Violent Crime and Capital Market Punishment:

How Violent Crime Affects the Supply of Debt to Municipal Mexico

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Abstract: Research shows that violent and organized crime reduces foreign direct investment and that armed conflict lowers sovereign credit ratings. Building on these insights, I argue violent crime reduces financial institutions' confidence in the capacity of governments to repay loans, raising the costs attached to loans and reducing government debt through a "supply-side" logic. Yet, the supply-side logic is difficult to test. Governments can render lenders indifferent to violent crime by accepting higher borrowing costs, resulting in no observed relationship between them. It is for this reason that analysis of the effect of violent crime on credit ratings alone cannot tell us much about its effect on government debt. In this study, I explain how analysis of subnational debt from welfare-minded public banks and profit-minded private lenders can distinguish the supply-side logic from the null hypothesis. Cross-sectional time-series analysis of homicide rates and municipal debt in Mexico demonstrates support for the supply-side logic. Evidence of the supply-side logic reveals that those governments most in need of cost-efficient financing are most likely to be charged higher prices for it or priced out of capital markets altogether, signaling the need for market intervention in these cases.

Keywords: criminal violence, armed conflict, sovereign debt, subnational debt, subnational capital markets, international capital markets, Mexico

Word count: 9,994 (text, tables and figures, references)

Introduction

Does violent crime affect governmental debt? There is no research on this topic to date but two things suggest there may be a link. First, violent and organized crime affect the distribution of domestic and foreign direct investment within and across nations (e.g, Detotto et al. 2010; Daniele et al. 2011; Camacho et al. 2013). That investors perceive risks associated with violent crime suggests that financial institutions may as well. Second, armed conflict – interstate wars, civil wars, and terrorism – lowers sovereign credit ratings and raises corporate risk insurance premiums (e.g., Gupta et al. 2004; Jensen et al. 2008). That ratings agencies and risk insurers react negatively to armed conflict suggests that financial institutions might as well. Building on these insights, I argue that violent crime lowers financial institutions' confidence in the capacity of governments to repay loans, affecting governmental debt loads through a "supply-side" logic.

At first glance, it seems a simple task to detect a supply-side logic linking violent crime to governmental debt. However, at second glance, it is difficult to isolate this effect from other national and international economic (e.g., Erb et al. 2000; Chuhan et al. 1998), political (e.g., Mosley 2003; Garrett 1998) and institutional factors (e.g., Gray 2009; Koremenos et al. 2001; Biglaiser et al. 2008; Lee et al. 2012) that also affect capital markets. It is also difficult to detect a supply-side link because governments can compensate lenders for the additional risk they assume in lending through higher interest rates and stricter terms and conditions attached to loans. There thus may be no observed

effect of violent crime on governmental debt, even if the supply-side logic drives up borrowing costs, if governments accept additional borrowing costs.

With these difficulties in mind, my aim in this study is twofold. First, I seek to demonstrate support for the argument that criminal violence affects sovereign debt through a supply-side logic. Second, I seek to demonstrate the benefit of testing the supply-side logic at the subnational level. Scaled-down analysis serves three purposes. It allows scholars to control for the impact of international and national factors affecting capital markets, by holding them constant across subnational units. It allows scholars to leverage the presence of spatial variation in criminal violence across subnational units to estimate its effects on subnational debt. It allows scholars to leverage the presence of private and public lenders in subnational capital markets to distinguish the supply-side logic from competing ones, as explained below. Because it is unlikely that the impact of violent crime on subnational debt would be absent from or contrary to that at the national level, detection of a subnational supply-side link suggests its national presence.

To achieve these aims, I examine the impact of violent crime on municipal debt in Mexico. Mexico is a good case for analysis due to municipal political accountability for violent crime, and to variation in violent crime rates and debt loads across municipal governments over time. Cross-sectional time-series analysis of violent crime and municipal debt from public and private sources provides support for the supply-side logic.

Identifying the supply-side logic linking violent crime to government debt has important policy implications. As noted, examination of the effect of violent crime on

governmental credit ratings or risk premiums attached to loans cannot tell us much about whether violent crime matters for government debt, because governments may or may not accept higher borrowing costs. However, because governments rely on capital markets to finance public policy, capital investment, and counter-cyclical spending (Mosley 2003; Sáez 2016; Alesina et al. 1997), evidence that criminal violence affects governmental debt though a supply-side logic would mean that those governments most in need of debt-financed public spending are being forced to pay more for loans or are being priced out of the market. International financial institutions and supranational development banks might need to intervene to ensure cost-efficient financing to national governments facing criminal violence. National governments might need to direct public development banks to target loans to subnational entities beleaguered by violent crime. The failure to do so might prevent governments from addressing violent crime, as well as from attenuating economic downturns and political instability resulting from them.

The Supply-Side Logic

There are no studies examining whether and how violent crime affects sovereign debt but two lines of research suggest there may be a link. Research on the impact of violent and organized crime on firm behavior and direct investment reveals a negative relationship between them. Violent and organized crime act like a "tax" on firms, raising business costs (Detotto et al. 2010; Daniele et al. 2011) and reducing gross capital formation (Daniele et al. 2011; Pshisva et al. 2006). Scholars have thus shown that violent

and organized crime reduces firm competitiveness, leading companies to exit markets (Gaviria 2002; Detotto et al. 2010; Camacho et al. 2013) and foreign direct investors to (re)locate operations in safer nations or regions within them (Peri 2004; Pan et al. 2012; Detotto et al. 2010; Escobar Gamboa 2013), although investors in some economic sectors are less deterred by such crime than others (Ashby et al. 2013; Samford et al. 2014).

Research examining the impact of armed conflict on foreign direct investment, investment risk insurance, and sovereign credit ratings also shows this negative relationship. Political risk – expropriation risk, transfer risk, and violence risk – reduces investor confidence in economies and governments (Archer et al. 2007; Jensen et al. 2008; Bechtel 2009; Biglaiser et al. 2008; Jensen 2003; Jensen 2008; Bernhard et al. 2006; Jensen et al. 2005; Busse et al. 2007). Governments facing armed conflict thus often find their foreign investment flows curtailed (Li et al. 2010; Busse et al. 2007), sovereign credit ratings downgraded (Brewer et al. 1990; Procasky et al. 2016; Chen et al. 2004; Castañeda et al. 2012), and corporate risk insurance premiums raised (Jensen et al. 2008).

Building on these insights, I argue that financial institutions lending to governments should react in similar ways to violent crime. Criminal violence and armed conflict can raise governments' military and security expenditures (Drakos et al. 2014; Olavarria-Gambi 2007; Gupta et al. 2004; DiGiuseppe 2015), reducing resources available for other fiscal obligations like debt repayment. Violent crime and armed conflict can undermine investment and economic growth (Daniele et al. 2011; Pshisva et al. 2006; Collier 1999; Enders et al. 2012; Gupta et al. 2004; Chen et al. 2004; Ismail et al. 2014;

Peri 2004; Cardenas 2007; Detotto et al. 2010), further weakening government fiscal positions and raising the chances of default. Violent crime can affect the capacity of governments to repay loans, raising the risk of delayed payments or default, increasing financial institutions' capital risk in lending. That is, violent crime might affect capital markets through a "supply-side" logic, where rising capital risk raises the costs attached to loans, putting downward pressure on governments' capacity to access credit.

Testable Expectations

Testing the relationship between violent crime and debt is complicated by the different types of financial institutions supplying credit and by the fact that governments can choose to accept higher interest rates and stricter terms and conditions attached to loans. Most national governments access financing through bond emissions marketed to profit-minded investors in national and international capital markets. If governments compensate lenders for additional risk, there would be no observed relationship between violent crime and bond debt even if violent crime raised the costs attached to loans, leaving us unable to distinguish the supply-side and null hypotheses. Some developing nations access credit mainly through publicly-funded international financial institutions or supranational development banks whose mission is to help nations to meet their developmental goals or address economic crises. If governments accept loans from such lenders, there would be a positive relationship between violent crime and development bank debt, leaving us unable to distinguish the supply-side logic from a possible

competing demand-side rationale where violent crime leads governments to demand greater debt and lenders meet this demand without concern for capital risk.

However, it is possible to distinguish the supply-side logic through examination of the relationship between violent crime and subnational debt. Instead of concentrating borrowing among either private or public lenders, subnational governments (with debt rights) can seek financing from both in subnational capital markets. Because private financial institutions respond to investors whose goal is to maximize profits but public development banks respond to national governments whose goal is to ensure development, these lenders should react differently to criminal violence under the supply-side logic compared to the demand-side and null hypotheses. The development of joint expectations for how violent crime would affect private and public lending under each of these logics combined with comparison of the empirical evidence for each can be used to test the supply-side logic and to distinguish it from competing ones, especially the null hypothesis.

Specifically, if a simple supply-side logic is present, high rates of violent crime will either lower or have no impact on private bank debt but raise public development bank debt. If subnational governments are unwilling to compensate private lenders for any additional risks to repayment associated with violent crime, private financial institutions will be reluctant to lend, resulting in a negative relationship between violent crime and debt from private sources. If subnational governments accept higher borrowing costs, private financial institutions will be willing to lend, leaving them indifferent to violent crime and no observed relationship between violent crime and debt from private sources.

Meanwhile, public development banks' developmental missions should lead them to supply loans to subnational governments with higher violent crime, leading to a positive relationship between violent crime and development bank debt. These joint expectations are summarized in Table 1.

–Table 1 Here–

If a simple demand-side logic is present, high rates of violent crime will raise subnational debt from both private and public sources. National governments facing violent crime or armed conflict sometimes divert resources to security and military expenditures and demand greater debt as a result (Drakos et al. 2014; Olavarria-Gambi 2007; Gupta et al. 2004; DiGiuseppe 2015). National governments are also known to engage in countercyclical spending (Alesina et al. 1997). If subnational economic downturns are caused by crime-related divestment, violent crime would drive debt-financed countercyclical spending. If subnational governments suffering violent crime demand greater debt, and private banks meet this demand without concern for capital risk, then a simple demand-side logic would be present. Under this logic, public banks would also meet subnational debt demand as well, without concern for capital risk due to their developmental missions. See Table 1.

It is also possible that both the supply-side and demand-side logics might be present, with their combined effect on subnational debt depending on whether governments accept higher costs attached to loans. If subnational governments are unwilling to accept higher borrowing costs, private lenders will be unwilling to meet their

demand and the supply-side logic will prevail in determining the relationship between violent crime and debt. If subnational governments are willing to accept higher borrowing costs, private lenders will be willing to lend and the demand-side logic will prevail in determining the relationship between violent crime and debt. See Table 1.

If there is no link between violent crime and debt (the null hypothesis), it should have no effect on lending from private and public sources. See Table 1. The joint expectations for each of the logics reveal how examination of the effect of violent crime on government credit ratings or loan risk premiums cannot tell us much about whether violent crime affects government debt. Although violent crime might raise the cost of financing and/or limit credit supply, lower credit ratings and/or higher risk premiums need not translate into lower debt if governments accept additional borrowing costs.

Empirical Strategy

To test the supply-side logic, I examine municipal Mexico. Mexico is a federal system with 31 states, a Federal District (which became a state in 2016), and 2,443 municipalities (depending on the year). Mexico is a good case for analysis due to variation in municipal violent crime rates – the independent variable – and in municipal debt loads – the dependent variable – from both private and public lenders.

Mexico has faced rising and increasingly varied municipal violent crime rates President Felipe Calderón – from the right-leaning National Action Party (PAN) – announced his strategy for combating organized crime after taking office in December

2006 (Ley 2017). Between 2005 and 2014, the years under study (explained below), homicide rates (intentional homicides per 100,000 people) averaged 17, with a minimum 0 and a maximum of 2,271 observed in the municipality of General Treviño in the northern state of Nuevo León in 2010, with the next closest values 1,623 in Vallecillo in the northern state of Nuevo León and 1,613 from Tubutama in the northern state of Sonora (standard deviation 45). There is considerable debate about the factors contributing to the rise of violent crime in Mexico (see, for example, Phillips 2015; Calderón et al. 2015; Duran-Martinez 2015; Osorio 2015; Ley 2017; Trejo et al. 2016; Vilalta et al. 2016). What is useful here are the rising and varied municipal violent crime rates.

Although Mexico's municipalities are only responsible for basic public security (regular and transit police), and state and federal police are responsible for investigating and prosecuting most violent crimes, municipal incumbents share political responsibility for violent crime with state and federal officials (Ley 2017). Municipal, state, and federal police forces operate side-by-side (Ley 2017), often amidst joint military operations, blurring the lines of responsibility. National officials also publicly blame state and municipal governments for their failure to address criminal violence, especially for their failure to root out police complicit with criminal groups, even though municipal governments also publicly blame state and national officials for their security policy failures (Ley 2017; Trejo et al. 2016). In this context, mayors are regularly punished for violent crime at the polls (Ley 2017).

Direct investors can also react to violent crime by relocating operations, with falling municipal economic activity and tax revenues giving voters another reason to punish incumbents. The relative powerlessness of mayors to address violent crime directly but their political accountability for it thus might encourage them to raise debt-financed public spending. Of course, mayors might raise expenditures on police forces but they can also raise expenditures on basic public services – like potable water, sewage and drainage systems, trash collection, public lighting and roads, public transportation, and public parks and markets – whose improved provision can appease voters and compensate direct investors. It is the municipal political accountability for violent crime, and the pressures to municipal fiscal finances associated with it, that makes both the demand- and supply-side logics plausible in Mexico.

Mexico has also faced rising and increasingly varied municipal debt. Between 2005 and 2014, municipal debt liabilities averaged 90 pesos per capita, with a minimum of 0 and a maximum of 133,887 in 2009 in Juárez in the northern state of Coahuila, with the next closest value 123,675 in this same municipality in 2010, followed by 6,805 in Chilchotla, Puebla (standard deviation 1,184). I am concerned about the impact of violent crime on municipal debt loads, but regulatory failures also allowed rising municipal debt, while socio-economic, fiscal, and political factors contributed to its rising cross-municipal variation. I discuss regulatory failures here and the other factors below.

Subnational governments in Mexico can only borrow in Mexican pesos in the domestic capital market. The main financing instruments available are public development

bank loans, private commercial bank loans, and private bond emissions. The mission of the public development bank, the National Bank of Public Works and Services (Banobras), is to foment public investment outlined in six-yearly national economic development plans. Banobras does not provide subsidized credit but makes financing available to subnational entities where investment is deemed crucial for development. It also provides financial services (financial training, loan guarantees, and fiduciary services) to facilitate access to private capital. Subnational governments contract commercial bank loans directly but bond emissions occur through private financial underwriters that purchase the full issuance and remarket it to investors. Although the nation's public development and private commercial banks have long lent to subnational governments (Hernández Trillo et al. 2002), the subnational bond market only became operational in 2001 (Revilla 2013).

Regulations governing Mexico's subnational capital market for long-term financing date to public debt, fiscal, and banking reforms between 1997 and 2000, that structured this market until 2016 (and the approval of new measures). The framework regulates long-term debt contracts; short-term bridge loans used to close year-end fiscal accounts are unregulated and unreported. The framework was designed to improve oversight over subnational (long-term) debt decisions, strengthen the link between subnational fiscal solvency and public/private (long-term) lending, and eliminate bailout expectations for (long-term) debt (Revilla 2013; Hernández-Trillo et al. 2009). However,

several gaps in the framework allowed subnational governments to borrow and financial institutions to lend at rising and, in some cases, unsustainable levels.

According to the constitution, subnational debt can only be assumed for "economically productive" purposes. The framework made state legislatures responsible for authorizing state and municipal debt (and municipal councils municipal debt) and for setting subnational debt limits. However, "economic productivity" was often interpreted to include debt buybacks and refinancing, often of unregulated short-term bridge loans used to close year-end fiscal accounts but also of long-term debt (Revilla 2013; Hurtado et al. 2013). State governors usually controlled state legislatures (and municipal mayors municipal councils), weakening legislative oversight, and debt limits (only enacted in about half of states) usually went unenforced (Hurtado et al. 2013). Weak legislative oversight allowed subnational governments to run up large debt loads both licitly and illicitly, as occurred in Coahuila where one governor falsified documents showing state legislative approval for loans in the late 2000s.

The framework also established that subnational lending from private sources had to be based on credit ratings, private financial institutions had to hold capital reserves based on these ratings, and all long-term loans had to be registered with the national finance ministry. Development bank lending was based on internal bank assessments of subnational fiscal solvency. The failure to secure ratings or register private sector loans resulted in the loan's automatic capital risk re-weighting by bank regulators (Revilla 2013; Hernández-Trillo et al. 2009). However, subnational fiscal accounts are notoriously

nontransparent, and debt to service providers, short-term bridge loans, and contingent liabilities unregulated and unreported (Hurtado et al. 2013; Hernández-Trillo et al. 2009). Financial institutions were unable to assess fiscal solvency (Hernández-Trillo et al. 2009).

Questionable fiscal solvency amidst poor legislative oversight should have led financial institutions to perceive greater risk in lending. However, subnational governments collateralized debt with unearmarked fiscal transfers (the usual practice), own source revenues, and some earmarked fiscal transfers, with these resources intercepted by fiduciaries and debt payments made before reaching treasuries. (States are dependent on national transfers, and municipalities mostly on state transfers, although both enjoy tax rights.) Debt collateralization and the use of fiduciaries improved the timeliness of payments (Standard & Poor's Ratings Services 2007). Of course, the history of bailouts raised the expectation of future ones (Hernández-Trillo et al. 2009), but this perception likely weakened as none occurred during 2001-2016. Instead, fiscally insolvent subnational governments delayed payments to public employees and service providers, defaulted on short-term bridge loans (refinancing them with long-term debt), and restructured long-term debt (Fitch Ratings 2011; Hurtado et al. 2013; Revilla 2013).

Subnational governments thus appeared willing to take great strides to avoid default. Even so, debt collateralization and the apparent willingness to repay loans did not eliminate financial institutions' capital risk. Fiscal transfers and own source revenues declined during economic downturns, while state transfers to municipalities faced politically-motivated delays or adjustments, affecting the resources flowing to fiduciaries

and raising the economic and political risks to repayment (Standard & Poor's Ratings Services 2007). Subnational governments also sometimes diverted resources from fiduciaries to treasuries (Standard & Poor's Ratings Services 2007). Although the willingness of subnational governments to undertake long-term debt restructuring (with municipal restructuring often orchestrated by state governments) reduced the chances of default, it did not fully eliminate the risk of missed or delayed payments. There has thus been notable variation in loan interest rates that does not reflect credit ratings or collateralization (Hurtado et al. 2013). Both public and private lenders likely perceived some capital risk, or at least risk to timely repayment, with this risk varying among subnational governments. Low quality economic and fiscal information alongside salient economic and political risk are known to lead financial institutions to consider a wide range of criteria when assessing capital risk (Mosley 2003). In Mexico, this includes violent crime.

The Data

The data consist of 2,443 Mexican municipalities across 10 years or 24,430 observations. Mexico's finance ministry has only reported municipal debt separately from state debt since 2005, so I examine the 2005-2014 period for which I have complete data. I exclude the Federal District as it operated like a state during this period.

The dependent variables include yearly per capita municipal public development bank debt, private commercial bank debt, and private bond debt. Online Appendix 8

reports summary statistics. Between 2005 and 2014, 1,459 municipalities reported long-term debt liabilities and 984 municipalities reported no long-term debt. Among those with long-term debt, 913 had public development bank loans, 445 had public development bank and private commercial bank and/or bond debt, and 88 had private commercial bank and/or bond debt, irrespective of "trust" and/or "other" debt. Another 13 had only "trust" and/or "other" debt. I do not examine "trust" and "other" debt due to lack of information on their financing sources.

I transform all yearly debt liabilities into per capita square roots to address nonlinearity and account for outliers. (I do not use a logarithmic transformation due to zero debt liabilities in some years.) All debt and fiscal data (below) are deflated to 2005 pesos. Data are from the Secretary of the Treasury and Public Credit (SHCP). I examine debt by type rather than the balance between instruments as the argument does not make predictions about this balance. Also, exogenous national and international economic factors can influence the relative amounts of funding available from public and private sources for reasons having nothing to do with subnational government or financial institution decisions. I examine debt per capita rather than debt-to-GDMP (gross domestic municipal product, data which are unavailable in Mexico) or debt-to-revenue ratios because exogenous economic and political factors having nothing to do with subnational government or financial institution debt decisions can inflate/deflate the denominators in these ratios. (But I include controls for these factors, discussed below.)

The main explanatory variable is the lagged annual municipal intentional homicide rate (exclusive of manslaughter and justifiable homicides), the most severe form of violent crime. Data on other types of violent crime, such as kidnappings, armed robbery, and extortion, are unreliable in Mexico as they often go unreported. I divide total yearly municipal homicides by the population and multiply this figure by 100,000, and then take the square root to address nonlinearity and outliers. Data are from Mexico's National Institute of Geography and Statistics (INEGI). See Online Appendix 8.

Financial institutions vary in their incentive to supply subnational governments with credit for reasons not having to do with criminal violence, contributing to considerable variation in municipal debt among Mexican municipalities and requiring the addition of several controls to the models. Larger municipalities with bigger budgets are known to enjoy greater access to debt (Thau 2011; Freire et al. 2004; Freire 2014; Hernández-Trillo et al. 2009); municipal population size (square root) and total municipal fiscal revenues (transfers plus own source revenues; per capita square root) capture this. Greater administrative capacity improves municipal policy-making, something that improves access to loans; human development levels (Avellaneda 2009; O'Toole et al. 1999) and vertical fiscal imbalances (Cabrero Mendoza 2004; Ibarra Salazar et al. 2001) are used as proxies. Mexico's marginality index captures human development and the ratio of transfer to total revenues captures vertical fiscal imbalances. These variables also act as proxies for (albeit questionable) municipal credit ratings (Thau 2011; Freire et al. 2004; Freire 2014; Hernández-Trillo et al. 2009). Even if accurate, municipal credit

ratings could not be included because only those municipalities choosing to access private sector credit sought them. The inclusion of credit ratings would lead municipalities without any debt or with only public sector debt to drop out of the analysis. Data are from the National Institute of Geography and Statistics (INEGI) and from the National Population Council (CONAPO). See Online Appendix 8.

Subnational governments vary in their incentive to demand credit beyond criminal violence, also contributing to the considerable variation in municipal debt and also requiring the addition of other controls in the models. Low socio-economic development may raise the need for debt-financed public investment (Sáez 2016), while high vertical fiscal imbalances may raise the incentive for fiscal profligacy (Rodden 2006). (Data are described above.) Opportunistic electoral concerns may lead subnational incumbents to ramp up spending ahead of elections, while politically weak mayors may be more compelled in this way (Sakurai et al. 2008; Veiga et al. 2007; Sáez 2016; Benton et al. 2016). Left-leaning governments may demonstrate a greater tolerance for expansionary fiscal policy and right leaning ones a lower tolerance for it (Rodden 2006; Franzese 2002; Benton et al. 2016; Hernández-Trillo et al. 2009). I include a municipal election year dummy, the margin of victory, and dummies to capture right-leaning National Action Party (PAN), the left-leaning Democratic Revolution Party (PRD), a PAN-PRD coalition, or other small party municipal control (with the centrist Institutional Revolutionary Party (PRI) the omitted case). Vertical partisan alignment with national presidents may raise fiscal restraint (Jones et al. 2000; Rodden et al. 2002); dummies capture municipal-federal

and municipal-state-federal alignment. State elections are used to determine municipal outcomes in Oaxaca state where most municipalities hold non-partisan contests; higher level elections reflect municipal politics there (Benton 2012). Models excluding Oaxaca produce the same results. (See the online appendices 1, 2, and 3.) Data are from the Center for Research on Development (CIDAC) and state electoral institutes. See Online Appendix 8.

Statistical Approach

I examine the data using linear Prais-Winston cross-sectional time-series analysis with panel-corrected standard errors (CSTS-PCSE). Unit-root (Levin-Lin-Chu, Harris-Tzavalis, and Breitung) tests on balanced versions of the dataset, assuming a common autoregressive process, show that the data are stationary ($p<0.01$), so I examine the data in level form. Modified Wald tests for group-wise heteroskedasticity in the residuals were significant ($p < 0.01$), so I correct for panel-level heteroskedastic errors, assuming no contemporaneous correlation across panels. Wooldridge tests for serial autocorrelation in the residuals were significant ($p<0.01$), even in models with lagged dependent variables, so I correct for the presence of a common autoregressive error process of order 1. (I could not correct for the possibility of cross-panel contemporaneous correlation or panel-specific autoregressive processes due to missing fiscal data.)

I include a lagged dependent variable in all models, as well as lagged variables for all other types of debt. The lagged dependent variable accounts for persistence, while this

variable alongside those for all other types of debt account for their effect, in total and by type, on subsequent municipal debt supply and/or debt demand. Year fixed effects control for inter-temporal variation in national economic dynamics that affect debt supply and/or demand common to all municipalities. A test for whether year dummy coefficients were jointly equal to zero rejects the null hypothesis that they were ($p < 0.01$). State fixed effects control for structural differences between them, such as variation in the competence of state police forces and judiciaries in the prevention, investigation, and prosecution of violent crimes, variation in state level legislative debt oversight or state level fiscal solvency that might affect municipal debt supply and/or demand. Municipal fixed effects are inappropriate as the main explanatory variable and several controls are highly sluggish or time invariant in many municipalities (Clark et al. 2015).

The Results

The first three models in Table 2 present results for the effect of homicide rates on public and private sector debt across the full sample of municipalities. Table 2 shows that homicide rates had a positive and significant ($p<0.01$) effect on development bank debt and no effect on commercial bank loans or bond emissions. The positive relationship between violent crime and public sector debt, amidst the lack of observed relationship between violent crime and private sector debt, demonstrates the presence of the supply-side logic, where violent crime leads financial institutions to question the capacity of municipal governments to repay loans. However, municipalities appear to accept any

additional costs attached to loans; had municipalities rejected additional costs, there would have been a positive relationship between violent crime and public debt but a negative one between violent crime and private debt. Violent crime does not lead municipalities to demand greater debt; had this been the case, there would have been a positive relationship between violent crime and both public and private sector debt instruments.

–Table 2 Here–

The effect of homicide rates on municipal debt is absent the impact of prior debt liabilities, as the models include lags for all types of debt. The effect of homicide rates on municipal debt is also absent any trade-off between debt types. Higher crime rate municipalities might have selected public development bank debt and lower crime rate municipalities selected private financing, something consistent with the lack of observed effect of violent crime on commercial bank and bond debt. However, models including controls for all current debt liabilities, in addition to their lags, do not change the results. (See online appendices 4, 5, and 6.) Had municipalities with higher violent crime rates traded private for public financing, the impact of violent crime on public debt would have disappeared with the addition of private sector debt controls, which was not the case. Models examining the impact of violent crime on the balance between public and private financing show that there was no effect. (See online Appendix 7.) Had municipalities with higher violent crime rates opted for public over private financing, the impact of violent crime on this balance would have been positive, which was not the case.

Because homicide rates are skewed toward zero values, with a large share of municipalities facing no homicides, the results might reflect some underlying difference between municipalities with and without homicides, rather than the impact of different homicide rates on municipal debt. To make sure that this was not the case, I reran all models excluding all municipalities with zero homicides during the entire period under analysis. These results are in Table 2 and are in line with those for the full sample. Because debt values are skewed toward zero debt liabilities, with a large share of municipalities acquiring no debt, it could be the case that the results reflect some underlying difference between municipalities with and without debt (not captured by the socio-economic, fiscal, and political controls). To make sure this was not the case, I reran all models excluding all municipalities that assumed no debt during the entire period under analysis. These results are in Table 2 and are in line with those for the full sample. In the case that the extreme homicide rates in General Treviño or the extreme debt values in Juárez affected the results, I reran all models excluding these two extreme cases. The results are in line with those in Table 2 (and are available upon request.)

Alternative Explanations

The results in Table 2 provide support for the supply-side logic. However, it could be argued that the lack of effect of homicide rates on commercial bank loans is the result of preexisting bank relations. Commercial banks often enjoy longstanding relationships with subnational governments, handling their retail banking needs (Freire et al. 2004;

Freire 2014), something well documented in Mexico (Hernández Trillo et al. 2002). Longstanding relations could leave commercial banks indifferent to the risks posed by violent crime. In contrast, financial institutions handling bond emissions do not usually offer retail services, limiting their subnational relations (Freire et al. 2004; Freire 2014). In Mexico, the newness of the bond market would further limit preexisting municipal relations. If preexisting bank relations were driving the results, then we should have observed Mexico's longtime commercial bank lenders undeterred by high rates of violent crime but its newer bond underwriters discouraged by them, which was not the case.

It could also be argued that homicide rates do not adequately test the demand-side hypothesis. Under a demand-side logic, municipal governments might be expected to react to changes in homicide rates, rather than aggregate homicide rates, given that upward shifts are more likely to harm their parties' political support and their own progressive political career ambitions [known to be strong in Mexico (Kerevel 2015)]. Under a supply-side logic, public and private financial institutions base their decisions on calculated homicide rates, rather than changes to them, because aggregate homicide rates would better reflect the risk that violent crime poses to the municipal capacity to repay loans. Analyses substituting the lagged first difference in the homicide rate for the lagged level homicide rate in Table 3 show that homicide rate changes had no effect on development bank debt, commercial bank debt, or bond emissions in the full sample or the two subsamples. This finding supports the supply-side rationale. Models in Table 4 with both the lagged level and lagged first difference homicide rates demonstrate the robustness

of the supply-side results: the lagged level homicide rate retained its positive significant effect on public development bank debt and its insignificant effect on private commercial bank and bond debt, while the lagged first difference homicide rate had no effect.

– Table 3 & Table 4 Here–

Long-Term and Substantive Effects

Tables 2, 3, and 4 thus support the conclusion that violent crime affects subnational capital markets according to a supply-side logic, at least in the short term (captured by the homicide rate variable coefficients). Estimates of the long-term effects of homicide rates on public and private debt provide additional support for this conclusion. Long-term effects (LTE) are estimated as $LTE_{x1} = \widehat{\beta} \,/\, (1 - \widehat{\Phi})$, where $\widehat{\beta}$ is the parameter estimate for the independent (homicide rate and homicide shock) variable of interest and $\widehat{\Phi}$ is the parameter estimate for the lagged dependent (type of debt) variable point of focus. Long-term effects of both homicide rates and the change in homicide rates in Table 5 (for the models in Table 4) are in line with the short-term results.

– Table 5 Here–

However, given the inclusion of a lagged dependent variable in all models, the models are dynamic (De Boef et al. 2008; Williams et al. 2012), with the most effective technique for interpreting dynamic models to conduct a "dynamic simulation." I thus follow Williams et al. (2012) and calculate the expected cumulated effect of lagged homicide rates over time on development bank debt for three plausible homicide rate

scenarios – zero yearly homicide rates, mean homicide rates, and homicide rates one standard deviation above the mean – holding all other covariates at their means.

Figure 1 shows that per capita development bank debt is expected to rise over time, even in zero homicide municipalities. In five years' time, however, municipalities with the mean rate of nine homicides per 100,000 people will enjoy an additional 10 pesos per capita development bank debt than places with zero homicides. Municipalities with 36 homicides per 100,000 people (one standard deviation above the mean) will enjoy another 12 pesos per capita development bank debt above this. In 2005, the average exchange rate was 10.89 Mexican pesos per US dollar, so municipalities with 36 homicides per 100,000 people enjoyed access to two dollars additional development bank debt per capita than municipalities with zero homicides. Although at first seemingly small, this difference can come to reflect important disparities in the level of public capital available to municipalities when aggregated across populations over time. A municipality with 15,000 people would enjoy 30,000 US dollars more in development bank financing after five years, a large sum for a small municipality in a developing nation. Figure 2 depicts the lack of cumulative impact of homicide rates on private sector commercial bank debt and bond emissions over time, in line with the supply-side logic.

– Figure 1 & Figure 2 Here–

Conclusion

The original aim of this study was twofold. The first was empirical: to explain whether and how violent crime affects government debt. To this end, I developed a supply-side argument how violent crime might affect governmental debt loads. Specifically, I argued that high violent crime rates should lower financial institutions' confidence in the capacity of governments to repay loans, raising lending costs and/or reducing the supply of credit, thereby affecting government debt. The second aim was methodological: to explain how analysis of subnational debt can be used to distinguish the supply-side logic from competing ones. Examination of the joint effect of violent crime on both public and private lending under a simple supply-side, simple demand-side, more complex supply-and-demand-side, and null logics can be used to determine the prevailing logic driving the impact of violent crime on debt. The presence of a positive effect of violent crime on public debt combined with its negative or null effect on private debt would indicate a prevailing supply-side logic. The presence of a positive effect of violent crime on public debt and its positive effect on private debt would indicate a prevailing demand-side logic. The presence of no effect of violent crime on public and private debt would indicate no causal link between them.

To make these points, I examined municipal homicide rates and debt loads in Mexico. Cross-sectional time-series analysis reveals a positive relationship between homicide rates and public debt but an insignificant one between homicide rates and private credit. Because it is unlikely that the supply-side logic linking violent crime to public sector debt would be wholly absent from or contrary to that driving private sector

lending in the same subnational capital market, I conclude that the positive effect of homicide rates on public bank loans is the result of a supply-side dynamic driving this relationship. I also conclude that this same supply-side logic drives private sector lending, but that the lack of effect of homicide rates on private debt loads reveals that municipalities compensate private lenders for the additional risk they assume in lending.

Beyond these original aims, the study shows the importance of accounting for subnational debt dynamics when analyzing the impact of violent crime (or armed conflict) on aggregate national debt. In many federal and decentralized unitary nations, subnational governments enjoy considerable policy authority in areas that matter for violent crime or for voters' and investors' reactions to it. However, subnational fiscal and debt accounts are often determined and reported separately from national ones, while national fiscal and debt accounts are determined and reported exclusive of subnational ones. As such, analysis of the impact of violent crime that focuses solely on the national level – regardless of whether examining credit ratings, risk premiums, or debt – risks inaccurate estimation of its aggregate effects when subnational effects are excluded.

Table 1: Competing Mechanisms Linking Violent Crime to Public/Private Debt

	SIMPLE SUPPLY-SIDE LOGIC		***BOTH SUPPLY-SIDE & DEMAND-SIDE LOGICS***		***SIMPLE DEMAND-SIDE LOGIC***	***NULL HYP.***
	Govt Rejects Private Debt Costs	*Govt Accepts Private Debt Costs*	*Govt Rejects Private Debt Costs*	*Govt Accepts Private Debt Costs*	*No Rising Private Debt Costs*	*No Rising Private Debt Costs*
Private Debt	**–**	**NE**	**–**	**+**	**+**	**NE**
Public Debt	**+**	**+**	**+**	**+**	**+**	**NE**
	PREVAILING SUPPLY-SIDE MECHANISM			***PREVAILING DEMAND-SIDE MECHANISM***		***NE***

Note: NE = No Effect.

Table 2: The Impact of Homicide Rates on Municipal Debt, 2005-2014

	All Municipalities (Full Sample)			*Excluding Municipalities With Zero Homicides*			*Excluding Municipalities with Zero Debt*		
	PUBLIC	***PRIVATE***	***PRIVATE***	***PUBLIC***	***PRIVATE***	***PRIVATE***	***PUBLIC***	***PRIVATE***	***PRIVATE***
	Develop. Bank Debt	**Commercial Bank Debt**	**Bond Debt**	**Develop. Bank Debt**	**Commercial Bank Debt**	**Bond Debt**	**Develop. Bank Debt**	**Commercial Bank Debt**	**Bond Debt**
Homicide Rate	0.0377***	-0.00626	-0.00135	0.0358***	-0.00583	-0.00148	0.0519**	-0.0138	-0.00747
	(0.0109)	(0.0128)	(0.00289)	(0.0113)	(0.0129)	(0.00298)	(0.0221)	(0.0281)	(0.00628)
Lag Dev. Bank Debt	0.728***	-0.000825	0.0162***	0.721***	-0.000858	0.0162***	0.657***	-0.0103	0.0128***
	(0.00882)	(0.0128)	(0.00396)	(0.00901)	(0.0134)	(0.00410)	(0.00975)	(0.0154)	(0.00420)
Lag Com. Bank Debt	-0.00961	0.172	-0.00142	-0.00943	0.170	-0.00150	-0.0221***	0.224	-0.00231
	(0.00647)	(0.206)	(0.00407)	(0.00654)	(0.206)	(0.00406)	(0.00827)	(0.202)	(0.00406)
Lag Bond Debt	0.0460**	0.00421	0.687***	0.0413*	0.00304	0.675***	0.0514**	-0.00323	0.672***
	(0.0233)	(0.0240)	(0.0398)	(0.0248)	(0.0255)	(0.0424)	(0.0258)	(0.0237)	(0.0419)
Lag Trust Debt	-0.0198	0.198**	-0.00998	-0.0199	0.197**	-0.0105	-0.0888	0.160*	-0.0124
	(0.0706)	(0.0864)	(0.0109)	(0.0716)	(0.0870)	(0.0109)	(0.0774)	(0.0867)	(0.0101)
Lag Other Debt	-0.105	0.0780	-0.000412	-0.106	0.0770	0.0000449	-0.153*	0.0655	-0.000370
	(0.0932)	(0.0575)	(0.0619)	(0.0935)	(0.0576)	(0.0618)	(0.0930)	(0.0567)	(0.0618)
PAN Municipality	-0.484***	-0.617***	0.0992**	-0.475***	-0.658***	0.107**	-0.640***	-0.919***	0.145**
	(0.123)	(0.132)	(0.0476)	(0.131)	(0.141)	(0.0506)	(0.200)	(0.196)	(0.0736)
PRD Municipality	-0.296***	-0.298***	0.101***	-0.276**	-0.313***	0.0879***	-0.397**	-0.453***	0.172***
	(0.114)	(0.0862)	(0.0339)	(0.122)	(0.0920)	(0.0336)	(0.176)	(0.129)	(0.0505)
PAN-PRD Muni.	-0.0389	0.0316	-0.178***	-0.0685	0.0258	-0.194***	-0.310	0.0926	-0.305***
	(0.122)	(0.0800)	(0.0317)	(0.142)	(0.0888)	(0.0349)	(0.266)	(0.136)	(0.0574)
Other Municipality	0.191	-0.186*	-0.0323	0.175	-0.197*	-0.0312	0.171	-0.348**	-0.0320
	(0.153)	(0.108)	(0.0383)	(0.160)	(0.115)	(0.0386)	(0.224)	(0.158)	(0.0542)
Muni-State-Nat Align.	0.540***	-0.0859	0.0544	0.529***	-0.0794	0.0605	0.757***	-0.148	0.0860
	(0.136)	(0.180)	(0.0387)	(0.143)	(0.188)	(0.0405)	(0.206)	(0.255)	(0.0582)

Muni-Nat Align.	0.465***	0.318***	-0.00151	0.465***	0.346**	0.00536	0.600***	0.568***	0.0166
	(0.126)	(0.123)	(0.0437)	(0.137)	(0.138)	(0.0474)	(0.218)	(0.212)	(0.0713)
Margin of Victory	-0.320	0.348	-0.0449	-0.341	0.341	-0.0611	-0.0776	0.417	-0.214
	(0.236)	(0.271)	(0.0672)	(0.270)	(0.306)	(0.0776)	(0.479)	(0.455)	(0.139)
Muni. Election Year	-1.988***	-0.0792	0.105***	-2.073***	-0.0789	0.113***	-3.123***	-0.1000	0.177***
	(0.0864)	(0.0622)	(0.0168)	(0.0897)	(0.0648)	(0.0182)	(0.128)	(0.0992)	(0.0277)
Total Fiscal Assets	-0.00324	0.00757	0.00147	-0.00305	0.00766	0.00173	0.0155**	0.0132	0.00516*
	(0.00280)	(0.00781)	(0.00116)	(0.00326)	(0.00962)	(0.00134)	(0.00691)	(0.0247)	(0.00276)
Vertical Fiscal Imbal.	-1.351***	-0.296	-0.0333	-1.449***	-0.379	-0.0167	-1.289**	-0.801	0.0895
	(0.316)	(0.307)	(0.0926)	(0.347)	(0.354)	(0.101)	(0.580)	(0.788)	(0.170)
Human Develop.	0.0215	-0.0402	-0.0000691	0.0279	-0.0335	-0.00399	-0.00152	-0.0602	-0.0124
	(0.0486)	(0.0451)	(0.0209)	(0.0538)	(0.0532)	(0.0228)	(0.0888)	(0.110)	(0.0356)
Population	0.00122***	0.00989***	-0.000109	0.00122***	0.00997***	-0.0000983	0.000690	0.00941***	-0.0000272
	(0.000331)	(0.00249)	(0.000167)	(0.000342)	(0.00251)	(0.000171)	(0.000429)	(0.00248)	(0.000165)
Constant	2.937***	-1.565**	-0.220	3.102***	-1.529*	-0.266	2.715***	-1.432	-0.678*
	(0.589)	(0.763)	(0.247)	(0.622)	(0.838)	(0.258)	(0.881)	(1.770)	(0.348)
Observations	18705	18705	18705	17064	17064	17064	11419	11419	11419
Groups	2437	2437	2437	2220	2220	2220	1456	1456	1456
Min. Obs.	1	1	1	1	1	1	1	1	1
Avg. Obs.	7.675	7.675	7.675	7.686	7.686	7.686	7.843	7.843	7.843
Max. Obs.	9	9	9	9	9	9	9	9	9
R-Squared	0.633	0.0857	0.677	0.622	0.0843	0.667	0.535	0.116	0.672
Chi-Squared	20090.8***	904.1***	8837.3***	18462.3***	893.6***	8528.0***	10737.6***	1073.9***	9362.3***
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Linear Prais-Winston cross-sectional time-series analysis with panel-corrected standard errors (in parentheses). Groups in full sample models do not total 2,443 due to missing data. * p<0.10, ** p<0.05, *** p<0.01.

Table 3: The Impact of Homicide Rate Change on Municipal Debt, 2005-2014

	All Municipalities (Full Sample)			*Excluding Municipalities With Zero Homicides*			*Excluding Municipalities with Zero Debt*		
	PUBLIC	***PRIVATE***	***PRIVATE***	***PUBLIC***	***PRIVATE***	***PRIVATE***	***PUBLIC***	***PRIVATE***	***PRIVATE***
	Develop. Bank Debt	**Commercial Bank Debt**	**Bond Debt**	**Develop. Bank Debt**	**Commercial Bank Debt**	**Bond Debt**	**Develop. Bank Debt**	**Commercial Bank Debt**	**Bond Debt**
Homicide Rate Chg.	0.00703	-0.00585	0.00101	0.00670	-0.00599	0.000886	0.0170	-0.0140	-0.000781
	(0.00995)	(0.00710)	(0.00194)	(0.00991)	(0.00705)	(0.00194)	(0.0211)	(0.0172)	(0.00430)
Lag Dev. Bank Debt	0.727***	-0.00133	0.0114***	0.721***	-0.00115	0.0113***	0.658***	-0.0116	0.00853**
	(0.00924)	(0.0144)	(0.00361)	(0.00945)	(0.0151)	(0.00374)	(0.0102)	(0.0173)	(0.00386)
Lag Com. Bank Debt	-0.00935	0.180	-0.00400*	-0.00903	0.172	-0.00407*	-0.0211**	0.225	-0.00467**
	(0.00658)	(0.221)	(0.00217)	(0.00664)	(0.221)	(0.00216)	(0.00842)	(0.217)	(0.00222)
Lag Bond Debt	0.0542**	0.0211	0.704***	0.0493*	0.0213	0.695***	0.0498*	0.0190	0.690***
	(0.0240)	(0.0245)	(0.0325)	(0.0255)	(0.0262)	(0.0343)	(0.0268)	(0.0245)	(0.0339)
Lag Trust Debt	-0.0165	0.191**	-0.00771	-0.0179	0.190**	-0.00821	-0.0915	0.141	-0.0114
	(0.0740)	(0.0880)	(0.0108)	(0.0751)	(0.0886)	(0.0108)	(0.0816)	(0.0892)	(0.00979)
Lag Other Debt	-0.108	0.0791	-0.00245	-0.109	0.0780	-0.00206	-0.158	0.0643	-0.00260
	(0.0981)	(0.0603)	(0.0627)	(0.0984)	(0.0605)	(0.0627)	(0.0977)	(0.0595)	(0.0629)
PAN Municipality	-0.499***	-0.642***	0.108**	-0.493***	-0.688***	0.114**	-0.679***	-0.977***	0.151**
	(0.127)	(0.148)	(0.0450)	(0.136)	(0.157)	(0.0477)	(0.207)	(0.215)	(0.0697)
PRD Municipality	-0.350***	-0.331***	0.162***	-0.336**	-0.357***	0.150***	-0.490**	-0.537***	0.267***
	(0.125)	(0.105)	(0.0357)	(0.134)	(0.112)	(0.0349)	(0.196)	(0.159)	(0.0539)
PAN-PRD Muni.	0.0258	0.0379	-0.147***	-0.000764	0.0340	-0.160***	-0.112	0.126	-0.265***
	(0.128)	(0.0945)	(0.0225)	(0.148)	(0.104)	(0.0244)	(0.287)	(0.158)	(0.0397)
Other Municipality	0.197	-0.222*	0.00165	0.177	-0.238*	0.00702	0.159	-0.415**	0.0228
	(0.163)	(0.125)	(0.0359)	(0.171)	(0.133)	(0.0354)	(0.239)	(0.184)	(0.0507)
Muni-State-Nat Align.	0.551***	-0.110	0.0593	0.536***	-0.108	0.0668*	0.751***	-0.201	0.101*
	(0.141)	(0.193)	(0.0373)	(0.147)	(0.202)	(0.0388)	(0.214)	(0.275)	(0.0555)

Muni-Nat Align.	0.395***	0.281**	0.0759**	0.383***	0.310**	0.0871**	0.486**	0.528**	0.142**
	(0.130)	(0.137)	(0.0386)	(0.142)	(0.153)	(0.0418)	(0.226)	(0.235)	(0.0630)
Margin of Victory	-0.198	0.474	-0.0426	-0.211	0.489	-0.0592	0.117	0.661	-0.232*
	(0.254)	(0.316)	(0.0665)	(0.290)	(0.356)	(0.0770)	(0.513)	(0.530)	(0.141)
Muni. Election Year	-2.067***	0.0407	0.0775***	-2.165***	0.0444	0.0841***	-3.311***	0.0889	0.133***
	(0.0935)	(0.0799)	(0.0154)	(0.0973)	(0.0830)	(0.0165)	(0.140)	(0.128)	(0.0256)
Total Fiscal Assets	-0.00353	0.00726	0.00139	-0.00335	0.00707	0.00154	0.0165**	0.0126	0.00461*
	(0.00300)	(0.00866)	(0.00116)	(0.00348)	(0.0107)	(0.00133)	(0.00742)	(0.0276)	(0.00268)
Vertical Fiscal Imbal.	-1.461***	-0.400	0.0135	-1.568***	-0.473	0.0481	-1.411**	-0.905	0.233
	(0.337)	(0.286)	(0.0895)	(0.372)	(0.325)	(0.0976)	(0.627)	(0.760)	(0.166)
Human Develop.	0.0356	-0.0517	-0.0206	0.0425	-0.0447	-0.0269	0.00830	-0.0809	-0.0494*
	(0.0524)	(0.0483)	(0.0147)	(0.0581)	(0.0579)	(0.0164)	(0.0965)	(0.121)	(0.0274)
Population	0.00130***	0.0103***	-0.000174	0.00127***	0.0105***	-0.000160	0.000707	0.00976***	-0.000102
	(0.000359)	(0.00278)	(0.000161)	(0.000370)	(0.00281)	(0.000165)	(0.000463)	(0.00277)	(0.000154)
Constant	3.360***	-1.566*	-0.556***	3.482***	-1.532*	-0.619***	3.152***	-1.418	-1.167***
	(0.636)	(0.839)	(0.196)	(0.671)	(0.915)	(0.210)	(0.953)	(1.900)	(0.320)
Observations	16609	16609	16609	15131	15131	15131	10087	10087	10087
Groups	2418	2418	2418	2201	2201	2201	1444	1444	1444
Min. Obs.	1	1	1	1	1	1	1	1	1
Avg. Obs.	6.869	6.869	6.869	6.875	6.875	6.875	6.985	6.985	6.985
Max. Obs.	8	8	8	8	8	8	8	8	8
R-Squared	0.638	0.0936	0.801	0.628	0.0894	0.798	0.543	0.122	0.798
Chi-Squared	19513.3***	924.8***	10607.8***	18047.3***	898.9***	10340.0***	10651.2***	1078.1***	11306.0***
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Linear Prais-Winston cross-sectional time-series analysis with panel-corrected standard errors (in parentheses). Groups in full sample models do not total 2,443 due to missing data. * p<0.10, ** p<0.05, *** p<0.01.

Table 4: The Impact of Homicide Rates and Homicide Rate Changes on Municipal Debt, 2005-2014

	All Municipalities (Full Sample)			*Excluding Municipalities With Zero Homicides*			*Excluding Municipalities with Zero Debt*		
	PUBLIC	***PRIVATE***	***PRIVATE***	***PUBLIC***	***PRIVATE***	***PRIVATE***	***PUBLIC***	***PRIVATE***	***PRIVATE***
	Develop. Bank Debt	**Commercial Bank Debt**	**Bond Debt**	**Develop. Bank Debt**	**Commercial Bank Debt**	**Bond Debt**	**Develop. Bank Debt**	**Commercial Bank Debt**	**Bond Debt**
Homicide Rate	0.0477***	0.000620	0.00253	0.0462***	0.00300	0.00302	0.0534*	0.00253	0.00193
	(0.0143)	(0.0182)	(0.00349)	(0.0151)	(0.0190)	(0.00362)	(0.0284)	(0.0364)	(0.00735)
Homicide Rate Chg.	-0.0177	-0.00617	-0.000275	-0.0170	-0.00752	-0.000650	-0.0103	-0.0153	-0.00176
	(0.0124)	(0.00619)	(0.00262)	(0.0126)	(0.00661)	(0.00268)	(0.0256)	(0.0148)	(0.00569)
Lag Dev. Bank Debt	0.724***	-0.00134	0.0114***	0.719***	-0.00120	0.0113***	0.657***	-0.0117	0.00850**
	(0.00926)	(0.0144)	(0.00361)	(0.00946)	(0.0151)	(0.00374)	(0.0103)	(0.0173)	(0.00385)
Lag Com. Bank Debt	-0.00907	0.180	-0.00399*	-0.00875	0.172	-0.00406*	-0.0208**	0.226	-0.00467**
	(0.00659)	(0.221)	(0.00217)	(0.00665)	(0.221)	(0.00216)	(0.00840)	(0.217)	(0.00222)
Lag Bond Debt	0.0553**	0.0211	0.704***	0.0503**	0.0214	0.695***	0.0512*	0.0190	0.690***
	(0.0241)	(0.0245)	(0.0325)	(0.0256)	(0.0262)	(0.0344)	(0.0268)	(0.0245)	(0.0339)
Lag Trust Debt	-0.0158	0.191**	-0.00771	-0.0169	0.190**	-0.00819	-0.0916	0.140	-0.0114
	(0.0743)	(0.0880)	(0.0108)	(0.0753)	(0.0886)	(0.0108)	(0.0816)	(0.0892)	(0.00980)
Lag Other Debt	-0.109	0.0791	-0.00249	-0.110	0.0780	-0.00209	-0.159	0.0643	-0.00263
	(0.0980)	(0.0603)	(0.0627)	(0.0983)	(0.0605)	(0.0627)	(0.0975)	(0.0595)	(0.0629)
PAN Municipality	-0.503***	-0.642***	0.108**	-0.495***	-0.688***	0.114**	-0.679***	-0.977***	0.151**
	(0.127)	(0.148)	(0.0450)	(0.136)	(0.157)	(0.0477)	(0.206)	(0.215)	(0.0697)
PRD Municipality	-0.353***	-0.331***	0.163***	-0.340**	-0.357***	0.150***	-0.487**	-0.537***	0.267***
	(0.125)	(0.105)	(0.0358)	(0.134)	(0.112)	(0.0350)	(0.196)	(0.159)	(0.0539)
PAN-PRD Muni.	0.0274	0.0378	-0.147***	0.000389	0.0340	-0.160***	-0.125	0.125	-0.265***
	(0.129)	(0.0946)	(0.0225)	(0.149)	(0.104)	(0.0244)	(0.287)	(0.157)	(0.0399)
Other Municipality	0.194	-0.222*	0.00158	0.176	-0.238*	0.00704	0.160	-0.415**	0.0228
	(0.164)	(0.125)	(0.0359)	(0.171)	(0.133)	(0.0355)	(0.239)	(0.184)	(0.0507)
Muni-State-Nat Align.	0.543***	-0.110	0.0590	0.528***	-0.109	0.0664*	0.751***	-0.201	0.101*
	(0.141)	(0.192)	(0.0374)	(0.147)	(0.202)	(0.0389)	(0.213)	(0.275)	(0.0555)

Muni-Nat Align.	0.400***	0.282**	0.0760**	0.387***	0.310**	0.0872**	0.490**	0.528**	0.142**
	(0.130)	(0.137)	(0.0385)	(0.142)	(0.153)	(0.0417)	(0.225)	(0.235)	(0.0629)
Margin of Victory	-0.206	0.474	-0.0429	-0.233	0.489	-0.0604	0.110	0.660	-0.232*
	(0.254)	(0.316)	(0.0665)	(0.290)	(0.356)	(0.0770)	(0.512)	(0.530)	(0.141)
Muni. Election Year	-2.068***	0.0407	0.0776***	-2.166***	0.0444	0.0841***	-3.309***	0.0890	0.133***
	(0.0934)	(0.0799)	(0.0154)	(0.0972)	(0.0830)	(0.0165)	(0.140)	(0.128)	(0.0256)
Total Fiscal Assets	-0.00290	0.00727	0.00142	-0.00318	0.00709	0.00155	0.0166**	0.0127	0.00462*
	(0.00302)	(0.00867)	(0.00117)	(0.00349)	(0.0107)	(0.00133)	(0.00742)	(0.0276)	(0.00268)
Vertical Fiscal Imbal.	-1.439***	-0.401	0.0143	-1.560***	-0.473	0.0484	-1.400**	-0.906	0.233
	(0.338)	(0.285)	(0.0898)	(0.372)	(0.325)	(0.0977)	(0.627)	(0.759)	(0.166)
Human Develop.	0.0192	-0.0520	-0.0214	0.0302	-0.0456	-0.0277*	-0.00365	-0.0816	-0.0499*
	(0.0526)	(0.0486)	(0.0149)	(0.0581)	(0.0579)	(0.0166)	(0.0964)	(0.120)	(0.0277)
Population	0.00122***	0.0103***	-0.000179	0.00120***	0.0105***	-0.000166	0.000633	0.00975***	-0.000104
	(0.000360)	(0.00278)	(0.000159)	(0.000371)	(0.00281)	(0.000163)	(0.000463)	(0.00277)	(0.000150)
Constant	3.287***	-1.566*	-0.559***	3.446***	-1.534*	-0.621***	3.105***	-1.420	-1.168***
	(0.638)	(0.837)	(0.197)	(0.672)	(0.914)	(0.211)	(0.953)	(1.893)	(0.322)
Observations	16609	16609	16609	15131	15131	15131	10087	10087	10087
Groups	2418	2418	2418	2201	2201	2201	1444	1444	1444
Min. Obs.	1	1	1	1	1	1	1	1	1
Avg. Obs.	6.869	6.869	6.869	6.875	6.875	6.875	6.985	6.985	6.985
Max. Obs.	8	8	8	8	8	8	8	8	8
R-Squared	0.637	0.0938	0.801	0.628	0.0893	0.798	0.543	0.122	0.798
Chi-Squared	19425.9***	931.5***	10636.5***	18006.7***	904.3***	10350.6***	10667.6***	1084.7***	11357.4***
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Linear Prais-Winston cross-sectional time-series analysis with panel-corrected standard errors (in parentheses). Groups in full sample models do not total 2,443 due to missing data. * p<0.10, ** p<0.05, *** p<0.01.

Table 5: Long Run Effect of Homicide Rates on Debt (per capita, sqrt)

	Coef.	Std. Err.	P-Value	CI-Lower Bound	CI Upper Bound
Development Bank Debt					
Homicide Rate (sqrt)	0.1731946	0.0519946	0.001	0.071	0.275
Homicide Shock (sqrt)	-0.0640946	0.0449023	0.153	-0.152	0.024
Commercial Bank Debt					
Homicide Rate (sqrt)	0.0007564	0.0221827	0.973	-0.043	0.044
Homicide Shock (sqrt)	-0.0075184	0.0080363	0.350	-0.023	0.008
Bond Debt					
Homicide Rate (sqrt)	0.0085342	0.0119209	0.474	-0.015	0.032
Homicide Shock (sqrt)	-0.0009303	0.0088705	0.916	-0.018	0.016

Note: Based on Table 3's Full Sample Models

Figure 1: Long-Term Effect of Homicide Rates on Development Bank Debt

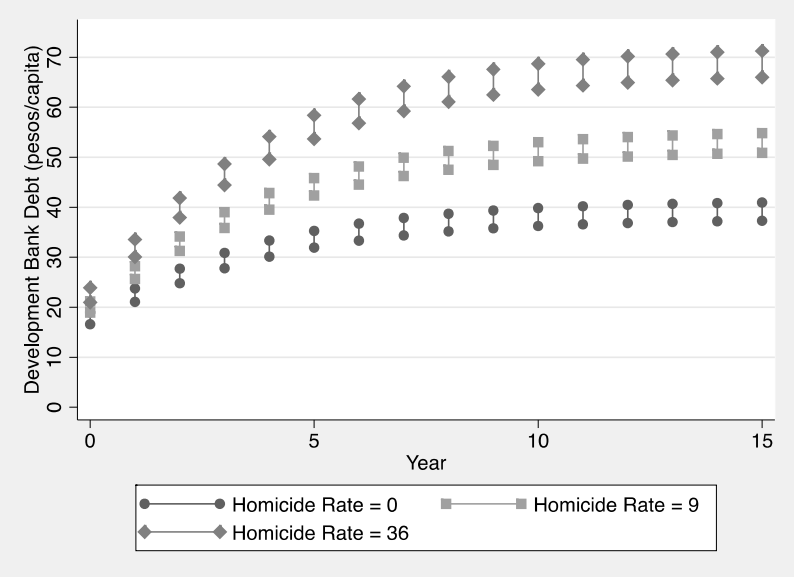
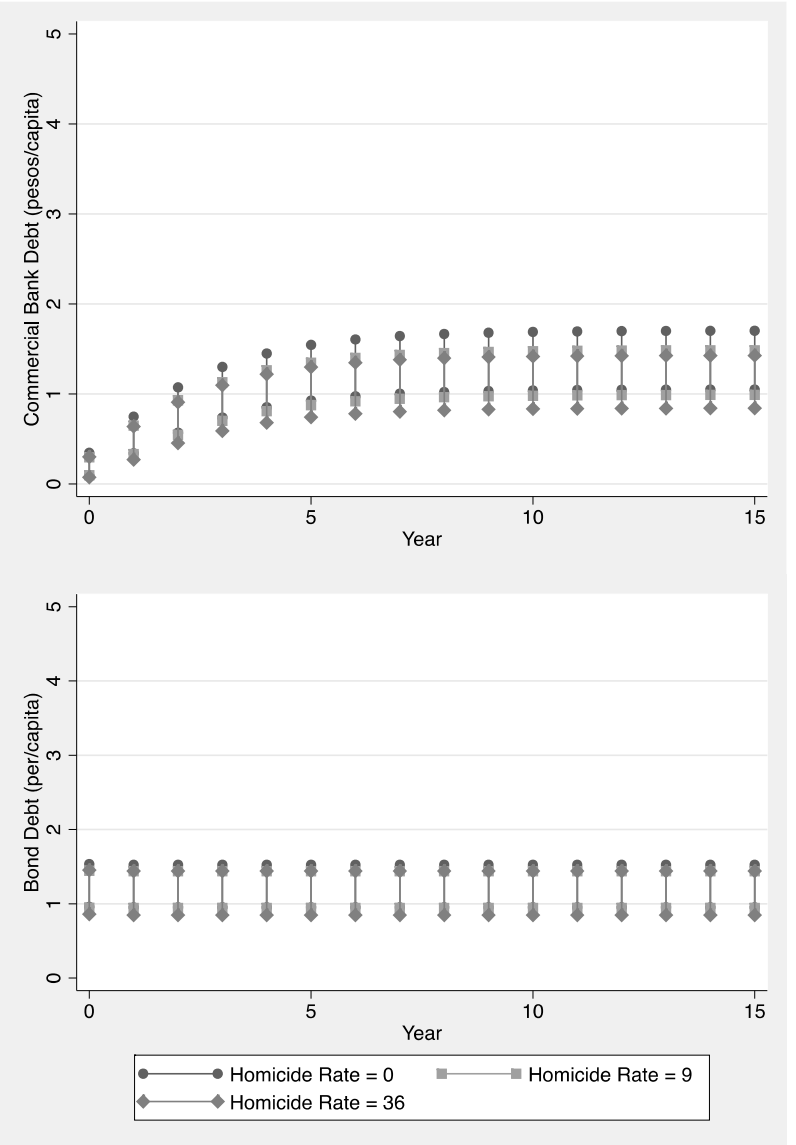


Figure 2: The Lack of Long-Term Effect of Homicide Rates on Private Sector Commercial Bank and Bond Debt



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