014	0.006	0.001

Panel B: Difference CAR		
	(2) min (3)	(2) min (5)
Difference in Means	1.11%	-0.01%
(p-value)	0.030	0.504

Panel C: Cumulative Abnormal Returns if (either) event coincides with E.A.					
Sample	*Full*	*Non Pre-Announced*	*Pre-Announced*	*Pre-Announced*	*Pre-Announced*
N	65	45	20	36	51
Variable	*Deal CAR*	*Deal CAR*	*Deal CAR*	*PreAnn CAR*	*Total CAR*
Mean	1.02%	1.68%	-0.47%	0.23%	**-0.18%**
(p-value)	0.208	0.161	0.626	0.431	**0.547**

Table 2.5 Timing of preannouncement

This table presents results of the timing of opting vesting, exercising and selling surrounding preannouncement. Total number of stock options is calculated for each deal that vest in the 22 months (467 trading days) centered around the preannouncement where the CEO has been granted stock options at least a full year prior to the preannouncement. The proportion of stock options is then calculated that vest in each trading day and the average value of this proportion for each of the 467 trading days. The average proportion is then regressed on a dummy for the -/+21 trading day window and report the results in panel A. Alternatively, we split this 42 trading day window in two dummies, one for the period before (-21, -1) and one for the period after (0, 21) the preannouncement. Panel B runs the same regression with the important distinction that we do not calculate the average proportions per relative trading day, but rather keep each relative trading day as the unit of analyses. This allows to control specific characteristics, such as firm size or whether a trading coincides with a blackout period or earnings announcement. All variables are defined in Appendix A2. The symbols ***, **, and * denote statistical significance at p<0.01, p<0.05, and p<0.010 levels respectively.

Panel A	(1) *Vesting*	(2) *Vesting*	(3) *Exercise*	(4) *Exercise*	(5) *Selling*	(6) *Selling*
Constant	0.212***	0.212***	0.226***	0.226***	0.229***	0.229***
	[12.45]	[12.46]	[12.17]	[12.21]	[9.52]	[9.56]
[-21; 21]	0.195***		0.074		0.061	
	[3.47]		[1.21]		[0.78]	
[0; 21]		0.127*		0.188**		0.225**
		[1.66]		[2.26]		[2.09]
[-21; -1]		0.266***		-0.045		-0.110
		[3.39]		[-0.53]		[-0.99]
N	467	467	467	467	467	467
Adjusted R^2	2.30%	2.50%	0.10%	0.70%	-0.10%	0.80%
R^2	2.50%	2.80%	0.30%	1.17%	0.13%	1.18%

Panel B	(1) *Vesting*	(2) *Vesting*	(3) *Exercise*	(4) *Exercise*	(5) *Selling*	(6) *Selling*
Constant	0.215***	0.215***	0.218**	0.217**	0.216	0.215
	[2.59]	[2.60]	[2.12]	[2.11]	[1.59]	[1.58]
[-21; 21]	0.222***		0.124*		0.124	
	[3.70]		[1.95]		[1.46]	
[0; 21]		0.148*		0.224***		0.265**
		[1.95]		[2.79]		[2.48]
[-21; -1]		0.312***		0.005		-0.048
		[3.79]		[0.05]		[-0.41]
Relative Size	0.010	0.010	0.012	0.012	0.007	0.007
	[0.11]	[0.11]	[0.11]	[0.11]	[0.06]	[0.05]
Size	-0.001	-0.001	0.000	0.000	0.001	0.001
	[-0.07]	[-0.07]	[0.02]	[0.02]	[0.04]	[0.04]
Blackout	-0.091	-0.106	-0.100	-0.081	-0.130	-0.100
	[-1.33]	[-1.54]	[-1.38]	[-1.11]	[-1.35]	[-1.02]

EAday	0.822**	0.839**	0.040	0.008	0.153	0.110
	[2.42]	[2.47]	[0.11]	[0.02]	[0.32]	[0.23]
N	46,903	46,903	46,403	46,403	31,959	31,959
Adjusted R^2	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%
R^2	0.04%	0.05%	0.01%	0.02%	0.01%	0.02%

Table 2.6 Multivariate analysis of stock market reactions to asset sales by deal type

This table reports the OLS regressions of cumulative abnormal returns to asset sales. The sample consists of asset sales by U.S. listed firms from 2005-2019, as further described in the sample selection section. The dependent variable in columns (1) to (8) is the cumulative abnormal return (CAR) of the deal and the combined cumulative abnormal return (CAR) of the preannouncement and the deal announcement of the preannounced deals in columns (9) to (11). All variables are defined in Appendix A2. The symbols ***, **, and * denote statistical significance at p<0.01, p<0.05, and p<0.010 levels, respectively.

	Non-preannounced		*Preannounced (Deal)*			*Preannounced (Preannouncement)*			*Preannounced (Combined)*		
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Size	-0.002	-0.001	-0.002	-0.002	-0.002	-0.005*	-0.006**	-0.005*	-0.009***	-0.009***	-0.010***
	[-0.97]	[-0.50]	[-1.01]	[-1.04]	[-0.92]	[-1.96]	[-2.25]	[-1.92]	[-3.34]	[-3.65]	[-3.80]
Relative Size	0.085***	0.072**	0.011	0.010	0.011	0.057*	0.051*	0.051*	0.063**	0.057**	0.062**
	[2.97]	[2.50]	[0.41]	[0.38]	[0.44]	[1.91]	[1.72]	[1.70]	[2.22]	[2.03]	[2.22]
Leverage	-0.010	-0.023	0.016	0.015	0.016	-0.042*	-0.052**	-0.052**	-0.030	-0.040*	-0.042*
	[-0.56]	[-1.23]	[0.79]	[0.71]	[0.77]	[-1.82]	[-2.22]	[-2.19]	[-1.39]	[-1.83]	[-1.94]
Pre-BHAR	-0.029**	-0.024*	-0.001	-0.001	0.003	-0.026	-0.026	-0.032	-0.032	-0.032	-0.031
	[-2.18]	[-1.82]	[-0.07]	[-0.07]	[0.14]	[-1.18]	[-1.19]	[-1.41]	[-1.55]	[-1.56]	[-1.49]
Foreign Asset	-0.002	-0.002	0.007	0.008	0.002	-0.009	-0.009	-0.012	-0.001	-0.000	-0.009
	[-0.17]	[-0.20]	[0.79]	[0.80]	[0.23]	[-0.84]	[-0.84]	[-1.08]	[-0.09]	[-0.04]	[-0.82]
Main Industry	-0.014*	-0.016*	-0.002	-0.002	-0.005	0.003	0.003	0.006	0.001	0.002	0.004
	[-1.71]	[-1.92]	[-0.21]	[-0.20]	[-0.58]	[0.28]	[0.32]	[0.61]	[0.11]	[0.22]	[0.36]
New CEO	-0.003	-0.003	0.011	0.011	0.012	-0.002	-0.001	-0.003	-0.002	-0.002	-0.004
	[-0.33]	[-0.32]	[1.11]	[1.09]	[1.23]	[-0.17]	[-0.10]	[-0.26]	[-0.18]	[-0.18]	[-0.32]
PE	-0.011	-0.011	-0.002	-0.002	0.001				-0.006	-0.009	-0.003
	[-1.17]	[-1.11]	[-0.17]	[-0.21]	[0.06]				[-0.60]	[-0.85]	[-0.27]
Related Buyer	0.002	0.001	0.003	0.003	0.004				0.013	0.010	0.014
	[0.24]	[0.14]	[0.28]	[0.25]	[0.40]				[1.18]	[0.94]	[1.23]
EA (Deal)	-0.007	-0.009	-0.012	-0.011	-0.014						
	[-0.62]	[-0.76]	[-0.84]	[-0.84]	[-0.99]						
E.A. (PreAnn)						-0.017	-0.018	-0.017			

						[-1.41]	[-1.48]	[-1.42]			
E.A. (Either)									-0.033***	-0.033***	-0.031***
									[-3.23]	[-3.23]	[-3.07]
Vesting Deal				-0.003	-0.002		-0.026**	-0.023*		-0.027**	-0.029**
				[-0.30]	[-0.17]		[-2.00]	[-1.68]		[-2.20]	[-2.34]
Focus		-0.010			0.015*			-0.011			-0.003
		[-1.37]			[1.88]			[-1.10]			[-0.28]
Efficiency		0.033**			0.007			-0.012			-0.022*
		[2.48]			[0.39]			[-0.74]			[-1.71]
Synergy		0.020			0.051***			-0.014			0.055***
		[0.97]			[2.68]			[-0.30]			[2.82]
Debt		0.022***			0.000			0.002			-0.013
		[2.68]			[0.01]			[0.14]			[-1.20]
Equity		0.002			-0.013			0.015			-0.000
		[0.14]			[-1.13]			[0.79]			[-0.03]
Cash		0.013			0.015*			0.007			0.016*
		[1.64]			[1.67]			[0.73]			[1.70]
Reinvest		-0.007			-0.004			0.004			-0.000
		[-0.91]			[-0.48]			[0.32]			[-0.04]
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	433	433	199	199	199	199	199	199	398	398	398
Adj. R-squared	0.058	0.083	0.002	-0.003	0.036	0.077	0.094	0.080	0.122	0.131	0.155

Table 2.7 Robustness tests with segment data

This table presents results of Probit regressions for the determinants of preannouncing an asset sales in addition to the variables included in table 2.3. The dependent variable in all models is PreAnn, an indicator variable that takes on the value of one in case the deal was preannounced and zero otherwise. The sample consists of asset sales by U.S. listed firms from 2005-2019. All variables are defined in Appendix A2. The symbols ***, **, and * denote statistical significance at p<0.01, p<0.05, and p<0.010 levels, respectively.

	(1)	(2)
Segment Gone	0.102	-0.034
	[0.61]	[-0.18]
Segment RoA	-0.157	-0.044
	[-0.49]	[-0.33]
TCR Score		0.112
		[1.28]
Year FE	Yes	Yes
Industry FE	Yes	Yes
Observations	463	383
Pseudo. R-squared	0.188	0.212

Table 2.8 Robustness tests with additional variables

This table reports the OLS regressions of cumulative abnormal returns to asset sales and add variables in addition to variables included in Table 2.6. The sample consists of asset sales by U.S. listed firms from 2005-2019, as further described in the sample selection section. The dependent variable in columns (1) to (8) is the cumulative abnormal return (CAR) of the deal and the combined cumulative abnormal return (CAR) of the preannouncement and the deal announcement of the preannounced deals in columns (9) to (11). All variables are defined in Appendix A2. The symbols ***, **, and * denote statistical significance at p<0.01, p<0.05, and p<0.010 levels, respectively.

	Non-preannounced				**Preannounced (Deal)**				**Preannounced (Preannouncement)**				**Preannounced (Combined)**			
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**	**(11)**	**(12)**	**(13)**	**(14)**	**(15)**	**(16)**
Nr. Analysts	0.001	0.001	0.001	0.001	-0.001	-0.000	0.001	0.001	0.000	0.000	0.001	0.002	-0.000	-0.000	0.001	0.001
	[1.35]	[1.51]	[0.56]	[0.61]	[-0.81]	[-0.43]	[0.72]	[0.51]	[0.44]	[0.30]	[0.60]	[1.11]	[-0.34]	[-0.26]	[0.73]	[0.87]
RoA	-0.028	-0.005	0.032	0.015	-0.008	-0.001	0.017	0.009	-0.039	-0.056	-0.059	-0.011	0.005	-0.002	0.015	0.041
	[-0.66]	[-0.12]	[0.54]	[0.25]	[-0.19]	[-0.03]	[0.29]	[0.14]	[-0.77]	[-1.08]	[-0.86]	[-0.16]	[0.11]	[-0.04]	[0.24]	[0.60]
Tobin's Q	-0.000	-0.001	0.004	0.008	0.004	0.007	-0.003	-0.001	0.011	0.011	0.010	0.016	0.012	0.012	0.009	0.010
	[-0.00]	[-0.18]	[0.40]	[0.83]	[0.52]	[0.81]	[-0.33]	[-0.06]	[1.22]	[1.10]	[0.83]	[1.15]	[1.37]	[1.39]	[0.84]	[0.86]
Board Size		0.001	0.003	0.003		0.003	0.007**	0.006		0.003	0.001	0.001		0.005**	0.005	0.006
		[0.67]	[1.34]	[1.41]		[1.17]	[2.00]	[1.53]		[1.16]	[0.19]	[0.21]		[2.12]	[1.48]	[1.45]
Executive Ratio		-0.103	-0.089	-0.070		-0.146**	-0.212**	-0.260**		-0.095	-0.155	-0.130		-0.184**	-0.327***	-0.336***
		[-1.65]	[-1.15]	[-0.90]		[-2.06]	[-2.25]	[-2.31]		[-1.19]	[-1.43]	[-1.02]		[-2.49]	[-3.25]	[-3.01]
Segment Gone			-0.004	-0.006			0.017	0.006			-0.019	-0.006			-0.004	-0.015
			[-0.35]	[-0.55]			[1.53]	[0.48]			[-1.35]	[-0.40]			[-0.32]	[-1.07]
Segment RoA			0.002	-0.001			-0.002	-0.002			0.005	0.005			0.001	0.000

			[0.14]	[-0.07]			[-0.48]	[-0.48]			[1.25]	[1.14]			[0.30]	[0.11]
TCR Score				-0.004				-0.004				0.002				-0.009
				[-0.64]				[-0.65]				[0.22]				[-1.43]
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	433	410	322	264	199	190	143	117	198	190	141	122	397	380	284	240
Adj. R-squared	0.082	0.094	0.177	0.222	0.023	0.035	0.029	0.032	0.067	0.060	0.127	0.116	0.155	0.160	0.166	0.237

Chapter 3

Crime and Covenants

Abstract

Uncertainty in borrowers' actions induces creditors to increase debt covenant intensity. This paper examines whether the U.S. states' property crime rate is a source of uncertainty that induces lenders to tighten covenants as a result of increased risk. I find that greater crime exposure of the borrower leads lenders to impose more and tighter covenants. Instrumental variable analysis and various robustness tests confirm my findings. A difference-in-difference test shows that firm's relocation to a higher crime-prone state significantly increases the covenant intensity. I investigate two potential channels that drive the effect of property crime: earnings volatility and reduced collateral value of firms operating in crime-ridden states. I find that covenants and spreads are complementary factors, not substitutes, in the presence of higher property crime.

Keywords: Covenant intensity, Covenant tightness, Property crime

JEL: D81, G21, G30, G32

3.1. Introduction

Covenants are standard precautionary instruments used by lenders in corporate loan contracts to safeguard borrowers' future repayments. Jensen and Meckling (1976), Myers (1977), Dichev and Skinner (2002), Tirole (2006), Christensen et al. (2016), and Prilmeier (2017), among others, provide rationales for the presence of debt covenants in loan contracts. Their research focuses exclusively on the role of debt contract design in controlling borrowers' moral hazard by restricting their actions. This agency-based explanation is based on the assumption that the borrower has an information advantage over the lender and the borrower action is the only determinant of future repayment. However, a borrower's ability to repay a loan is a function of two indicators: the borrower's actions and outside factors such as the state of the business environment in which the borrower operates (Demerjian, 2017). Based on the assumption that the borrower and creditor have the same information set upon loan initiation, they are equally ignorant of outside shocks or uncertainty. Nonetheless, these events can affect the borrower's ability to repay a loan. Therefore, concerns beyond the borrower's action should also be important for lenders when entering a loan contract.

This study examines how the presence of an outside risk factor, property crime rates across U.S. states where firm headquarters are located, affects the debt covenant design (covenant intensity and tightness) in private corporate loans. It is motivated by recent evidence that costs associated with crime can be detrimental to firms. Higher crime levels can discourage a firm's entry into a market, domestic and foreign, its expansion, and capital expenditures (Krkoska and Robeck, 2009). Firms also suffer financial losses from reduced operations, productivity losses, loss of reputation, supply chain interruptions, and reduced employee well-being (Goldberg, 2014). Huck (2018) links crime with stock market returns and proposes crime as a measure of revealed marginal utility, providing evidence that relative wealth and crime are negatively related. Furthermore, the frequency of property crime offenses is high in the United States, with a property crime committed every 4.4 seconds and an annual cost of $16.4 billion.[30] Anecdotal evidence also suggests how costly crime can be for firms. For example, the jeans brand Diesel USA was forced to file for chapter 11 bankruptcy, and the company blamed the amount of theft it suffered as one of the reasons for its predicament.[31] A similar fate was suffered by DEP Marketing LLC, when a pair of costly thefts hampered its ability to repay loans to creditors and led the company to file for chapter 11 bankruptcy.[32]

Prior literature points out that uncertainty can hinder loan repayment and, consequently, the debt contracting design. For example, in Aghion and Bolton's (1992) model, an entrepreneur has an investment opportunity but limited capital. The creditor has the money and is willing to lend to the entrepreneur. Ex ante, the entrepreneur's optimal future action is state contingent. It is not possible for the parties to consider all possible future states ex ante and the contract based on future state contingencies. The contract is therefore incomplete and lenders must force borrowers to take correct actions based on ex post information. Demerjian (2017) extends this argument by adding a key factor that can also hinder the borrower's repayment ability: uncertainty in the outside environment in which the borrower operates. Uncertainty refers to a situation in which the future outcome is unknown and unquantifiable (Knight, 1921). Demerjian (2017) adapts the Knightian concept of uncertainty and referred to it as a future event that can affect the borrower's creditworthiness. Therefore, creditors are likely to impose a higher number of covenants in the presence of uncertainty.

[30] However, Miller et al. (2020) claim that the monetary value of the cost alone is $623 billion.

[31] Available at https://www.reuters.com/article/us-dieselusa-bankruptcy-idUSKCN1QM2DP, accessed April 19, 2021.

[32] Available at https://www.heraldtribune.com/article/LK/20070314/News/605204779/SH, accessed April 14, 2021.

Although conceptually very similar to Demerjian (2017) study, this paper focuses on quantifiable risk rather than unquantifiable uncertainty regarding the borrower's operating environment. The outside known source of risk in this study is the property crime rate in the U.S. states where the firms' headquarters are located, including burglary, larceny, and motor vehicle theft.[33]

I argue that both the lender and the borrower are aware of the property crime risk and can quantify the likelihood of crime from the past distribution of crimes, but the timing and magnitude of the event are unknown. Since covenant intensity is primarily determined by the credit risk of the borrower (Demiroglu and James, 2010), if the lender is aware of the potential risk of property crime and its ability to influence the loan repayment capacity of the firm, the covenant intensity will be greater and the covenants will be tighter.

I test the hypotheses on a large sample of covenant information on U.S. firms available from the Loan Pricing Corporation (LPC) DealScan database. The analysis starts by examining covenant intensity, defined as the number of covenants incorporated in the loan contract. Covenants are broadly classified into two types: financial covenants and general covenants. Financial covenant intensity is defined as the number of financial and net worth covenants. These covenants require firms to maintain a specific financial ratio. General covenant intensity is the number of general covenants included in a loan contract These covenants restrict other firm behaviors, such as dividend payments to shareholders, debt sweep, and collateral requirements.34 A higher number of covenants limits borrower actions that could hurt lenders

[33] This study mainly focuses on property crime because business firms are most likely to be susceptible to these crimes rather than violent crimes such as homicide.

[34] A list of financial and general covenants is provided in Appendix A.

or strengthen lender rights conditioned on adverse future events (Demiroglu and James, 2010). These restrictions are conjectured to be more important for firms that operate in high-crime states than for those in the low-crime states.

I find that covenant intensity is positively related to property crime. Firms operating in higher-crime states have more covenants in loan terms than their counterparts operating in lower-crime states. I show that both financial and general covenants are used significantly more to mitigate concerns of adverse future events. These results are economically significant and show that, if the level of property crime increases by one standard deviation, a firm will have 0.378 more financial and net worth covenants. This finding is robust to alternative definitions of covenant intensity and endogeneity. It is possible that the results suffer from omitted variable bias or reverse causality. The endogeneity concerns are addressed in two ways. First, I use two instrumental variables, namely, the rate of poverty across states and the rate of illicit drug use among individuals 12 years or older across states. Second, I use a difference-in-difference (DiD) setting to test the relation between covenant intensity and property crime when firms move to a higher- or lower-crime state from its' current location. Both of these methods support the original findings. I explore two potential channels that drive the effect of property crime: cash flow volatility and the reduced collateral value of firms operating in crime-ridden states. I find that, in the presence of higher property crime risk, spread and covenants are complementary, whereas in low–property crime states, they are substitutes. Further, collateral value affects covenants through property crime.

Additionally, I use other covenant intensity definitions widely used in the literature, such as the performance- and capital-based covenants proposed by Christensen and Nikolaev (2012) and the covenant index developed by Bradley and Roberts (2015). Christensen and Nikolaev (2012) classify financial covenants into two types, performance-based covenants and capital-based covenants. The argue that capital-based covenants align debtholder-shareholder incentives by forcing firms to maintain sufficient equity capital and thus control agency problems. In contrast, performance-based covenants serve as an early indicator of deteriorating performance that facilitates the early transfer of control and renegotiation. The covenant index of Bradley and Roberts (2015) indicates the degree of covenant intensity. This index ranges from zero to six, with a higher index indicating greater covenant intensity. I find that lenders use significantly more capital- and performance-based covenants to lower their loan risk. I also find similar results using Bradley and Roberts (2015) covenant index.

Covenant tightness can convey information to lenders about the borrower's potential for future risk shifting (Demiroglu and James, 2010) and is measured by the distance between the covenant's ratio threshold and the firm's actual financial ratio. However, this measure has limitations, since covenant definitions differ across contracts. Therefore, calculations of tightness using Compustat's GAAP-based financials and DealScan's covenant threshold could contain errors. The two financial covenants for which tightness can be measured most reliably are the minimum current ratio covenant and the maximum debt-to-EBITDA covenant (Hollander and Verriest, 2016). Therefore, I focus on the minimum current ratio and maximum debt-to-EBITDA covenants and use cluster analysis following Demiroglu and James (2010) to measure covenant tightness. Covenants are defined as tight if the covenant choice in each cluster is more restrictive than the cluster median. As predicted, I find that both the current ratio and debt-to-EBITDA ratio covenants tighten significantly when firms are exposed to higher crime rates. To test the robustness of this result, I use the probability of the aggregate covenant violation measure developed by Demerjian and Owens (2016). The results are similar to the original finding that firms operating in states with higher property crime have tighter financial covenants.

This paper contributes to the literature by studying the relation between property crime, an outside risk factors that affect firm creditworthiness and debt covenant design. Debt covenants and interest rates are the two key components that lenders use to reduce their credit and price risk. Brushwood et al. (2016) examine the impact of property crime on firms' cost of debt or interest rate. They identify crime as a systematic risk and show that firms located in states with greater property crime have more uncertain earnings and higher costs of debt and equity financing. However, the authors ignore property crime's impact on another important component of private debt contracts, the debt covenant design. This study fills this gap by showing that the presence of property crime induces borrowers to impose higher and tighter covenants. This study also contributes to the literature by departing from the agency-based view in explaining covenants in private loan contracts (Jensen and Meckling, 1976; Bradley and Roberts, 2015; Prilmeier, 2017), as well as from investigating the effect of corruption-based crime on firms, which has been extensively studied (Shleifer and Vishny, 1994). This study presents a new perspective on loan contracting design. The concern is not the borrower's moral hazard, but rather the absence of relevant information during the initial loan contract due to the presence of outside risk (Aghion and Bolton, 1992; Demerjian, 2017). This study also suggests potential channels through which property crime could affect covenants.

The remainder of this paper proceeds as follows. Section 2 details the data collection process and descriptive statistics. Section 3 presents the results for covenant intensity, while Section 4 discusses the mitigation of potential endogeneity issues. Section 5 performs additional robustness checks, and Section 6 concludes the paper.

3.2. Data, Methodology and Summary Statistics

I obtain data from several sources. The data on debt covenants on private loan contracts are from the Loan pricing Corporation's (LPC) Dealscan database. I merge the Dealscan data with the Compustat data using the Compustat- Dealscan link table from Chava and Roberts (2008). With this data, I combine the Dealscan Package data and the state-level property crime data, using firm headquarter states as its location. However, Compustat only provides the most recent headquarter location of the firm. Therefore, following prior studies, I supplement the headquarter data with SEC's EDGAR data that records the firm's actual headquarter state.[35] The study period extends from 1992 to 2018. I exclude financial firms (SIC 6000-6999) , utility firms (SIC 4900-4999), non-US firms, and the firms for which accounting and covenant related variables are missing. Following prior literature, I exclude loans that report no covenants (Christensen and Nikolaev, 2012; Demiroglu et al., 2012; Hollander and Verriest, 2016).

The data on property crime comes from the Federal Bureau of Investigation's (FBI) Uniform Crime Reporting (UCR). FBI's definition of property crime includes burglary, larceny-theft, motor vehicle theft, and arson. Theft-type crimes involve taking money or property without any force to the victim. Arson involves the destruction of property. The victim can be subjected to force FBI reports the arson data separately from the total property crime because local law enforcement agencies follow different procedures to collect this data. Therefore, to be consistent with the FBI data, we only include the data of burglary, larceny-theft, and motor vehicle theft in our definition of property crime, per 100,000 inhabitants.

[35] This extracted data from SEC's EDGAR is available at the University of Notre Dame's website.

Covenants of a firm can be affected by various firm-specific characteristics. Therefore, I use a wide range of firm-level control variables. I use natural logarithm of asset to measure size, long-term debt scaled by total asset to measure leverage, operating cashflow to asset as a proxy of cash availability, Tobin's Q to measure investment opportunity, a rating dummy to measure whether the firm is rated or not, asset tangibility to measure asset's resale value, and Altman z-score to measure the risk of financial distress.

I also control for several state-level variables. To estimate the states' strength of legal and policy environment, we use a dynamic latent variable of 148 state policies that range from social welfare to civil rights to income tax rate (Caughey and Warshaw, 2018). GDP growth rate is used as a proxy of the business cycle. I also control economic policy uncertainty, a country-level categorical variable that includes a range of sub-indexes based on economic uncertainty, and policy terms from over 2,000 US newspapers (Baker et al., 2016) . A detailed description of the variables is provided in Appendix A3.

Since the dependent variable, covenant intensity, is a count variable, I use negative binomial regression with the following equation:

$$Intensity_{i,t+1} = \alpha + \beta_1 Lnpcrime_{j,t} + \theta controls_{i,t} + \delta controls_{j,t} + \varepsilon_{i,t} \qquad (3.1)$$

Where, intensity is the different intensity measures such as financial (number of financial, net worth and tangible net worth covenants) and general covenant intensity (number of excess cash flow sweep, asset sales sweep, debt issuance sweep, equity issuance sweep, dividend restrictions sweep, insurance proceeds sweep, collateral release). i indexes firms, j indexes states, t indexes time, ln (property crime) is the natural logarithm of property crime, controls are firm and state-level control variables.

Fixed effect models conventionally assume constant firm and year effect on the dependent variable. However, this assumption will produce biased standard errors if the firm or time effect is not fixed or within-cluster variation is minimal (Petersen, 2009; Gow et al., 2010). For example, firms may not enter into a loan contract every year or the property crime rate within a state can be very slow changing overtime (the within-variation of property crime in the sample is only 0.11). Therefore, this study does not assume a constant firm or year effect. Following Cameron et al. (2009) and Thompson (2011), I adjust firm and time effects by clustering standard errors on firm and year concurrently. As this approach does not assume constant effects, the results are free from the related estimation bias (Kuang and Qin, 2013).[36]

Before discussing the summary statistics, one caveat is in order. Since this study uses the state-level property crime to measure the firm's exposure to these types of risk, one important assumption in this study is that if the state has higher property crime, the firm's operating in those states is also exposed to higher property crime. Although the association appears intuitive, it could be argued that business firms are also better positioned to safeguard themselves from this type of crime as they hold both knowledge and expertise to take measure against these crimes. However, as mentioned in the introduction, crime against business firms are substantial. To provide further assurance, I construct a simple graph (figure 1) that shows the burglary rate against commercial houses as a percent of total burglary during 2001 to 2018.[37]

[Insert figure 1 here]

[36] However, as a robustness, I use time-period fixed effect by pooling 3 year lapses. In this way we get more within variation in the time period that allow me to included both time-period fixed effect and state-time period fixed effect. The results are reported in Appendix A3.

[37] This data is available from 2001.

Figure 1 shows that commercial burglary ranges between 25% to 35 % over this period, which is high.[38] The impact of these results becomes more substantial if we assume that businesses are also in a better position to install protective mechanisms against these criminal activities than households.

Summary statistics are reported in Table 3.1 and depicts the mean, standard deviation, 25th percentile, median, and 75th percentile values of various firm-level, state-level, and country-level variables.

[Insert Table 3.1 here]

Panel A Table 3.1 shows the across the distribution, financial covenants have an average of 2.52 covenants per loan package, whereas general covenants have 2.44. Panel B of the table shows the frequency of different type of financial and general covenants in corporate loan contracts.

I also document the rate of property crime incidents per 100,000 inhabitants by the state over the period from 1992-2018. Table 3.2 reports the results.

[Insert Table 3.2 here]

Table 3.2 portrays a considerable variation in property crime across the U.S. states. The average property crime rates in these states during the study period ranges between 2162 to 4820 per 100,000 inhabitants with the lowest crime rate in South Dakota and the highest crime rate in Arizona.

[38] This is high in comparison to the 125,000 firms covered in the World Bank Enterprise Survey around the world in 2018 (does not include the U.S. firms) where 17.7% of the firms report to have suffered property crime against them. These crimes lead to about 5.6% loss in their annual sales.

I further test whether debt covenant intensity increases with the rate of property crime. To conduct this test, we divide the states into five quintiles, from lowest property crime to the highest property crime. We then calculate the average covenant intensity in each of these quintiles. Table 3.3 reports the results.

[Insert Table 3.3 here]

I find that covenant intensity increases with the increase in property crime. The average covenant intensity in the lowest crime quintile is 1.952, whereas it is 2.935 in the highest crime quintiles. A significant *t*-statistic confirms the difference in covenant intensity between the highest and lowest crime quintiles.

Table 4 documents the Pearson correlation matrix. The table indicates a significant positive correlation between the property crime and both financial and general covenants.

[Insert Table 3.4 here]

3.3. Results

3.3.1 Baseline Results

Table 3.5 presents the coefficients of the negative binomial regression. In the first four columns, the dependent variable is financial covenants (fincov), the number of financial and net worth covenants. Column (1) only includes the main variable of interest, ln(property crime). The subsequent three columns then append column (1) by adding the firm-level, loan-level and state-level control variables. The dependent variable in column (5) is general covenants (gcov) that include excess cash flow sweep, asset sales sweep, debt issuance sweep, equity issuance sweep, collateral, insurance proceed sweep, and dividend restriction sweep. The definitions of control variables are provided in Appendix A3. Continuous variables are winsorized at 1st and 99th percentiles. All the regression includes an intercept, industry fixed effects (two-digit SIC), and state fixed effects with subsequent extension with loan type and loan purpose fixed effects. The standard errors, robust to heteroscedasticity and clustered by borrower firms and year, are reported in parentheses.

[Insert Table 3.5 here]

Consistent with the prediction, the effect of property crime on covenant intensity is positive and this effect is statistically significant. This means that the lenders use more covenants as a protective mechanism to compensate for the additional risk that comes from the borrower's operating environment. According to column (5), the coefficient of ln(property crime) is positive and significant. Following convention in the literature, I report the coefficients of the negative binomial regression in the table. For economic significance, I use margins. In margin terms, for one unit increase in property crime, covenants increase by 1.42 times. I multiply the marginal effect of ln(property crime) by its standard deviation to assess the economic significance of this result. It shows that if the level of property crime increases by one standard deviation, the average firm will have 0.376 more financial and net worth covenants. Control variables also provide expected signs and are consistent with the prior literature. Unrated firms, firms with higher leverage, and more operating cash flow are likely to have more covenants in the loan contract, whereas large firms, with more investment opportunities, are likely to have fewer covenants. These results are consistent with prior studies' findings (Demerjian, 2017; Prilmeier, 2017). Column (8) provides similar results and confirms our estimates of the relationship between property crime and general covenant intensity.

3.3.2 Endogeneity of Property Crime

So far, the regression estimates suggest a positive relation between crime and covenants. But this relation could be driven by a potential endogeneity problem. Clearly, a firm’s location depends on its' individual characteristics, and it might be systematically related to unobserved determinants of debt covenant intensity. For example, Hollander and Verriest (2016) argue that firms usually have a higher covenant intensity if they borrow from remote lenders. As a result, we may infer a false causal association between covenant and property crime when there is an unobserved factor that drives both locations – and thus, crime rates – and covenants intensity. Besides, estimating the indirect price of crime also poses a concern. Firm headquarters are likely to be located in the big cities where the majority of the clients, investors and peer firms are because it facilitates the firm's business. Property crime is also likely to be higher in these areas because the pecuniary return to property crime is high (Glaeser and Sacerdote, 1996). These effects can, nevertheless, bias our findings so far. To address this problem, I use two approaches: (1) an instrumental variable approach and (2) a difference-in-differences design.

3.3.2.1 Instrumental Variable Approach

To establish whether property crime is endogenous, I turn to prior finance and criminology literature that identifies factors related to crime but unrelated to debt covenant decisions. I identify and use two instruments: the state poverty rate and the degree of illicit drug abuse. Mehlum et al. (2005) find that poverty enhances property crime, leading to inefficient economic outcomes. According to Kelly (2000), poverty significantly raises property crime. Property crime explains the economic theory of crime because the expected return for committing these crimes is higher for poor individuals. Similar findings are also reported by Garmaise and Moskowitz (2006) where they document an increase in property crime due to a reduction in economic activity through bank merger. I collect the data of poverty from the U.S. census Bureau that uses a dollar value threshold to define poverty and calculates the percentage of people below the states' threshold.

The second instrument is based on the instrument used by Brushwood et al. (2016), the percentage of illicit drug use by 12 years old or older across the U.S. states (in thousands).[39] The data is collected from the National Survey on Drug Use and Health (NSDUH). In the U.S. Department of Justice drug-related factsheet, drug use is labeled as a major contributor to crime in the U.S. states. Drugs tend to have a pharmacologic effect that stimulates the user's need of continuous use. Illicit drug users are more likely to commit property crime in need to finance undisruptive drug use (U.S. Department of Justice, 1994). In 2007, approximately 1.2 million arrests were made only for larceny-theft related crime of persons under drug influence (UCR, 2007).

To implement the instrumental variable analysis, the first-stage model is estimated by regressing the natural logarithm of the property crime rate on the two instruments and the same set of control variables from Table 3.5 in the OLS setting. In the second-stage, negative binomial regression is used, and ln(Property crime) is replaced with the fitted values obtained from the first-stage regression, and control variables from Table 3.5 are added. The results of these two instrumental variable analyses are reported in Table 3.6.

[Insert Table 3.6 here]

The first-stage results presented in column (1) show that the estimated coefficients on the instrumental variables, poverty, and drug abuse are statistically significant and have the expected positive signs. Further, the post regression *F*-statistic for the instruments' joint significance is 150.63, which provides evidence of the instruments' validity. Column (2) and (3) present the second-stage results for financial and column (4) and (5), general covenants,

[39] Brushwood et al. (2016) use alcohol consumption.

which show that firms headquartered in states with higher property crime rates have significantly higher financial and general covenant intensity.

3.3.2.2 Difference-in-differences Design

The second approach used to address the endogeneity problem is Difference-in-differences (DiD). DiD controls for unobserved variables that bias estimates. This idea of this approach is similar to the DiD design proposed by Brushwood et al. (2016) but differs methodically. Consistent with their argument, relocation decisions are unlikely to be driven by firms pursuing a lower covenant. Still, the evidence that covenant intensity changes with relocation will support that crime affects covenants. Therefore, if a firm relocates to a higher property crime state, the covenant intensity should increase. We should observe the opposite if firm relocates to a low crime state.

I define two treatment dummies depending on the relocation. For the L to H dummy, *(i)* all relocating firms are from below mean property crime states of that year, and *(ii)* the dummy is equal to one if a firm relocates to a state in which the property crime is above the mean property crime that year (treatment) and zero otherwise (control). For the H to L dummy, *(i)* all the relocating firms are from the above mean property crime states of that year and *(ii)* dummy is equal to one if a firm relocates to a state in which the property crime is below the mean property crime that year (treatment) and zero otherwise (control). Firms that have at least two-year pre and post relocation data available are included, and firms that relocated due to mergers are excluded.[40] 312 firms relocated to a high crime area, and 216 firms relocated to a low property

[40] For example, if the relocation take place at the beginning of data period or at the end of data period, these firms are excluded since they do not have enough observations to run a DiD test.

crime area during the sample period. The DiD model is implemented with the following equation, and the results are reported in Table 3.7.

$$Intensity_{i,t+1} = \alpha + \beta_1 Treat_i \times Post_{i,t} + \beta_2 Post_{i,t} + \beta_3 Treat_i + \varepsilon_{i,t} \qquad (3.2)$$

where $Intensity_{i,t+1}$ is the number of covenants for firm i during the year t; $Treat_i$ is an indicator variable that takes 1 for treatment firms, and 0 for control firms; $Post_{i,t}$ is an indicator variable that equals 1 for periods after the relocation, and 0 for periods prior to relocation; $Treat_i \times Post_{i,t}$ is a dummy variable indicating whether the outcome is observed in the treatment group and it is observed after relocation. Under the assumption that (1) treatment and control firms share parallel trends in covenant intensity prior to relocation, and (2) no other event occurred pre and post relocation, β_1 is our main variable of interest.

[Insert Table 3.7 here]

Table 3.7 shows interesting results. The primary variable of interest, Treat × Post, in column (1) to (3) show that firms that relocate to high property crime states have significantly higher covenants. However, relocation to a lower property crime state, depicted in columns (4) to (6), does not considerably affect the covenant intensity. This indicates that firms that relocate to a higher property crime state are usually regarded riskier and have a higher number of covenants on average but relocating to a lower property crime state might not reduce the covenant intensity. In the latter case, the covenant is stickier. This is also evident from the Figures 3(a) and (b) that portrays parallel trend assumptions using the marginal predictions from column (2) and (5) of Table 3.7, respectively. In Figure 3(a), we observe a parallel trend pre-treatment between firms that relocate from low crime to low crime states and firms that relocate from low crime to high crime states. After relocating, we observe an apparent increase in covenant intensity in firms that relocate to high crime states from low crime ones. In Figure 3(b), we do not observe a discernible pattern and no parallel trend pre-treatment between firms that relocate from high crime to high crime or to low crime states.

[Insert Figure 3 (a) and 3(b) here]

3.4. Potential Channels

I identify two potential channels. First, the firm operating in more criminogenic states are likely to face more earnings volatility. Lenders are likely to seek protection against such risks if these risks can affect borrowers' future repayment ability by including more covenant. Second, property crime is expected to reduce the collateral value of the firm’s asset. Lenders might impose more covenants to protect themselves due to this reason.

3.4.1 Cashflow Volatility

Brushwood et al. (2016) show that firms operating in high property crime-ridden states have higher earnings volatility and higher cost of debt. Roberts and Bradley (2015) argue that covenants are priced. If the cost of debt, measured by spread, increases, the number of covenants that a firm receives decreases. In this regard, cost of debt and covenant intensity work as substitutes. Therefore, if earnings volatility increases and spread increases, the number of the covenant should be lower. However, it is unclear ex-ante if this relation hold if property crime is introduced. If the lender considers property crime as source of risk that they cannot fully incorporate in the contracting design, they may include more covenant for the firms operating in high property crime states. In this case, spread and covenant might act as a complement rather than a substitute.

To capture whether the earnings volatility and spread channel can explain higher covenants for the firms in high property crime states, I use the interaction of three variables; cash flow volatility as a measure of earnings volatility, spread, and ln(property crime). Table 3.8 documents the results.

[Insert Table 3.8 here]

The coefficient of interaction terms of the three variables in column (1) and (2) of Table 3.8 is positive and significant. It shows that for the firm that operates in high property crime-ridden states, spread and covenant intensity work as complements. Specifically, they have a higher spread as well as higher covenant intensity than the firms that operate in low crime-ridden states. These results suggest that earnings volatility is a potential channel through which property crime affects covenant intensity.

3.4.2 Reduction in Collateral Value

The second potential channel through which property crime can increase covenant is through the reduction of the collateral value of real assets. Literature in economics has long identified crime as a major determinant of real estate prices and argues that crime drastically reduces property prices (Thaler, 1978; Lynch and Rasmussen, 2001; Schwartz et al., 2003; Troy and Grove, 2008). Therefore, if the real estate value gets eroded for firms that operate in higher crime-prone states, the value of these assets as collaterals declines as a consequence. In this situation, the lender might increase covenants as an additional protective mechanism for themselves.

This channel is tested by using the Housing Price Index (HPI) data from the Federal Housing Price Agency. This data is available for all U.S. states. Since the interaction between HPI and ln(property crime) will be used to measure the covenant intensity, a negative coefficient of the interaction term is expected. The results are reported in column (3) and (4) of Table 3.8.

The negative and significant coefficient of the interaction term in column (3) and (4) of Table 3.8 suggests that as property crime increases, covenant intensity decreases if the HPI increases and vice versa. Specifically, as property crime increases, the effect of falling housing price on covenant intensity becomes more and more positive. Therefore, these results provide some indication that collateral value is a potential channel through which property crime affects covenant intensity.

Interpreting three-way and two-way interaction terms with continuous variables, however, pose serious challenges, especially in non-linear models. Therefore, I use contour plots for both cashflow volatility and collateral channels to see how covenant intensity relates to the interaction terms. A contour plot generates a three-dimensional view and are useful for investigating outcome values and operating conditions (Luciano and Schoutens, 2006).[41]

I begin by creating a contour graph for the cashflow volatility channel that interacts property crime, cashflow volatility and spread. For the ease of interpretation, I divide property crime into two groups: high property crime, the states where the property crime is above average and low property crime, where the property crime is below average. I use predictive margins of the model to calculate the predictive covenant intensity for all combinations of cashflow volatility and spread. Figure 4 portrays the contour plot.

[Insert Figure 4(a) here]

The contour plot in Figure 4(a) shows the relation between spread and cashflow volatility in high and low crime states and the probability of covenant intensity assigned by the lender. The darker region indicates higher covenant intensity. The dominant characteristic of the contour plot is the upward concave non-linear appearance which implies strong positive interaction

[41] Specifically, contour plot represents a three-dimensional surface by plotting fixed slices of z on a two-dimensional format, connecting the (x,y) coordinates where that z value occurs.

effect. In the lower crime states, as cashflow volatility increases, the number of covenants decreases with an increase in spread, shown by gradually lighter contours implying that covenants are priced. However, when the cashflow volatility increases in high crime states, both spread and covenant intensity increases as shown by gradually darker contours implying covenants and spreads are complementary.

Figure 4(b) shows the contour plot for the collateral channel. It portrays the relation between property crime, the HPI and the predicted covenant intensity assigned by the lender. Similar to figure 3, I use the predictive margins of the model over all ranges of property crime and HPI. The darker region indicates higher covenant intensity.

[Insert Figure 4(b) here]

The downward concave non-linear appearance of the contours implies strong negative interaction effect. As the property crime increase, the HPI declines and covenant intensity increases, shown by gradually darker contours. However, when the property crime and HPI is low, so is the number of covenants imposed by the lenders. Figure 3 and 4, thus, support the conclusion drawn from the interactions in Table 3.8.

3.5. Additional analysis

3.5.1 Covenant Tightness

A major limitation of covenant tightness measures is that lenders often customize the terms and the accounting numbers to reflect the characteristics of the borrower (Demiroglu and James, 2010). Consequently, there is lack of uniformity of the same covenant across borrower heterogeneity, which implies that the covenant tightness variables reported in the Dealscan might differ from how the covenant were originally set. This increases the likelihood of measuring covenant tightness with an error. Two economically important covenants, the minimum current ratio covenant and the maximum Debt/EBITDA covenant, suggests the lowest measurement error (Demiroglu and James, 2010). Therefore, following prior literature, I focus on these two covenants.

Prior studies measure covenant tightness as the distance between the level of the covenant variable at the initiation of the loan agreement and the minimum (or maximum) covenant threshold permitted by the loan contract (Chava and Roberts, 2008). Greater distance implies greater slack in covenant tightness. However, this method does not consider the covenant offered to heterogeneous borrowers and the slack being offered. To reduce this limitation, I compare the covenant choices of borrowers of similar characteristics using cluster analysis following Demiroglu and James (2010). Clustering discovers the unknown data structure to minimize the variance within clusters and maximize the variance between clusters. Within each cluster, borrowers are sorted by their accounting ratios at the time of loan initiation and covenant threshold. Borrowers that have similar financial ratios at the time of the loan agreement should be offered similar covenants although the threshold will depend on the specific borrower characteristics. If the covenant threshold is higher than the median of that cluster, then it is classified as more restrictive and tighter. [42]

In theory, a higher the current ratio means a firm is more capable of paying its short-term obligations. A declining debt/EBITDA ratio is better as it implies that the company can pay off its longer-term debt. The theory suggests that the firm will be assigned greater control rights when monitoring and renegotiation are costlier. The investors are allocated more control rights when uncertainty about the firm's prospects are high. However, the empirical evidence

[42] Following Colla et al. (2013), I apply a stopping rule based on the Calinski/Harabasz index to select the optimum number of clusters.

regarding ex-ante adverse selection on the allocation of control rights is mixed. Therefore, it is unclear ex-ante if any or both the ratios should be tighter from a lender's perspective in the presence of property crime. However, Demiroglu (2010) finds that current ratio covenants are associated with observationally riskier loans and borrowers but the evidence on debt/EBITDA covenants and their relation to borrower riskiness is mixed. The results of the logistic regression of covenant tightness on property crime are reported in Table 3.9.

[Insert Table 3.9 here]

In Table 3.9, the dependent variable is the current ratio tightness in column (1) to (3), and Debt/EBITDA tightness in column (4) and (6). The results suggest that firms operating in high crime-prone states have significantly tighter current ratio and debt/ EBITDA covenant. In marginal terms it means that firms operating in crime-ridden areas are likely to have 0.416% tighter covenants than the firm's operating in low crime-ridden states. Firms are considered riskier when they operate in states with higher property crime rates than firms that operate in lower crime states.

I also use the alternative definition of covenant tightness developed by (Demerjian and Owens, 2016). By using simulation, they measure the aggregate probability of covenant violation in a loan package. Table 10 reports the results.

[Insert Table 3.10 here]

In Table 3.10, the dependent variable in column (1), (2) and (3) is the probability to violate financial covenants, performance covenants and capital covenants respectively. The positive and significant results reiterate our original findings that firms operating in crime-ridden states have tighter covenants.

3.5.2 Quartile Regressions

To test the robustness of the results, I use quantile regressions. Quantile regression examines the effect of the independent variables over the varying quantiles. For example, Barnes and Hughes (2002) model the stock return and test whether the conditional CAPM hold at different points of the distribution apart from the mean. They also stress that quantile regression alleviates various statistical shortcomings such as omitted variable bias, errors in variables and sensitivity to outliers. Since the argument in this study is that covenant intensity increases with property crime, quantile regression could provide helpful insight in this regard.

The quantile regression is tested on 25^{th} , 50^{th} , and 75^{th} percentiles. The expectation is that the covenant intensity will increase as the percentile increases because a higher percentile will mean higher property crime. The results are reported in Table 3.11.

[Insert Table 3.11 here]

In Table 3.11, the dependent variable in column (1) to (3) is financial covenant intensity. The results suggest that property crime has a positive and significant relation to covenant intensity. Further, as property crime increases, so does the covenant intensity.

3.5.3 Alternative Definitions

To verify the robustness of the results, I also use alternative definitions of covenant intensity suggested by prior literature, namely, covenant intensity index (covindex) proposed by Bradley and Roberts (2015) and performance covenants (P-cov) and capital covenants (C-cov) intensity proposed by Christensen and Nikolaev (2012). Covindex includes six covenants, namely, asset sales sweep, debt issuance sweep, equity issuance sweep, collateral, more than two financial covenants, and dividend restriction sweep. Performance covenants include cash interest coverage ratio, debt service coverage ratio, level of EBITDA, fixed charge coverage ratio, interest coverage ratio, debt to EBITDA, and, senior debt to EBITDA. Capital-based covenants include quick ratio, current ratio, debt-to-equity ratio, loan-to-value ratio, debt-to tangible net worth ratio, leverage ratio, senior leverage ratio, and net worth requirement. The results are reported in Table 3.12.

[Insert Table 3.12 here]

The dependent variable in column (1) of Table 3.12 is the covenant intensity index. In column (2) and (3), I repeat the test with performance and capital covenants. The results suggest that property crime has a positive and significant impact on covenant intensity, irrespective of the definition of intensity used.

3.5.4 Single-segment Firms

In this study, a firm location is measured by the location of its headquarter. However, it could be argued that if the firm operates in multiple locations, then measuring the effect of property crime in its headquarter state on covenant intensity might inflate the results. To mitigate this problem, I consider the single business segment firms only. The assumption is that single segment firms are more likely to operate locally. Although it does not fully address the disperse operation concern, it will provide some assurance about the baseline results. The results are reported in Table 3.13.

[Insert Table 3.13 here]

The positive and significant coefficients in all the columns of Table 3.13 suggest that covenant intensity is positively related to property crime. These results are similar to the baseline results.

3.6 Conclusion

The presence of outside risk factors such as property crime can affect the borrowers credit worthiness. Lenders are likely to be stricter in their debt covenant design when this risk is high. Consistent with the hypothesis, I find that firms that operate in high crime-prone states are likely to have higher covenant intensity. This result is robust to using various firm-level, state-level, and country-level control variables and different model specifications. I also address the potential endogeneity concern by using the instrumental variable approach where property crime is instrumented poverty and illicit drug use among twelve years or older. Further, I use difference-in-differences design to test how firm relocation to high or low property crime states affect the covenant intensity. I also use several robustness tests and find that the baseline results are robust to these tests.

The findings of this paper have broad implications for firms and policymakers. The results suggest that crime can significantly affect the firms' debt contract design by influencing its repayment capacity. It induces a higher number of covenants, greater earnings volatility, interest rate, tighter covenants, and lower capital expenditure for the firm. Improving the operating environment of the firm provides higher borrower and creditor protection, increases growth opportunities through sustainable investments and reduces overall cost of doing business.

Despite the strong findings, some qualifications remain. First, while we can observe that being headquartered in a crime-ridden state is likely to increase covenant intensity and tightness for firms, we might not fully capture the effect when the firm has dispersed operations. While it can be argued that the major credit decisions are taken by the firm's headquarters and thus, these average effects apply to wide range of firms, our results are likely to be understated if other branches of firms' operation undertake credit decisions. Second, we cannot fully observe the role of insurance due to data unavailability. This is because no firm reports how much insurance they claim if any incidence take place. I leave these aspects for future research.

Appendix A3. Variable description and sources

Variable	Definition	Source
Financial covenants	Number of financial, net worth and tangible net worth covenants. It includes maximum capex, maximum debt to ebitda, maximum debt to equity , maximum debt to tangible net worth , maximum leverage ratio, maximum loan to value, maximum senior debt to ebitda, maximum senior leverage, minimum cash interest coverage , minimum current ratio , minimum debt service coverage, minimum ebitda, minimum equity to asset ratio , minimum fixed charge coverage , minimum interest coverage, minimum net worth to total asset, minimum quick ratio , other ratio, net worth, tangible net worth	Dealscan
General covenants	Excess cash flow sweep, asset sales sweep, debt issuance sweep, equity issuance sweep, dividend restrictions sweep, insurance proceeds sweep, collateral release	Dealscan
Covenant index (covindex)	The index ranges from 0 to 6 and includes the following covenants: asset sales sweep, debt issuance sweep, equity issuance sweep, dividend restrictions sweep, collateral and more than two financial covenants (Bradley and Roberts, 2015).	Dealscan
Performance covenants	(1) Cash interest coverage ratio; (2) Debt service coverage ratio; (3) Level of EBITDA; (4) Fixed charge coverage ratio; (5) Interest coverage ratio; (6) Debt to EBITDA; and	Dealscan

	(7) Senior debt to EBITDA. See (Christensen and Nikolaev, 2012)	
Capital covenants	(1) Quick ratio; (2) Current ratio; (3) Debt-to-equity ratio; (4) Loan-to-value ratio; (5) Debt-to tangible net worth ratio; (6) Leverage ratio; (7) Senior leverage ratio; and (8) Net Worth requirement. See (Christensen and Nikolaev, 2012).	Dealscan
Size	Natural logarithm of firm's assets	Compustat
Tangibility	Firm's net property, plant and equipment scaled by total assets	Compustat
Cash flow/ Asset	Firm's operating cash flow scaled by total asset	Compustat
Tobin's Q	Sum of the market value of equity and debt scaled by total asset	Compustat
Leverage	Long-term debt scaled by the market value of assets	Compustat
Unrated	Dummy variable that gets a value of 1 if the firm has an S & P credit rating and o otherwise	CapitalIQ
Z-score	Altman Z score: 1.2 ((act-lct)/at) + 1.4 (re/at) + 3.3((oibdp-dp)/at) + 0.6 (mvequity/lt) + 0.999 (sale/at)	Compustat
Current ratio	Current asset divided by current liabilities	Compustat
Net worth	Total asset-total liabilities	Compustat
Tangible net worth	Current asset + net property, plant and equipment++other assets-total liabilities. See Chava and Roberts (2008).	Compustat
Loan size	Ln (Dollar amount of credit granted)	Dealscan

Loan maturity	Ln (months to maturity)	Dealscan
Revolving	1 if revolving loan exists, 0 otherwise	Dealscan
Ln (spread)	Natural logarithm of all-in drawn yield spread	Dealscan
Debt/EBITDA	Long-term debt/ EBITDA	Compustat
Pviol	Aggregate probability of covenant violation across all covenants included on a given loan package	Demerjian and Owens (2016)
Pviol_Pcov	Aggregate probability of covenant violation across all performance covenants included on a given loan package.	Demerjian and Owens (2016)
Pviol_Ccov	Aggregate probability of covenant violation across all capital covenants included on a given loan package.	Demerjian and Owens (2016)
Cash flow volatility	The standard deviation of the ratio of operating cash flow to book assets over the next 3 years relative to year t. Operating cash flow equals income before extraordinary items and depreciation and amortization.	Compustat
Property crime	Incidents of property crime (burglary, larceny, and motor vehicle theft) per 100,000 inhabitants	Uniform crime reporting, FBI
Ln (property crime)	Natural logarithm of property crime incidents per 100,000 inhabitants	Uniform crime reporting, FBI
Policy environment	A dynamic latent variable that measures the states policy liberalism based on 148 policies collected over time.	(Caughey and Warshaw, 2016)
GDP growth rate	Growth rate of Gross Domestic Product (GDP) across states	U.S. Bureau of Economic Analysis (BEA)

Poverty	Percentage of people that lie below poverty line across states.	U.S. Census Bureau
Drug abuse	Percentage of any illicit drug use among 12 years old or older across the U.S. states	National Survey on Drug Use and Health (NSDUH)
Economic policy uncertainty	Categorical data include a range of sub-indexes based on economic, uncertainty, and policy terms from over 2,000 US newspapers.	(Baker et al., 2016)
HPI	Housing Price Index	Federal Housing Price Agency

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Table 3.1 Summary statistics

Panel A

This table presents summary statistics for the variables used in the analysis. The data of covenants are collected from Dealscan, the data of the main variable of interest, property crime, is collected from FBI. All variables are defined in Appendix A3. Variables sample size varies depending on data availability.

Variables	Obs	Mean	Std.dev	p25	Median	p75
Firm-level variables						
Financial covenants	7715	2.526	1.112	2.000	2.000	3.000
General covenants	7715	2.445	2.203	1.000	2.000	4.000
Covenant index	7715	2.403	1.918	1.000	2.000	4.000
Performance covenants	7715	1.690	0.900	1.000	2.000	2.000
Capital covenants	7715	0.303	0.534	0.000	0.000	1.000
Leverage	7715	0.186	0.159	0.060	0.153	0.274
Size	7715	6.753	1.709	5.608	6.736	7.887
Cash flow/ Asset	7715	0.134	0.090	0.090	0.131	0.177
Tobin's Q	7715	1.452	1.009	0.848	1.161	1.692
Unrated	7715	0.494	0.500	0.000	0.000	1.000
Tangibility	7715	0.312	0.237	0.123	0.242	0.453
Z-score	7715	3.541	2.978	1.861	2.907	4.404
Loan size	7715	19.189	1.477	18.258	19.337	20.212
Loan maturity	7715	3.725	0.595	3.584	4.078	4.094
Revolving	7715	0.748	0.434	0.000	1.000	1.000
Ln (spread)	7715	5.056	0.716	4.605	5.165	5.521
Cash flow volatility	7652	0.054	0.152	0.012	0.025	0.054
Current ratio	635	1.810	1.134	1.015	1.535	2.34
Debt/EBITDA	4826	3.123	6.164	1.095	2.153	3.711
State-level variables						
Ln (property crime)	7715	8.148	0.265	7.947	8.169	8.350
Policy environment	7715	0.381	1.308	-0.776	0.211	1.750
GDP growth rate	7715	1.987	2.165	0.700	2.100	3.400
Poverty	5754	13.707	2.912	11.100	13.300	15.800
Drug abuse	5754	0.022	0.015	0.018	0.019	0.021
HPI	7715	463.688	186.034	338.22	400.57	554.27
Country-level variables						
Economic policy uncertainty	7715	143.061	29.828	116.483	132.812	160.830

Panel B: Frequency of financial and general covenant types

This table reports the frequency of different financial and general covenant in the loan contracts of the nonfinancial and non-utility US firms for which the covenant information is available for the period of 1992-2018.

Financial covenants	Percent	General covenants	Percent
Max. Capex	8.89	Excess cash flow sweep	15
Max. Debt to EBITDA	26.13	Asset sales sweep	31.76
Max. Debt to Equity	0.5	Debt issuance sweep	23.14
Max. Debt to Tangible Net Worth	6.01	Equity issuance sweep	19.23
Max. Leverage ratio	6.77	Dividend restrictions sweep	71.28
Max. Loan to Value	0.02	Insurance proceeds sweep	20.37
Max. Senior Debt to EBITDA	3.1	Collateral release	44.11
Max. Senior Leverage	0.07		
Min. Cash Interest Coverage	0.49		
Min. Current Ratio	5.08		
Min. Debt Service Coverage	4.24		
Min. EBITDA	3.53		
Min. Equity to Asset Ratio	0.02		
Min. Fixed Charge Coverage	16.67		
Min. Interest Coverage	16.07		
Min. Net Worth to Total Asset	0.01		
Min. Quick Ratio	2.36		
Other Ratio	0.04		
Net worth covenant	15.49		
Tangible Net worth covenant	19.22		

Figure 3.1 Commercial burglary as a percent of total burglary

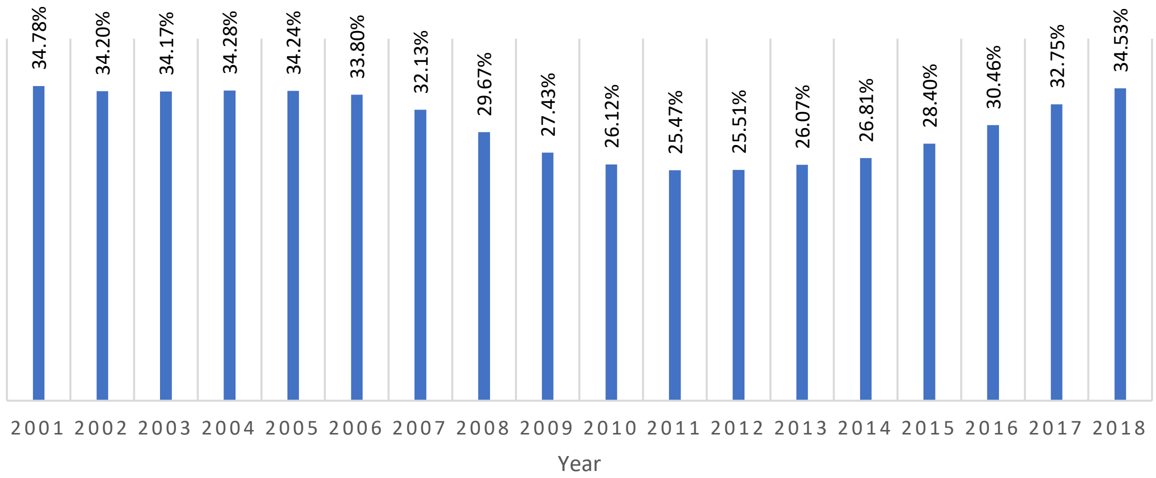

Fig 3.1 This figure plots the percentage of commercial burglary among total burglaries in the United States over 2001-2018. Commercial burglary is defined as the burglary in store, offices, and other non-resident properties. Source: Uniform crime reporting, FBI.

Table 3.2 Incidence of property crime by state

This table demonstrates the rate of property crime incidents per 100,000 inhabitants by state over the period from 1992-2018. Burglary, larceny-theft and motor vehicle theft are the averages of these crimes during this period. Total property crime is the summation of these three crimes. All the values are rounded to the nearest whole numbers. Source: Uniform Crime reporting (UCR), FBI.[43]

States	Burglary	Larceny-Theft	Motor Vehicle Theft	Total Property crime
Alabama	935	2559	288	3782
Alaska	597	2666	378	3642
Arizona	972	3164	684	4820
Arkansas	980	2528	252	3759
California	740	2030	606	3376
Colorado	656	2531	376	3563
Connecticut	530	1900	330	2760
Delaware	725	2533	291	3549
Florida	1046	3003	460	4508
Georgia	926	2781	429	4136
Hawaii	785	3237	490	4512
Idaho	534	1955	151	2639
Illinois	703	2301	313	3318
Indiana	648	2254	327	3228
Iowa	590	2002	170	2762
Kansas	768	2638	270	3676
Kentucky	641	1727	205	2573
Louisiana	1030	2952	390	4372
Maine	536	1869	100	2505
Maryland	717	2469	479	3665
Massachusetts	560	1648	338	2546
Michigan	704	2072	435	3212
Minnesota	558	2280	247	3085
Mississippi	959	2098	247	3303
Missouri	756	2584	398	3738
Montana	438	2601	216	3256
Nebraska	525	2460	288	3273
Nevada	955	2233	653	3841
New Hampshire	370	1683	119	2172
New Jersey	539	1735	349	2623
New Mexico	1117	2845	431	4393
New York	455	1734	273	2462
North Carolina	1147	2582	258	3987
North Dakota	360	1729	175	2264
Ohio	807	2343	298	3449
Oklahoma	1002	2537	366	3905
Oregon	718	3017	424	4159
Pennsylvania	449	1704	244	2397
Rhode Island	605	1938	348	2891
South Carolina	1014	2893	348	4255
South Dakota	409	1634	119	2162

43 Available at https://ucr.fbi.gov/crime-in-the-u.s/2018/crime-in-the-u.s.-2018/topic-pages/property-crime

Tennessee	968	2564	395	3927
Texas	908	2819	410	4137
Utah	611	3043	301	3955
Vermont	556	1803	97	2456
Virginia	428	2076	201	2706
Washington	893	3032	519	4443
West Virginia	560	1549	167	2276
Wisconsin	484	2084	238	2806
Wyoming	460	2375	135	2970

Figure 3.2 Quintiles of the US states in terms of average property crime over the period 1992-2018

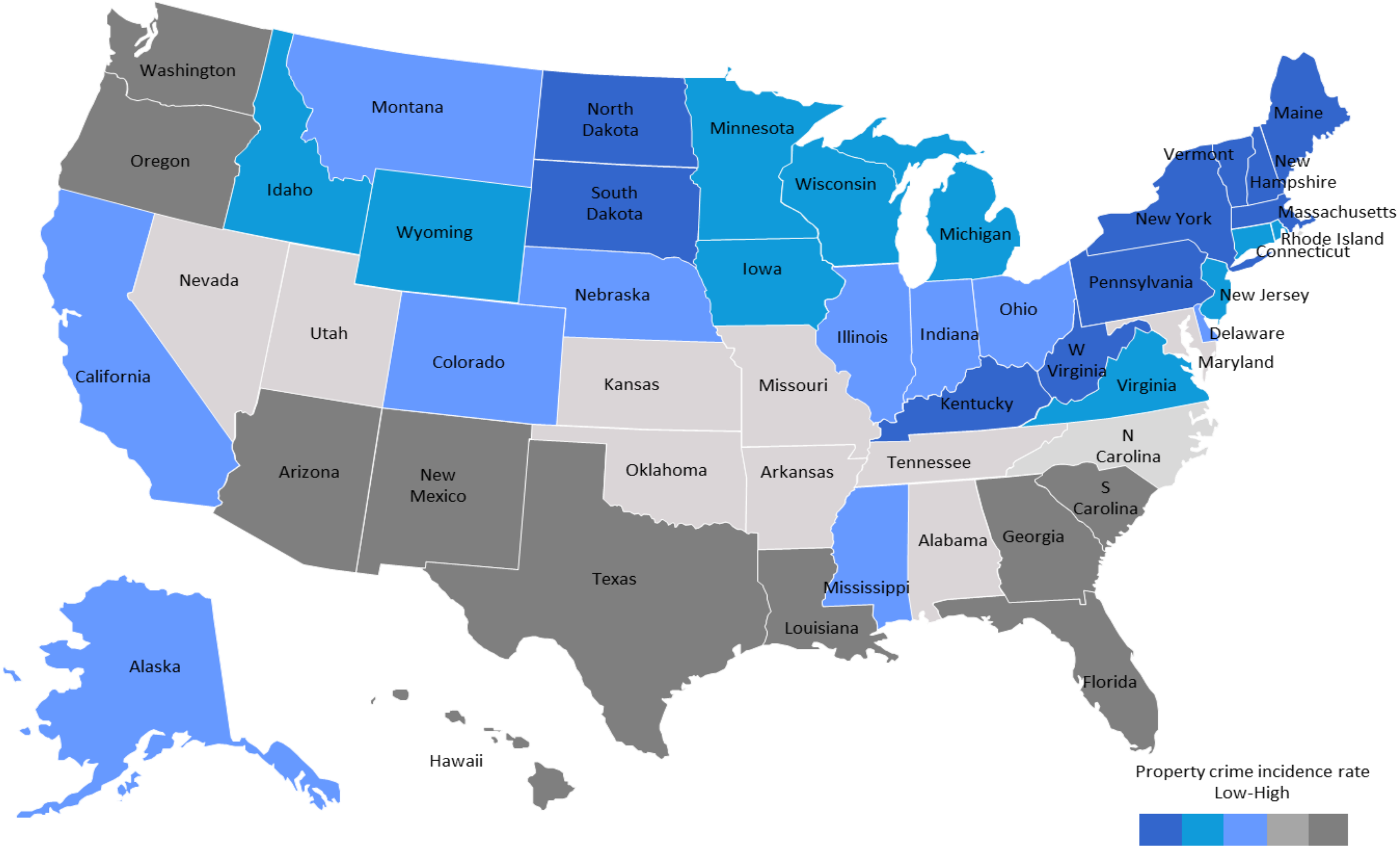

Table 3.3 Debt covenant intensity for the portfolio of firms formed on location and property crime

Table 3.3 presents the portfolio of firms partitioned into state-crime-firm observations into five financial and net worth covenant quintiles. The lowest quintile (portfolio 1) contains firms that operate in the lowest criminogenic states and the highest quintile (portfolio 5) contains firms that operate in the highest criminogenic states. The difference in mean between portfolio 5 and portfolio 1 is calculated. t-statistic is in parenthesis.

Quintiles	N	Property Crime per 100,000 inhabitants (Mean)	Covenant intensity (Mean)
1	1,747	2682.733	1.952
2	1,642	3134.985	2.340
3	1,512	3429.830	2.683
4	1,532	3591.391	2.882
5	1,282	3988.485	2.935
High covenant intensity (5)-Low covenant intensity (1)			**0.983**
t-statistics			(26.763)

Table 3.4 Pearson Correlations

This table presents Pearson correlations among the variables and indicates the significance. ***,** and * denote significance at 1%, 5% and 10%, respectively. Variable description are provided in Appendix A3.

	Variables	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**	**(11)**	**(12)**	**(13)**	**(14)**	**(15)**	**(16)**
(1)	Fincov																
(2)	Ln (pc)	0.21***															
(3)	Leverage	0.12***	0.08***														
(4)	Size	-0.40***	-0.26***	0.12***													
(5)	CF/ asset	-0.05***	0.00	-0.18***	0.07***												
(6)	Tobin's q	-0.07***	-0.00	-0.37***	-0.11***	0.41***											
(7)	Unrated	0.24***	0.09***	-0.30***	-0.65***	-0.02	0.11***										
(8)	Tangibility	-0.01	0.22***	0.25***	0.07***	0.09***	-0.10***	-0.10***									
(9)	Z-score	-0.02**	0.01	-0.54***	-0.19***	0.43***	0.69***	0.26***	-0.19***								
(10)	Loan size	-0.30***	-0.21***	0.11***	0.81***	0.16***	-0.02	-0.55***	0.08***	-0.14***							
(11)	Loan maturity	-0.00	-0.10***	0.11***	0.10***	0.05***	-0.07***	-0.08***	0.03***	-0.08***	0.30***						
(12)	Revolving	0.03***	0.01	-0.06***	-0.10***	0.01	-0.03***	0.11***	0.00	0.05***	-0.03**	0.33***					
(13)	Spread	0.27***	0.03**	0.28***	-0.32***	-0.28***	-0.22***	0.16***	0.02*	-0.22***	-0.30***	0.03***	-0.07***				
(14)	Policy	-0.00	-0.52***	-0.15***	-0.01	-0.03**	0.10***	0.07***	-0.29***	0.10***	-0.04***	-0.03***	-0.03**	-0.06***			
(15)	EPU	-0.14***	-0.28***	0.03**	0.19***	-0.03***	-0.09***	-0.08***	-0.02*	-0.07***	0.14***	0.07***	0.02	0.05***	0.01		
(16)	GDP growth	0.15***	0.17***	-0.04***	-0.20***	0.01	0.10***	0.08***	-0.01	0.10***	-0.17***	-0.12***	-0.03***	-0.04***	0.12***	-0.44***	
(17)	Gcov	0.33***	0.058**	0.28***	-0.17***	-0.08***	-0.11	0.01	-0.02***	-0.17***	0.01	0.19	-0.08***	0.45***	-0.03***	-0.06***	0.02

Table 3.5 Regression of debt covenant intensity on property crime

This table presents the coefficients of negative binomial regression that regress covenant intensity on property crime. The dependent variable in column (1) to (4) is financial covenants, a total of financial and net worth covenants. In columns (5) to (8), the dependent variable is general covenants that include excess cash flow sweep, asset sales sweep, debt issuance sweep, equity issuance sweep, collateral, insurance proceed sweep and dividend restriction sweep. The main variable of interest, Ln (property crime), is the rate of property crime per 100,000 inhabitants in the borrowers headquarter state (in Ln). The definitions of control variables are provided in Appendix A3. Continuous variables are winsorized at 1st and 99th percentiles. All the regression includes intercept, industry fixed effect (two-digit SIC) and state fixed effect. The standard errors, robust to heteroscedasticity and clustered by borrower firms and year, are reported in parentheses. ***,** and * denote significance at 1%, 5% and 10%, respectively.

	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Variables	fincov	fincov	fincov	fincov	gcov	gcov	gcov	gcov
Ln (property crime)	0.778***	0.533***	0.563***	0.552***	0.260***	0.340***	0.252***	0.225***
	(0.028)	(0.030)	(0.035)	(0.035)	(0.064)	(0.057)	(0.067)	(0.068)
Leverage		0.254***	0.245***	0.243***		0.652***	0.661***	0.668***
		(0.038)	(0.038)	(0.038)		(0.065)	(0.065)	(0.065)
Size		-0.090***	-0.086***	-0.082***		-0.241***	-0.237***	-0.223***
		(0.006)	(0.006)	(0.006)		(0.011)	(0.011)	(0.011)
Cash flow/ Asset		0.230***	0.237***	0.240***		0.265**	0.260**	0.270***
		(0.062)	(0.061)	(0.061)		(0.104)	(0.104)	(0.102)
Tobin's Q		-0.038***	-0.040***	-0.039***		-0.040***	-0.044***	-0.045***
		(0.007)	(0.007)	(0.007)		(0.013)	(0.013)	(0.013)
Unrated		0.028**	0.030**	0.030**		-0.065***	-0.061***	-0.063***
		(0.013)	(0.013)	(0.013)		(0.023)	(0.023)	(0.023)
Tangibility		-0.021	-0.022	-0.019		-0.266***	-0.255***	-0.242***
		(0.030)	(0.030)	(0.029)		(0.055)	(0.055)	(0.055)
Z-score		0.005**	0.005**	0.005**		-0.008*	-0.008	-0.007
		(0.002)	(0.003)	(0.003)		(0.005)	(0.005)	(0.005)
Loan size		0.026***	0.025***	0.018***		0.262***	0.260***	0.225***
		(0.006)	(0.006)	(0.006)		(0.012)	(0.012)	(0.012)
Loan Maturity		0.016*	0.018**	-0.012		0.203***	0.203***	0.200***

		(0.009)	(0.009)	(0.013)		(0.019)	(0.019)	(0.028)
Revolving		0.015	0.016	0.083***		-0.102***	-0.100***	-0.013
		(0.011)	(0.011)	(0.026)		(0.021)	(0.021)	(0.067)
Spread		0.104***	0.107***	0.097***			0.610***	0.592***
		(0.008)	(0.008)	(0.008)			(0.018)	(0.019)
Policy environment			0.062***	0.061***			0.037*	0.037*
			(0.011)	(0.011)			(0.021)	(0.021)
Economic policy uncertainty			0.000	0.000			-0.002***	-0.002***
			(0.000)	(0.000)			(0.000)	(0.000)
GDP growth			0.010***	0.010***			-0.001	-0.001
			(0.002)	0.552***			(0.005)	(0.005)
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Loan purpose FE	No	No	No	Yes	No	No	No	Yes
Loan type FE	No	No	No	Yes	No	No	No	Yes
Observations	7,715	7,715	7,715	7,715	7,715	7,715	7,715	7,715
Pseudo R-squared	0.021	0.041	0.042	0.043	0.013	0.112	0.114	0.118

Table 3.6 Property crime and debt covenant intensity-Instrumental variables

This table reports the results of OLS regression in the first stage and negative binomial regression in the second stage. Column 1 shows the first-stage results, where property crime is instrumented by poverty and drug abuse, poverty is the percentage of people below a dollar value threshold across states, and drug abuse denotes illicit drug abuse among individuals 12 years or older. Columns 2 and 3 present the second-stage results for financial covenants and columns 3 and 4 present those for general covenants. The definitions of the control variables are provided in the Appendix. Continuous variables are winsorized at the first and 99th percentiles. All the regressions include intercept, industry (at the two-digit SIC) fixed effects, and state fixed effects. Standard errors, robust to heteroscedasticity and clustered by borrower firm and year, are reported in parentheses. ***, **, and * denote significance at 1%, 5%, and 10%, respectively.

	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Variables	**ln(property crime)** 1st stage (OLS)	**fincov** 2nd stage (Negative Binomial)	**fincov** 2nd stage (OLS)	**gcov** 2nd stage (Negative Binomial)	**gcov** 2nd stage (OLS)
Instrumented ln(property crime)		1.193***	0.465***	1.373***	0.718**
		(0.050)	(0.180)	(0.105)	(0.355)
Poverty	0.027***				
	(0.001)				
Drug abuse	0.889***				
	(0.147)				
Controls	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	No	Yes	No	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
State FE	No	Yes	No	Yes	No

Loan purpose FE	No	Yes	Yes	Yes	Yes
Loan type FE	No	Yes	Yes	Yes	Yes
Observations	5754	5754	5754	5754	5754
Model fits:					
F-Test for joint significance	377.80***				
Test of weak identification					
Cragg-Donald Wald F statistic			437.244		437.244
Stock and Yogo (2005) 10% maximal IV size (critical value)			19.930		19.930
Test of under identification					
Kleibergen-Paap rk LM			548.369		548.369
p-value			0.000		0.000
Test of overidentification					
p-value of Hansen J statistic			0.547		0.107

Table 3.7 Firm relocation and covenant intensity-Difference-in-Difference

This table reports the results of difference-in-difference analysis of firm relocation and covenant intensity. Two treatment dummies are defined depending on the firm relocation. For the Low to High dummy, (i) all relocating firms are from below mean property crime states of that year and (ii) the dummy is equal to one if a firm relocate to a state in which the property crime is above the mean property crime that year (treatment) and zero otherwise (control). For the High to Low dummy, (i) all the relocating firms are from the above mean property crime states of that year and (ii) dummy is equal to one if a firm relocate to a state in which the property crime is below the mean property crime that year (treatment) and zero otherwise (control). Firms that have at least two-year pre and post data available are included and firms that relocated due to merger are excluded. Ln (property crime), is the rate of property crime per 100,000 inhabitants in the borrowers headquarter state (in Ln). The definitions of control variables are provided in Appendix A3. Continuous variables are winsorized at 1st and 99th percentiles. All the regression includes intercept, industry fixed effect (two-digit SIC) and state fixed effect. The standard errors, robust to heteroscedasticity and clustered by borrower firms and year, are reported in parentheses. ***,** and * denote significance at 1%, 5% and 10%, respectively.

	Low to High			**High to low**		
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Variables	fincov	fincov	fincov	fincov	fincov	fincov
Post	0.013	0.088	0.074	-0.468***	-0.514***	-0.480***
	(0.095)	(0.083)	(0.080)	(0.092)	(0.097)	(0.107)
Treat	0.153*	0.380***	0.301***	-0.065	-0.241	-0.335*
	(0.082)	(0.087)	(0.078)	(0.274)	(0.191)	(0.186)
Treat×Post	0.263***	0.509***	0.418***	0.002	-0.148	-0.185
	(0.094)	(0.109)	(0.108)	(0.229)	(0.205)	(0.206)
Leverage		-0.066	-0.000		-0.332	-0.288
		(0.220)	(0.191)		(0.265)	(0.274)
Size		-0.129***	-0.131***		-0.207***	-0.197***
		(0.030)	(0.031)		(0.044)	(0.051)
Cash flow/ Asset		-0.106	-0.025		-0.760	-1.051
		(0.500)	(0.485)		(0.785)	(0.787)
Tobin's Q		-0.074	-0.071		-0.144*	-0.086
		(0.051)	(0.051)		(0.073)	(0.081)
Unrated		0.060	0.017		-0.340**	-0.257*
		(0.130)	(0.115)		(0.149)	(0.141)
Tangibility		-0.406	-0.470**		-0.127	-0.184
		(0.258)	(0.235)		(0.280)	(0.326)
Z-score		0.003	0.000		-0.005	-0.001
		(0.012)	(0.012)		(0.038)	(0.036)
Loan size		0.038	0.051		-0.012	0.011
		(0.039)	(0.033)		(0.036)	(0.037)
Loan Maturity		0.053	0.024		0.043	-0.228**
		(0.045)	(0.075)		(0.080)	(0.099)
Revolving		-0.066	-0.061		-0.191***	0.418**
		(0.043)	(0.136)		(0.073)	(0.195)
Spread		0.055	0.056		-0.044	-0.047
		(0.039)	(0.041)		(0.069)	(0.041)
Policy environment		0.017	0.027		-0.050	-0.110
		(0.020)	(0.019)		(0.038)	(0.077)
Economic policy uncertainty		-0.001	-0.001		-0.001	-0.001
		(0.001)	(0.001)		(0.001)	(0.001)
GDP growth		0.008	0.003		0.002	0.005

		(0.009)	(0.009)		(0.013)	(0.015)
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Loan purpose FE	No	No	Yes	No	No	Yes
Loan type FE	No	No	Yes	No	No	Yes
Observations	289	289	289	154	154	154
Pseudo R-squared	0.072	0.082	0.089	0.063	0.080	0.089

Figure 3.3 (a)

Parallel trend in covenant intensity for firms located in a low crime state and relocate to a low crime or a high crime state

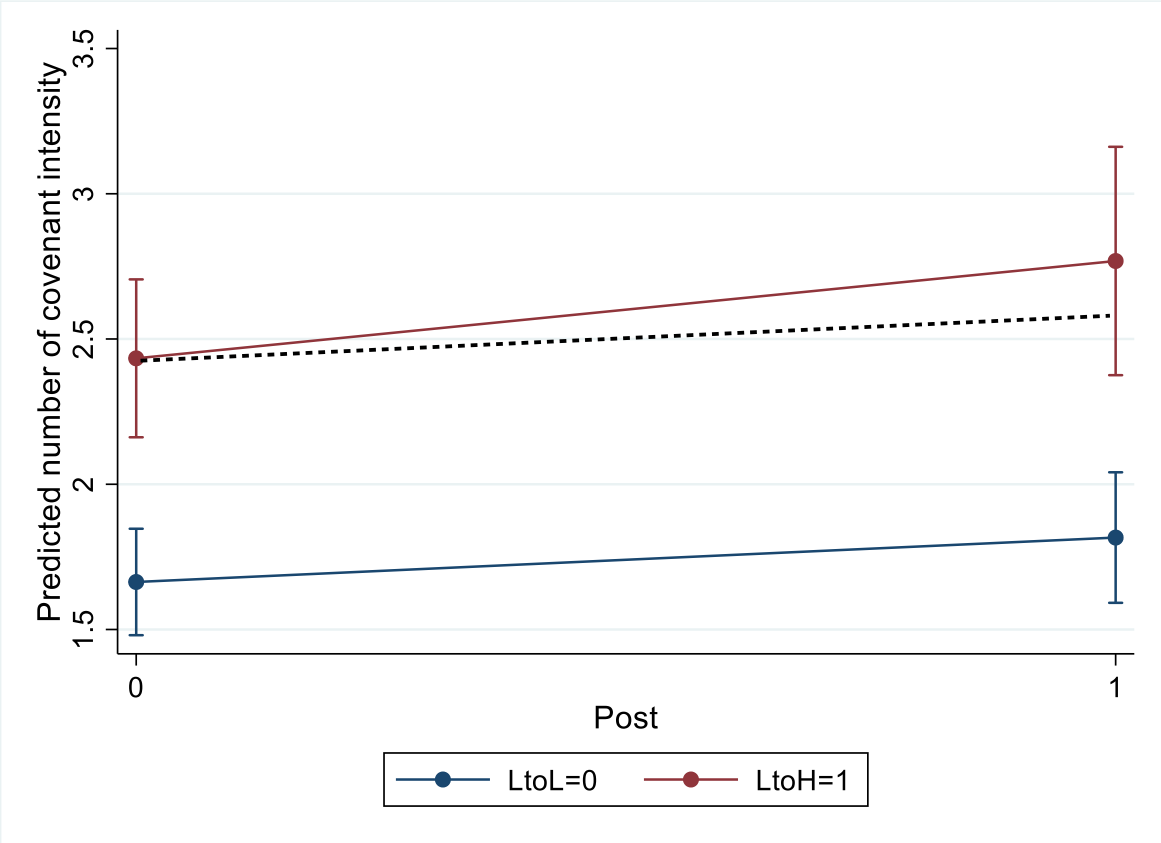

Figure 3.3 (a): This predictive margin graph shows the parallel trend in covenant intensity for firms located in a low crime state and relocate to a low crime or a high crime state. The dashed line corresponds to the counterfactual.

Figure 3.3 (b)

Parallel trend in covenant intensity for firms located in a high crime state and relocate to a low crime or a high crime state

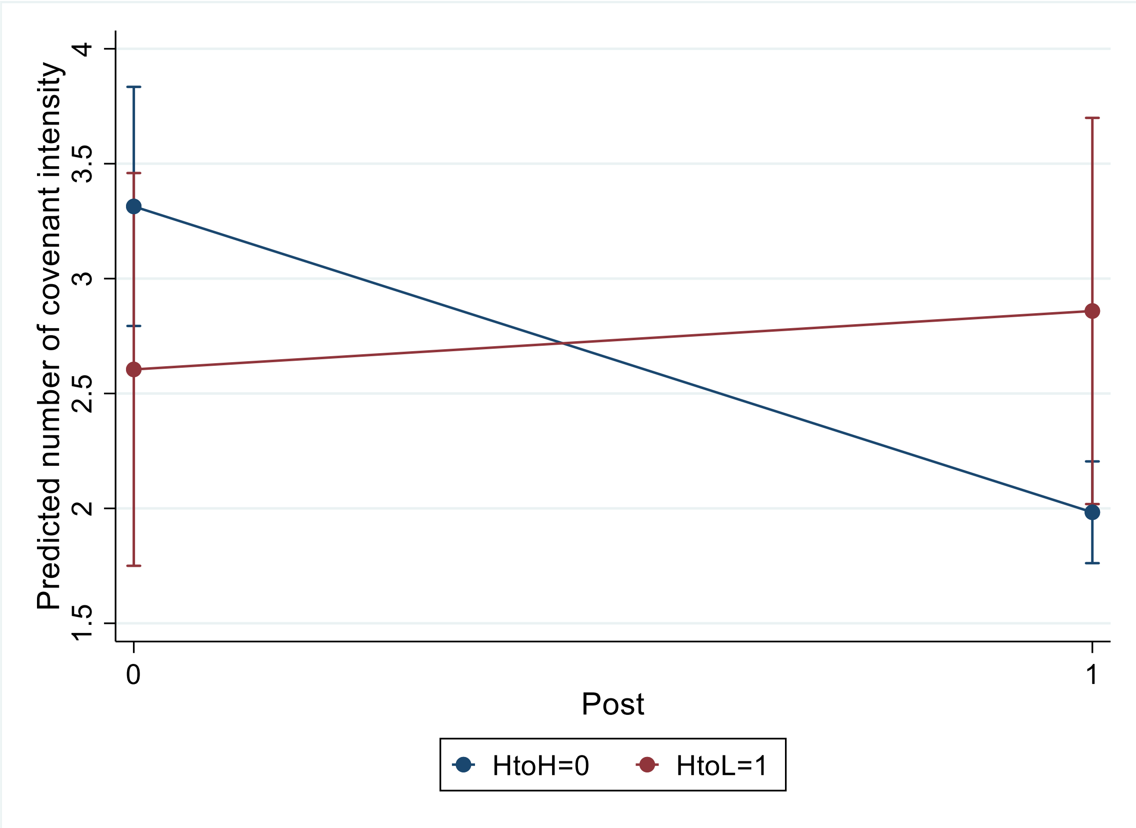

Figure 3.3 (b): This predictive margin graph shows the predicted covenant intensity for firms located in a high crime state and relocate to a low crime or a high crime state.

Table 3.8 Covenant intensity and property crime-potential channels

This table presents the coefficients of negative binomial regression that regress covenant intensity on property crime and the various channels through which property crime affects covenants. The dependent variable in column (1) to (2) is financial covenants, a total of financial and net worth covenants. In column (1), three interaction terms, namely, cashflow volatility, spread and ln (property crime) are used. In column (2), two interaction terms, namely, ln (property crime) and HPI and are used. The main variable of interest, Ln (property crime), is the rate of property crime per 100,000 inhabitants in the borrowers' headquarter state (in Ln). The definitions of control variables are provided in Appendix A3. Continuous variables are winsorized at 1st and 99th percentiles. All the regression includes intercept, industry fixed effect (two-digit SIC) and state fixed effect. The standard errors, robust to heteroscedasticity and clustered by borrower firms and year, are reported in parentheses. ***,** and * denote significance at 1%, 5% and 10%, respectively.

	(1)	**(2)**	**(3)**	**(4)**
Variables	fincov	fincov	fincov	fincov
Ln (property crime)	0.697***	0.658***	0.752***	0.741***
	(0.133)	(0.132)	(0.071)	(0.072)
Cf volatility	46.834***	45.625***		
	(13.439)	(13.479)		
Ln (property crime) × Cf volatility	-5.711***	-5.572***		
	(1.616)	(1.619)		
Spread	0.292	0.238	0.107***	0.096***
	(0.212)	(0.211)	(0.008)	(0.008)
Ln (property crime) × Spread	-0.021	-0.016		
	(0.026)	(0.026)		
Cf volatility × Spread	-7.955***	-7.756***		
	(2.397)	(2.401)		
Cf volatility × Spread× Ln (property crime)	0.964***	0.941***		
	(0.287)	(0.288)		
HPI			0.005***	0.004***
			(0.001)	(0.001)
HPI × Ln (property crime)			-0.001***	-0.001***
			(0.000)	(0.000)
Leverage	0.233***	0.230***	0.235***	0.232***
	(0.038)	(0.038)	(0.038)	(0.038)
Size	-0.084***	-0.080***	-0.085***	-0.081***
	(0.006)	(0.006)	(0.006)	(0.006)
Cash flow/ Asset	0.206***	0.209***	0.240***	0.242***
	(0.063)	(0.063)	(0.061)	(0.061)
Tobin's Q	-0.032***	-0.031***	-0.039***	-0.039***
	(0.007)	(0.007)	(0.007)	(0.007)
Unrated	0.032**	0.032**	0.029**	0.029**
	(0.013)	(0.013)	(0.013)	(0.012)
Tangibility	-0.020	-0.016	-0.028	-0.024
	(0.030)	(0.029)	(0.029)	(0.029)
Z-score	0.003	0.002	0.005*	0.005*

	(0.003)	(0.003)	(0.002)	(0.002)
Loan size	0.022***	0.015**	0.025***	0.017***
	(0.006)	(0.006)	(0.006)	(0.006)
Loan Maturity	0.017*	-0.014	0.021**	-0.009
	(0.009)	(0.013)	(0.009)	(0.013)
Revolving	0.015	0.085***	0.017	0.086***
	(0.011)	(0.026)	(0.011)	(0.026)
Policy environment	0.059***	0.230***	0.055***	0.054***
	(0.011)	(0.038)	(0.011)	(0.011)
Economic policy uncertainty	0.000	-0.080***	0.000	0.000
	(0.000)	(0.006)	(0.000)	(0.000)
GDP growth	0.010***	0.209***	0.010***	0.010***
	(0.002)	(0.063)	(0.002)	(0.002)
Industry FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Loan purpose FE	No	Yes	No	Yes
Loan type FE	No	Yes	No	Yes
Observations	7,652	7,652	7,715	7,715
Pseudo R-squared	0.043	0.044	0.043	0.045

Figure 3.4 (a)

Predicted covenant intensity by cashflow volatility and spread in high and low crime states

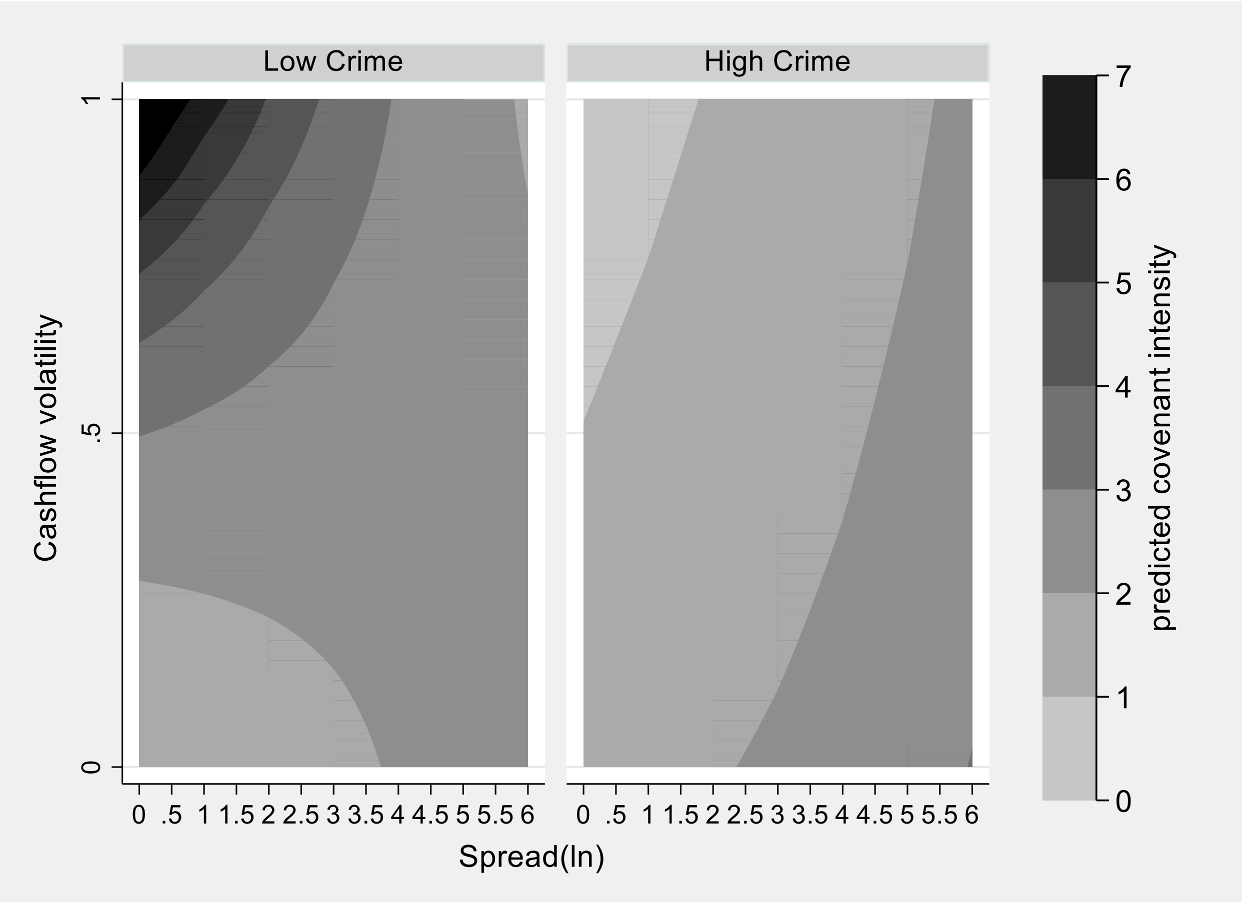

Figure 3.4 (a): This contour plot shows the relation between spread and cashflow volatility in high and low crime states and the predictive margins of the model to calculate the predictive covenant intensity assigned by the lender for all combinations of cashflow volatility and spread. The darker region indicates higher covenant intensity. The upward concave non-linear appearance implies a strong positive interaction effect.

Figure 3.4 (b)

Predicted covenant intensity by crime and HPI

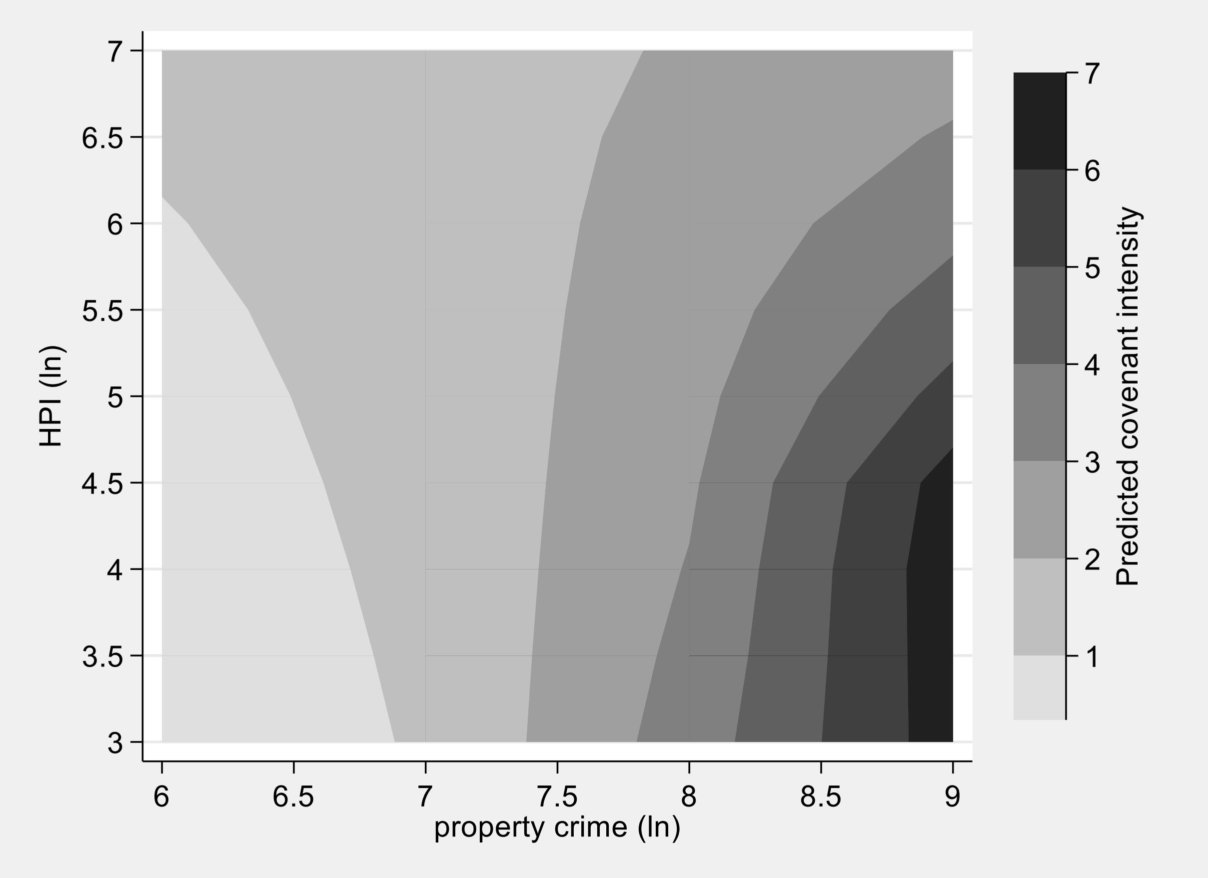

Figure 3.4 (b): This contour plot shows the relation between property crime and the HPI and the predictive margins of the model to calculate the predictive covenant intensity assigned by the lender for all combinations of property crime and the HPI. The darker region indicates higher covenant intensity. The downward concave non-linear appearance of the contours implies strong negative interaction effect.

Table 3.9 Regression of debt covenant tightness on property crime

This table presents the coefficients of logistic regression that regress covenant tightness on property crime. The dependent variable in column (1) to (2) is current ratio tightness, and in column (3) to (4) debt/EBITDA tightness using cluster analysis. Within each cluster, borrowers are sorted by their accounting ratios at the time of loan initiation and covenant threshold. If the covenant threshold is higher than the median of that cluster, then it is classified as more restrictive and tight. Stopping rule, based on the Calinski/Harabasz index, is used to select the optimum number of cluster following Colla, Ippolito and Li (2006).The main variable of interest, Ln (property crime), is the rate of property crime per 100,000 inhabitants in the borrowers' headquarter state (in Ln). The definitions of control variables are provided in Appendix A3. Continuous variables are winsorized at 1st and 99th percentiles. All the regression includes intercept, industry fixed effect (two-digit SIC) and state fixed effect. The standard errors, robust to heteroscedasticity and clustered by borrower firms and year, are reported in parentheses. ***,** and * denote significance at 1%, 5% and 10%, respectively.

	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Variables	Current ratio tightness	Current ratio tightness	Current ratio tightness	Debt/EBITDA tightness	Debt/EBITDA tightness	Debt/EBITDA tightness
Ln (property crime)	3.930***	3.916***	3.786***	0.496*	1.430***	1.435***
	(1.041)	(1.422)	(1.436)	(0.283)	(0.385)	(0.392)
Leverage		0.664	0.093		1.850***	1.888***
		(1.247)	(1.383)		(0.386)	(0.388)
Size		0.044	0.086		0.012	0.007
		(0.196)	(0.198)		(0.059)	(0.061)
Cash flow/ Asset		-2.102	-2.729*		-2.730***	-2.713***
		(1.304)	(1.405)		(0.713)	(0.715)
Tobin's Q		-0.215	-0.193		0.180**	0.168**
		(0.194)	(0.210)		(0.076)	(0.078)
Unrated		0.481	0.613		-0.381***	-0.386***
		(0.484)	(0.514)		(0.125)	(0.126)
Tangibility		-3.441***	-3.426***		-1.516***	-1.545***
		(1.100)	(1.129)		(0.331)	(0.333)
Z-score		0.051	0.061		-0.077**	-0.076**
		(0.056)	(0.058)		(0.035)	(0.035)
Loan size		0.141	0.089		0.324***	0.324***
		(0.181)	(0.193)		(0.065)	(0.070)
Loan Maturity		-0.225	0.006		0.214**	0.176
		(0.229)	(0.335)		(0.105)	(0.149)
Revolving		0.417	1.162		0.909***	0.852***
		(0.318)	(0.854)		(0.105)	(0.110)
Spread		0.192	0.240		-0.148	0.284
		(0.346)	(0.405)		(0.107)	(0.364)
Policy environment		0.374	0.348		0.171	0.189
		(0.321)	(0.335)		(0.125)	(0.127)
Economic policy uncertainty		-0.010	-0.009		-0.004**	-0.004**
		(0.007)	(0.007)		(0.002)	(0.002)
GDP growth		-0.106	-0.105		0.011	0.009
		(0.088)	(0.087)		(0.024)	(0.024)
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes

Loan purpose FE	No	No	Yes	No	No	Yes
Loan type FE	No	No	Yes	No	No	Yes
Observations	567	567	567	4,680	4,680	4,680
Pseudo R-squared	0.382	0.415	0.436	0.119	0.230	0.238

Table 3.10 Regression of debt covenant tightness on property crime-alternative measure

This table presents the coefficients of OLS that regress covenant tightness on property crime using the measure developed by (Demerjian and Owens, 2016). The dependent variable in column (1), (2) and (3) is the probability to violate financial covenants (Pviol), performance covenants (Pviol_Pcov) and capital covenants (Pviol_Cov) respectively. The main variable of interest, Ln (property crime), is the rate of property crime per 100,000 inhabitants in the borrowers headquarter state (in Ln). The definitions of control variables are provided in Appendix A3. Continuous variables are winsorized at 1st and 99th percentiles. All the regression includes intercept, industry fixed effect (two-digit SIC), state fixed effect, loan purpose fixed effect and loan type fixed effects. The standard errors, robust to heteroscedasticity and clustered by borrower firms and year, are reported in parentheses. ***,** and * denote significance at 1%, 5% and 10%, respectively.

	(1)	**(2)**	**(3)**
Variables	**Pviol**	**Pviol_Pcov**	**Pviol_Ccov**
Ln (property crime)	0.256***	0.201***	0.111***
	(0.036)	(0.036)	(0.023)
Leverage	0.321***	0.328***	0.045*
	(0.040)	(0.041)	(0.026)
Size	-0.015**	-0.007	-0.014***
	(0.006)	(0.006)	(0.004)
Cash flow/ Asset	-0.588***	-0.558***	-0.207***
	(0.071)	(0.077)	(0.053)
Tobin's Q	0.009	0.003	0.015***
	(0.007)	(0.008)	(0.005)
Unrated	0.011	0.013	0.000
	(0.013)	(0.012)	(0.008)
Tangibility	-0.029	-0.105***	0.117***
	(0.032)	(0.031)	(0.020)
Z-score	-0.010***	-0.009***	-0.003
	(0.003)	(0.003)	(0.002)
Loan size	-0.022***	-0.023***	-0.003
	(0.006)	(0.006)	(0.004)
Loan Maturity	-0.020	-0.005	-0.033***
	(0.013)	(0.013)	(0.008)
Revolving	-0.015	-0.018	0.022
	(0.026)	(0.026)	(0.018)
Spread	0.143***	0.145***	0.006
	(0.008)	(0.008)	(0.005)
Policy environment	0.028**	0.023*	0.008
	(0.012)	(0.013)	(0.007)
Economic policy uncertainty	-0.000	-0.000	0.000*
	(0.000)	(0.000)	(0.000)
GDP growth	0.007***	0.007***	0.003*
	(0.002)	(0.002)	(0.001)

Industry FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Loan purpose FE	Yes	Yes	Yes
Loan type FE	Yes	Yes	Yes
Observations	6,894	6,894	6,894
R-squared	0.279	0.273	0.158

Table 3.11 Covenant intensity and property crime-Quantile regression

This table presents the coefficients of quantile regression that regress covenant intensity on 25th, 50th and 75th percentile of property crime in column (1), (2) and (3) respectively. The dependent variable in all the three columns is fincov, the total of financial and net worth covenants. The main variable of interest, Ln (property crime), is the rate of property crime per 100,000 inhabitants in the borrowers' headquarter state (in Ln). The definitions of control variables are provided in Appendix A3. Continuous variables are winsorized at 1st and 99th percentiles. All the regression includes intercept, industry fixed effect (two-digit SIC), state fixed effect, loan purpose fixed effect and loan type fixed effects. The standard errors, robust to heteroscedasticity and clustered by borrower firms and year, are reported in parentheses. ***,** and * denote significance at 1%, 5% and 10%, respectively.

	(1)	**(2)**	**(3)**
Variables	fincov (25th)	fincov (50th)	fincov (75th)
Ln (property crime)	1.045***	1.154***	1.528***
	(0.117)	(0.101)	(0.120)
Leverage	0.317***	0.728***	1.097***
	(0.122)	(0.105)	(0.125)
Size	-0.171***	-0.208***	-0.238***
	(0.019)	(0.016)	(0.019)
Cash flow/ Asset	0.883***	0.672***	-0.033
	(0.195)	(0.167)	(0.199)
Tobin's Q	-0.102***	-0.126***	-0.104***
	(0.022)	(0.019)	(0.022)
Unrated	0.107***	0.030	0.047
	(0.041)	(0.035)	(0.042)
Tangibility	0.053	-0.045	-0.107
	(0.102)	(0.088)	(0.105)
Z-score	0.011	0.018**	0.028***
	(0.008)	(0.007)	(0.009)
Loan size	0.039*	0.028	0.013
	(0.020)	(0.017)	(0.020)
Loan Maturity	-0.001	-0.071**	-0.140***
	(0.041)	(0.035)	(0.042)
Revolving	0.151*	0.222***	0.243***
	(0.085)	(0.073)	(0.087)
Spread	0.103***	0.238***	0.360***
	(0.026)	(0.023)	(0.027)
Policy environment	0.093**	0.108***	0.216***
	(0.039)	(0.034)	(0.040)
Economic policy uncertainty	0.000	0.000	-0.000
	(0.001)	(0.001)	(0.001)
GDP growth	0.008	0.017**	0.024***
	(0.008)	(0.007)	(0.008)
Industry FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Loan purpose FE	Yes	Yes	Yes
Loan type FE	Yes	Yes	Yes
Observations	7,715	7,715	7,715
R-squared	0.090	0.172	0.192

Table 3.12 Alternative definitions of covenant intensity and property crime

This table presents the coefficients of negative binomial regression that regress alternative definitions of covenant intensity on property crime. The dependent variable in column (1) is the covenant intensity index (covindex) proposed by Bradley and Roberts (2015) that includes six covenants namely asset sales sweep, debt issuance sweep, equity issuance sweep, collateral, more than two financial covenants and dividend restriction sweep. The dependent variable in column (2) and (3) are performance covenants (P-cov) and capital covenants (C-cov) intensity proposed by Christensen and Nikolaev (2012). Performance covenants include cash interest coverage ratio, debt service coverage ratio, level of EBITDA, fixed charge coverage ratio, interest coverage ratio, debt to EBITDA, and, senior debt to EBITDA. Capital-based covenants include quick ratio, current ratio,) debt-to-equity ratio, loan-to-value ratio, debt-to tangible net worth ratio, leverage ratio, senior leverage ratio, and net worth requirement. The main variable of interest, Ln (property crime), is the rate of property crime per 100,000 inhabitants in the borrowers' headquarter state (in Ln). The definitions of control variables are provided in Appendix A3. Continuous variables are winsorized at 1st and 99th percentiles. All the regression includes intercept, industry fixed effect (two-digit SIC), state fixed effect, loan purpose fixed effect and loan type fixed effects. The standard errors, robust to heteroscedasticity and clustered by borrower firms and year, are reported in parentheses. ***,** and * denote significance at 1%, 5% and 10%, respectively.

	(1)	(2)	(3)
Variables	covindex	Pcov	Ccov
Ln (property crime)	0.365***	0.255***	1.400***
	(0.059)	(0.044)	(0.152)
Leverage	0.614***	0.289***	-0.961***
	(0.056)	(0.047)	(0.164)
Size	-0.198***	-0.089***	0.071***
	(0.010)	(0.007)	(0.022)
Cash flow/ Asset	0.242***	0.590***	-0.949***
	(0.092)	(0.083)	(0.192)
Tobin's Q	-0.054***	-0.006	-0.147***
	(0.012)	(0.009)	(0.028)
Unrated	-0.033	0.034**	0.021
	(0.021)	(0.015)	(0.052)
Tangibility	-0.165***	-0.191***	1.006***
	(0.047)	(0.037)	(0.127)
Z-score	-0.001	-0.009***	0.050***
	(0.004)	(0.003)	(0.008)
Loan size	0.177***	0.083***	-0.221***
	(0.011)	(0.008)	(0.023)
Loan Maturity	0.134***	0.142***	-0.403***
	(0.024)	(0.018)	(0.046)
Revolving	0.093	-0.017	0.249***
	(0.061)	(0.041)	(0.086)
Spread	0.498***	0.148***	-0.456***
	(0.016)	(0.011)	(0.031)
Policy environment	0.049***	0.035**	0.068
	(0.018)	(0.014)	(0.059)

Economic policy uncertainty	-0.001***	0.000	0.001*
	(0.000)	(0.000)	(0.001)
GDP growth	0.003	-0.000	0.040***
	(0.004)	(0.003)	(0.010)
Industry FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Loan purpose FE	Yes	Yes	Yes
Loan type FE	Yes	Yes	Yes
Observations	7,715	7,715	7,715
Pseudo R-squared	0.122	0.042	0.165

Table 3.13 Covenant intensity and property crime-Single segment firms

This table presents the coefficients of negative binomial regression that regress covenant intensity on property crime, considering single segment firms only. The dependent variable in all the three columns is fincov, the total of financial and net worth covenants. The main variable of interest, Ln (property crime), is the rate of property crime per 100,000 inhabitants in the borrowers' headquarter state (in Ln). The definitions of control variables are provided in Appendix A3. Continuous variables are winsorized at 1st and 99th percentiles. All the regression includes intercept, industry fixed effect (two-digit SIC), state fixed effect, loan purpose fixed effect and loan type fixed effects. The standard errors, robust to heteroscedasticity and clustered by borrower firms and year, are reported in parentheses. ***,** and * denote significance at 1%, 5% and 10%, respectively.

	(1)	**(2)**	**(3)**	**(4)**
Variables	fincov	fincov	fincov	fincov
Ln (property crime)	0.632***	0.574***	0.514***	0.488***
	(0.092)	(0.099)	(0.111)	(0.114)
Leverage		0.242**	0.251**	0.259**
		(0.108)	(0.108)	(0.110)
Size		-0.050***	-0.048***	-0.048***
		(0.017)	(0.017)	(0.018)
Cash flow/ Asset		-0.224*	-0.215*	-0.217*
		(0.131)	(0.128)	(0.130)
Tobin's Q		-0.031*	-0.032*	-0.029*
		(0.017)	(0.017)	(0.017)
Unrated		0.085**	0.077*	0.074*
		(0.041)	(0.042)	(0.042)
Tangibility		-0.029	-0.031	-0.016
		(0.108)	(0.106)	(0.106)
Z-score		0.019***	0.019***	0.018***
		(0.006)	(0.006)	(0.006)
Loan size		0.039**	0.036**	0.041**
		(0.016)	(0.016)	(0.019)
Loan Maturity		-0.015	-0.015	-0.042
		(0.029)	(0.029)	(0.045)
Revolving		0.034	0.046	0.123
		(0.032)	(0.033)	(0.089)
Spread		0.040	0.043*	0.041
		(0.025)	(0.025)	(0.028)
Policy environment			0.026	0.018
			(0.024)	(0.025)
Economic policy uncertainty			-0.001	-0.001
			(0.001)	(0.001)
GDP growth			0.016	0.017*
			(0.010)	(0.010)

Industry FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Loan purpose FE	No	No	No	Yes
Loan type FE	No	No	No	Yes
Observations	624	624	624	624
Pseudo R-squared	0.036	0.043	0.044	0.045

Appendix A3.1

Table A3.1 Covenant intensity and property crime-Including time period fixed effects

This table presents the coefficients of negative binomial regression that regress covenant intensity on property crime, considering single segment firms only. The dependent variable in all the three columns is fincov, the total of financial and net worth covenants. The main variable of interest, Ln (property crime), is the rate of property crime per 100,000 inhabitants in the borrower's headquarter state (in Ln). The definitions of control variables are provided in Appendix A3. Continuous variables are winsorized at 1st and 99th percentiles. All the regression includes intercept. The standard errors, robust to heteroscedasticity and clustered by borrower firms and year, are reported in parentheses. ***,** and * denote significance at 1%, 5% and 10%, respectively.

	(1)	**(2)**
Variables	fincov	fincov
Ln (property crime)	0.093**	0.152***
	(0.040)	(0.044)
Leverage	0.160***	0.151***
	(0.036)	(0.037)
Size	-0.074***	-0.074***
	(0.006)	(0.006)
Cash flow/ Asset	0.247***	0.254***
	(0.059)	(0.060)
Tobin's Q	-0.035***	-0.037***
	(0.007)	(0.007)
Unrated	0.050***	0.046***
	(0.012)	(0.012)
Tangibility	-0.040	-0.057**
	(0.028)	(0.028)
Z-score	0.005*	0.005**
	(0.002)	(0.002)
Loan size	0.029***	0.029***
	(0.006)	(0.006)
Loan Maturity	0.023*	0.027**
	(0.013)	(0.013)
Revolving	0.106***	0.101***
	(0.026)	(0.026)
Spread	0.117***	0.121***
	(0.008)	(0.008)
Policy environment	0.025**	0.029***
	(0.010)	(0.011)
Economic policy uncertainty	0.000	0.000
	(0.000)	(0.000)
GDP growth	-0.001	0.000
	(0.002)	(0.003)
Industry fixed effects	Yes	Yes
State fixed effects	No	Yes
Time period fixed effects	No	Yes
State-time period fixed effects	Yes	No
Loan purpose fixed effects	Yes	Yes

Loan type fixed effects	Yes	Yes
Observations	7,715	7,715
Pseudo R-squared	0.050	0.054